

How Does Emissions-Charging Influence House Prices? Evidence From London's ULEZ

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Abstract

In 2019, the Ultra Low Emissions Zone (ULEZ) was implemented in London: a daily charge on all environmentally non-compliant cars driven in a specified region. This zone was expanded in 2021 and again in 2023. I use data from HM Land Registry to find that the ULEZ led house prices in these expansion zones to fall on average by roughly 3-4%, when using a local control region. This corresponds to a reduction of around £20,000 in the sale price of the average house in the region, or close to half of London's median annual salary. The significance of this result is robust to considerable weakening of the parallel trends assumption. The result is also robust to different methods of dealing with spatial spillovers, and I find that the ULEZ also leads prices of houses just outside the region to fall by roughly 2%. I also use a simple adaptation of the k -means clustering algorithm, repurposed for matching, to construct an alternative control region formed of areas in other English cities, and find results broadly consistent with those of the local comparison when using this instead. This coherence suggests the effect is not driven by suburbanisation, ruling out the COVID pandemic as an alternative explanation. Comparison of Londoners' transportation preferences to other European cities suggest my results are externally valid.

JEL Classification: C23, Q53, Q58, R21, R31

Keywords: emissions charges, house prices, difference-in-differences, environmental policy, spatial spillovers

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1 Introduction

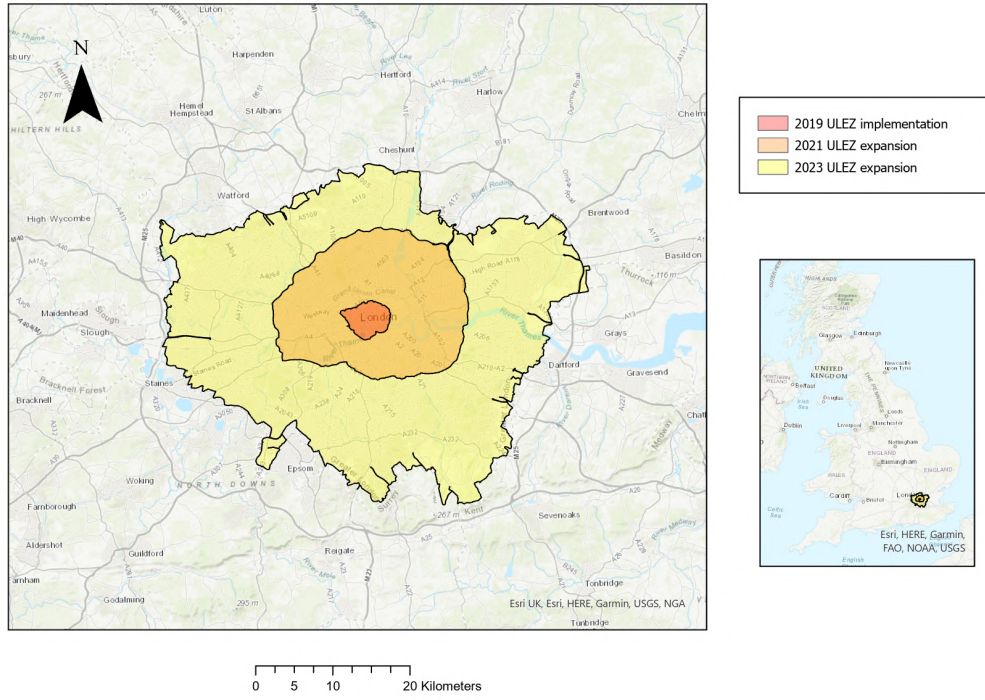
In 2018, the European city suffering the worst costs from air pollution was London – they were 79.5% higher than second-placed Bucharest (CE Delft, 2020). To address this, the Mayor of London established the Ultra Low Emissions Zone (ULEZ), within which drivers of non-compliant vehicles are charged £12.50 per day. The ULEZ is one of the largest and strictest emissions-charging zones worldwide, making a comprehensive understanding of its overall impact crucial. Importantly, this must extend further than its intended effects on air pollution, to include both positive (e.g. reduced traffic and noise) and negative (e.g. increased transport costs, firm relocation) unintended effects.

The best way to ascertain the overall effect of the policy on affected residents is to examine its effect on the attractiveness of living in the regions subject to the policy, proxied for by their house prices. This connection is grounded in the hedonic pricing model, where house prices are modelled as the sum of the value of various internal and external components, including air quality, congestion, and transportation costs. Changes in these individual valuations change house prices accordingly: these aggregate every possible dimension of the ULEZ effect together. This paper therefore examines the effect of the ULEZ on house prices in London.

By creating a novel dataset through combining house sale data from HM Land Registry with geographic information system (GIS) software, and using the local surrounding area as a control, I find that the ULEZ led house prices to fall by roughly 3-4%. This result suggests the loss of freedoms due to travel restrictions far outweigh any gains from, for example, improved air quality and health. The effect follows a U-shaped profile over time – relative prices fall sharply, plateau out at around 4%, then begin to recover, which is rationalisable as a product of asymmetric information transmission. I also find that areas just outside the ULEZ experienced spillover effects of around a 2% drop in prices.

To confirm the interpretation of the observed effects as stemming from the ULEZ, I run a similar analysis with a new, long-distance control region formed of areas within other towns and cities across England. To do this, I implement a novel matching algorithm, which is a simple adaptation of the standard k -means clustering algorithm. The resulting difference-in-difference analysis lends support to the conclusions from the local analysis, particularly in the case of the 2021 expansion. This helps invalidate explanations of the observed effect that center around a sudden spike in urban-rural movement around the London area, such as the COVID pandemic.

Figure 1: The geography of the ULEZ, as it was implemented and then expanded



The paper proceeds as follows. Section 2 explains the charge and its expansion process, and section 3 reviews the literature on the broad spectrum of effects of emissions charges in London and elsewhere. Section 4 outlines the data collection and processing, followed by a methodological outline in section 5. Section 6 presents my results, discussed in section 7, and section 8 concludes.

2 Background

The ULEZ, first announced in March 2015, was implemented in April 2019 in a 20km² region in Central London, with the explicit aim of improving air quality (Transport for London, 2025). The policy imposes a daily £12.50 charge on all vehicles driven in the zone that do not adhere to certain environmental standards. The ULEZ expanded in October 2021 to the North and South circular roads, as announced in June 2018. It then expanded again in August 2023, as announced in November 2022, to cover almost all 1,572km² of Greater London, where it remains today. I illustrate these expansion zones in Figure 1.

The original 2019 ULEZ differed in several ways to the zones it later expanded into. Firstly, until the 2021 expansion, residents of the 2019 zone were exempt from the charge. Further, the 2019 zone had already been subject to some emissions-related driving restrictions – the Toxicity Charge (operating 2017-2019) and the Congestion Charge (operating 2003-present). I therefore disregard the 2019 zone from my analysis, as residents had a fundamentally different experience of the ULEZ to those in the later expansion zones. Aside from a separate Low Emissions Zone aimed at commercial vehicles, the 2021 and 2023 expansion zones had never experienced emissions-charging, and so their experience resembles the conventional introduction of a ‘traditional’ (in the sense that residents are charged) emissions charge.

Since 2019, the charge has remained fixed at £12.50, as have both the targeted vehicles and the environmental standards. As such, ignoring spillover effects, the ‘treatment’ of London’s houses to the ULEZ has been homogeneous, although staggered, which I leverage in my analysis.

3 Literature Review

This paper contributes to the emerging intersection between environmental and urban economics, by building on the literature surrounding the effects of emissions-charging policies. In particular, the direct effects of such charges are well-evaluated. The gradual implementation of broadly similar LEZs in German cities is naturally conducive to event-study designs, which have identified their positive effect on air quality (Wolff, 2014) and health outcomes (Pestel and Wozny, 2021; Margaryan, 2021). Similar findings have emerged from LEZ-type policies in other European cities (Simeonova et al, 2019, Panteliadis et al, 2014, Bernardo et al, 2021). In London, the effects of the Congestion Charge (LCC) and the Low Emission Zone (LEZ) have been more ambiguous, though potentially stemming from simply their limited scope. Zhai and Wolff (2021) and Ellison et al (2013) fail to find much evidence supporting the effect the LEZ had on air quality, and Green et al (2020) share this conclusion for the LCC. Ma et al (2021) extend this ambiguity to the 2019 implementation of the ULEZ, but London City Hall (2024) suggest that the expansions were considerably more successful. In general, large emissions charges achieve their main objective.

There is less consensus on their indirect effects. An important one is road congestion, and Moral-Carcedo (2024) finds that Madrid’s LEZ led congestion to fall inside the zone, but rise outside it. Studies in Germany find no effect on traffic volumes inside or outside the zone (Pestel and Wozny, 2021; Wolff, 2014). However, Herzog (2024) constructs an ‘exposure index’

to London’s LCC, and uses it in a difference-in-differences framework to find it reduced traffic both in the charge zone and on suburban roads heading in the zone’s direction. He then builds this into a structural model which suggests positive distributional effects of the LCC, since the areas benefitting most from the traffic reductions house predominantly low-skilled jobs. This feature of London’s structure may imply similarly positive secondary effects for the ULEZ.

House prices are often understood through hedonic pricing frameworks, established in Rosen (1974), within which they are determined through the sum of the values of distinct internal and external components, including air quality, congestion and local transportation costs. However, while research has established the connection between these individual components and house prices (particularly air quality – e.g. Chay and Greenstone, 2005), very few studies have convincingly collected them together to assess the overall impact of emissions-charging. Jabbar et al (2024) finds a positive effect of the 2019 introduction of the ULEZ on house prices, but uses a sharp RDD with the cutoff at the zone’s border, therefore ignoring spatial spillovers. The zone’s peculiarities mean this effect is also not reflective of more conventional emissions-charges. In Milan, Percoco (2014) finds the Ecopass emissions charge reduced house prices in the region by roughly 1.5%, but fails to account for spillovers, anticipation, and the dynamics of the result. Gruhl et al (2022) use a stacked difference-in-difference design to find a positive effect of Germany’s many LEZs on apartment rents, as well as other housing types, but again their results change when adjusting for spatial spillovers. An alternative evaluation of Germany’s LEZs on overall welfare comes from Sarmiento et al (2023), who conversely find that they led residents’ subjective wellbeing to fall. Convincing literature on the overall effects of an emissions charge is sparse - an important and very policy-relevant gap this paper aims to fill.

This paper contributes to the discourse on the effects of emissions-charges, by using a novel dataset to provide a holistic overview of one of the largest and strictest - London’s ULEZ. Given the increasing policy-relevance of emissions-charging, this analysis matters both because of the scale of the ULEZ, and because existing literature generally ignores unintended secondary effects. This paper also makes a methodological contribution, applying recent theoretical research to handle both spatial spillovers and parallel trends more comprehensively, as well as proposing a novel matching algorithm that addresses the challenges associated with matching in this setting.

4 Data

In my analysis, I use the Price Paid Data from HM Land Registry, from which I take the sale price, address and characteristics of all houses sold in England and Wales from January 2015 to December 2024. I then use ArcGIS to relate these sales to the ULEZ expansion, by geocoding each sale address and calculating its shortest planar Euclidean distance to each ULEZ border, then signing the distances according to whether the house is within the border in question (negative distances signify houses within the border). Full details of the data collection and processing, are in Appendix A.1.

I am left with 8.3 million geocoded observations house sales across the region in the period, whose numeric characteristics are presented in panel (a) of Table 1. However, the bulk of my analysis is ‘local’, in the sense that the control region lies in the suburbs of London. For this purpose, I keep only houses within 20km of the 2023 ULEZ border, which leaves me with 1.3 million observations, whose numeric characteristics are summarised in panel (b) of Table 1. It is worth noting that the Price Paid Data provides relatively few house characteristics, only giving indicators for the house type, lease type, and whether it was newly built at the point of sale. However, while this could imperil my identifying assumptions, I harness new econometric research to illustrate the robustness of my results to violations of these assumptions. My lack of controls isn’t a big problem.

My unit of analysis is the house, but I consider treatment (i.e. status as within the ULEZ or not) to be assigned at the postcode-sector level¹. In some cases, the ULEZ boundary cuts through a postcode sector. I split any such postcode sector in two at the border, treating them as separate ‘pseudo-postcode sectors’ (which I henceforth simply call postcode sectors, for clarity). The data for the local analysis contains house sales from 1705 postcode sectors over 40 quarters, the default time period I consider.

The data needs further refinement for baseline analysis. Since I focus on the effect of the 2021 and 2023 expansions, I drop all observations within the original 2019 zone, but more steps are required to ensure important identifying assumptions hold. People living outside the zone pay the charge each time they drive into it, and any improvements in air quality, congestion and other relevant factors will all percolate out of the zone’s border, so people living in houses near an active ULEZ will experience similar effects to those within the zone – spatial spillovers are

¹UK postcodes take the form AB1 2CD. A postcode sector is the collection of houses that share the AB1 2xx component of their postcode.

Table 1: Summary statistics for the numeric variables in the data

(a) For the data across England and Wales

VARIABLES	N	mean	sd	min	max
Log(sale price)	8,263,415	12.430	0.659	0	18.42
Detached?	8,263,415	0.259	0.438	0	1
Semidetached?	8,263,415	0.286	0.452	0	1
Terraced?	8,263,415	0.278	0.448	0	1
Flat?	8,263,415	0.177	0.382	0	1
New?	8,263,415	0.114	0.317	0	1
Leasehold?	8,263,415	0.227	0.419	0	1
In 2019 ULEZ?	8,263,415	0.003	0.050	0	1
Distance from 2019 ULEZ (km)	8,263,415	166.6	108.800	-1.942	491.2
In 2021 ULEZ?	8,263,415	0.042	0.200	0	1
Distance from 2021 ULEZ (km)	8,263,415	158.0	108.700	-9.191	482.0
In 2023 ULEZ?	8,263,415	0.099	0.299	0	1
Distance from 2023 ULEZ (km)	8,263,415	145.4	108.2	-14.76	473.7

(b) For the data local to London

VARIABLES	N	mean	sd	min	max
Log(sale price)	1,344,447	13.07	0.588	0	18.42
Detached?	1,344,447	0.119	0.324	0	1
Semidetached?	1,344,447	0.189	0.392	0	1
Terraced?	1,344,447	0.269	0.443	0	1
Flat?	1,344,447	0.423	0.494	0	1
New?	1,344,447	0.114	0.317	0	1
Leasehold?	1,344,447	0.433	0.496	0	1
In 2019 ULEZ?	1,344,447	0.016	0.124	0	1
Distance from 2019 ULEZ (km)	1,344,447	17.60	12.09	-1.942	46.42
In 2021 ULEZ?	1,344,447	0.255	0.436	0	1
Distance from 2021 ULEZ (km)	1,344,447	9.415	11.65	-9.191	37.82
In 2023 ULEZ?	1,344,447	0.611	0.487	0	1
Distance from 2023 ULEZ (km)	1,344,447	-0.148	9.093	-14.76	20.00

an issue. To address this, I remove all house sales outside, but within 5km from, the 2019, 2021 and 2023 borders. These will be most susceptible to spillovers and so by removing them I remove the bulk of the relevant bias from the analysis. The idea of assuming a reasonable distance beyond which spillovers end is a common way to define treatment and control groups (e.g. Gupta et al, 2022), and I take the 5km figure from Gruhl et al (2022) who use it for the same purpose in their robustness checks. If spillovers extend past 5km from the zone, they will

5 Methodology

I aim to examine the impact that the ULEZ expansions had on house prices in affected regions. To form a prior over what I expect to find in the analysis, I use kernel density smoothing to approximate the distributions of house prices in each of the two expansion regions, before and after the relevant expansion, and compare them to the same distribution in the control zone before and after the same point in time, as plotted in Figure 3. As would be expected, inflation shifts the distributions rightwards over time. There is very little difference in the 2023 comparison, but the 2021 comparison shows a larger rightward shift in the control zone than the 2021 zone, particularly around the peak of the distribution. Similar conclusions can be drawn from comparisons of CDFs and histograms, presented in Appendix B. These figures suggest a drop in house prices associated with the implementation of ULEZ. More rigorous analysis is required isolate this fall and deal with causality.

5.1 Baseline Local Analysis

I begin by analysing the evolution of house prices in the treated regions in comparison to their evolution in the nearby control region in suburban London outlined above ('local' analysis). A naïve comparison of house prices within and outside the ULEZ suffers from selection issues: houses exposed to the ULEZ are closer to central London and other useful amenities, for example. To more convincingly isolate a causal effect, I utilise the staggered, homogeneous expansion of the ULEZ, which lends itself to a staggered difference-in-differences (DiD) design. As a baseline, I estimate the static treatment effect in the standard two-way fixed-effects (TWFE) form, estimating the following equation by OLS:

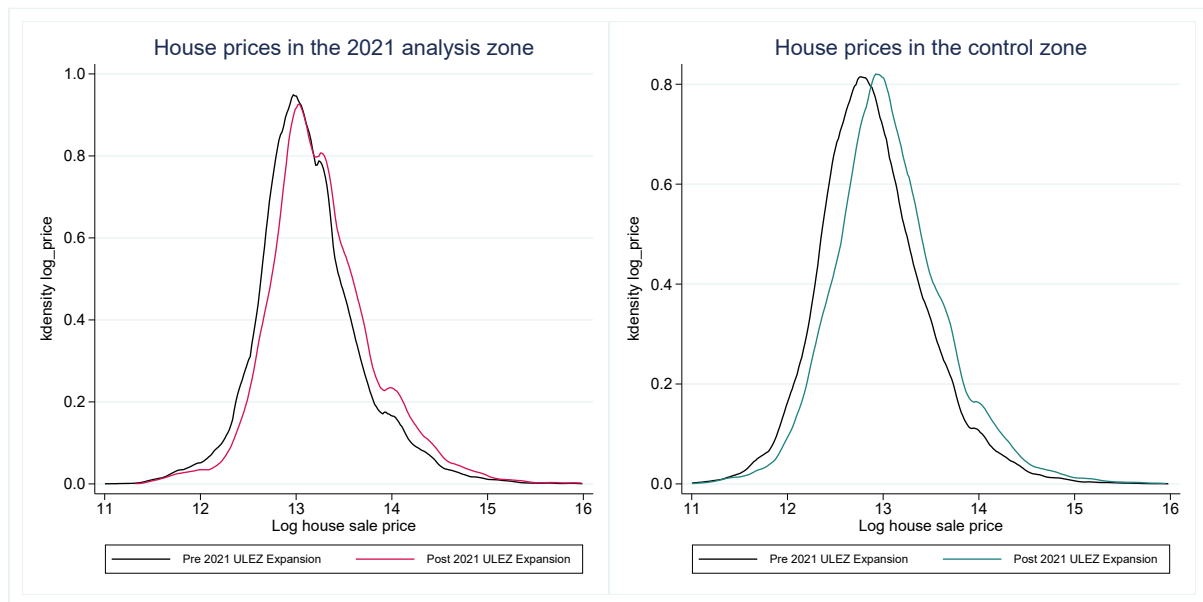
$$y_{ipt} = \alpha_p + \delta_t + \beta ULEZ_{pt} + \rho \mathbf{X}_{it} + \varepsilon_{ipt}$$

where y_{ipt} is the log of the sale price of house i in postcode sector p , sold in quarter t , with postcode-sector and time fixed-effects α_p and δ_t , a vector of covariates \mathbf{X}_{it} (shown in Table 1), and $ULEZ_{pt}$ an indicator for whether the postcode sector containing house i is within the ULEZ in quarter t . The coefficient of interest, β , represents the 'extensive margin' overall treatment effect.

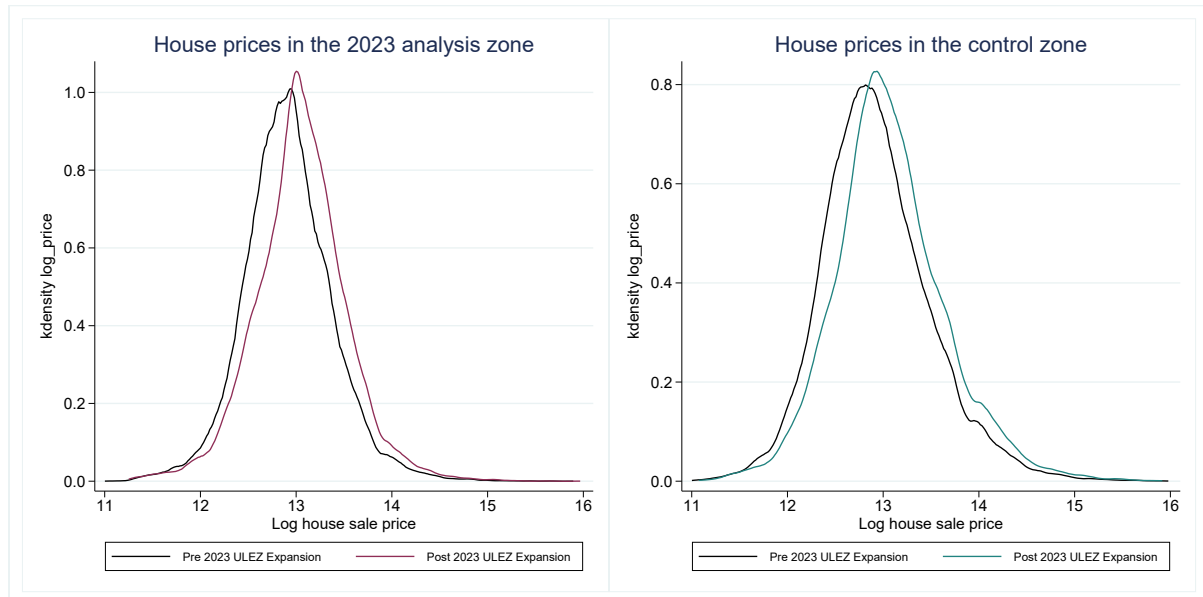
A more comprehensive approach involves estimating a dynamic version of this staggered DiD, alternatively known as an event study. I estimate the following dynamic TWFE specification via OLS:

$$y_{ipt} = \alpha_p + \delta_t + \sum_{j \neq j_{base}} \beta_j \mathbb{1}(t - G_p = j) + \rho \mathbf{X}_{it} + \varepsilon_{ipt}$$

Figure 3: (a) Distribution of log house sale price in the 2021 analysis and control zones, before and after the 2021 expansion; (b) As in (a), but replacing 2021 with 2023



(a)



(b)

where in addition to the above, G_p is the first quarter in which postcode sector p became part of the ULEZ, and j_{base} is a pre-period coefficient I normalise to zero (conventionally $j = -1$). Essentially, I replace the dummy $ULEZ_{pt}$ with a set of ‘event-time’ dummies that pick out all houses observed exactly j periods after they first came under the ULEZ. My coefficients of interest are the β_j , which isolate the average treatment effect on the treated (ATT) of being in the ULEZ on house prices j quarters after it was first implemented in the region, under a set of assumptions detailed below. This specification allows insight into the extensive-margin effect, but also information on the ‘intensive margin’ about the dynamics of the effect over time. These dynamics are important from a policy perspective, enabling a distinction between short-run and long-run effects. In all regressions, standard errors are clustered at the postcode-sector level.

Certain assumptions must be made to ensure my identification strategy isolates causality. Most importantly, I assume that without treatment, the expected log house price would have evolved in the same way in the treated and control groups in the post-treatment period, conditional on my covariates. This is the (conditional) parallel trends assumption. This assumption cannot be directly tested, but if the pre-treatment dynamic coefficients are consistently near-zero, then relative prices did not evolve differently between treatment and control groups before treatment, suggesting the same would hold in the post-treatment period. The classic approach is to formally test the joint insignificance of these ‘pre-trends’, and apply the above logic. However, recent literature advocates against this test: it is low powered, and conditioning analysis on good pre-treatment behaviour implicitly introduces bias when estimating post-treatment effects (Roth, 2022). I therefore use them as no more than suggestive evidence that parallel trends holds. New research from Rambachan and Roth (2023) shows it is possible to perform inference under a more relaxed version of the parallel trends assumption, evaluating the robustness of results to violations of parallel trends. I deal with the assumption formally in this way.

Another important assumption is that there is no pre-treatment anticipatory response. Anticipatory effects may be present here, since the expansions were announced well before their implementation. However, if parallel trends holds, this assumption can be dealt with simply by considering ‘treatment’ as starting at the point where these anticipatory effects begin.

I also assume any effect does not come from confounding events in the region. The two events that threaten this assumption most are the opening of London’s Elizabeth line, and the construction of the UK’s HS2 railway. However, the effects of these events are both very local, so postcode-sector fixed-effects will capture most price variation they may cause. Furthermore, the Elizabeth line opened in 2022, while HS2 construction has been gradually implemented

since 2020, out of time with the ULEZ expansions. The COVID pandemic may have influenced London’s relative house prices, but again we are likely to see these effects well before the expansions. This should enable distinction between the effect of the ULEZ and any COVID-related shock, but as a confirmatory check I also run this analysis with a long-distance control group that will enable further distinction (see section 5.3). Finally, to complete my set of assumptions, I assume no switching between treatment groups: houses clearly can’t move.

The baseline TWFE model above is a workhorse model in applied microeconomics, but recent research has demonstrated that OLS estimates of the ATT in this specification are inconsistent when the effect is heterogeneous between treatment groups and/or over time. The issue is that the estimates are just weighted averages of all possible simple 2x2 difference-in-differences estimates between all groups at each time period, including ‘bad’ ones which compare later treated groups to earlier ones (Goodman-Bacon, 2021). Figure 3 suggests I have heterogeneity across groups, so my estimates of the ATT will be inconsistent in this framework. I therefore also use two new estimators of the β_j in the above model, both robust to this inconsistency problem: those from Abraham and Sun (2021), and Gardner (2021). Intuitively, Abraham and Sun solve this problem by estimating each 2x2 comparison individually and ‘manually’ putting them together correctly, while Gardner divides the process into two stages, estimating group and period effects using only untreated observations to avoid bad comparisons. Both will provide more reliable estimates than the basic TWFE model.

5.2 Flexible Spillover Treatment

To deal with the issue of spillovers more flexibly, I also use the estimator from Butts (2024), which extends Gardner (2021). In this framework, I set a distance by which I assume spillovers end, and then estimate the fixed-effects only on observations beyond this distance from the ULEZ at the given time, to remove the influence of spillovers. I then don’t have to drop the observations close to inner borders outlined in section 4: the issues they pose are dealt with in the estimation. This feature makes my estimated treatment effects more precise, and more representative of the effect across London. Furthermore, in the second stage, I can estimate not only the direct treatment effect, but also the effect of exposure to spillovers, allowing me to ascertain the effect the ULEZ had on house prices nearby but outside the zone.

Another important advantage of retaining these troublesome observations is that I am less restricted in where I assume spillovers end. Previously, assuming they ended at any further

than 5km leaves me with few remaining observations in the zones after cutting out rings of this width (see Figure 2). I therefore extend this distance to 10km, reducing the spillover bias in my estimation of both the treatment effect and the spillover effect. I must then enlarge the outer control zone, as otherwise after the second expansion all houses are exposed, leaving none on which to estimate the later time fixed-effects. I therefore extend the outer zone to 15km outside the 2023 border, and again check robustness to failures of parallel trends using the methodology from Rambachan and Roth (2023). I still omit all houses within 5km of the 2019 ULEZ border, because of their fundamentally different treatment.

5.3 Long-Distance Analysis

The identification strategy outlined above hinges on the parallel trends assumption. Because I use as my control group house sales from a suburban region on the outskirts of London, I am therefore assuming that within London and its suburbs, the evolution of average house prices in an area is invariant to how urban that area is. The evolution of the coefficients, particularly the presence of a significant trend in the pre-treatment coefficients, will signal the presence of any gradual suburbanisation, but this is a strong assumption that should be addressed more thoroughly.

To improve confidence that any observed effect is indeed a result of the ULEZ, I therefore undertake similar analysis to the above, but with a new control group, comprised of houses in cities elsewhere in England that resemble London’s 2021 and 2023 expansion zones (henceforth ‘long-distance’ analysis)². Under the assumption that in the absence of treatment, average house prices in each of the expansion zones evolve identically to those in the areas of the towns and cities used to form their control group, this identification strategy isolates the effect of the ULEZ on house prices - crucially, issues with suburbanisation are removed. Of course, this is also a strong assumption, and identification in this way trades off one assumption for another, with no prior knowledge of which one is more plausible. However, if the results align with those observed from the previous identification strategy, we can claim with more confidence that the observed effect is indeed a product of the ULEZ - this approach therefore serves as a useful confirmatory exercise.

When using the outskirts of London as a control, the selection of the specific control region was

²I have HM Land Registry data for Wales, but further covariate information is unavailable, forcing me to disregard Welsh houses from this analysis.

constrained by the fact that in the vicinity of London, areas further from the city become quickly unrepresentative of areas in the city. Combined with the desire to avoid spillover effects, this left very few areas to choose from, which motivated the crude control-selection process based on distance from the 2023 ULEZ border. In this long-distance regime, the selection of the control region is far more free, because of how many areas in and around other English cities seem like a plausible control. I take advantage of this by selecting control regions for each of the two expansion regions separately, allowing for more accurate representation. Having done this, I will use a simple dynamic difference-in-differences design to estimate the ATT separately for the 2021 and 2023 regions.

The size of the pool of possible control areas also makes it much more difficult to select the most appropriate one. The most obvious way of doing so is to match areas to the treated region based on their covariates. Since this approach serves as a confirmatory check for the local analysis, it is important that we are able to match all of the treated region, rather than just a subset, to guard against heterogeneity of the treatment effect that may be correlated with ease of matching. In general, this comes at the cost of the 'quality' of the match, in some sense, but it seems clear that in this situation we prefer a universal match to a higher quality match.

However, matching is particularly difficult in this setting, for three reasons. Firstly, I have repeated cross-sectional data, rather than panel data, which means I can't match on the house and must instead match on characteristics of the regions in which they lie. In this instance, the obvious choice is to match on the characteristics of the postcode sectors, especially because it is easy to access postcode-sector-level covariates fairly easily. However, this rules out many common methods of incorporating matching into the analysis that are built under the assumption of panel data, for example the synthetic difference-in-differences estimator of Arkhangelsky et al (2021), and in general there is very little information on best practice for matched difference-in-differences with repeated cross-sectional data. Secondly, the geography of the ULEZ gives more information on the treatment assignment mechanism than is usually available: namely, all areas outside of Greater London have a treatment probability of exactly 0, because the ULEZ is implemented by the Mayor of London, whose influence extends no further than this border. This invalidates any type of propensity-score-type matching, which is another very common method. Finally, London is far larger than other English cities, and will likely have systematically different characteristics to most other areas of England. As such, the pool of suitable control regions outside of the surrounding area may in fact be very small, and so I must match on as many covariates as possible to have the best chance of finding a valid control group. This means that even in the rare cases where long-distance matching

has been used in this setting (e.g. Gruhl et al (2022)), the methods used are not applicable here.

It is hard to find a theoretically well-grounded matching algorithm that can produce a control group for the whole of the treated region (in both the 2021 and 2023 cases), subject to the constraints set out in the previous paragraph. As a solution, I propose a simple modification of the intuitive and common k -means clustering algorithm, repurposed for matching (henceforth ‘ k -means matching’). Specifically, I use the standard k -means algorithm, but fix the cluster assignment for the treated postcode sectors throughout. In more detail, suppose we have information on d numeric covariates for units numbered $n = 1, \dots, N$, where units $n = 1, \dots, N_C$ are untreated, and units $n = N_C + 1, \dots, N$ are treated³. Then the k -means matching algorithm runs as follows:

1. Set the desired number of clusters, $k \in \mathbb{N}$
2. Assign each control unit $n = 1, \dots, N_C$ a cluster value from $\{1, \dots, k\}$, uniformly at random
3. Assign each treated unit $n = N_C + 1, \dots, N$ the fixed cluster value 1
4. Until a stopping criterion is reached, repeat the following:
 - (a) For each cluster, calculate the mean value of each covariate for units in that cluster, to form a vector of means in \mathbb{R}^d (its ‘centroid’)
 - (b) For each control unit $n = 1, \dots, N_C$, assign it the cluster whose centroid is closest to its vector of covariates (in terms of Euclidean distance)

This is a simple adaptation of the basic k -means clustering algorithm presented in Appendix A.2. I then simply take the untreated postcode sectors in the same cluster as the treated ones as my control group.

Because of the size of the possible control area, to protect from spillovers I can afford to remove from consideration more of the area around London than in the local analysis. As such, I drop all postcode sectors with centroids less than 50km from the outer 2023 zone. I also remove from consideration all postcode sectors in areas of England that have implemented a low-emission zone of similar stringency to the ULEZ: namely Birmingham, Bristol and Oxford. Importantly, because this analysis is aimed at confirming the local analysis, the treated regions I am trying to match to are the same ‘rings’ as before - the 2021 and 2023 analysis zones⁴. If parallel trends

³These covariates should be normalised to each have zero mean and unit variance, as is standard for k -means clustering.

⁴More precisely, all postcode sectors with centroids in these rings

Table 3: Results from the static specifications

	(1)	(2)	(3)	(4)
ULEZ	-0.022*** (0.0065)	-0.033*** (0.0059)	-0.045*** (0.0084)	-0.014* (0.0073)
Treated Group	2021 & 2023	2021 & 2023	2021	2023
Postcode Sector FEs?	✓	✓	✓	✓
Quarter FEs?	✓	✓	✓	✓
Controls?		✓	✓	✓
Observations	494,556	494,556	244,837	367,179
R-squared	0.372	0.693	0.682	0.708

Robust standard errors, clustered at the postcode-sector level, in parentheses

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

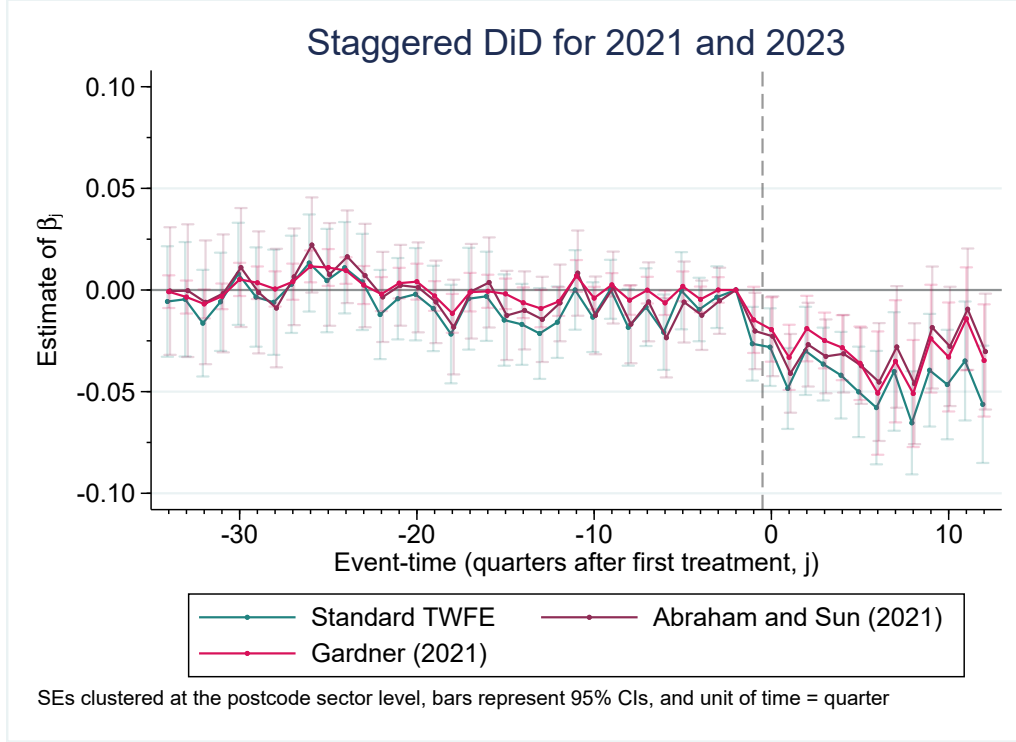
holds, then the simple dynamic DiD for these expansion zones - with house sales from these matched regions across England as the control group - will also identify the ATT of the ULEZ in these zones on their house prices.

6 Results

6.1 Baseline Local Analysis

Table 3 presents results from the baseline static staggered DiD, which shows a negative effect of the ULEZ on house prices. Without controls, in specification (1), prices are estimated to fall by 2.2%, but the more reliable specification with controls extends this estimate to 3.3%. Comparing the 2021 and 2023 expansions individually with the control zone through simple static DiD designs, in specifications (3) and (4) respectively, shows a much larger effect in the 2021 zone than the 2023 zone, aligning with the exploratory analysis. This is surprising given that more suburban regions further from central London are likely more car-reliant, but regardless, the effect is significantly negative in both areas. Heterogeneity analysis, in Appendix A.3, indicates the negative house-price effect is concentrated within lower-priced houses, and particularly flats, which fits with the regressive nature of the charge. This Appendix also reports results with different assumed distance thresholds beyond which the parallel trends assumption fails, and spillover effects end, rather than just using 10km and 5km respectively. I am of course constrained in this heterogeneity analysis by the requirement that parallel trends holds and spillovers are suitably accounted for, but within these constraints I am able to vary these

Figure 4: Estimates of β_j for each of the three event-study estimators above



distance thresholds and demonstrate robustness of this result.

Figure 4 now plots the coefficient estimates from each of the three event-study estimators outlined above: standard TWFE estimated via OLS, and the robust estimators from Abraham and Sun (2021) and Gardner (2021). Appendix A.4 gives a regression table containing these results, alongside all subsequent event-studies. I have addressed pre-emptive behaviour by normalising to zero the coefficient two quarters before first treatment (i.e. event time $j = -2$), since the coefficient estimates are remarkably flat up to event time $j = -1$, where they then exhibit a sudden drop. The figure has three important features. Firstly, in each case, the pre-treatment coefficients are generally close to zero and insignificant, suggesting no differential pre-treatment evolution of house prices and therefore supporting the parallel trends assumption. Second, the post-treatment coefficients are negative and generally significant, lying fairly consistently around -0.035 in the first two years after treatment, implying the ULEZ led house prices in treated regions to fall by roughly 3-4% over that time. The charge, alongside any other negative effects of the ULEZ, have an overwhelmingly larger impact on housing valuations than the positive effects. Finally, the treatment effect begins to recover over time when robustly estimated, and beyond two years after treatment they begin to become insignificant. I discuss

this in section 7. Appendix A.5 presents an alternative visualisation of these effects, plotting the evolution of region-wise control-adjusted means.

Three further points must be made. Firstly, although both expansions were announced long in advance, the absence of a behavioural response until near implementation, as observed in Figure 4, is theoretically reasonable. Policies implemented by authorities frequently change in nature after their initial announcement - in fact, the 2023 expansion was challenged in the High Court, and only approved less than two months before it was implemented. Waiting until confident in the exact details of the expansion before adjusting behaviour seems rational in these circumstances. Notably, the sharp drop at event-time $j = -1$ is present not just in Figure 4, but also when comparing the 2021 and 2023 implementations individually with the control group - these dynamic DiD results are presented in Appendix A.6. This consistency further supports the theoretical justification of no pre-emptive behaviour before $j = -1$.

Secondly, there are two signs that suggest the effects of the COVID pandemic is not driving this result. The UK's first lockdown began seven quarters before the first ULEZ expansion, in March 2020, and was gradually eased through summer of that year. The housing market was frozen for seven weeks, but reopened in May 2020, marking the first opportunity for Londoners to move out of the city. Lockdowns were reimplemented over the winter of 2020-21, but if the pandemic was contributing to the effect, then we would expect to see a drop in the coefficients well before when we actually see it in the data. Graphs of the sale count in Appendix B also shows that there was an surge in the number of house sales in London around the time of the restrictions being lifted, but again this occurs before the point at which we observe a response in our coefficients. In fact, after this surge, the sale-count series reverts very quickly to the same seasonal trend it exhibited before the pandemic, uniformly across the three regions in our analysis. Furthermore, the proportion of sales in the 2015-2024 period that occur in this surge is qualitatively identical in the 2021 Analysis Zone as in the other two zones. This would be very surprising if the effect was coming from COVID-induced suburbanisation, since homeowners in inner London have the choice of moving anywhere, not just into one of the other zones we consider here. While the long-distance analysis will provide a more thorough check of whether COVID was responsible for these observed effects, these features of the local analysis provide useful qualitative support.

Finally, for event times $j > 5$, I only observe observations from the 2021 zone. This is a small issue, that I accept to avoid the alternatives: restricting my sample to only five periods after treatment obscures much of the dynamics of the effect, while presenting analysis for each zone

separately sacrifices precision from not-yet treated zones serving as controls for already-treated zones. Regardless, even before $j = 5$, the treatment effect is significantly negative and large.

6.2 Flexible Spillover Treatment

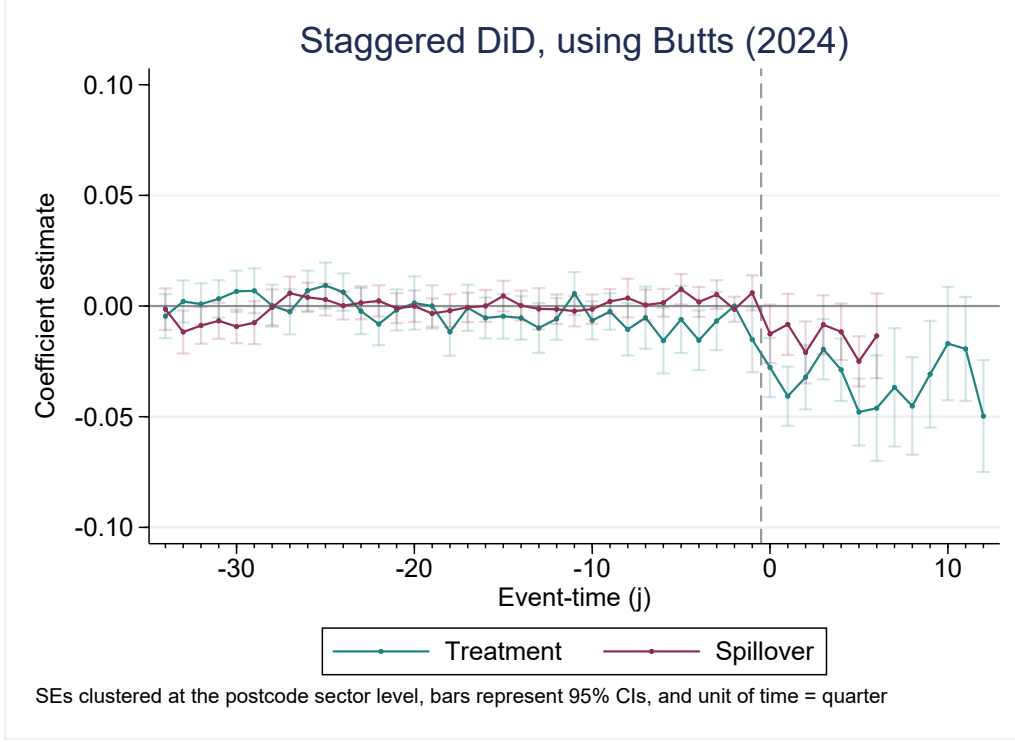
Figure 5 presents the results from the spillover-robust estimator from Butts (2024), which gives a less biased and more representative estimate of the overall ATT across Greater London. Encouragingly, these estimates align with what was observed before. The pre-treatment coefficient estimates for the ATT, as well as the spillover effect, are largely insignificant and non-trending, supporting parallel trends in both cases. The ATT is also similar in magnitude and dynamics to the event studies above. Once the full negative effect of the ULEZ is realised, the coefficient estimates suggest it removes roughly 4% off the price of houses in the treated regions, and this effect remains fairly stable for about two years. Prices then begin to recover, as was also seen before.

The spillover effect is also significant in a few post-treatment periods, as well as visually convincing. The implementation of the ULEZ leads house prices to fall by roughly 2% in areas within 10km of the border, and this effect appears to be sustained over a period of almost two years. It evolves in the same way as the direct effect does, suggesting the factors driving them are the same. Notably, I have fewer post-treatment coefficients for my spillover effects than for my direct treatment effects. To be exposed for longer than six quarters while still not treated, a house must be within 10km of the 2021 expansion zone but outside the 2023 expansion zone. Few houses satisfy this criteria for event times $j > 6$, so the estimates are very imprecise and I omit them.

6.3 Parallel Trends Analysis

In sum, assuming parallel trends, there is substantial evidence to suggest that the ULEZ has a large negative impact on house prices, both within the zone and just outside it. However, while the parallel trends assumption is imperative in allowing a causal interpretation of the observed effect, it is fundamentally untestable, as it is an assumption on the counterfactual. The classic approach to this issue is to test the joint insignificance of the pre-trends, and argue their insignificance implies these non-differential trends would continue past treatment, but as justified in section 5, this approach has severe issues. I therefore do not present formal tests for the joint insignificance of my pre-treatment coefficients.

Figure 5: Estimates of the treatment and spillover effect, using Butts (2024)



However, my results hinge heavily on parallel trends, so it is important to address their robustness to this assumption in some way. To do this, I use the methodology in Rambachan and Roth (2023). The intuitive idea here is that rather than assuming parallel trends holds, I bound how badly it can fail, thus making more post-treatment trends feasible under the null of no treatment effect. I then seek to show that under reasonable bounds, what I observe in my event studies above is still not feasible under this regime - i.e. the result is significant.

In more detail, I replace the parallel trends assumption with the following: any deviation from parallel trends between consecutive periods in the post-period can be no larger than a multiple M of the maximum such deviation in the pre-period. More formally, using the decomposition from the paper of β_j into the sum of a treatment effect τ_j and the difference in trends that would have occurred without treatment, δ_j , this assumption states that for all $t \geq 0$, $|\delta_{t+1} - \delta_t| \leq M \cdot \max_{s < 0} |\delta_{s+1} - \delta_s|$. Importantly, M is left up to the researcher, but the paper suggests that if there are an equal number of pre and post periods, then $M = 1$ is a reasonable and intuitive choice - post-treatment deviations in parallel trends between consecutive periods are bounded by exactly the greatest such deviation before treatment.

I also act on two further important remarks in the paper (on p. 2563, footnote 7). Firstly, to mitigate against seemingly large pre-treatment deviations in parallel trends arising simply from stochastic ‘bouncing’ around a zero trend, I pool time periods together, from the quarter level to the year level. Second, the paper also suggests that pre-treatment periods closer to the treatment point will be more informative than those further back, and so I should allow M to be higher for these earlier periods. There is no obvious way of doing this within the statistical programming package the authors provide. As an alternative (somewhat crude) solution, and guided by the advised condition when choosing $M = 1$, I restrict in my analysis the pre-period back only as far as the post-period extends, so that the number of pre- and post-treatment coefficients are equal.

Figure 6 gives the confidence intervals for each of the post-treatment coefficient estimates from each of the four estimators above, under this new assumption with $M = 1$. It should be first noted that as a result of the method, the confidence intervals get wider for periods further past the treatment point. This is intuitively because the permitted deviation from parallel trends is allowed to compound over time - by allowing multiple consecutive ‘large’ deviations from parallel trends, I am implicitly enlarging the permitted failures in periods further on from treatment. However, the key point is that in each of the three robust models, the upper bound of the 95% confidence interval for the average treatment effect two years after treatment is below zero, indicating that my results are robust to large, repeated failures of parallel trends. In two of these models, the estimate of the treatment effect a year after treatment is also significantly negative at the 5% level under this more flexible specification. As such, while there is good evidence from the event study plots to argue in the conventional way that parallel trends holds in my analysis, I have shown that it holds ‘as much as it needs to’ in order for me to claim that the treatment effect is significantly negative. It is also important to note that this analysis does not affect my point estimates of the effects - it just provides further evidence to support their robustness.

The fact that M is the choice of the researcher also allows me to report the largest M , denoted \bar{M} , at which a given coefficient estimate retains its significance. This sensitivity analysis is a key feature of the method that the paper provides. Table 4 reports these maximal \bar{M} values, for coefficients corresponding to one and two years after treatment, i.e. event times $j = 1$ and $j = 2$, which are the time periods in which the most pronounced negative effects are observed. It indicates that even if I was to allow these post-treatment consecutive parallel trends deviations to be as much as 25% larger than the maximum in the pre-period, and occur repeatedly

Figure 6: 95% confidence intervals for the treatment effect, from Rambachan and Roth (2023)

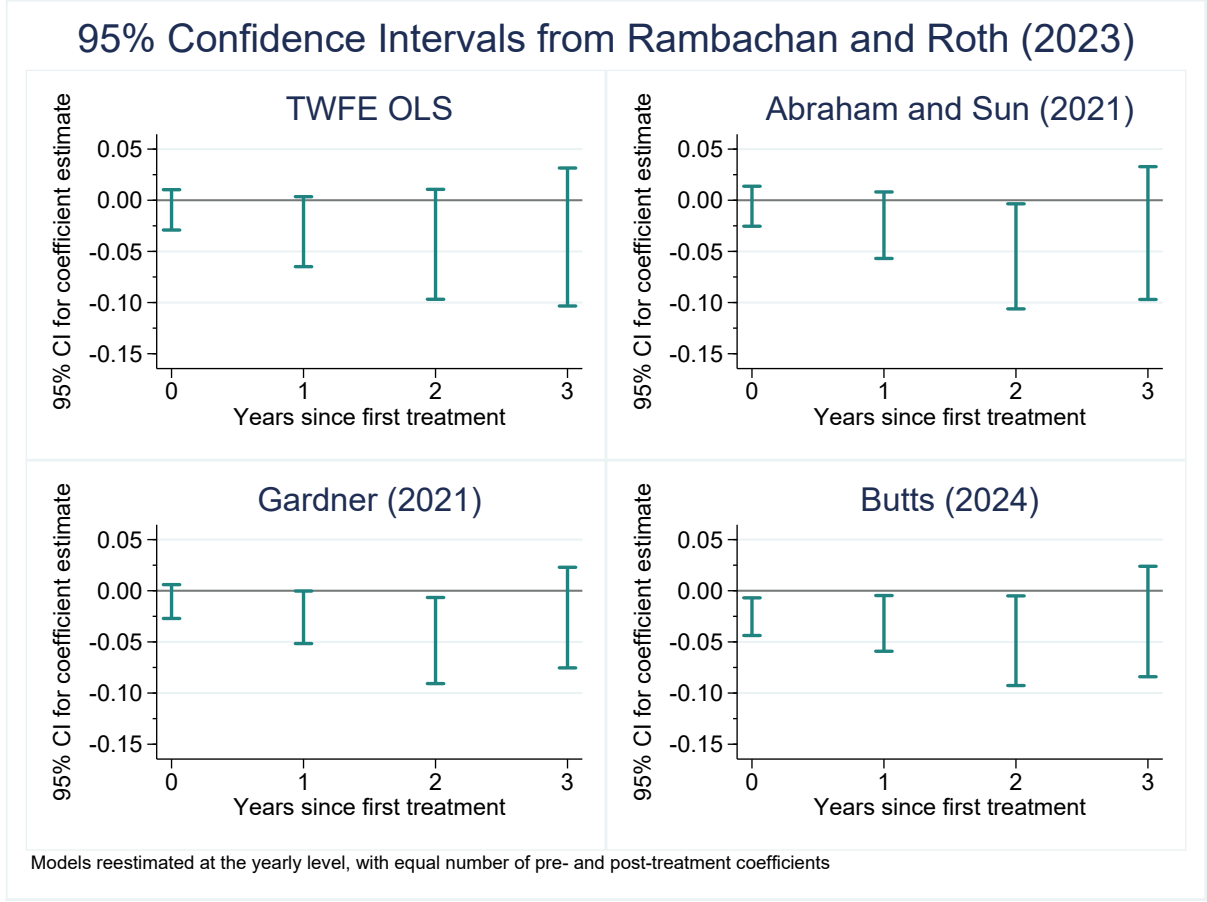


Table 4: Maximal M (\bar{M}) from Rambachan and Roth (2023) that retains significance of the relevant coefficient (nearest 0.05, rounded down)

Estimator	\bar{M}	
	$j = 1$	$j = 2$
TWFE OLS	0.85	0.75
Abraham and Sun (2021)	0.7	1.05
Gardner (2021)	1	1.25
Butts (2024)	1.2	1.1

j := number of *years* since the ULEZ first implemented in the region

over the post-period, I would still find that my results are significant at the 5% level according to the estimator from Gardner (2021). It should be emphasised how flexible this weakening of the parallel trends assumption is - this robustness analysis indicates that my initial results are strongly robust to possible violations in the parallel trends assumption.

6.4 Long-Distance Analysis

To find suitable control groups for both analysis regions across the whole of England, I apply the k -means matching algorithm to the collection of postcode sectors across England and Wales for which data is available. I use as covariates the proportion of houses sold in a given postcode sector in the period 2015-2019 that are detached, semi-detached, flats, new-builds, and leasehold, as well as the same proportions in the postcode district that contains the relevant postcode sector⁵. I also collect data on the Index of Multiple Deprivation (a standard measure of deprivation across the UK) and the population density at the LSOA level for 2019, from the Ministry of Housing, Communities and Local Government and the Office of National Statistics respectively⁶. I then use a postcode to LSOA best-fit lookup (also from the ONS) to map these values to the postcode level, which I then aggregate at the postcode sector and postcode district level, and use as further covariates. Having removed all postcode sectors with missing covariate information, those with centroids within 50km of the 2023 ULEZ border, and those in cities that also implemented a LEZ of similar stringency over the sample period, I have 5,190 postcode sectors outside London, in addition to 177 in the 2021 analysis zone, and 252 in the 2023 analysis zone. I set $k = 15$ in the algorithm, so that the mean group size is roughly double the number of treated postcode sectors. In Appendix B, I show my results are robust to different values of k , and different initial group assignments.

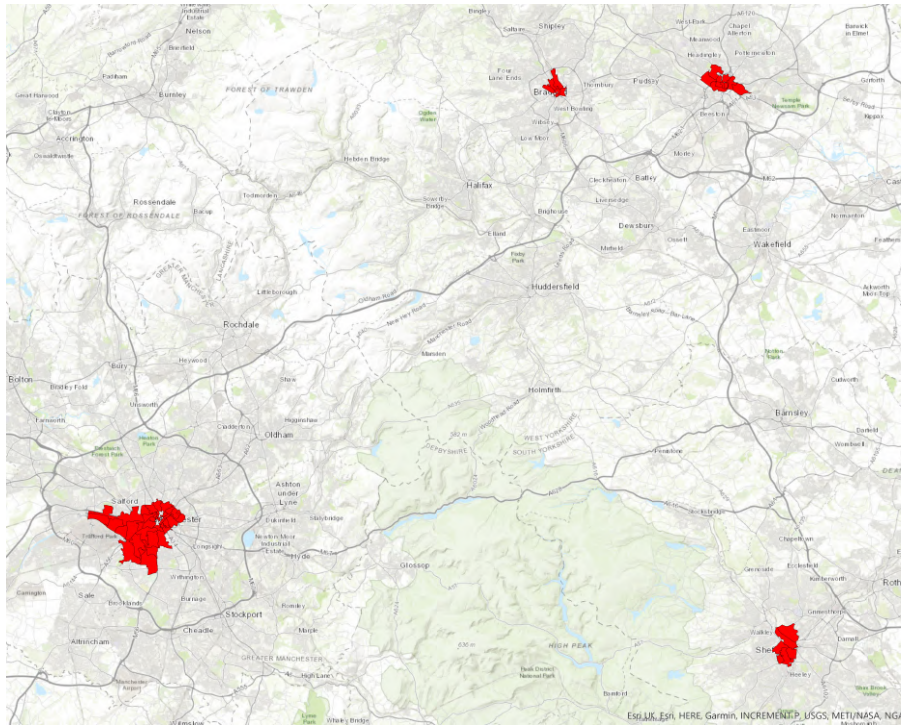
Appendix A.7 gives full maps of the derived control regions for each zone, but for the sake of exposition and clarity, I present zoomed in versions of the area around the Manchester and Leeds in Figure 7, which illustrate well their properties. As we would expect, the algorithm picks out the cities in the region to be used as controls, omitting more rural areas. The 2021 control group is limited only to the centres of Manchester, Leeds, Bradford and Sheffield, which seems intuitively sensible for regions supposed to represent inner London. The 2023 control group contains these central areas, but in addition it includes more suburban areas in Manch-

⁵Recall that a postcode sector is a collection of houses all sharing the AB12 3XX component of their postcode.

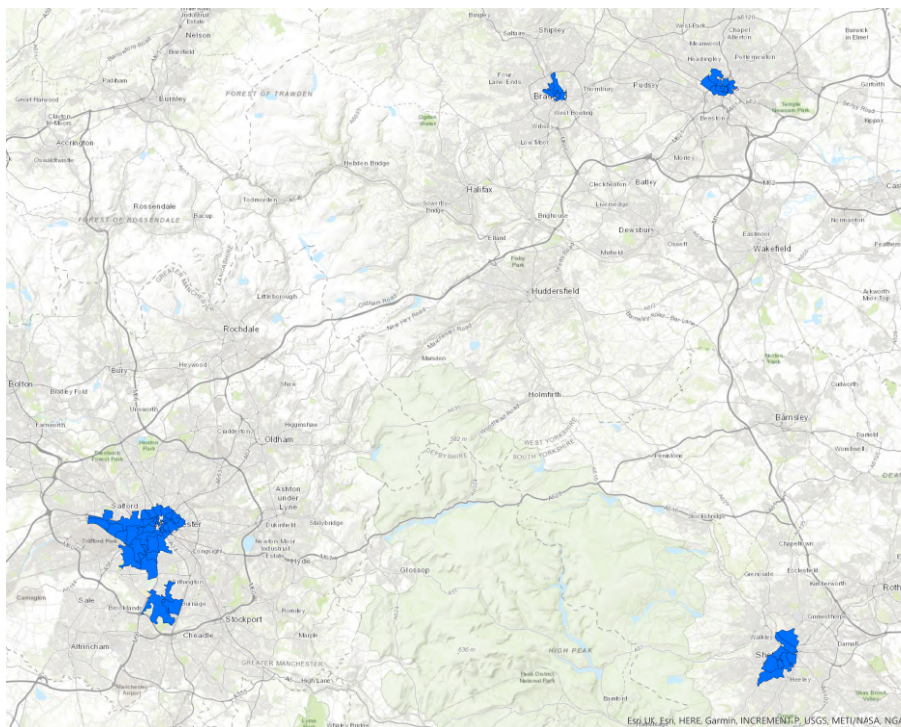
A postcode district is the collection of houses that share the AB12 XXX component.

⁶LSOA stands for Lower layer Super Output Area. These are small geographic areas that partition the UK - each contains between 400 and 1200 households.

Figure 7: Control regions derived from k -means matching, restricted to the Manchester-Leeds area, for (a) the 2021 analysis zone, and (b) the 2023 analysis zone



(a)



(b)

ester and Sheffield. Again, this seems sensible - we get some overlap, since the 2021 and 2023 analysis zones are both formed of similarly built-up areas in London, but since the 2023 zone is slightly further out, the algorithm also picks out more suburban regions of the other smaller cities around England, as well as centres of smaller cities and towns, which can be seen in Appendix A.7. Intuitively, the clustering appears to have chosen the control regions sensibly.

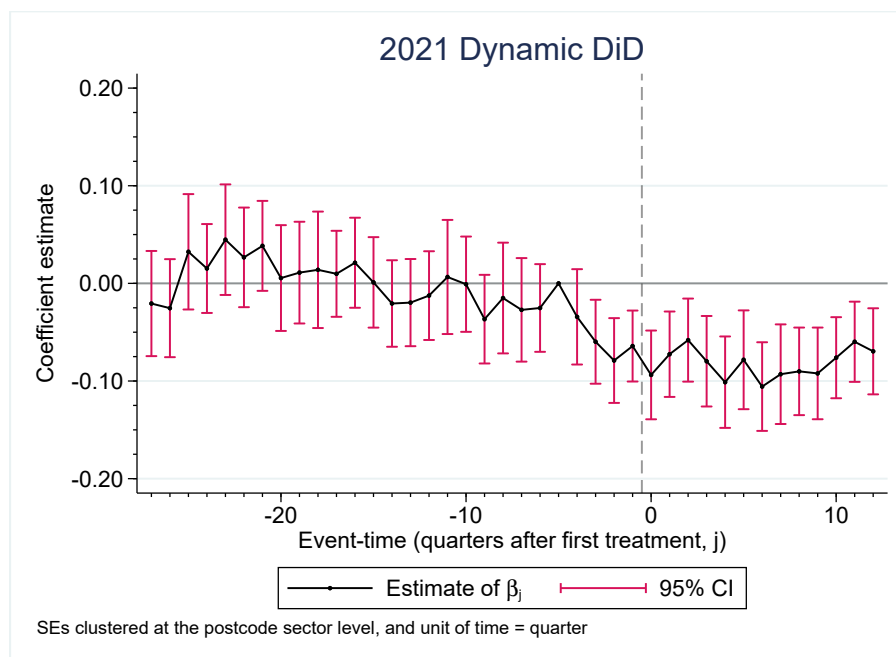
Figure 8 presents the results of the dynamic difference-in-differences regressions using the long-distance control regions derived from the matching. The 2021 results suggest pre-emption extending as far back as four quarters before treatment starts, hidden in the local analysis - for this reason, I normalise the coefficient at event-time $j = -5$ to 0. Having done this, the pre-treatment coefficients are all insignificant, but there is a fairly clear downward trend, which prevents us from claiming that parallel trends holds here. However, the regularity of the downward trend reflects gradual and consistent net movement away from London and into other cities in England, consistent with net domestic migration out of the city, perhaps supported by government efforts at ‘levelling up’⁷. This may therefore exaggerate the size of the post-treatment coefficients, but regardless of this, there is a visible drop in the coefficient value around the ULEZ treatment. The post-treatment coefficients are in general around -0.075, but accounting for the influence of this downward trend suggests a smaller effect, similar to the one observed in Table 2 and Appendix A.6.

To investigate this more formally, I would want to include time-trends interacted with treatment status in the regression. The treatment effect would then be identified under the assumption that parallel trends holds up to divergence between the two groups which is linear over time. As noted, this can be rationalised as a product of gradual movement out of London and into other cities around England. Unfortunately, doing so would require a restriction on the flexibility of the treatment effect evolution, since the interaction of this trend with the event-time dummies would otherwise be collinear. As such, we must settle for the intuitive argument that de-trending the coefficient path according to the pre-treatment coefficients would yield a similar path to what was seen in the local analysis. This analysis is clearly not perfect, but the results suggest a negative effect of a similar size to the effect found in the local analysis: this lends support to our conclusions on the effect of the ULEZ in the 2021 analysis zone.

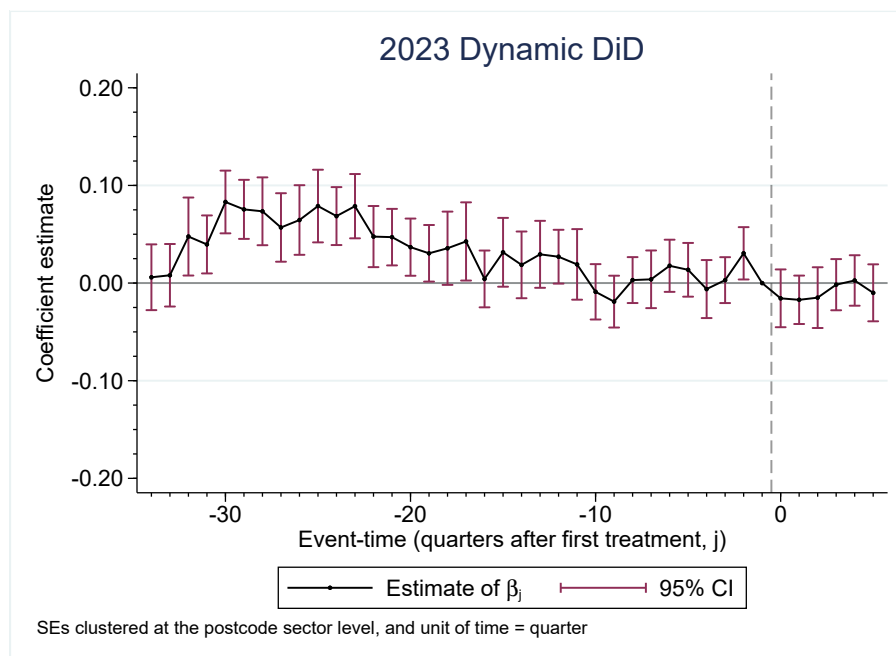
The 2023 results are less useful. Again there is a downward trend, but it is far steeper and the pre-treatment coefficients are largely positive and significant, particularly earlier in the sample. It is unclear whether there is much pre-emptive behaviour, and so I normalise the coefficient

⁷See, for example, [here](#), [here](#) or [here](#)

Figure 8: Result from the dynamic DiD with long-distance controls, for (a) the 2021 analysis zone, and (b) the 2023 analysis zone



(a)



(b)

at event-time $j = -1$ to 0, as is conventional. The post-treatment coefficients are insignificant, but because of the extent to which the parallel trends assumption fails in the pre-period, we cannot draw from this any conclusions about the true effect. However, it should be noted that the post-treatment coefficients do not deviate far from the downward trend of the pre-treatment coefficients, which suggests that if there is an effect, it is small. This is consistent with what we see in Table 2 and Appendix A.6. Any conclusions taken from this figure must be made with extreme caution, but there is nothing here that majorly contradicts the local analysis.

The conclusion from the long-distance analysis, particularly when examining the 2021 region, is that once we account for linear trends in these coefficients (rationalisable as a product of consistent net-negative domestic migration from London to elsewhere in England), the treatment effects observed here are similar in size to those observed in the local analysis. This suggests they are not driven by any sharp suburbanisation around that time that might have been induced by, for example, the COVID pandemic. The results demonstrate that the additional net movement out of central London into its suburbs after the introduction of the ULEZ roughly equals the additional net movement from central London to other English cities. If COVID was the driver of this effect, then this would imply that it drove people to move to other city centres in the same numbers as to suburban areas around London, which seems ridiculous given that this movement was generally understood to be motivated by a desire to escape built-up areas. There are two possible alternative explanations: the first is that a shock around the time of the ULEZ expansion made central London less attractive to live in, without making suburban London any more or less attractive than the centres of cities elsewhere in England. The second is that similarly timed shocks of similar sizes hit both suburban London and the centres of the other English cities to make them equally more attractive than central London. The first seems far more plausible than the second, which strongly suggests the observed results are a result of the ULEZ.

7 Discussion

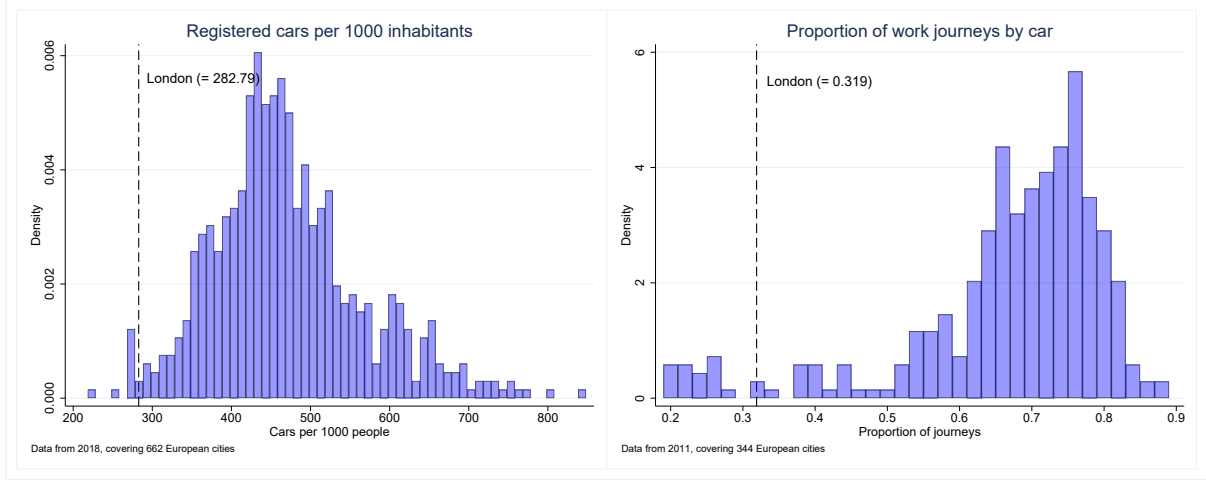
The above analysis found that the ULEZ reduced house prices by roughly 3-4%, consistently among different estimators. As well as statistically significant, this effect is economically significant, with huge implications. Critically, it will impose huge welfare costs on London's population. The average price of houses sold in the expansion zones since the start of 2021 was around £573,000 - a 3.5% fall in this price corresponds to a drop in the market value of the average house in the region of roughly £20,000, over the space of a year. To put this in perspective, the

median full-time salary in London in 2024 was £47,455 (ONS, 2024). The short term holistic effect of the ULEZ, framed in terms of its effect on house prices, was to cost the average London homeowner almost half the median salary in the region – a huge cost that cannot be ignored in favour of positive impacts of the ULEZ elsewhere. These welfare implications also raise concerns over inequality, particularly given that the effect falls disproportionately on lower-priced houses (see Appendix A.3). A household’s welfare loss is also clearly independent of their polluting behaviour - two houses on the same street will suffer the same adverse effects even if one has a new electric car in their driveway, and the other has an old diesel one. This feature may provoke local aggravations and further amplify a general sentiment of discontent with the policy.

The results also point towards issues associated with non-compliance. The reduction in house prices shows London has become less attractive to live in as a result of ULEZ, but the people whose opinions on London are being changed by the policy are precisely the people who will be most affected by the charge - i.e. the highest polluters. The results therefore imply that polluters are being pushed away from the region, which improves its air quality not by correcting the behaviours at the root of the problem (i.e. driving old high-polluting cars), but by simply moving the offenders elsewhere. This presents a more ethical dilemma: does a region with an emissions zone successfully deal with its air pollution if it just relocates the worst offenders? If the answer is no, then my results suggest that the ULEZ may not be classifiable as successful, even if studies find that London’s air quality has improved. Note also that this movement elsewhere will actually bias estimates of the effect on air quality upwards in magnitude within any difference-in-differences framework, and by the same token, my results may be slightly upward biased too. Despite this, I can be confident that my effect is significantly negative - if it wasn’t, there would be no upward bias as there would be no outward movement of polluters.

However, to understand the long-term impact of the ULEZ, the dynamics of the effect must be accounted for – in particular, the main local analysis suggests the ATT begins to recover over time. This may be a product of asymmetric information transmission. Recalling the hedonic house valuation process, the driving restriction itself will be quickly incorporated into prices, because it is very quick and easy to observe and understand. Conversely, the positive effects the ULEZ has on air pollution and congestion are less obvious, take longer to materialise, and have implications on health and welfare in ways that are difficult to immediately understand, so will take longer to be reflected in house prices. This dynamic may explain the late signs of recovery. The long-run effect depends on the relative size of the positive and negative effects of the ULEZ, not their relative pace of emergence, and so it may be the case that the observed effect is just temporary. As such, long-term conclusions about the effect of the ULEZ on local

Figure 9: Histograms of transportation statistics for different European cities



house prices cannot be made yet.

Finally, a comment must be made on the external validity of my results. The broad house price effects of the ULEZ stem from the effects it has on drivers of non-compliant cars, who now have to pay either directly, via paying the charge, or indirectly, via upgrading their cars or shifting to other transportation methods, for example. As such, the cities where such stringent emissions charges are likely to have the worst effects are those where cars are most widely used, and so the external validity of my results can be ascertained by examining how London's car and public transportation usage compare to other major cities. Figure 9 plots data collected from Eurostat on a variety of European cities, illustrating the distribution across these cities of two important indicators of car-dependency: the number of registered cars per 1000 inhabitants, and the share of journeys to work by car. In both cases, I take the year in which data for London was last recorded. The results show that London is clearly one of the least car-dependent cities in Europe. This suggests that if a similar policy were to be implemented in another European city, the effects it would have on house prices would be to reduce them by far more than they have fallen in London.

8 Conclusion

London was in 2018 the European city suffering the greatest costs from its air pollution (CE Delft, 2020). To combat the problem, the Mayor of London implemented the Ultra Low Emis-

sions Zone in 2019, which was then expanded in 2021 and again in 2023. As a pioneer among worldwide emissions zones, both in terms of its size and its strictness, it is crucial to ascertain a holistic evaluation of total effect of the policy, to judge its overall effectiveness and inform similar policy in the future. This paper addresses this highly policy-relevant question, by looking at how the ULEZ affected the region’s house prices.

The results from the above analysis indicate that the expansions of the ULEZ caused house prices in the affected regions to fall by roughly 3-4%. This corresponds to a reduction of about £20,000 in the value of the average house sold in the region, or close to half the median local salary. This effect is similar among different estimators, and robust to more explicit ways of dealing with spillovers, as well as to a large weakening of the key identifying assumption of parallel trends. The ULEZ also had significant spillover effects, reducing house prices in regions within 10km of the zone by roughly 2%. Using a simple adaptation of the k -means clustering algorithm to repurpose it for matching, I also find that the main effect is reasonably robust when changing the control group from the suburban areas around London to a collection of representative areas of cities elsewhere in England, which rules out explanations of the effect related to suburbanisation, including the COVID pandemic. London’s relatively low car-dependence means these adverse short-term effects are highly externally valid.

It should be emphasised that this paper is concerned explicitly with the immediate effects of the ULEZ, rather than its longer term consequences. The dynamics of the observed effects may be influenced by asymmetric information transmission, where residents understand the negative effects before the positive ones. We cannot therefore make any comments on the long-term effect of the policy on house prices until all information has been fully transmitted, by which time the effect size may have changed. The analysis has also implicitly been working under the assumption that there are no sample selection issues in who decides to sell their house after the ULEZ - a more formal treatment, as in Heckman (1979), may be useful. Another task for future research is a rationalisation of why the ULEZ has such a negative impact on the 2021 expansion zone, but a nearly negligible one on the more car-dependent 2023 zone. This appears counter-intuitive, but to fully understand the intricacies of emissions-charging policy and environmental policy more broadly, a coherent logical foundation must be established.

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Appendix A.1 - Data Processing

Having downloaded the UK Price Paid data from HM Land Registry, I must geocode the addresses in order to relate them to the ULEZ. I do this in ArcGIS, using the OS Open Names Locator API from the Ordnance Survey and ESRI⁸. Once this is done, I need feature layers giving the borders of the ULEZ in 2019, 2021 and 2023, to create my distance variables. The London Datastore provides shapefiles for the ULEZ in 2019, 2021 and 2023, from which I extract the borders in ArcGIS⁹. Each of these shapefiles use both a geographical coordinate system (GCS) and a projected coordinate system (PCS), and in each case the projection of the PCS is centred in Britain. In particular, their GCS is some variant of OSGB, and their PCS is some variant of British National Grid, which ensures consistency and accuracy.

I then perform a series of Near operations and Spatial Joins in ArcGIS to calculate the Euclidean (crow-flies) distance from each house to each border, as well as which of these zones each house is in. Because of the spatial referencing of the shapefiles from the London Datastore, particularly the fact that they all use a PCS centered in Britain, I calculate planar distances, rather than geodesic ones, as they will be more accurate.

The one small issue with this process is that the Open Names Locator API locates houses at the postcode level. This means all houses with the same postcode have the same spatial reference, which means they all share the same calculated distances. However, this is a very minor issue, since postcodes are small in area and so any errors in distance calculations will be very small relative to the actual calculated distances.

⁸For more information, see [here](#)

⁹The 2019 ULEZ shapefile is available from [here](#), the 2021 shapefile [here](#), and the 2023 shapefile [here](#)

Appendix A.2 - k -means Clustering and k -means Matching

Section 5.3 outlines the k -means matching algorithm I use in the long-distance analysis. This is a very simple adaptation from the basic k -means clustering algorithm used in economics and many other disciplines, which is set out as follows: suppose we have information on d numeric covariates for units numbered $n = 1, \dots, N$. Then the k -means clustering algorithm runs as follows:

1. Set the desired number of clusters, $k \in \mathbb{N}$
2. Assign each unit $n = 1, \dots, N$ a cluster value from $\{1, \dots, k\}$, uniformly at random
3. Until a stopping criterion is reached, repeat the following:
 - (a) For each cluster, calculate the mean value of each covariate for units in that cluster, to form a vector of means in \mathbb{R}^d (its ‘centroid’)
 - (b) For each unit $n = 1, \dots, N$, assign it the cluster whose centroid is closest to its vector of covariates (in terms of Euclidean distance)

The objective of this algorithm is to take a group of unlabelled observations and put them into groups based on similarity of a set of covariates, where ‘similar’ here means ‘close in Euclidean distance’. The only difference with k -means matching is that we now distinguish between treated and control units, and keep the treated units all in one group throughout. The logic is that the control units that end up in the same group as the treated units not only have similar covariate vectors to each other, but also to the treated units, hence forming a suitable control group.

Appendix A.3 - Heterogeneity and Robustness

Table 5: Results from the static specification, across different house types and price quartiles

	(1)		(2)
ULEZ*detached	-0.0235 (0.0150)	ULEZ*quartile1	-0.0602*** (0.00609)
ULEZ*semi-detached	0.0303*** (0.00650)	ULEZ*quartile2	-0.0100** (0.00388)
ULEZ*terraced	0.0232*** (0.00551)	ULEZ*quartile3	0.00985** (0.00391)
ULEZ*flat	-0.0904*** (0.00947)	ULEZ*quartile4	0.0202*** (0.00571)
Observations	494,556	Observations	494,556
R-squared	0.694	R-squared	0.845
Treated Group?	2021 & 2023	Treated Group?	2021 & 2023
Postcode Sector FEs?	✓	Postcode Sector FEs?	✓
Quarter FEs?	✓	Quarter FEs?	✓
Controls?	✓	Controls?	✓

Robust standard errors, clustered at the postcode-sector level, in parentheses

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

In both of these regressions, I take the static specification and replace the $ULEZ_{pt}$ dummy with a set of its interactions with each house type in (1), and each price quartile in (2). I also include controls for the price quartile in (2) - in (1) the controls for house type are already there. This regression has a valid difference-in-differences interpretation as long as the average price differences between quarters and postcode sector, conditional on house type/price quartile, are constant - this seems reasonable and I assume it to be true. Also, I calculate the price quartile in (2) within quarters - i.e. I compare each house to only houses sold in the same quarter. Quartile 1 represents the lowest quartile, and 4 the highest.

Table 6: Results from the static specification, across different assumed cutoff distances

	(1)	(2)	(3)	(4)
ULEZ	-0.0324*** (0.0053)	-0.0370*** (0.0065)	-0.0385*** (0.0047)	-0.0166** (0.0075)
Treated Group?	2021 & 2023	2021 & 2023	2021 & 2023	2021 & 2023
Parallel Trends Distance	12.5km	7.5km	10km	10km
Spillover Distance	5km	5km	2.5km	7.5km
Postcode Sector FEs?	✓	✓	✓	✓
Quarter FEs?	✓	✓	✓	✓
Controls?	✓	✓	✓	✓
Observations	559,206	435,841	790,850	245,833
R-squared	0.700	0.689	0.686	0.698

Robust standard errors, clustered at the postcode-sector level, in parentheses

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

I am unable to vary the distances much more than this - extending the spillover distance far past 5km gives very few observations in the 2021 zone, and contracting the parallel trends distance much shorter than 10km gives very few observations in the control zone. Unfortunately, these are the directions of distance change I would be most interested in examining. However, careful analysis of the parallel trends assumption and spillover effects using Rambachan and Roth (2023) and Butts (2024) respectively should convince the reader that my results are robust to these assumptions.

Appendix A.4 - Event Study Regression Table

Table 7: Results from the event study regressions, for selected event times

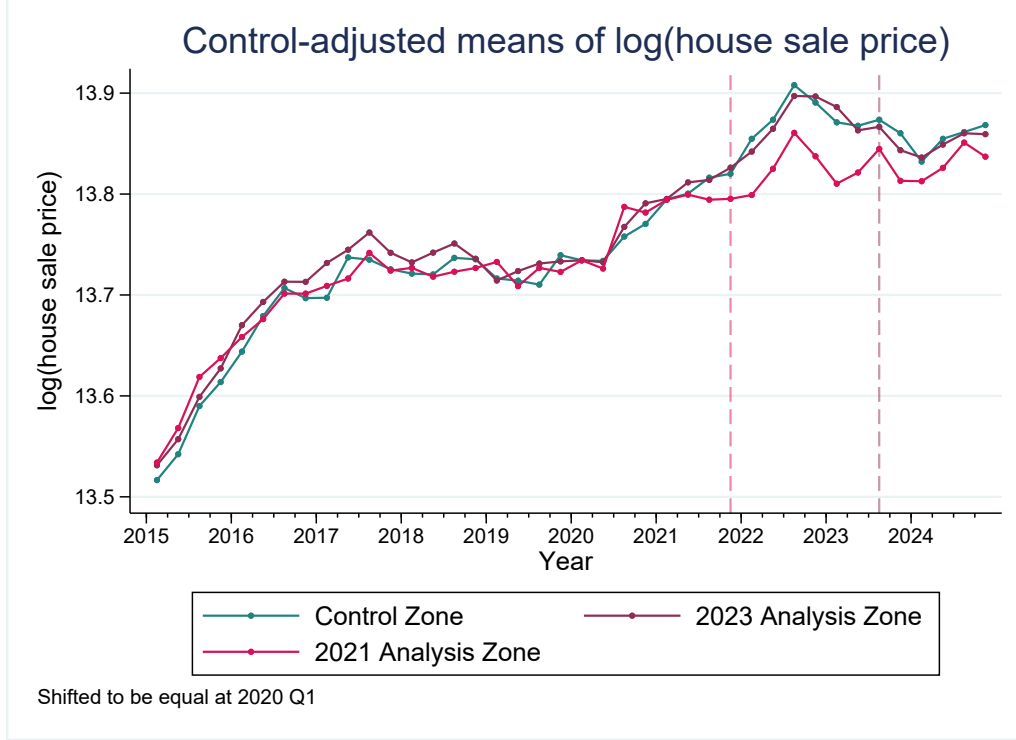
Event time (j)	(1)	(2)	(3)	(4)	(5)
$j = -5$	-0.0004 (0.0097)	-0.0059 (0.0102)	0.0018 (0.0037)	-0.0061 (0.0077)	0.0076** (0.0035)
$j = -4$	-0.0124 (0.0080)	-0.0131 (0.0087)	-0.0046 (0.0032)	-0.0155** (0.0069)	0.0018 (0.0034)
$j = -3$	-0.0053 (0.0077)	-0.0084 (0.0083)	0.0000 (0.0032)	-0.0067 (0.0067)	0.0052 (0.0033)
$j = -2$	- (-)	- (-)	- (-)	- (-)	-0.0015 (0.0029)
$j = -1$	-0.0264*** (0.0092)	-0.0202** (0.0096)	-0.0147* (0.0083)	-0.0151** (0.0076)	0.0059 (0.0040)
$j = 0$	-0.0281*** (0.0097)	-0.0228** (0.0100)	-0.0194** (0.0081)	-0.0277*** (0.0068)	-0.0126* (0.0068)
$j = 1$	-0.0484*** (0.0101)	-0.0410*** (0.0099)	-0.0331*** (0.0082)	-0.0407*** (0.0068)	-0.0083 (0.0070)
$j = 2$	-0.0300*** (0.0110)	-0.0269** (0.0105)	-0.0190** (0.0082)	-0.0322*** (0.0074)	-0.0210*** (0.0071)
$j = 3$	-0.0364*** (0.0091)	-0.0326*** (0.0093)	-0.0248*** (0.0070)	-0.0195*** (0.0070)	-0.0084 (0.0068)
$j = 4$	-0.0419*** (0.0109)	-0.0314*** (0.0097)	-0.0284*** (0.0082)	-0.0288*** (0.0072)	-0.0116* (0.0065)
$j = 5$	-0.0501*** (0.0113)	-0.0373*** (0.0096)	-0.0360*** (0.0092)	-0.0479*** (0.0077)	-0.0250*** (0.0058)
Observations	494,556	494,556	494,556	1,004,653	1,004,653
Estimand	Treatment Effect	Treatment Effect	Treatment Effect	Treatment Effect	Spillover Effect
Estimator	TWFE OLS	Abraham and Sun (2021)	Gardner (2021)	Butts (2024)	Butts (2024)
Controls?	✓	✓	✓	✓	✓

Robust standard errors, clustered at the postcode-sector level, in parentheses

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

Appendix A.5 - Alternative Visualisation of Treatment Effects

Figure 10: Evolution of mean log(house price) over time, in each region



In the above, I plot the evolution of the quarterly mean of the natural log of house prices over time in each zone, once adjusted for fixed-effects and controls. Specifically, I run the following regression:

$$Y_{ipt} = \delta_t \cdot zone_p + \alpha_p + \rho \mathbf{X}_{ipt} + \varepsilon_{ipt}$$

where $zone_p$ is just an indicator of the zone p is in. I then take the fitted values for each quarter-zone combination, setting everything else to 0 (and therefore taking the baseline post-code sectors in each region). Importantly, the mean price values are arbitrary, because I shift them all to be equal at 2020 Q1, but their *differences* are meaningful. The same key features are observed as in Figure 4. Before the first treatment, all paths evolve very similarly, supporting the parallel trends assumption. One period before treatment, in both the 2021 and 2023 zones, the mean price drops relative to the untreated and not-yet treated regions, and in the 2021 case this is sustained until the end of the period. Finally, in the 2021 case, there is a recovery in the difference over time. It is also clear from this graph how much more the 2021 zone contributes to the overall effect than the 2023 zone.

Appendix A.6 - Individual Dynamic DiD Results

The following figures give the estimated dynamic treatment effects from the individual dynamic DiDs of the 2021 zone vs the control zone, and the 2023 zone vs the control zone, respectively.

Figure 11: Coefficient plot from the dynamic DiD for the 2021 analysis zone individually

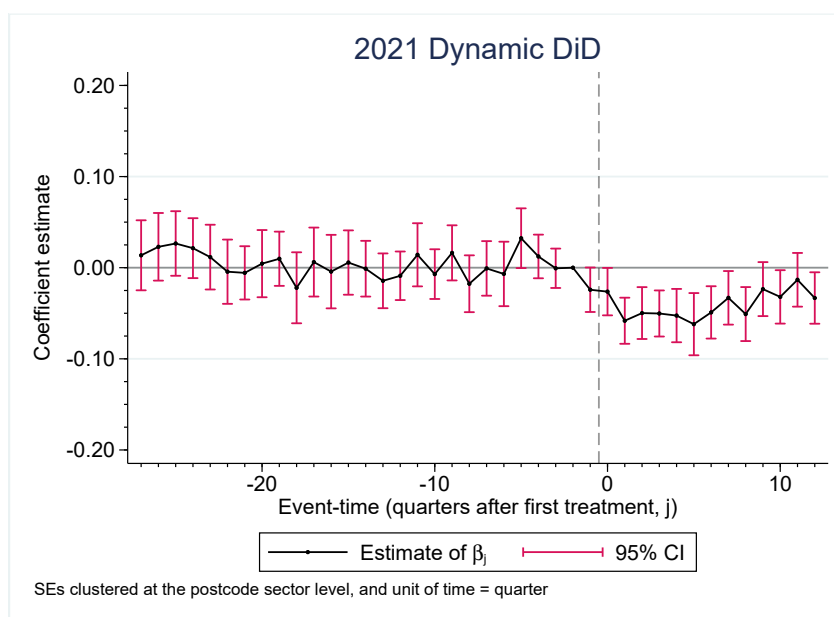
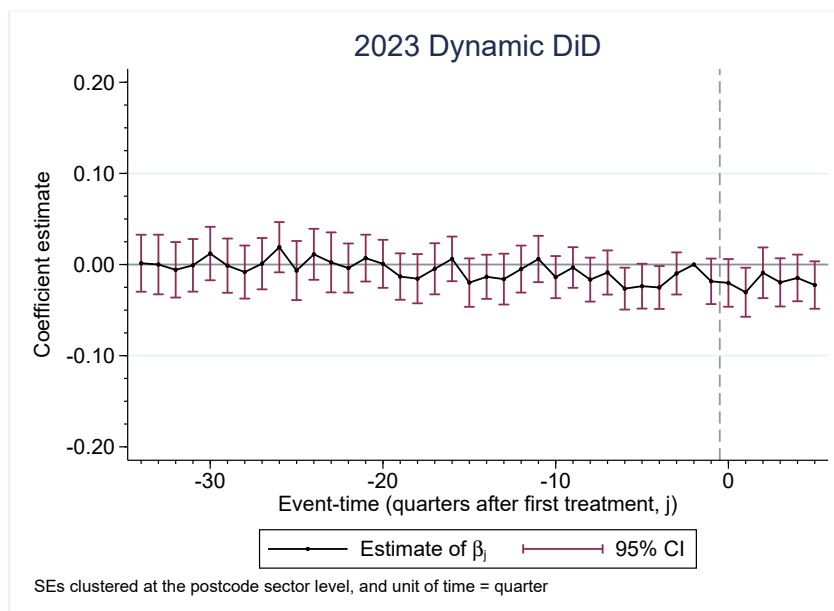


Figure 12: Coefficient plot from the dynamic DiD for the 2023 analysis zone individually



Appendix A.7 - Long-Distance Control Zone Maps

Figure 13: The control zone for the 2021 analysis zone, derived from k -means matching

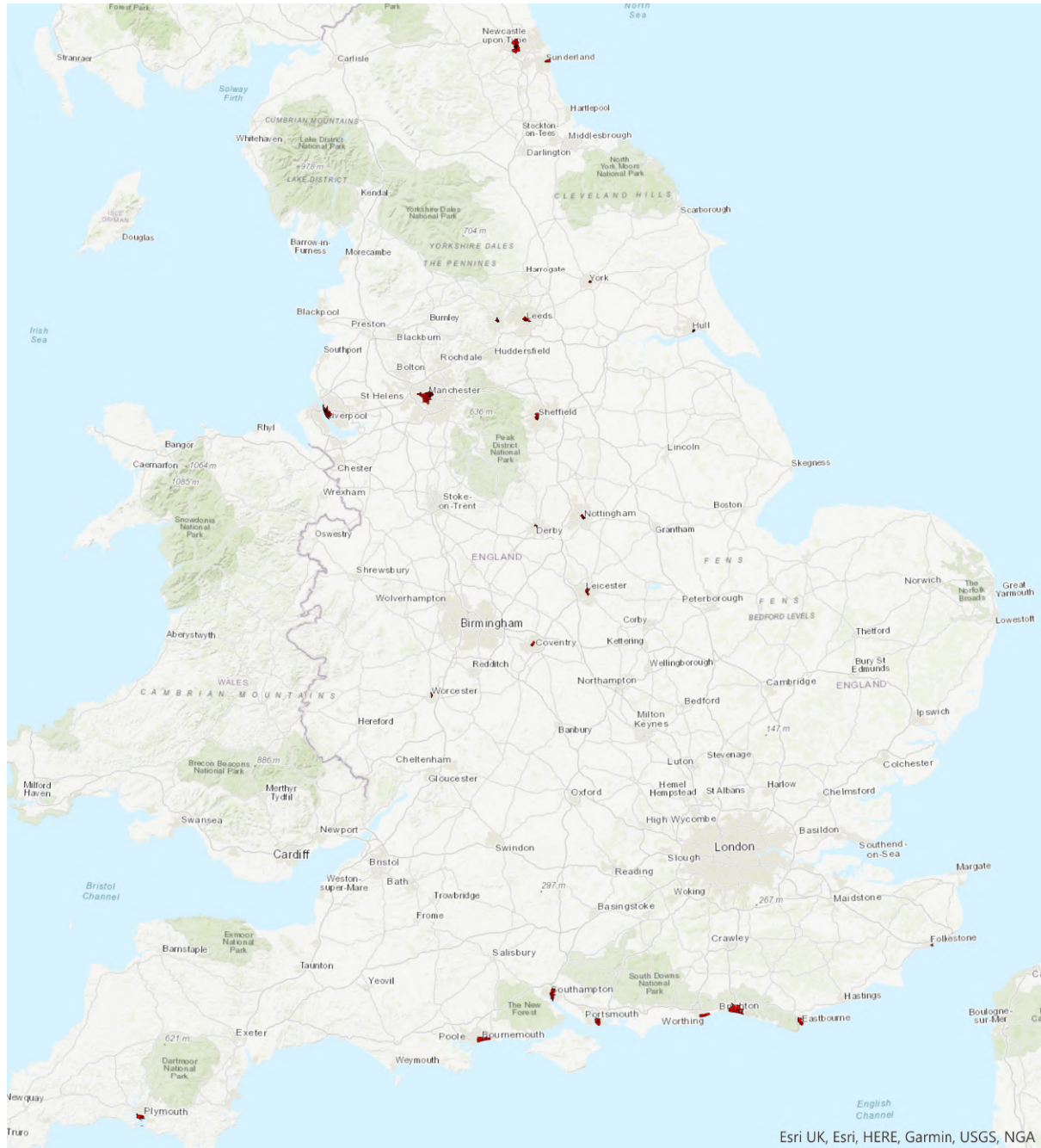
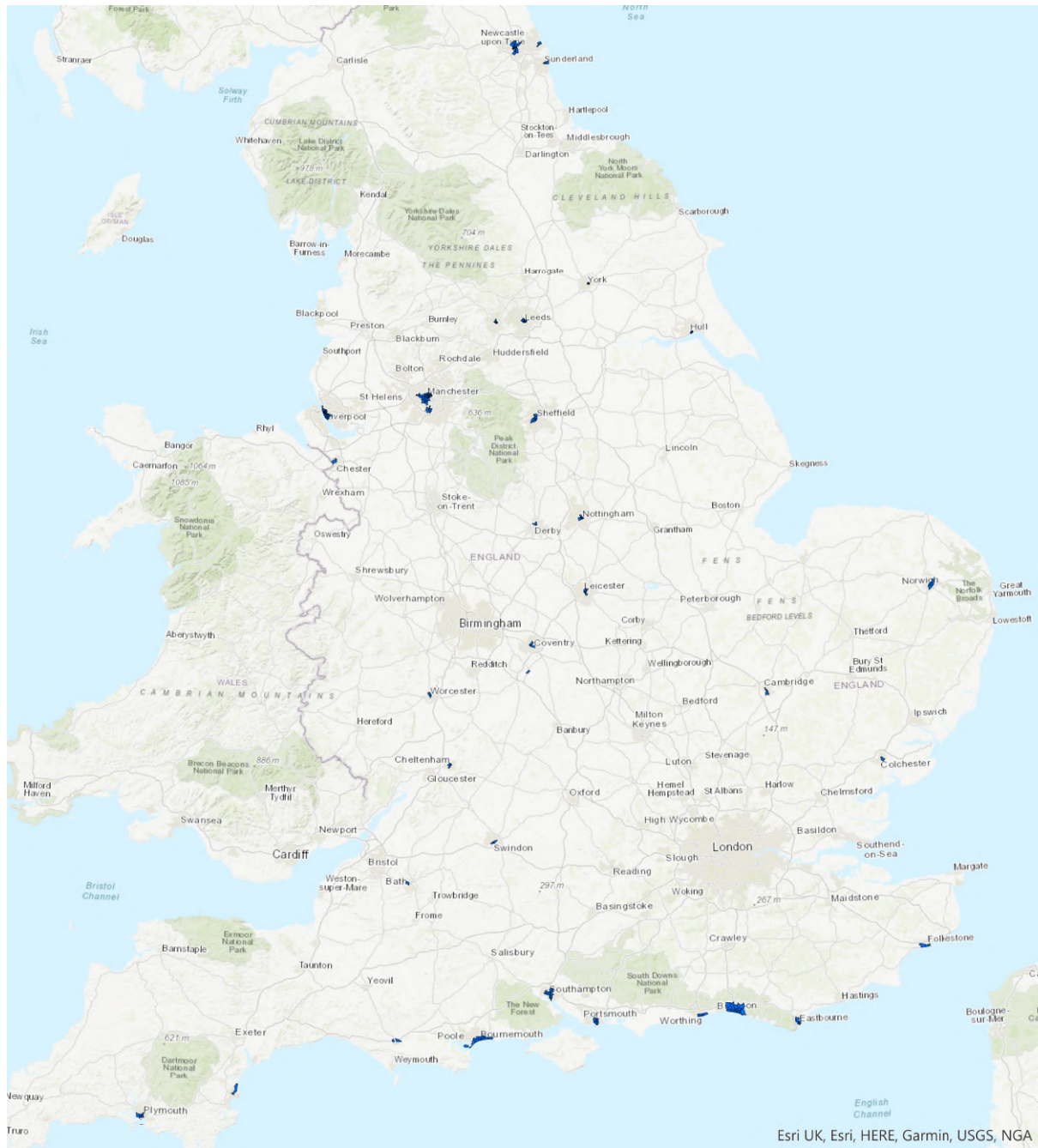


Figure 14: The control zone for the 2023 analysis zone, derived from k -means matching



Appendix B - Additional Figures

Figure 15: CDFs comparing the 2021 analysis zone with the control zone

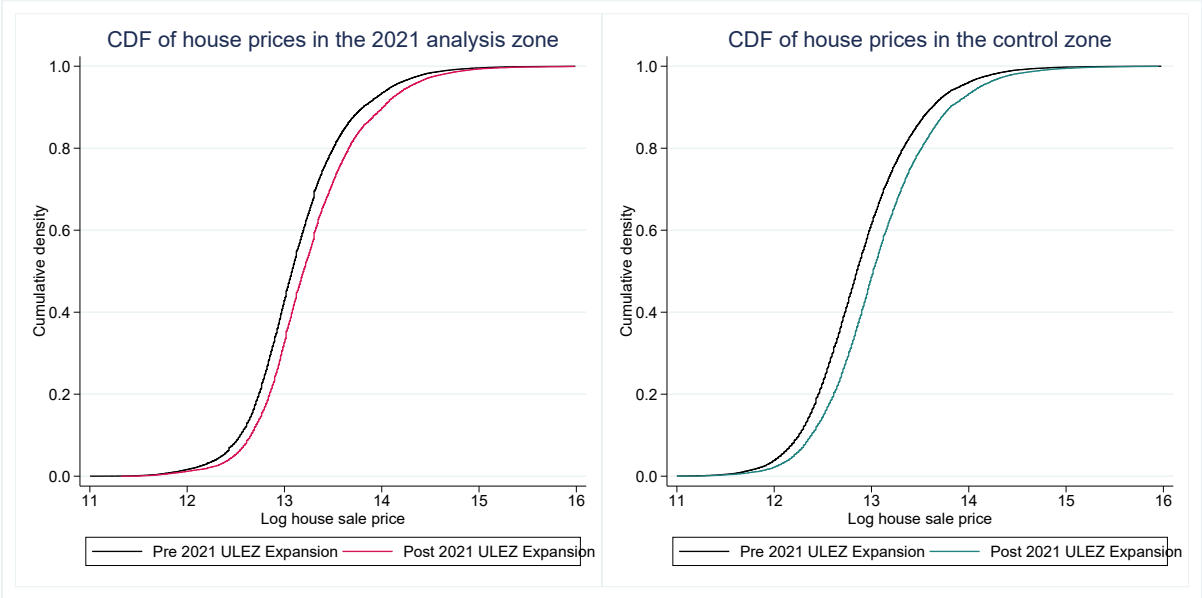


Figure 16: CDFs comparing the 2023 analysis zone with the control zone

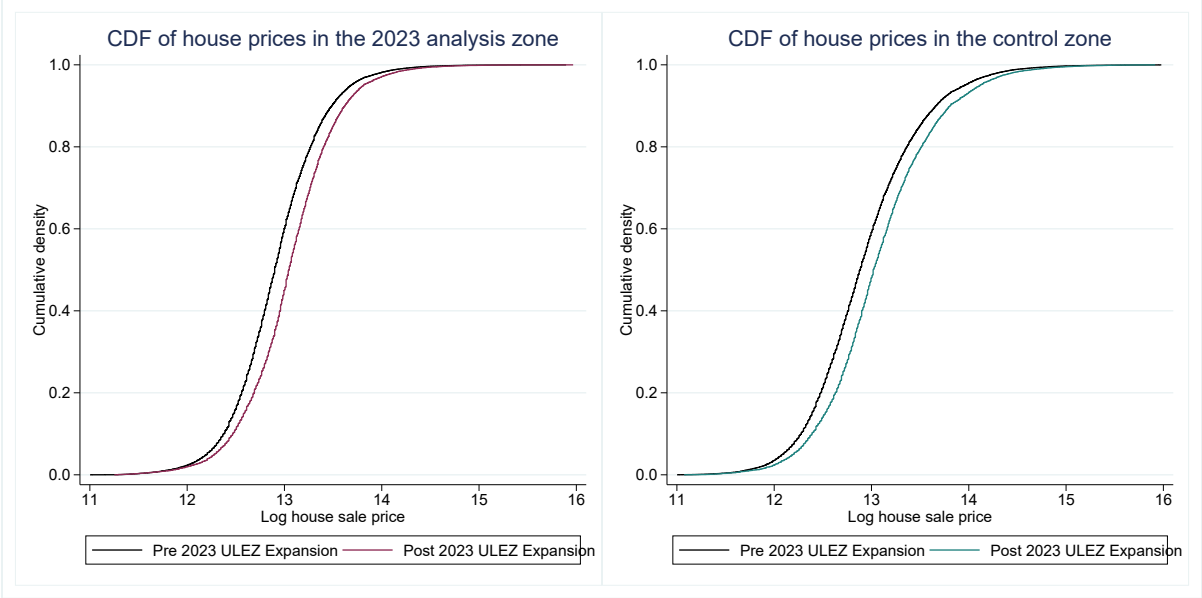


Figure 17: Histograms comparing the 2021 analysis zone with the control zone

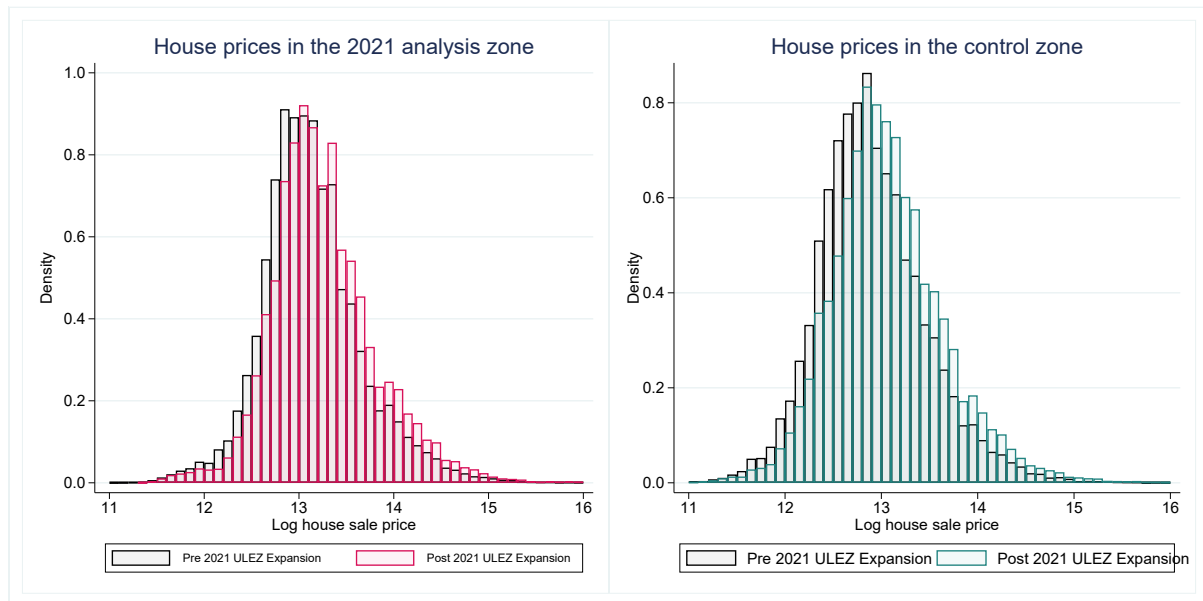


Figure 18: Histograms comparing the 2023 analysis zone with the control zone

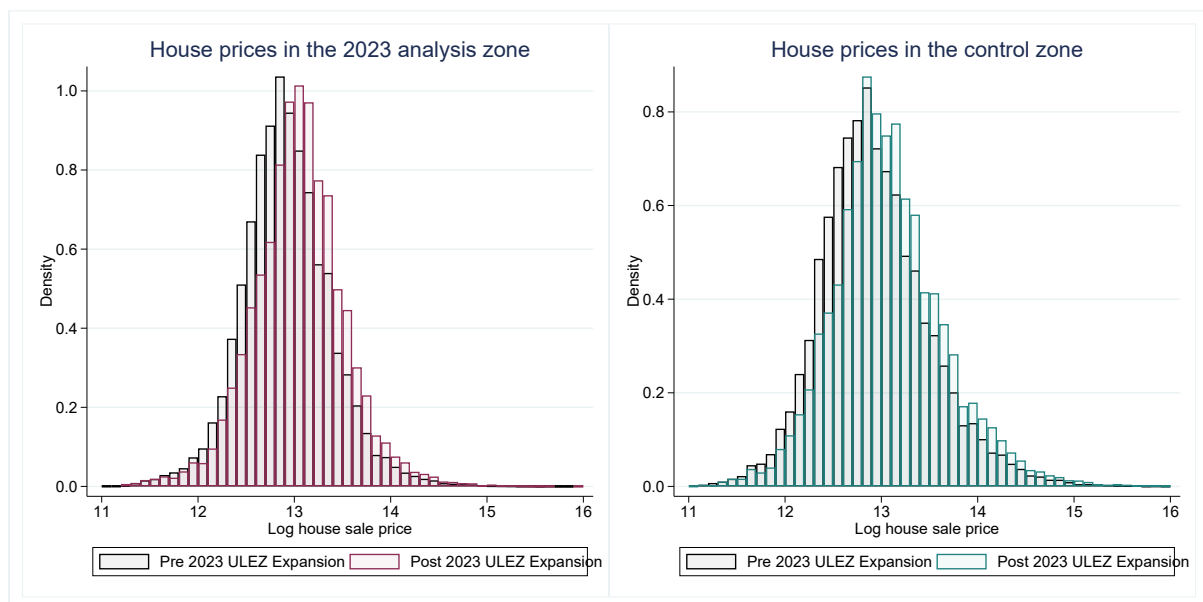


Figure 19: Evolution of the sale count in each zone

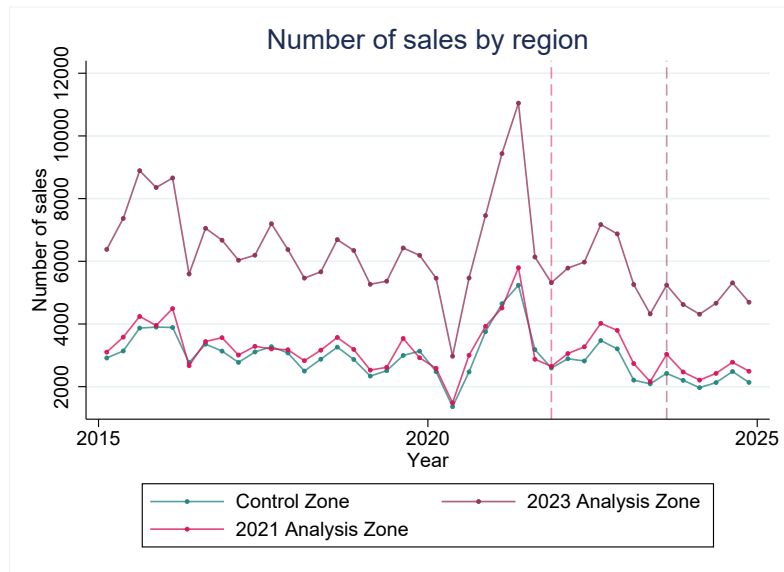


Figure 20: Evolution of the sale count in each zone, as a proportion of total sales in the sample period

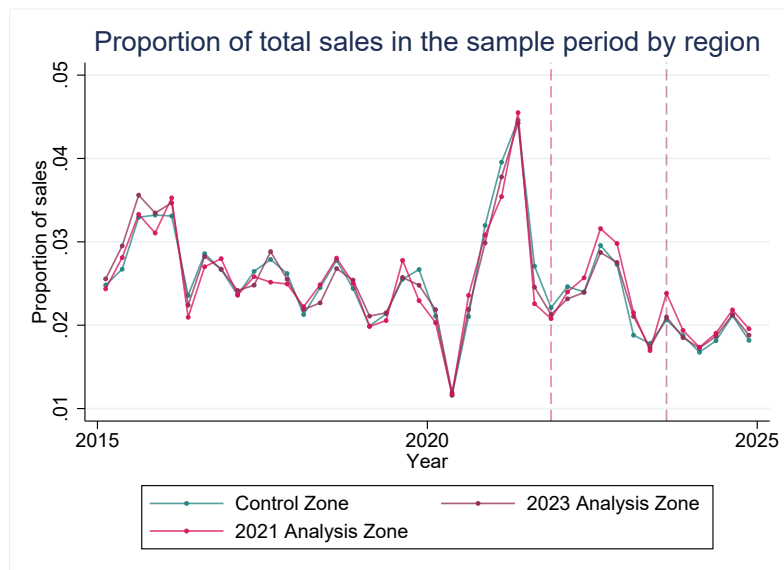
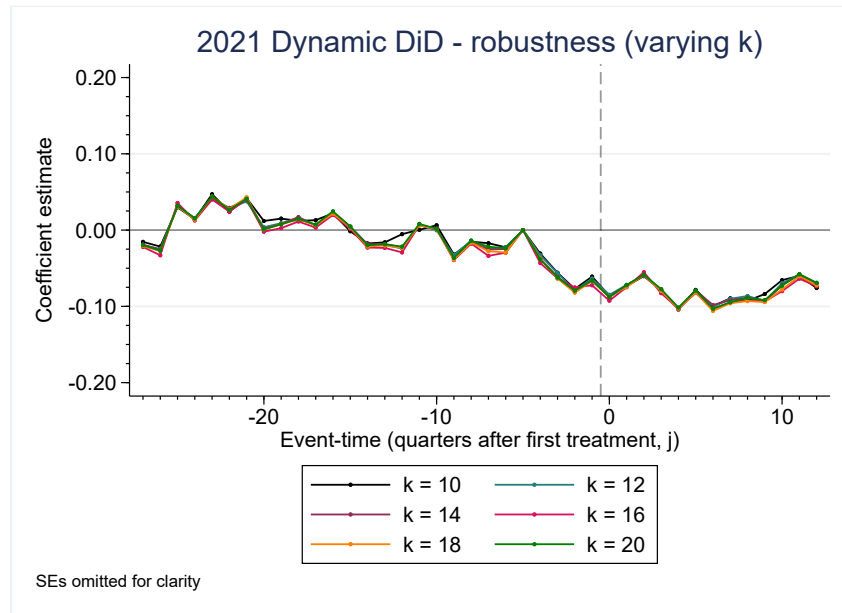
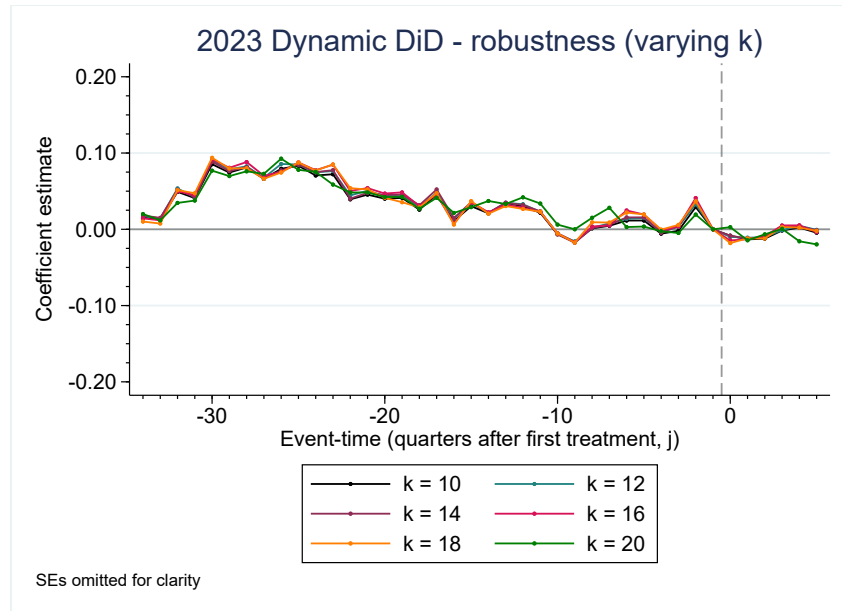


Figure 21: Robustness checks for k -means clustering: long-distance analysis results for various k ($k \in \{10, 12, \dots, 20\}$)

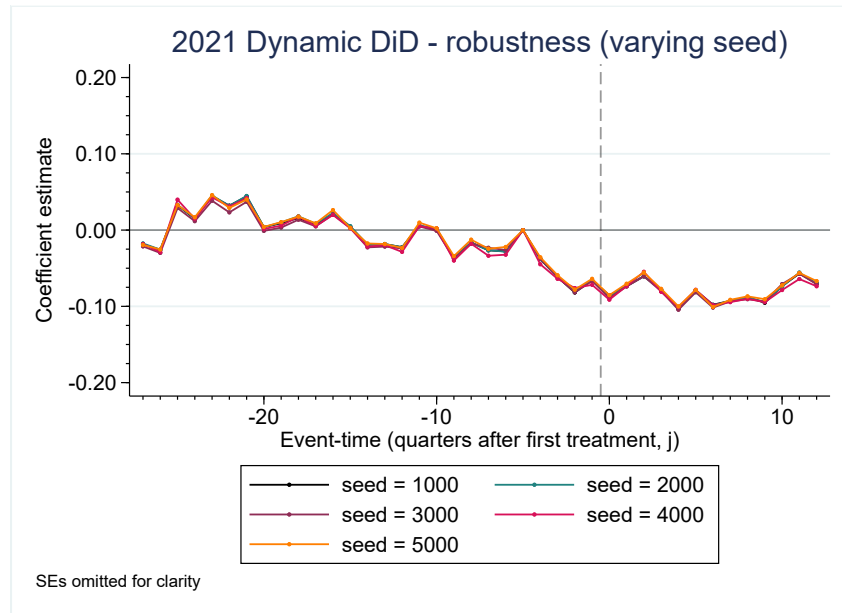


(a) 2021

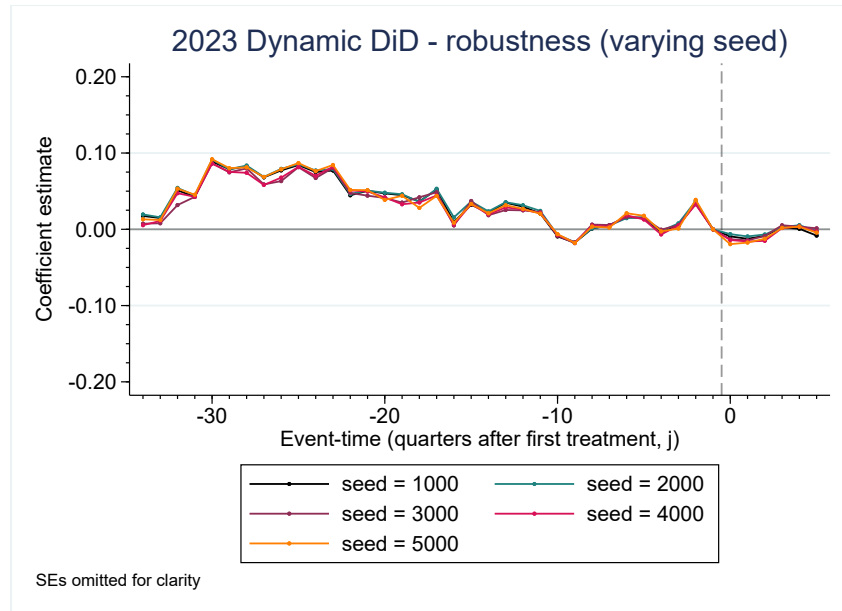


(b) 2023

Figure 22: Robustness checks for k -means clustering: long-distance analysis results for various initial allocations (seeds $\in \{1000, 2000, \dots, 5000\}$)



(a) 2021



(b) 2023