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Traffic Externalities and Housing Prices: Evidence from the London Congestion Charge

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Abstract
This study exploits the introduction of the London Congestion Charge (CC) that greatly improves traffic conditions in Central London to provide new evidence on the capitalization of traffic externalities onto housing values. The Congestion Charge restricts traffic into the cordoned area by imposing a flat fee on drivers whenever they enter during charging periods. I show that the introduction of the CC in the Western Extension Zone (WEZ) increases home prices by 3.68\% relative to untreated housing units within 1 kilometre away from the CC boundary. These estimates, which measures the marginal willingness to pay to avoid negative traffic externalities, are robust to many changes in specifications, suggesting that homeowners pay to avoid traffic so as to reduce commuting time, to enjoy better air quality and less traffic noise, and to travel on safer roads.

Keywords: housing prices, capitalization effects, congestion charge, traffic externalities, marginal willingness to pay, difference-in-difference
JEL Classifications: R21; R31; R38; R41; Q25

1 Introduction

Traffic congestion is an unintended consequence with agglomeration of economic activities, rapid expansion of cities and proliferation of auto-mobiles. This is particularly problematic in Central London as average on-road commuting speed in the 1990s was slower than that at the beginning of twentieth century before travelling on auto-mobiles become prevalent (Newbery, 1990). By 2002, traffic speed for motor vehicles during morning peak hours fell by almost 30% compared to that in 1974, from 14.2 to 10.0 miles per hour, and drivers spend, on average, 27.6% of their on-road time stationary (Department of Environment & the Regions, 1998). An effective way to mitigate traffic congestion is to explicitly tax road users (Pigou, 1924; Vickrey, 1963). Imposing a Congestion Charge (CC) that equates the marginal private and social cost of transport ensures that drivers incorporate external congestion externalities into their private cost. Tackling bottlenecks is imperative as congestion could affect economic growth (Boarnet, 1997; Fernald, 1999; Graham, 2007), employment (Hymel, 2009) and wage (De Borger, 2009) levels.

Despite its effectiveness, many cities, including Hong Kong, New York, Manchester and Edinburgh, find it hard to implement a CC due to strong resistance from the public, perceiving the charge as merely a tax increase. On 17th of February 2003, London[1] successfully implemented the system to control traffic into the city center. A flat £5 daily charge was enforced on private vehicles driving into the cordoned Congestion Charge Zone (CCZ) from 7:00am to 6:30pm on weekdays. The effects were immediate. Six months into implementation, the volume of private cars into Central London fell by 27% drop and average travelling speed was 20% higher than before (TfL, 2003a).

Alleviating congestion could affect residential location choices (Larsen *et al.*, 2008) as traffic generates negative externalities. Auto-mobile fumes are one of the key contributors to air pollution (Ernst *et al.*, 2003) and declines in traffic flow can enhance air quality and improve health outcomes (Currie & Walker, 2011; Knittel *et al.*, 2016). Cleaner air makes a neighbourhood more attractive and thus increases housing values (Palmquist, 1982; Chay *et al.*, 2005). Lighter traffic further allows residents to reduce commuting time and better transportation accessibility is associated with higher home values (Mohring, 1961; Boarnet & Chalermpong, 2001; Gibbons & Machin, 2005). For those who drive, less traffic could increase the ease of securing parking spaces and that could influence home prices as well (Van Ommeren *et al.*, 2011). Better traffic conditions also make roads safer by reducing accident or causality rates (Li *et al.*, 2012; Green *et al.*, 2016).

Given how traffic conditions affect the desirability of neighbourhoods, considerable attention has been paid on measuring the marginal willingness to pay (MWTP) to avoid negative traffic externalities using the housing market. An association between property values and some measures of traffic externalities, including traffic volume and

[1]Other cities that managed to introduce the CC include Singapore, Dubai, Milan, Stockholm, Gothenburg and Durham.

noise pollution, are established in the literature. In practice, it is extremely challenging to estimate these hedonic functions as traffic is not randomly distributed across space. Areas with the heaviest traffic conditions are often near the Central Business District (CBD), a foci of economic activities with peculiar neighbourhood characteristics. When differences between areas are unobserved or imprecisely measured, it could severely bias the hedonic estimates on the MWTP to avoid negative traffic externalities. Furthermore, it is difficult to select the correct function form for hedonic functions (Halvorsen & Pollakowski, 1981).

This paper provides new evidence on the capitalization of traffic externalities on housing values by exploiting a natural experiment to tackle these econometric issues. The analysis relies on the introduction of the London Congestion Charge, which leads to a sharp persistent reduction in traffic flow within the charged area. Estimation is based on a quasi experimental difference-in-difference approach to compare price changes for housing transactions inside the zone before and after the implementation of the CC with price changes of proximate unaffected housing units outside the zone. Exploiting the numerous housing sales within Central London, I restrict the analysis to transactions that are within 1 kilometre of the Congestion Charge Zone boundary to mitigate unobserved neighbourhood differences between housing units inside and outside the cordoned area. Analysis is conducted with postcode fixed effects to partial out any time-invariant unobservables within a building[2]. All the major events associated with the CC that could possibly lead to substantial changes in traffic conditions are examined. This include the initial implementation of Congestion Charge Zone (CCZ) in 2003 and Western Extension Zone (WEZ) in 2007, the subsequent hikes in 2005, 2011 and 2014, and the removal of WEZ in 2010 on housing prices. (Refer to Figure 2 for time-line of events)

Another aim of this paper is to evaluate the effectiveness of the Congestion Charge. Although highly unpopular and controversial, with many perceiving it as merely a tax instrument, the CC remains one of the most effective policy to discourage additional congestion (Anas & Lindsey, 2011). By explicitly taxing drivers using tolls, households incur higher commuting cost and this induces them to relocate nearer to the city centre (Anas & Xu, 1999; Anas & Rhee, 2006). Demand for land in the city centre drives up land prices that encourages more efficient use of space in the centre (Solow, 1973; Wheaton, 1998). Other methods, such as increasing fuel taxes and constructing new roads, might not be able to achieve their intended aims. Increasing fuel taxes could induce drivers to switch to more fuel efficient vehicles - hybrid or electric cars - instead of deterring them to drive. Building more extensive road networks could exacerbate congestion by making locations more accessible by car and driving more pleasant. Thus, it is imperative to evaluate whether the CC is able to achieve its intended aims.

There are, however, still several challenges towards identifying the marginal willingness to pay (MWTP) to avoid negative traffic externalities. First, the estimates of the CC on home prices could be capturing more than the

[2]On average 17 housing units share one postcode in United Kingdom. These units are either in the same block, development or are very close to one another.

MWTP to avoid traffic as residents in the zone enjoy a 90% discount on the charge. I disentangle the willingness to pay for the waiver of the CC by examining price changes for a sub-sample of properties that are entitled to the concession despite being located outside the zone. Secondly, there could be heterogeneity across individuals in the tastes for better traffic (Small *et al.*, 2005). Higher income earners, who incur a larger time-delay externality from congestion, could be more incentivised to relocate into the charged zone. Sorting of higher income households into the zone could lead to "better units" within the postcode being sold after the CC implementation. I test this assumption with a battery of balancing tests for a sub-sample of transactions from the Nationwide database that entails a larger set of housing characteristics (such as Floor Area, Number of Bedrooms, Age etc. See Table 3 for more details).

I find that homeowners moving into the cordoned charge zone pay more than those buying homes near but outside the zone to avoid negative traffic externalities. After the implementation of the CC in the Western Extension Zone (WEZ), homeowners pay 3.68% (£29,783) more for homes in the cordoned area relative to untreated units 1 kilometres from the CC boundary to enjoy a decrease in traffic by 3.58%. This finding is robust across a range of alternative specifications and falsification tests that address the challenges towards the identification strategy. The estimates are not spuriously driven by the CC discount and change in the composition of sales. These estimates, although much larger than those reported previously, are credible considering the array of benefits associated with better traffic.

The remainder of this paper is structured as follows. Section 2 provides an overview on the Congestion Charge in London. Section 3 describes the existing literature on this subject. Section 4 outlines the data used in this paper and Section 5 illustrates the identification strategy for this paper. Findings will then be discussed in Section 6 and Section 7 concludes the study.

2 Road Pricing in London

The initial Congestion Charge Zone (CCZ[3]) covered a total of 21 square kilometres (slightly more than 1% of the Greater London Area) and encompassed the financial centre (Bank), parliament and government offices (Palace of Westminster), major shopping belts (Oxford Circus) and tourist attractions (Trafalgar Square, Westminster Abbey, Big Ben, St Paul Cathedral etc). Figure 1 depicts the CCZ, the area shaded in orange and enclosed by the red dashed line. The boundary was drawn to isolate the most congested areas in Central London. The charged zone was not demarcated by any physical features (such as railways, green spaces and rivers etc). It was bordered by major Inner Ring Roads such as Edgeware, Vauxhall Bridge, Pentonville, Park Lane, Marylebone, Tower Bridge and Victoria

[3]The initial Congestion Charge Zone will be abbreviated as the CCZ while the Western Extension Zone will be abbreviated as WEZ from this point onwards

to divert traffic displaced by the charge. Commuters travelling on these roads are not required to pay unless they turn into the zone. To protect residents and businesses outside the zone, off-street parking enforcement is improved to deter anyone from parking outside and walking into the cordoned zone to avoid paying the charge. The CCZ crosses the River Thames to the South and covers parts of the Lambeth and Southwark boroughs. Although this is an area not typically considered as Central London, it was incorporated for the ease of implementation and operation (Richards, 2006).

On the 17th of February 2003, a flat fee[4] of £5.00 was levied on commuters driving into the zone between 7:00am to 6:30pm from Monday to Friday, excluding public holidays. Residents living within the zone and some living outside but in proximity to the zone were entitled to a 90% waiver[5] to the CC for their first registered vehicle. Buildings in area shaded in grey (purple striped from the direction of North-East to South-West) outside the CCZ in Figure 1 were entitled to resident discount during which the original CCZ (WEZ) is in operation. This policy was an outcome of extensive consultations with various stakeholders. Other than reducing congestion, another aim of the CC is to generate revenues to improve the public transport system by increasing the frequencies and routes of buses and tube. Reduced travel time and enhanced reliability could encourage commuters to switch from private to public transport when commuting into the zone.

The tax levied was substantially increased to £8.00 on the 4th July 2005 to further reduce traffic and raise revenues. On the 19th of February 2007, charging was extended to Central West London (known as the Western Extension Zone - WEZ) because of congestion in that area. Operating hours of the CC were reduced by half an hour from 7:00am to 6:00pm. The westward extension is circumvented by Harrow Road, Scrubs Lane, West Cross Route, the Earls Court One-Way system, Chelsea Embankment and the River Thames [6] to the South. Refer to the area in pink-striped (in the direction of North-West to South-East) in Figure 1. However, under tremendous pressure from residents living in West London, on the 24th of December 2010, the WEZ was scrapped. Between 2011 to 2015, the charge in the original CCZ underwent another two hikes. The CC was raised from £8 to £10 on the 4th January 2011 and from £10 to £11.50 on 16th June 2014. Overall, the CC experienced an average 10.83% growth per annum since introduction and this might have a compelling effect on commuters relying on private transport. For a time-line for all the CC events refer to Figure 2.

[4]The rationale for levying a flat fee, other than the difficulty in imposing time varying fees to reduce congestion during peak hours, is that vehicular volume on roads seem fairly uniform across the day.

[5]Other groups excluded from the charge include public transport(taxis and buses), motorcycles, bicycles, environmentally friendly vehicles (battery powered or hybrid cars), vehicles driven by disabled individuals (blue badge holders), vehicles with 9 seaters or more and emergency service vehicles.

[6]Unlike the Original CCZ, the WEZ is bounded by physical features. There is a concern whether the WEZ dividing different neighbourhoods across the river. Hence, I limit the analysis to transactions the north (refer to Panel D of Table6). It has an immaterial effect on the results.

Initial impact assessment by Transport for London (TfL) showed that the implementation of the CC in 2003 is effective in alleviating congestion. These results are very consistent with those reported in this study. After implementation all day travel speeds were almost 20% higher (from 14.3km to 16.7km per hour), minutes of delay fell by 30% compared to uncongested traffic conditions and queue time at junctions decreased (TfL, 2003a). This was largely attributed to a 27% overall drop in the number of private auto-mobiles into Central London. A change in composition of inbound traffic into the zone was observed. There was an increase in the volume of bicycles (28%), buses (21%) and taxis (22%) after the CC is implemented. Surveys conducted echoed similar findings with the majority of the drivers switching to public transport and others choosing to travel during off-charging hours (TfL, 2005). Though the number of commuters using rail did not increase, a 38% increase in the number of bus passengers during morning peak periods was documented (TfL, 2004). There was no apparent displacement of traffic into neighbouring uncharged roads and weekends as traffic conditions were fairly similar compared to those during pre-charged periods. Improved traffic conditions reported by the TfL are consistent to those documented in this study. Finally, as for air quality, the implementation of the CC led to a 12% reduction in both NO and PM_{10} compared to pre-charging levels in the cordoned area (TfL, 2004).

3 Literature Review

To estimate the marginal willingness to pay (MWTP) to avoid negative traffic externalities, a hedonic property value approach is widely adopted in the existing literature. An association between traffic externalities, measured by traffic volume (Hughes & Sirmans, 1992; Kawamura & Mahajan, 2005; Li & Saphores, 2012; Larsen, 2012) or noise (Palmquist, 1992; Wilhelmsson, 2000; Andersson *et al.*, 2010; Swoboda *et al.*, 2015), and housing prices are established using regression adjusted for differences in observable housing and neighbourhood characteristics. For a review of the literature, refer to Bateman *et al.* (2001) and Nelson (2008). A meta-analysis of previous literature suggests that the doubling road traffic volume, and every decibel increase in traffic noise, is associated with average 0.5%-3.0% and 0.3%-0.6% reduction in transaction prices respectively. Estimates, however, appear to vary across studies that adopt different specifications. Perverse relationships are sometimes documented (Day, 2003; Day *et al.*, 2006). This highlight whether these cross-sectional estimates are biased due to omitted variables that are not observed but are correlated with traffic conditions. One approach recommended by Palmquist (1992) is to constraint the analysis to a single neighbourhood to attenuate unobserved differences between housing transactions. However, a lack of variation in traffic within a localized area could impede the identification of traffic externalities on housing prices.

To recover the MWTP for non-market amenities using the housing market, many studies address the issue of omitted confounders by focusing on "natural experiments" or by identifying some political or administrative

boundaries that lead to a discontinuity in the level of amenities conferred [7]. Chay *et al.* (2005) rely on the implementation of the Clean Air Act in the 1970s to identify exogenous variation in air quality and its impact on housing prices. Davis (2004) measures the MWTP to avoid the risk of cancer by comparing house prices from repeated sales of a secluded county in Nevada that experienced a sharp rise in paediatric leukaemia cases with home prices from a neighbouring county. Gibbons & Machin (2005) compute the MWTP for public transport accessibility by comparing house price changes near to stations before and after a new rail line is constructed with similar housing units further away. Making use of the variation of school quality between school districts, Black (1999) and Gibbons *et al.* (2013) compare home prices between proximate homes that share similar neighbourhood characteristics but are on different school districts to identify the MWTP for good schools. In similar fashion, this study utilizes a hybrid of "natural experiment" and boundary discontinuity to identify the MWTP to pay to avoid negative traffic externalities. The CC ensures a discrete shock only to traffic conditions within the cordoned area. Using a difference-in-difference strategy for transactions close to the CC boundary, I exploit the variation in traffic (1) before and after the charge is implemented within the zone and (2) between proximate areas across the CC boundary. This partials out the time invariant unobservables within postcode and attenuates time-variant unobserved neighbourhood differences across the boundary.

The important question is whether the Congestion Charge is effective in reducing traffic jams. Numerous papers, on top of the reports by TfL, provide evidence on this. Considering that automobile traffic is one of the main contributors to air pollution, many studies show that air quality inside the cordoned area improved after the charge is implemented (Beevers & Carslaw, 2005; Tonne *et al.*, 2008; Atkinson *et al.*, 2009; Percoco, 2015). In particular, Beevers & Carslaw (2005) highlight that levels of CO_2, NO and $PM10$ fell by 19.5%, 12% and 11.9% respectively relative to the levels before the CC is implemented. Similar results are echoed by Percoco (2015) with larger effects associated with the WEZ. His results, however, suggest that due to displacement of traffic, air quality is poorer outside the charge zone. Roads in the zone are also reported to be much safer after the CC is introduced due to less traffic. Li *et al.* (2012) show that car casualties fell by 5.2% although two wheelers related casualties experienced a 1.8% and 13.5% hike for motorcycle and bicycles respectively compared to pre charging levels. They suggest that this is largely due to the switch in transport mode to two wheelers to avoid the charge. Larger effects are observed in Green *et al.* (2016). The CC coincides with a 32%-36% decline in accident counts and a 25%-35% drop in casualty counts relative to the pre-treatment periods. No evident displacement of accidents to neighbouring areas are documented.

In view of the benefits associated with the CC, several studies attempt to quantify these welfare gains using the housing market. Most of the studies have documented insignificant or perverse effects. The closest to this study is unpublished research conducted by Zhang & Shing (2006), who examine the effect of the CCZ in 2003

[7] For the advantages associated with quasi-experimental approaches of hedonic methods for environmental valuation refer to Kuminoff *et al.* (2010).

on property prices in London. Using a DID approach for transactions from 2000 Q1 to 2006 Q1, they show that property prices inside the zone fell by 8.5% when compared to outside. Using a similar approach, Percoco (2014) investigate the effect of the Milan EcoPass on housing prices. His study is restricted to a small sample of 764 annualized property prices averaged at 192 Micro-zone (OMI) level between 2006 and 2009. The headline finding is that housing prices fell by 1.2% to 1.8%. The contradictory relationship documented in both studies could stem from omitted confounders[8] due to the lack of controls, the incorporating of transactions/neighbourhoods fairly far from the treatment boundary and the adoption of coarse spatial fixed effects. Agarwal *et al.* (2015) improve the estimation by conducting the analysis at a finer spatial scale with postcode fixed effects. They examine the effects of an increase in the Singapore Electronic Road Pricing (approximately £0.50) on retail, office and residential prices. While retail property values are adversely affected by the hike, residential property values remain unchanged. This is anticipated considering that an immaterial hike is unlikely to induce tangible changes to traffic that can influence housing prices[9]. Their paper, however, is constrained to a small sample of transactions in the cordoned area: 316 out of 15,212 transactions.

In contrast, this paper contributes to the existing literature on several fronts. Other than utilizing the London Congestion Charge as a natural experiment to address the issue of omitted confounders, this study employs a richer dataset of more than half a million housing transactions in the vicinity of the CCZ/WEZ, with more than 15% of the transactions from almost 8,000 different postcodes in the charged zone. Other than hikes, this study also examines the effects of the implementation of the CC in the CCZ and WEZ that could induce larger variation in traffic conditions. Thus, it is more likely to capture any price effects if there are any present compared to previous studies. Further, I examine whether the charge improves traffic conditions using annual road level traffic data in Central London. This is not only an important 'first stage' that justifies the use of the CC to identify shocks to traffic, but also sheds light on the effectiveness of the CC in reducing congestion. This is something not addressed in the previous literature due to the data limitations.

4 Data

Information on the boundaries of the CCZ and WEZ and the areas entitled to 90% resident discount are based on the shape-files provided by Transport of London (TfL). Using Geographic Information Systems (GIS) mapping, together with the official dates of implementation/announcement of the CC from TfL, I assign postcodes into treatment and control groups and compute euclidean distance of each postcode from the CC boundary. Further information on the locations of tube stations and bus stops, which measures public transport accessibility for each property,

[8]In fact, a similar relationship is documented in weaker specifications in this study (refer to Columns 2 and 3 of Table 4).

[9]This point is proven in Table 7. Most of the CC increments, except for the initial hike in 2005, do not have a perceptible effect on reducing traffic into Central London.

are retrieved from TfL Open data source. Distances of each property from public transport nodes are also computed using GIS. Data on the average annual traffic flow at each count point (CP) from 2000 to 2014 is retrieved from Department of Transport (DfT). Count points are monitoring stations along the roads that provide junction-to-junction street level traffic flow. There are a total of 2,774 CPs in London, most of them clustered around Central London.

Housing transactions from the 1st quarter of 2000 to the 4th quarter of 2015 are collected from Land Registry database. Property characteristics include sale price, property type (detached, semi-detached, terraced, flat or maisonette), tenure (leasehold or freehold) and whether the property is new or second-hand. Land Registry covers all the transactions made in United Kingdom over the sample period. Furthermore, considering that terrace and flat housing make up most of the transactions in Central London (close to 95% of the transactions within 5 kilometres from the CCZ), other property types are removed to reduce heterogeneity in the sample that could raise endogeneity concerns. All the transactions are geo-coded using the address postcode. For a subset of transactions, more property information, such as floor area, number of bathrooms and bedrooms and age, are merged from Nationwide transaction database for balancing and robustness test.

Demographic and neighbourhood characteristics at Output Area (OA) level are collected from Census 2001 and 2011. Controls, which measure the quality of neighbourhood for each property, include the percentage of (1) minority residents and (2) uneducated residents, (3) unemployment rate and the percentage of (4) lone parent households. Data from Census 2001 are merged with transactions that take place before 2006, while data from Census 2011 are merged with transactions that happen post 2006 based on the OA each property is located.

Shape files detailing the location of heritage buildings and parks are provided by MAGIC [10]. Using GIS, distances of each postcode from the nearest Grade 1 park - top 10% of all U.K parks with international and historical significance - is measured. For heritage buildings, a 200 meter is drawn for each postcode and the counts of Grade 1 heritage buildings are computed. Designation is done by Historic England and is determined by the age, historical and architecture significance of the building. Only the top 2.5% of the buildings are classified as Grade 1. Maps for Thames River is obtained from Digimap. Like before, a buffer of 200 meters is drawn from Thames River and postcodes inside this area are assumed to have a river view. Proximity to these features are expected to influence home prices (Ahlfeldt & Maennig, 2010; Bolitzer & Netusil, 2000; Lansford Jr & Jones, 1995).

5 Identification Strategy and Methodology

A postcode fixed-effects difference-in-difference estimation method is adopted in this study. The strategy is to compare the average change in housing prices in the CCZ/WEZ before and after the implementation of the CC

[10] For more information, refer to http://magic.defra.gov.uk/.

with the average change in housing prices in areas outside but near to the CC boundary. The baseline specification is:

$$ln(price_{ijkqt}) = \alpha_j + \gamma \mathbf{CC_{it}} + (\pi'_k * Year_t) + X'_{it}\phi + (W'_j * Year_t)\omega + (Qtr'_q * Year_t) + \varepsilon_{ijkqt}, \quad (1)$$

where $ln(price_{ijkqt})$ is the natural logarithm of the price paid for property transaction i in postal code j located at wedge k sold in quarter q of year t. The main variable of interest is $\mathbf{CC_{it}}$, which is a binary variable taking the value of one if property i is located in the CCZ/WEZ and is sold after the CC event in time t and zero otherwise. In this paper, a total of 6 events are examined. They are (1) the initial implementation of the CC in 2003 ($CCZ2003\_2005$); (2) the CC increase in 2005 from £5 to £8 ($CCZ2005\_2011$); (3) implementation of the WEZ in 2007 (WEZ); (4) the CC increase in 2011 from £8 to £10 ($CCZ2011\_2014$); (5) removal of the WEZ in 2011 ($RemWEZ$) and (6) the CC increase in 2014 from £10 to £11.5 ($CCZ2014$).

α_j is postcode fixed effects to partial out time-invariant characteristics at postcode level. This is to mimic repeated sales analysis, assuming that properties in the same building or very close together are fairly similar. π'_k is a set of 8 cross-boundary wedge fixed effects interacted with year dummies ($Year_t$). These wedges are created by drawing a circle from the centroid of CCZ/WEZ before splitting it into 8 equal areas[11]. See Figure 3 for illustration. Conceptually, the inclusion of these wedge dummies allow for house price trends to vary non-linearly across time at these smaller geographical areas to prevent the key estimates from capturing spurious local price trends. X'_{it} is a set of housing characteristics denoting whether property i is a new build, a terrace house and whether the tenure is leasehold. W'_j is a vector of time-invariant location and neighbourhood characteristics that vary at postcode j. Location controls include (1) counts of buildings with heritage value within 200 metres; (2) distance from the nearest 'Grade 1 Park' and (3) Thames River view (if within 200 metres from Thames River). Neighbourhood characteristics, which measures the attractiveness of the neighbourhood, include the percentage of (1) lone parent households, (2) unemployment rate, the percentage of residents of (3) minority race and (4) without education qualifications. All the time-invariant controls are interacted with year dummies. ($Qtr'_q * Year_t$) are year-quarter fixed effects. Table 1 summarizes all the different variables used in the analysis.

[11]The reason for creating these cross boundary wedges is because there are no statistical boundaries that crosses the CC boundary.

The main identification challenge is to address the problem of omitted confounders. Other than focusing on the CC to induce a shock to traffic conditions within the charge zone, I restrict the analysis to transactions within 1 kilometre from the CC boundary. The rationale of doing so is because the CC boundary is drawn to contain an area with the heaviest traffic. Non-random assignment of the CC treatment meant that houses in the cordoned zone are probably in neighbourhoods different from those elsewhere. Restricting the analysis to proximate treated and untreated units in similar neighbourhoods near to the boundary mitigates the risk of time-variant unobservable neighbourhood differences from driving a spurious correlation between the CC and housing price changes (Refer to Figure 3). If unobserved characteristics vary smoothly across space, Conditional Independence Assumption (CIA) is likely to hold as the distance between treated and untreated locations fall (Duranton *et al.*, 2011), supporting the identification strategy.

Another key identification assumption for the DID approach is parallel trends i.e housing units inside and outside the CCZ/WEZ experience similar pre-treatment price trends. To illustrate that, a pretreatment year-quarterly property price indices from 1995 Q1 (Base=100) to 2002 Q4 for transactions inside and outside the CCZ/WEZ is computed. Furthermore, to avoid capturing the overall upward house price trends over time, I restrict the analysis to sales that close to the treatment date (Refer to Figure 4 for more details). [12]. Extending the treatment window increases the risk of unobserved neighbourhood changes occurring post CC implementation from biasing key treatment estimates. To ensure that untreated postcodes are unaffected by the previous CC events, properties located within the WEZ are omitted from the CCZ regressions and vice versa.

By specifying the fixed effects at the postcode level, one assumption made is that within a building there is no variation between units in terms of unobserved property characteristics. This assumption, however, could be violated if some units are built differently to cater to heterogeneous consumer preferences. For instance, a penthouse could be bigger than other units in the same development. This concern is whether the properties sold after the CC is introduced is different from those sold before. If higher income households are to self-select into the cordoned area after the CC is introduced because they incur higher delay cost from congestion, better housing units could be sold. Differences in the quality of housing units sold within postcode over treatment period could bias the CC estimates. This could be problematic given the limited information on housing characteristics from the Land Registry database. Hence, to test the validity of the research design, I conduct a battery of balancing tests for a subsample of properties from Nationwide sales database with a richer set of hedonic characteristics. A specification

[12] The reason for stratification is that when I combine the various CC events into one regression (results available upon request), point estimates of the latter CC windows ($CCZ2014$) are almost always bigger despite having an immaterial effect on home prices in stratified analysis. This is even after I include year quarter dummies to partial out property time trends, suggesting that the time dummies are not able to disentangle the market trends from treatment estimates.

similar to equation (1) is employed but the dependent variable is replaced with the different hedonic characteristics. The aim is to find out whether the properties sold after the CC is implemented differ in property characteristics from those sold before.

Finally, there is a concern whether estimates of the CC on home prices could be confounded by other benefits conferred by the charge. Since homeowners living in the zone are entitled to a 90% waiver to the charge, homeowners could be paying more for their homes in the zone to enjoy this benefit. To investigate whether this is driving the estimates, I exploit a policy in London that permit some residents living near but outside the zone entitled to a 90% reduction to the charge. They are exempted because road networks force them to bypass the CCZ/WEZ to reach their homes. I augment equation (1) with an additional variable $Discount_{it}$ that denotes houses in the discount zone that were sold after the implementation of the CC (See Figure 1 for more information). The following specification is adopted:

$$ln(price_{ijkqt}) = \alpha_j + \gamma \mathbf{CC_{it}} + \theta \mathbf{Discount_{it}} + (\pi_k' * Year_t) + X_{it}'\phi + (W_j'\omega * Year_t) + (Qtr_q' * Year_t) + \varepsilon_{ijkqt}, \tag{2}$$

If homeowners do not pay more for the 90% discount for the CC, I should expect θ to be statistically insignificant. However, given that home owners living in this area are very close to the CCZ/WEZ, they might benefit from better traffic conditions[13]. If home owners are doing so, this could attribute to a upward bias to θ. To find out if the charge do affect traffic conditions in the discount zone, I re-estimate equation (2) with traffic volume as the dependent variable. However, as long as θ from equation (2) is insignificant, it is safe to say that the discounts for the CC are not driving the key estimates.

6 Empirical Results

6.1 Descriptive Statistics

Basic summary statistics computed for a sample of housing transactions within 5 kilometres of the CCZ/WEZ boundary from 2000 to 2015 are detailed in Table 2 Panel A. There are 557,631 transactions from 39,174 different property-postcodes. This means that, on average, there are around 14 repeated sales for each postcode (=557,631/39,174). Approximately 15% of the sample (84,509 transactions from 7,941 postcodes) took place within the cordoned area

[13]Consider the case of a one way route towards homes in the discount zone that forces homeowners to bypass the CCZ/WEZ whenever they drive out. If they are commuting towards the charged zone, they could enjoy better traffic conditions. However, they are not likely to benefit from better air and less traffic noise as they are located closer to the Inner Ring Roads that carries detouring vehicles avoiding the charge.

(for both the CCZ and WEZ) after the implementation of the CC[14]. Majority of all the sales in the estimation sample are flats (79%) or terraces (17%). This justifies the restriction of the analysis to these property types to reduce sample heterogeneity.

Panel B of Table 2 summarizes the difference in means for the various housing and location characteristics between sales in the CCZ/WEZ with those outside pre (2000 - 2003) and post implementation of the CC (from 2004 onwards). I streamline the sample from 5 kilometres to 1 kilometre from the CC boundary. The rationale is to observe if differences in observable characteristics disappear to support the identification strategy of restricting transactions just in and out of the CCZ/WEZ. A few notable observations can be made. Properties sold in the CCZ/WEZ are more likely to be flats with leasehold tenure. They are also nearer to parks, historical buildings with architecture value, and are less likely to have river front view, justifying the inclusion of such location controls into the analysis. Housing units on opposite sides of the boundary become more similar, in terms of both location and property characteristics, as the sample is reduced to units that are closer to the CC boundary. This is reflected by the diminishing differences moving from left to right of the table[15]. The differences appear rather consistent before and after the implementation of the CC, indicating that the composition of sales are fairly identical across the treatment period.

Next, to show that treated and untreated properties in proximity to the CC boundary have similar price trends, I compute a pre-treatment repeat sales year-quarterly property price index for both the CCZ and WEZ. Pre-treatment period for the CCZ is from 1995 Q1 (Base=100) to 2002 Q4 and for the WEZ is from 2001 Q1 (Base=100) to 2006 Q4. Plotted estimates in Figure 5 are the coefficients of the quarterly year dummies from a regression similar to that in equation (1) other than the omission of the treatment variable. The aim is to partial out other factors that could affect market trend estimates. Dashed line represents price trends for properties in the cordoned area (CCZ & WEZ) while full line represents properties outside but within 5 kilometres of the CC boundary. Property price trends are very similar during the pre-treatment period between properties inside and outside the boundary for both the CCZ and WEZ, ameliorating concerns of violating the parallel trend assumption.

[14]The sample in the CCZ/WEZ is largely unchanged when I streamline the sample from 5km to 3km because 3km almost cover the entire CCZ/WEZ.

[15]Although the differences in means for observable covariates are statistically significant across the boundary, it is not a major concern as differences are likely to partial out when I conduct the analysis within postcode. Moreover, these variables are controlled for in the estimation.

To further show that the composition of properties sold within postcode after the introduction of the CC is similar to the composition before, I examine the effects of the CC on the hedonic characteristics for a sub sample of properties from Nationwide. The specification is similar to that in Equation (1) but instead of housing prices, the dependent variables are the various housing characteristics, including floor area, availability of central heating and garage, number of bedrooms and bath, and the age of unit. The analysis incorporates transactions inside the CCZ and WEZ, and untreated units within 3 kilometres of the CC boundary. Results are summarized in Table 3. There are no significant changes in the composition of transactions within a postcode before and after the introduction of the CC, mitigating the risk that key estimates are driven by the change in quality of housing units[16].

6.2 Congestion Charge and Housing Prices

6.2.1 Baseline Estimates

Table 4 summarizes the results from estimating equation (1) for the different CC events - the initial implementation of CCZ ($CCZ2003\_2005$) and WEZ (WEZ) in 2003 and 2007 respectively, the subsequent hikes in 2005 ($CCZ2005\_2011$), 2011 ($CCZ2011\_2014$) and 2014 ($CCZ2014$) and the removal of WEZ in 2011 ($RemWEZ$). Only the coefficients and standard errors for the key treatment estimates $\mathbf{CC_{it}}$ are reported (total 6 events). The sample includes all transactions within 5 kilometres of the CCZ/WEZ boundary. Additional covariates are included into the estimation sequentially with increase stringency moving from left to right of the table. Column (1) has no control variables other than year quarter dummies and postcode fixed effects. Column (2) allows price trends to vary at a smaller geographical level over time with wedge fixed effects. In Column (3), a vector of housing characteristics is included. Column (4) includes a set of neighbourhood controls that capture the attractiveness of the neighbourhood. Column (5) introduces a vector of location characteristics. Finally, in column (6), the estimates are weighted inversely according to the distance of each transaction from the treatment boundary with larger weights given to transactions closer to the CC boundary. For details on the co-variates refer to Table 1. Standard errors are clustered at cross boundary wedges that are constructed in similar fashion as those in figure 3 but are more finely divided into 300 wedges to allow for serial correlation in errors over time and spatial correlation in price changes between proximate postcodes across the CC boundary.

[16] I also estimated equation (1) with additional controls on hedonic characteristics for the sample of transactions from Nationwide Database. The results are very similar to that reported in Table 5. However, due to the small sample size (less than 1,000 observations), I do not report the findings in this paper although it is available upon request.

In less robust specifications with no controls as detailed in columns (1) and (2), I observe large and significant associations between all the six CC events and housing prices. Positive housing price effects are documented for the CC hikes in 2005 and 2011 and for the implementation and removal of the WEZ. Conversely, negative price responses are reported for the initial implementation of the CCZ in 2003 and the increase of the CC in 2014. Negative price impacts of $CCZ2003\_2005$ are similar to that reported in Zhang & Shing (2006) and Percoco (2014) but my estimates are much smaller after partialing out time-invariant characteristics with postcode fixed effects. Allowing price trends to vary at smaller geographical areas appears to increase the point estimates marginally as seen in Column (2). These findings, however, should not be taken as causal estimates: as soon as I control for housing, neighbourhood and location characteristics, most of the effects, except for those in WEZ and RemWEZ, are eliminated as seen in Column (5). Point estimates are more than 27% smaller for WEZ and 50% for RemWEZ compared to that reported in Column (1). In column (6), estimates are weighted inversely according to the distance from the CC boundary. Greater weights are given to transactions that are closer to the boundary to ensure that the identification of effects predominantly stems from transactions that are less dissimilar. If anything, applying weights on the estimates appears to accentuate the effects as price response increases by almost two fold.

6.2.2 Estimates Restricted To Proximate Transactions

Although earlier estimates show significant capitalization effects associated with the CC, it could stem from unobserved neighbourhood changes across the boundary over treatment period. It is likely that the CC boundary is endogenously drawn to alleviate traffic in areas subjected to the heaviest traffic. Thus, properties in the cordoned zone, which is very close to the CBD, are likely to be in very different neighbourhoods from those outside especially when these units are located further from the CC boundary.

Properties in different neighbourhoods across the CC boundary could experience unobserved neighbourhood changes. If such changes are correlated with the implementation of the CC, this could attribute to a spurious association between the CC event and housing price changes. For instance, if a neighbourhood outside the zone underwent urban renewal during the implementation of the CC, this could bias the estimates if such changes are not accounted for. To mitigate this, the intuitive strategy is to restrict the analysis to housing units close to one another to ensure that other than being located on different sides of the CC boundary, they are in near-identical or same neighbourhood.

Results are summarized in Table 5 and the specification adopted is similar to that of Column (6) in Table 4. I restrict the sample of transactions to be analysed from 5 kilometres to 1 kilometre from the CC boundary (from

left to right of the table). Estimates are somewhat smaller than raw DID estimates. Like before, home prices in WEZ increase after the CC is implemented. Capitalization effects are around 9.36%[17] when compared to untreated units within 5 kilometres of the boundary. Within 4 kilometres, effects fall to around 7.86% and within 3 kilometres, effects further decrease to around 6.48%. Constraining to housing units just 2 kilometres from the boundary reduces price response to 5.45%. Comparing units not more than 1 kilometre in and out the CC boundary, which cuts the sample size by about 75%, reduce price impacts to around 3.68%. All of the estimates are significant at 1% level. In monetary values, the findings suggest that homeowners are paying between £29,783 and £75,753 to enjoy better traffic in the WEZ.

Similar albeit inconclusive effects are documented for the removal of WEZ ($RemWEZ$). Housing prices increase by 5.24% when compared to units within 5 kilometres for the boundary. Within 4 kilometres, the effects fall to 3.96%. Price impacts are about 2.60% for units within 3 kilometres from treatment boundary and is only significant at a 10% level. When I narrow the sample to within 2 kilometres from treatment boundary, the effects are no longer significant. This is expected since the removal of the WEZ confers no tangible benefits to homeowners in the WEZ other than the fact that they no longer need to pay 10% charge whenever they choose to drive during the charged periods. Overall, the drastic decline in estimates by more than 50% when I restrict the sample from 5 kilometres to 1 kilometre suggests that if one does not control for unobserved neighbourhood differences between transactions, one overestimates the MWTP for avoiding negative traffic externalities.

It remains puzzling why the housing prices did not react to the initial implementation of the CCZ ($CCZ2003\_2005$). There are several possible explanations for this. One is that housing prices have already responded during the announcement of the CC. This could be possible as announcements are made way before the actual implementation (refer to figure 2). There are also concerns whether early announcement could result in the sorting of households who care most about traffic externalities into the cordoned area. Hence, I repeat the analysis using the official announcement dates from TfL in Panel B. The treatment period is defined as the day the CC event is announced by TfL and ends the day before the CC event is implemented. As hikes are announced months before being implemented, announcement effects are computed only for the initial implementation of the CCZ and WEZ.

Results, which are summarized in Panel B of Table 5, suggest that housing prices only respond to the announcement of the WEZ, but not to the CCZ. The disclosure of the news of WEZ is linked with an increment of housing prices that ranged from 3.39% to 5.32% - equivalent to a price response of between £23,813 and £37,370. Announcement effects are comparatively modest to implementation effects and that is understandable as homeowners have yet to experience reductions in traffic. An absence of tangible effects surrounding the CCZ suggests that residents are unsure of the effectiveness of the novel policy initially. This is consistent with the survey conducted by TfL that

[17] As it is a log-linear model, capitalization effects are computed by taking the exponential of the point estimates before subtracting by one. For instance, $Exp(0.0895) - 1 \approx 9.36\%$

echoed the uncertainty among respondents on the effectiveness of the CC on reducing traffic and improving accessibility (TfL, 2003b). Conversely, homeowners are optimistic about the impact of the WEZ, possibly, after observing the effectiveness of the CCZ in curbing traffic congestion.

6.2.3 Robustness and Placebo Tests

Table 6 summarizes the findings from a battery of robustness and placebo tests that further addresses the challenges that impede identification to provide more assuring evidence.

Matched-Pair Analysis. Exploiting the sharp discontinuity in congestion charge benefits across the treatment boundary, I pair each housing transaction with the closest comparable on the opposite site of the CC boundary. This identification strategy has been widely adopted by many papers including Black (1999); Gibbons & Machin (2006); Fack & Grenet (2010); Gibbons *et al.* (2013). The one-to-one matching procedure is as follows: I first reduce the sample to transactions within 3 kilometres of the treatment boundary. A distance matrix is then created between each transaction in the CCZ/WEZ with each transaction outside. Any pairs that are more than 1 kilometre apart are removed to ensure that I am not matching units excessively far apart. Transactions are then matched according to property type (flats or terraces), tenure (freehold or leasehold) and whether the unit is a new build and any mismatches are omitted. Days between transaction dates are then computed and any pairs that are sold more than a year apart are removed to ensure that price differences are not due to by price changes over time. Finally, pairs are ranked according to distance and transaction dates apart and are selected based on the composite rank of the two criteria. Matching is done with replacement, meaning that it is possible for a housing unit outside the zone to be matched repeatedly with those inside. Based on the rigorous matching process, about 56% of the transactions in the cordoned area are matched. Estimates, summarized in Panel A of Table 6, are weighted inversely according to the distance between pairs, with larger weights allocated to pairs that are closer together. Sample size is significantly reduced because every housing transaction in the cordoned area (CCZ/WEZ) is matched with only one nearest neighbour[18]. Results are fairly consistent to before as positive capitalization effects are detected only during the implementation of the WEZ.

Shrunk/Expanded CC zones. One impediment of establishing casual inference, due to the overlapping of the cordoned area with Central Business District (CBD), is that the key estimates could be denoting house price changes due to proximity to the CBD. To allay this concern, I create artificial treatment areas by shrinking and expanding

[18]Increasing the number of neighbours and relaxing the matching parameters have an immaterial impact on the results.

the CCZ and the WEZ by 1 kilometre. I provide an illustration for the CCZ in Figure 6. For the shrunken zones, postcodes at 0 to 1 kilometre from the boundary inside the treatment area are denoted as counterfactuals (yellow shaded area) and postcodes beyond 1 kilometre from the boundary in the cordoned area are denoted as treated properties (unshaded green area). For the expanded CC zones, postcodes between 0 and 1 kilometre outside the actual CC zone are flagged as treated units (unshaded in white) while those between 1 and 2 kilometres outside the actual CC zone are denoted as counterfactuals (red gridded area). Any buildings originally in the WEZ (blue) are omitted from the expanded CC analysis. Results in Panel B and C of Table 6 indicate that none of the estimates is significant, confirming that earlier results are not spuriously driven by property price changes due to proximity to CBD.

North of Thames River. An additional concern is whether the CCZ/WEZ is drawn due to physical barriers (hills, rivers, forest etc.) or major infrastructures (railways, flyovers etc.). If boundaries are drawn due to these features, the CCZ/WEZ could be demarcating different neighbourhoods. Restricting to proximate housing units across these boundary will not wipe out the unobserved differences between them. While the CCZ crosses the Thames River due to the ease of charge implementation, the south of WEZ is bounded by Thames River. The concern that properties to the south of the river are different from those in the north is not unfounded as these areas are typically not considered as part of Central London. Thus, I exclude housing transactions beyond the south of Thames River from the analysis. As documented in Panel D of Table 6, removing these sales has no apparent effect on the estimates.

Randomization of treatment period. Next, I address the concern whether positive capitalization effects of WEZ[19] could be documented in non-treatment time periods. To do so I generate 1000 random treatment dates (t_{false}) during the pre-treatment period - between 1st of January 1996 and 31st of December 2002. Treatment period is between t_{false} and $t_{false}+2years$ and the sample period for each regression is from $t_{false}-2years$ to $t_{false}+2years$[20]. Due to the sheer number of randomized dates false treatment periods do overlap. The new key regressor - $WEZ * t_{false}$ - is the interaction of a binary variable of whether the property i is in the WEZ and the false treatment period. Placebo regressions incorporate transactions within 1 kilometre from the CC boundary. Cumulative probability and probability density of the estimated coefficients of $WEZ * t_{false}$ from 1000 different placebo regressions are plotted in Figure 7. As expected, the distribution of placebo estimates is centered around zero, indicating that most of the randomly created treatment windows do not materially affect housing prices. Only 3% of the $WEZ * t_{false}$ estimates are larger than the benchmark estimate of WEZ (denoted by the dashed line - 0.0361) from Column (5)

[19]Randomization is also conducted for CCZ. (results available upon request) Most of the placebo point estimates are centered around zero with a mean of -0.00421 and standard deviation of 0.02798.

[20]For instance, if the randomly generated event date is 12th of July 1998, the analysis will incorporate transactions made between 12th of July 1996 to 12th of July 2000 and the treatment period will be from 12th of July 1998 to 12th July 2000.

of Panel A of Table 5. Even so, only 4.5% out of the 1000 placebo estimates are significantly different from zero at a 10% level. Taken together, results from Figure 7 increase the confidence that treatment effects documented earlier are casual and not spurious.

Additional variables. A battery of additional variables is included in the robustness test. First, to further disentangle the CBD capitalization effects from my estimates, I include the elucidian distance of each property i from the CBD - denoted by the centroid of the CCZ - and the subsequent polynomials (up to 4) interacted with year dummies. Second, a vector of controls that measures the public transport accessibility - a binary variable denoting whether property i is within 200 metres from tube station and count of bus lines from bus stops within 200 metres from property i interacted with year dummies - are included into the specification. The concern is whether the appreciation of property prices in the cordoned area post treatment is correlated with an increase in the willingness to pay for properties more accessible to public transport. Thirdly, I relax the parallel trend assumption between treatment and untreated units by allowing property price trends to vary linearly across 300 cross boundary wedges. Results are summarized in Panel E of Table 6. Earlier estimates associated with WEZ and $RemWEZ$ remain robust even after the inclusion of these variables, confirming that the effects surrounding the CC are not spurious.

6.3 Congestion Charge and Traffic Flow

While earlier findings, across various specifications, show that the implementation of the CC in the WEZ is associated with positive house price reactions, it is uncertain whether the price effects are capturing the MWTP to avoid negative traffic externalities without a "first stage". Hence, to demonstrate that the CC is effective in reducing traffic, the following specification is run:

$$Traffic_{pt} = \alpha_p + \zeta \mathbf{CC_{pt}} + Year_t + \varepsilon_{pt} \tag{3}$$

where $Traffic_{pt}$ is the natural logarithm of the average annual four wheel or more vehicular flow at count point[21] p in year t. Key variable of interest is $\mathbf{CC_{pt}}$ that denotes count points within the CCZ/WEZ after implementation of the CC event at time t. Because traffic flow data is only available from 2000 to 2014, analysis excludes the hike of CC in 2014. ζ captures the impact of the CC event on traffic flow. Analysis is conducted with count point fixed effects (α_p). Year dummies are included as well ($Year_t$). For each of the five CC events, which includes data from year $t-2$

[21]Count Points are sample points on the road networks where traffic flow is accounted for.

to $t + 2$ where t = the year when the treatment is implemented, a total of 6 separate regressions that accounts for all CPs before streamlining the sample to CPs from five to one kilometres from the CC boundary. This specification is fairly similar equation (1) in that I narrow the analysis from roads to 1 kilometre from the CC boundary to ensure comparability with earlier results.

I further flag roads that are carrying traffic from WEZ to CCZ and vice versa. The implementation of WEZ might induce more in-zone commuting due to reduce commuting cost or less congested traffic. This might lead to a downward bias to the actual impact of the CC on traffic. These roads are defined by two criteria: (1) roads that are within 400 meters from the shared boundary of the CCZ and WEZ [22] and (2) roads that are carrying traffic in the direction of the CCZ or WEZ. An additional variable CCZ_to_WEZ (WEZ_to_CCZ) is included in the estimation for the CC events $CCZ2005\_2011$ (WEZ) to denote these roads.

Table 7 shows the estimates for equation (3). The introductions of CCZ and WEZ, and the hike (from £5 to £8) of the CC in 2005 curbed traffic moving into the cordoned area. After the introduction of the CC in 2003 ($CCZ2003\_2005$), traffic volume fell by 6.07% to 9.87%. The hike in 2005 ($CCZ2005\_2011$) further curtailed traffic by 6.37% to 9.05%. Subsequent increases in the charge has an immaterial effect on traffic. Traffic volume fell by 3.58% to 5.96% after the WEZ is introduced, validating that homeowners who are paying more for their homes in the WEZ enjoy much better traffic conditions. Also, there is no evidence indicating that the introduction of the WEZ lead to more in-zone commuting between the CCZ and WEZ. Finally, like what was documented by TfL[23], no significant rebound in traffic is observed after the removal of the WEZ ($RemWEZ$). This could explain why home prices remain fairly stable in Table 5.

To ensure that the estimates are not spurious, I conduct a battery of placebo and robustness tests like before. Results are summarized in Panel A to D in Table 8. First, I repeat the analyses with announcement dates of the CCZ and WEZ in Panel A to observe there are any effects that predate the implementation that could suggest that the effects could be spurious. There are concerns that before the introduction of the CC traffic conditions are already improving. These effects are fairly weak and disappear when I restrict to roads less than 4 kilometres from treatment boundary. Conversely, traffic became heavier in the WEZ before the introduction of the CC between 2005 and 2006.

Another concern is whether the estimates are capturing any trends in traffic flow due to the proximity to CBD. I address this by shrinking and expanding the CCZ/WEZ by 1 kilometre. Findings are detailed in Panel B and C.

[22] To have a clearer picture, refer to Figure 1. It is the boundary running adjacent to Vauxhall Road and Park Lane.

[23] Retrieved from https://tfl.gov.uk/info-for/media/press-releases/2011/June/tfl-announces-initial-results-following-removal-of-the-western-extension-of-the-congestion-charging-zone

None of the estimates, except for $CCZ2003\_2005$, are significant. Results suggest that traffic in the CCZ escalated more than charged roads closer to the border and this could be due to more driving by residents in the zone. Finally, in Panel E, I include the distance between each CP and the CBD and the subsequent polynomials (up to 4) interacted with year dummies. It is to partial out any non-linear trends of traffic flow with proximity to CBD from treatment estimates. The sample includes CPs within 3 kilometres of the CC boundary. All the results are fairly similar with earlier estimates in Panel A with significant traffic reduction effects documented for $CCZ2003\_2005$, $CCZ2005\_2011$ and WEZ.

It is baffling to observe an absence of capitalization effects for the initial introduction of the CCZ in 2003 that led to significant reductions of traffic volume in the zone. One explanation provided earlier is that homebuyers are unsure of the effectiveness of the CC when it is first introduced in 2003. Here, I explore other possible reasons. It could that traffic conditions did not improve despite the reduction in volume because there are more heavy vehicles, such as trucks and buses, on roads after the CC is implemented. This is plausible as TfL increased the number of bus services after the CC is introduced to encourage commuters to switch to public transport (Leape, 2006). Thus, I re-estimated equation (3) with the traffic flow of different vehicle types as dependent variable. I do not observe that traffic has worsened due to the presence of more buses or trucks on roads in the CCZ after 2003 [24].

Another possible reason is that homeowners living in the WEZ benefit more from the CC relative to those living in the CCZ. I find evidence supporting this. From the Census[25], I document that it is more probable for household in the WEZ (49%) to own a auto-mobile than in the CCZ (37%). For residents who drive, there is a higher tendency for them to drive to work if they are staying in the WEZ (25%) compared to residents from the CCZ (13%). This could be due to the fact that residents in the CCZ are living closer to the work place. 42% of homeowners in the CCZ lives less than 2 kilometres from their workplace compared to 25% of the residents in the WEZ. Furthermore, homeowners living in the WEZ (£4,095) earn, on average, much higher wages compared to homeowners living in the CCZ (£3,517). Collectively, evidence shows that homeowners from the WEZ are not only more reliant on driving but also incur larger time-delay externality from congestion due to higher wages. This could explain why they are more willing to pay for better traffic conditions compared to those living in the CCZ.

6.4 Congestion Charge Savings and Housing Prices

One of the main impediments of identifying the MWTP for avoiding traffic externalities is that the capitalization effects are partly capturing the pecuniary CC savings residents in the zone are entitled to. In other words, new residents may pay more for their houses because of the discount for the CC. To address this, I exploit the policy

[24] Results available upon request

[25] Data is collected from Census 2001 and 2011 and is weighted according to the geographical distribution of transactions analysed in the study.

that allows some homeowners residing outside but near to the CCZ/WEZ the entitlement to a 90% waiver to the CC using a difference-in-difference-in-difference framework as shown in equation (2). As highlighted earlier, these areas are shaded in purple-striped (grey) for the WEZ (CCZ) in Figure 1. As there are a limited number of 936 transactions outside CCZ that are eligible for the CC savings, the focus will be on the discount zone of the WEZ that has a larger sample of 15,976 transactions.

Panel A of Table 9 presents the estimates from equation (2). Home prices in the discount zone are not materially affected after the CC is enforced, indicating that earlier estimates associated with the WEZ are not spuriously driven by the discount to the CC. However, the traffic conditions in the discount zone might be affected by the charge. To verify, I re-estimated equation (2) with traffic volume as the dependent variable. Findings are summarized in Panel B. Most of the results show that the traffic conditions in the discount zone are not materially affected by the CC in the WEZ except when I restrict the analysis to roads within 1 kilometre from the CC boundary. Overall, results support the notion that the findings associated with the WEZ are capturing the MWTP to avoid negative traffic externalities.

7 Conclusion

This paper exploits a shock to traffic conditions induced by the London Congestion Charge (LCC) to provide new estimates of the marginal willingness to pay (MWTP) to avoid negative traffic externalities using the housing market. Using a credible natural experiment, this study is an improvement from traditional hedonic approaches widely adopted previously that are blighted by omitted variable bias due to non random assignment of traffic across space. Restricting the analysis to transactions just in and out the Congestion Charge (CC) boundary in Central London to further reduce unobserved neighbourhood differences between treated and untreated housing units, I show that homeowners pay, on average, 3.68% (around £30,000) more for homes in the Western Extension Zone (WEZ) relative to homes within 1 kilometres outside the cordoned area to enjoy better traffic conditions that fell by 3.58%. These estimates do not appear to be spurious as they are robust across a variety of specifications and placebo tests that relax the identification assumptions.

So are the estimates tenable? Although it is much larger than those previously reported, it is not entirely implausible. To illustrate this point, consider the benefits associated with less traffic. Homeowners living in the zone can enjoy a more conducive environment with better air, less noise, greater time savings from commuting and

and safer roads. According to TfL (2008), total suspended particulates ($PM10$) levels in the WEZ fell by 4.2%[26] after the CC is imposed. Taking the estimates from Chay *et al.* (2005), the improvement in air quality, by itself, should attribute to a housing price increment of around 0.84% to 1.68%. This is almost half of the capitalization effects captured in this study without accounting for additional perks such as reduced time delays from congestion and less traffic noise.

This study further provides credible evidence on the effectiveness of the CC in curtailing incoming traffic into Central London. By forcing drivers to internalize the cost of negative traffic externalities imposed on others, the CC enhances the living environment and this is valued by homeowners. The efficacy of the charge, however, is conditional on a well functioning public transportation system (Leape, 2006). Drivers must be provided with alternative transport modes to commute into the charge zone. Thus, it is of paramount importance to channel revenues from the charge to enhance the reliability of public transport system. Given that traffic congestion is fast becoming a salient issue across different cities with the exorbitant cost incurred on fuel, the time wasted on jams and the ineffectual alternative solutions (e.g constructing more roads), this could be an instrument that local governments could turn to.

[26] According to the Sixth Annual Report on the Congestion Charge Impact Monitoring conducted by TfL, the WEZ also reduces emissions of Nitrogen Oxide (NO) and Carbon Dioxide (CO_2) by 2.5% and 6.5% respectively.

Appendix

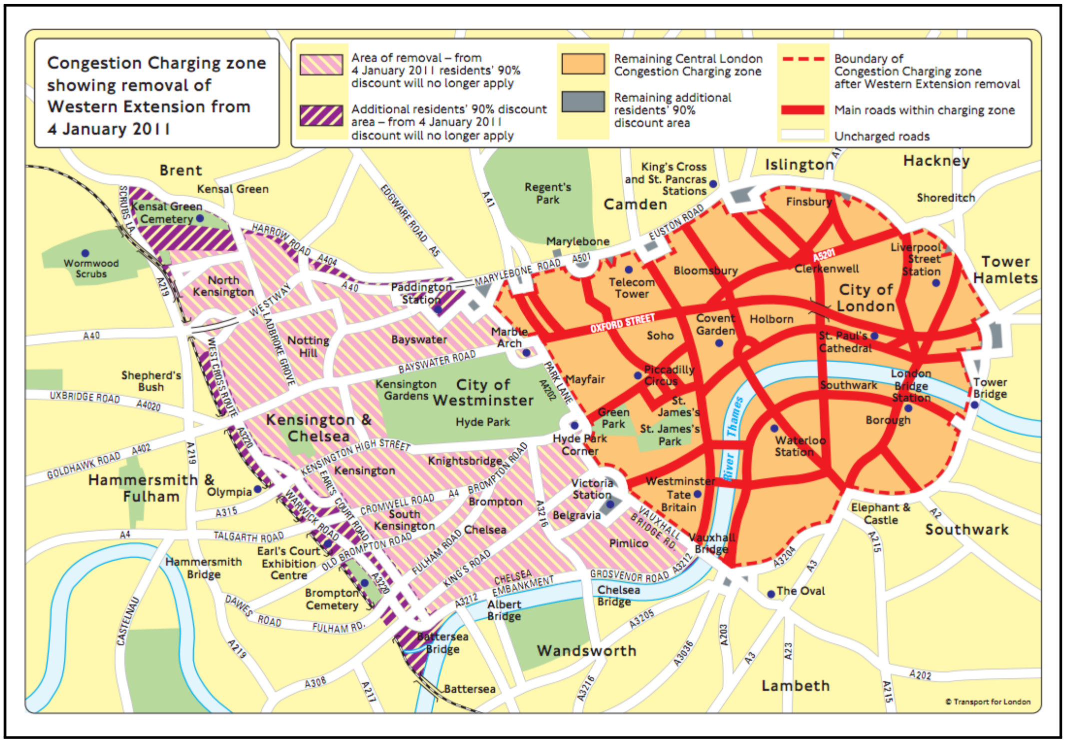

Figure 1: Map of the Original Congestion Charge Zone (CCZ) & Western Extension Zone (WEZ) Source: Transport for London (TfL)

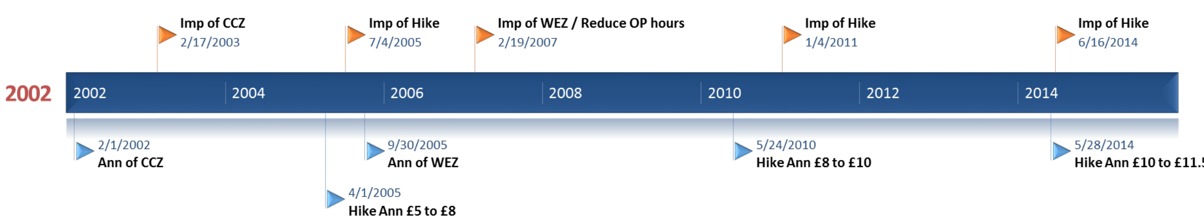

Figure 2: Timeline of The London Congestion Charge

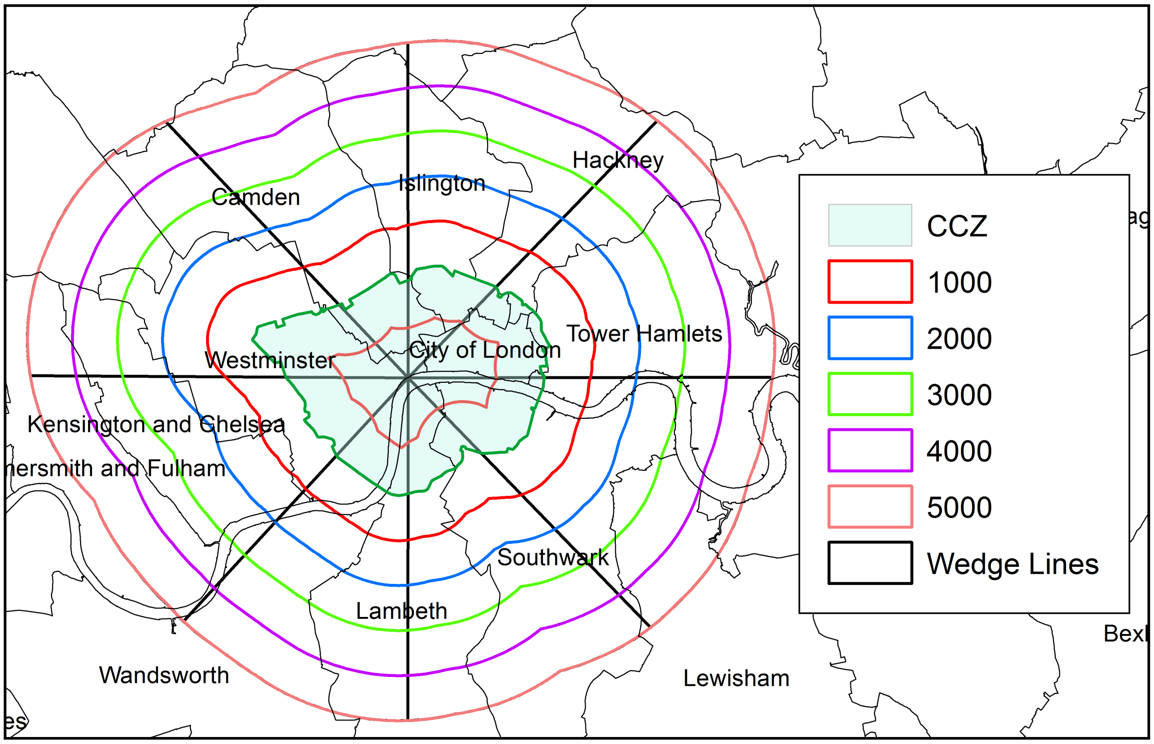

Figure 3: The CCZ, buffers (1 to 5 kilometres from the CC boundary) & wedge lines

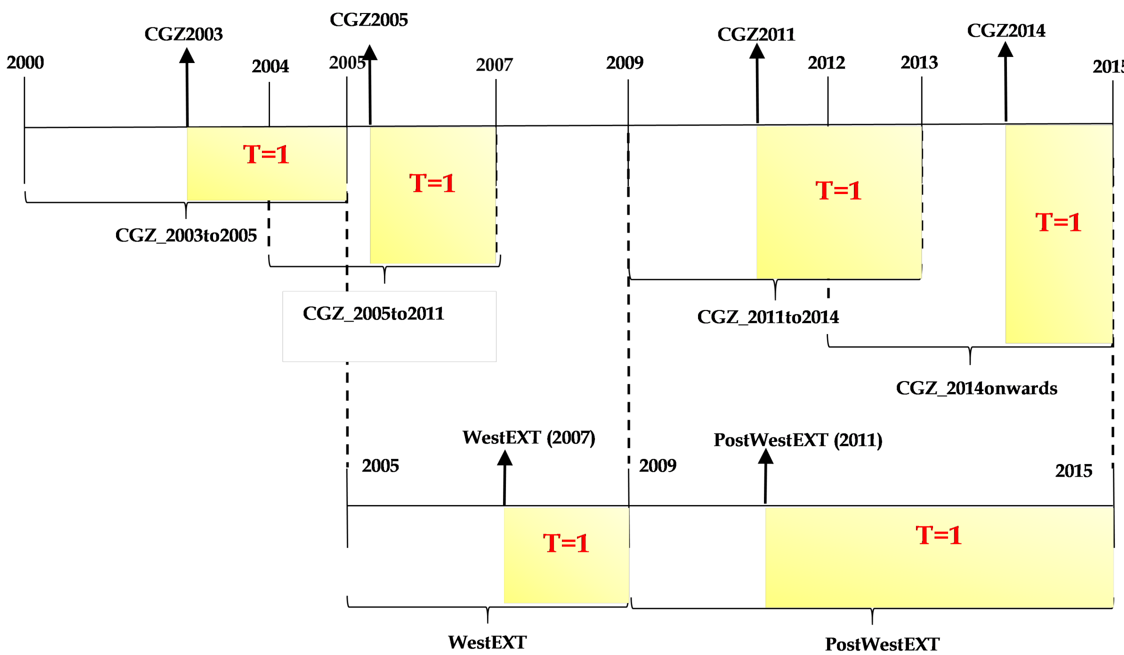

Figure 4: Sample window for the different regressions (T=1 denotes Treatment Period)

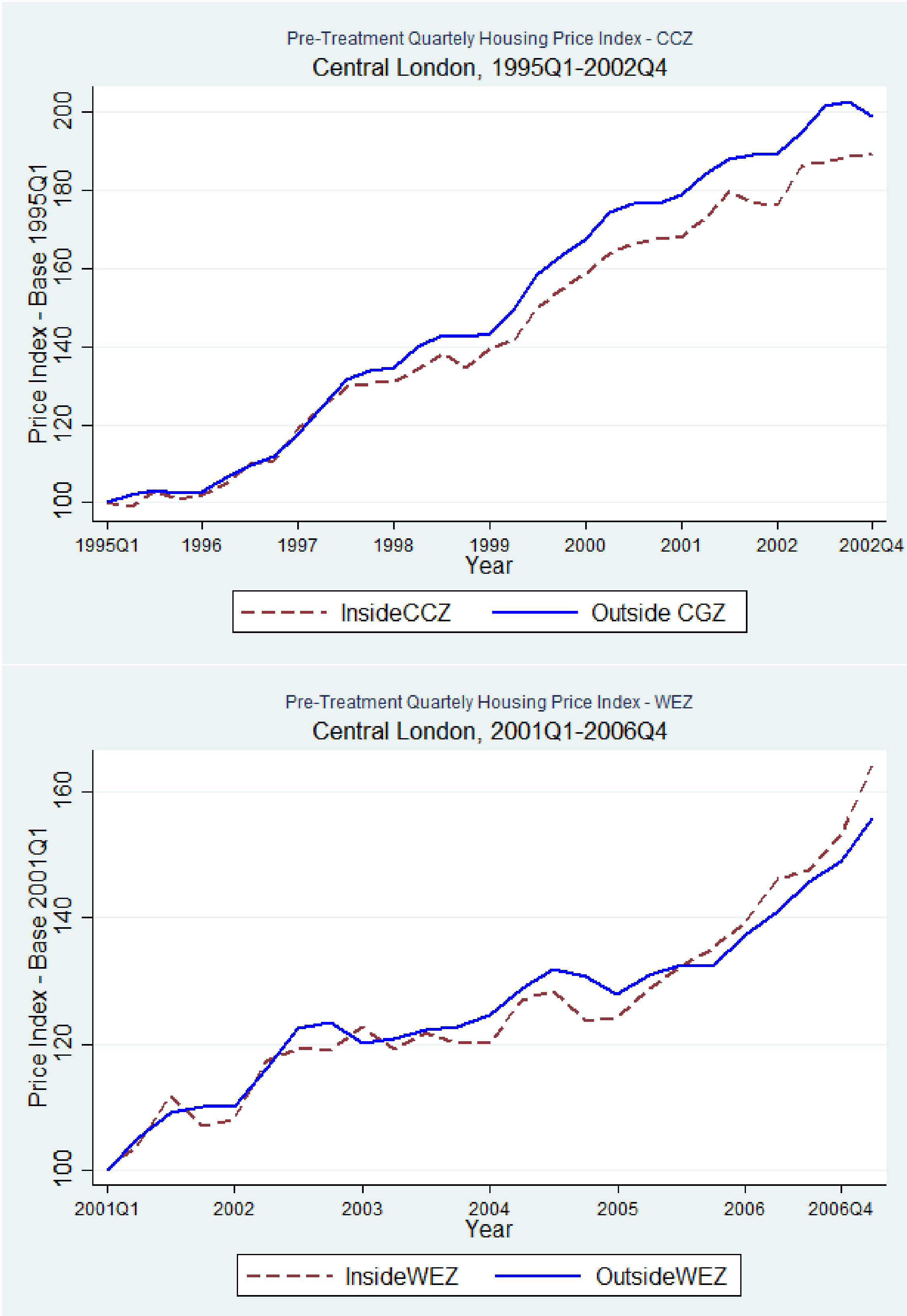

Figure 5: Pre-Treatment Repeated Sales Property Price Index: CCZ (Top) WEZ (Bottom)

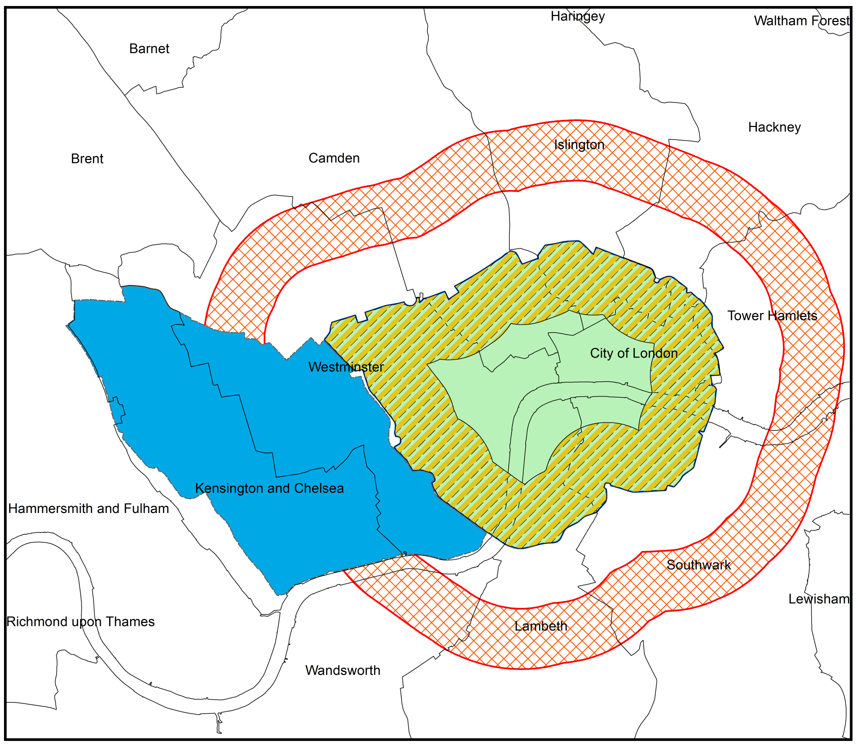

Figure 6: The Shrunken and Expanded CCZ and the WEZ (Blue)

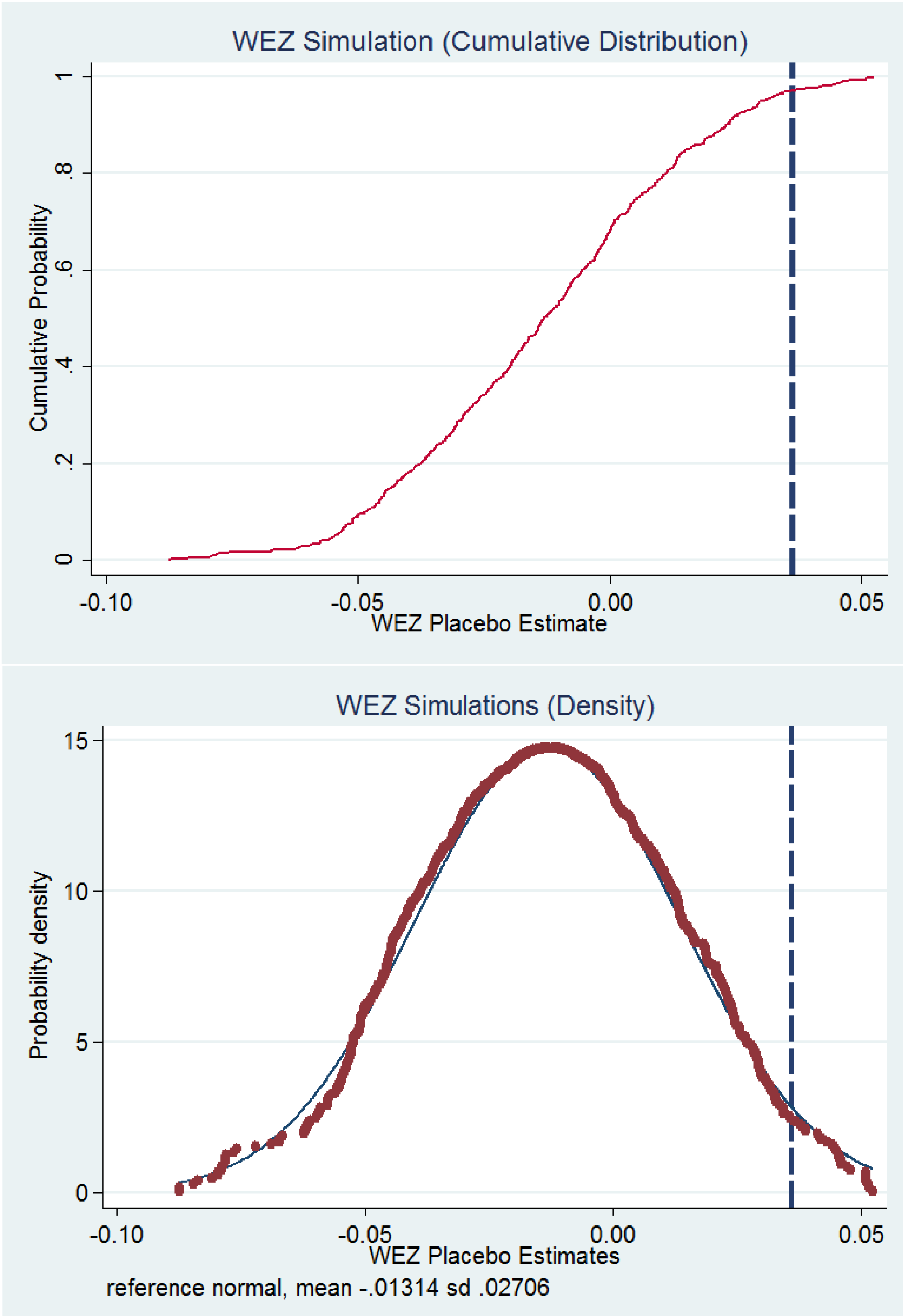

Figure 7: WEZ Placebo Estimates - CDF (top) & PDF (bottom)

Table 1: List of Variables

Variable	Source	Description
Panel A: Main Specification Variables		
Dependent Variable		
Housing Price ($ln(Price_{ijkqt})$)	Land Registry	Log of property price of transaction i at postcode j, wedge k at quarter q of year t
Traffic Flow ($Traffic_{pt}$)	Department Of Transport	Log of traffic flow from vehicles with 4 or more wheels in CP p at year t
Housing Characteristics		
New Sales	Land Registry	Dummy denoting whether transaction i is new build
Terrace	Land Registry	Dummy denoting whether the property type for transaction i is terrace
Leasehold	Land Registry	Dummy denoting whether the tenure for transaction i is leasehold
Location Characteristics (W_j')		
Distance to nearest Grade 1 Park	Magic	Elucidian distance of nearest Grade 1 Park from postcode j in km
Counts of Heritage Buildings	Magic	Number of Heritage buildings within 200m from postcode j
Thames River View	Digimap	Binary variable = 1 if postcode j within 200m from Thames River, 0 otherwise
Neighborhood Controls		
Minority race residents	Census 2001 & 2011	% of Asian/African/Middle Eastern and other minority race residents in OA
Unemployment rate	Census 2001 & 2011	% of unemployed working adults in OA
Uneducated residents	Census 2001 & 2011	% of residents in OA with no education qualifications
Lone parent households	Census 2001 & 2011	% of single-parent households in OA
Panel B: Robustness Variables		
Distance to CBD	TfL	Elucidian distance of postcode j from centroid of CCZ
Transport Controls		
Proximity to Tube station	TfL	Binary variable = 1 if postcode j within 200m from Tube Station, 0 otherwise
Bus Lines	TfL	Number of bus lines service postcode j computed using IDW based on distance of bus stops from j

Table 2: Descriptive Statistics for Estimation Sample from 2000 to 2015

	Panel A: Within 5km from the CC boundary				
	Mean			Standard Deviation	
Log sale price	12.76			0.71	
CCZ/WEZ Treatment	0.15			0.36	
New build	0.12			0.32	
Flat/Masionette	0.79			0.41	
Terraced house	0.17			0.38	
Leasehold	0.80			0.40	
Distance to Nearest Grade 1 Park (m)	2133.27			1377.81	
Counts of heritage building in 200m	0.14			0.66	
Thames River View	0.082			0.28	
	5km	4km	3km	2km	1km
Total Number of Sales	557631	443858	326307	214217	123081
Postcodes	39174	32959	26026	18965	11594
Sales in CCZ/WEZ after the CC is introduced	84509	84509	80580	67549	50536
Postcodes	7941	7941	7816	7507	6315

		Panel B: Difference In Means (Control - Treated)				
	Period	5km	4km	3km	2km	1km
New build	Pre	-0.0101***	-0.00906***	0.000401	0.00238	0.0625***
		(0.00209)	(0.00220)	(0.00254)	(0.00340)	(0.00500)
	Post	0.0353***	0.0326***	0.0346***	0.0315***	0.0436***
		(0.00125)	(0.00127)	(0.00139)	(0.00167)	(0.00223)
Flat	Pre	-0.124***	-0.0936***	-0.0750***	-0.0636***	-0.0584***
		(0.00270)	(0.00267)	(0.00278)	(0.00311)	(0.00392)
	Post	-0.109***	-0.0854***	-0.0702***	-0.0602***	-0.0361***
		(0.00154)	(0.00152)	(0.00155)	(0.00168)	(0.00199)
Terrace	Pre	-0.0955***	0.0721***	0.0553***	0.0451***	0.0485***
		(0.00253)	(0.00251)	(0.00260)	(0.00291)	(0.00372)
	Post	0.0767***	0.0609***	0.0503***	0.0451***	0.0310***
		(0.00144)	(0.00143)	(0.00146)	(0.00160)	(0.00192)
Leasehold	Pre	0.129***	-0.0997***	-0.0824***	-0.0726***	-0.0734***
		(0.00266)	(0.00263)	(0.00271)	(0.00299)	(0.00366)
	Post	-0.107***	-0.0837***	-0.0697***	-0.0614***	-0.0387***
		(0.00152)	(0.00150)	(0.00153)	(0.00165)	(0.00193)
Distance to Nearest Grade 1 Park	Pre	1602.1***	1521.0***	1395.5***	1022.2***	744.8***
		(7.980)	(7.551)	(7.171)	(7.938)	(9.961)
	Post	1579.9***	1468.6***	1284.9***	932.9***	619.0***
		(4.681)	(4.368)	(4.045)	(4.296)	(5.150)
Counts of Heritage Buildings in 200m	Pre	-0.423***	-0.413***	-0.418***	-0.507***	-0.373***
		(0.00465)	(0.00542)	(0.00694)	(0.0104)	(0.0152)
	Post	-0.385***	-0.376***	-0.378***	-0.418***	-0.237***
		(0.00236)	(0.00271)	(0.00334)	(0.00471)	(0.00625)
Thames River View	Pre	0.0418***	0.0386***	0.0408***	0.0505***	0.129***
		(0.00186)	(0.00190)	(0.00212)	(0.00270)	(0.00362)
	Post	0.0311***	0.0226***	0.0204***	0.0308***	0.0697***
		(0.00104)	(0.00103)	(0.00110)	(0.00135)	(0.00174)

Note: Panel A summarizes the descriptive statistics for all the housing transactions within 5 kilometres from the CCZ/WEZ boundary and the number of sales and postcodes as the sample is reduced to 1 kilometre from the CCZ/WEZ boundary. Panel B summarizes the paired t-test between transactions in the CCZ/WEZ and those outside the cordoned area from 5 kilometres to 1 kilometre from the CC boundary before (**Pre**: 2000 - 2003) and after (**Post**: 2004 - 2014) the implementation of the CC. Standard errors are reported in the parenthesis.

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

Table 3: Balancing Test for Housing Characteristics for a subsample of transactions from Nationwide within 3km from the CC boundary

	(1) Floor Area	(2) Bathrooms	(3) Bedrooms	(4) Central Heat	(5) Garage	(6) Age
CCZ	-2.349	0.0338	0.00295	-0.0122	-0.113	-3.041
	(3.699)	(0.125)	(0.0709)	(0.0933)	(0.212)	(6.795)
N	5288	5288	5288	5288	5288	5288
R^2	0.592	0.626	0.541	0.564	0.695	0.851
WEZ	-5.041	-0.102	0.0288	-0.238	-0.381	-1.628
	(11.22)	(0.116)	(0.193)	(0.251)	(0.349)	(4.779)
N	3283	3283	3283	3283	3283	3283
R^2	0.599	0.653	0.553	0.570	0.780	0.868

Note: Postal code fixed effects estimation for a subsample of transactions from Nationwide Database within 3km from CCZ/WEZ boundary. Dependent variable is the respective housing characteristics labelled below the columns. Floor area (1) is the size of unit in square meters. Bathrooms (2) and Bedrooms (3) is the count of Baths and Bedrooms in the unit. Central heating (4) and Garage (5) is a binary variable that denotes if unit has such facilities. Age (6) is the number of years since the unit is built. Sample of transactions for CCZ (WEZ) is from 2000 (2004) onwards, explaining why the sample is larger than that in Panel D of Table 6. Robust standard errors clustered at 300 cross-boundaries wedges are reported in parenthesis.

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

Table 4: The Effects of the CCZ/WEZ on Housing Prices: Baseline

	(1)	(2)	(3)	(4)	(5)	(6)
CCZ2003_2005	-0.0135	-0.0257***	-0.0311***	-0.0173**	-0.000415	-0.0148
	(0.00879)	(0.00864)	(0.00845)	(0.00812)	(0.00903)	(0.0126)
N	193663	193663	193663	193663	193663	193663
R^2	0.720	0.722	0.773	0.774	0.774	0.781
CCZ2005_2011	0.0188*	0.0194*	0.0181*	0.0168*	0.00829	-0.00860
	(0.0105)	(0.0106)	(0.0103)	(0.0100)	(0.0106)	(0.0142)
N	98630	98630	98630	98630	98630	98630
R^2	0.719	0.719	0.796	0.796	0.796	0.803
CCZ2011_2014	0.00772	0.00982	0.00757	0.00593	-0.00386	-0.0307
	(0.0119)	(0.0117)	(0.0115)	(0.0115)	(0.0125)	(0.0196)
N	90293	90293	90293	90293	90293	90293
R^2	0.726	0.727	0.802	0.802	0.802	0.811
CCZ2014	-0.0321*	-0.0321*	-0.0322*	-0.0234	-0.0155	-0.0266
	(0.0170)	(0.0170)	(0.0171)	(0.0169)	(0.0183)	(0.0228)
N	76572	76572	76572	76572	76572	76572
R^2	0.750	0.751	0.810	0.810	0.810	0.815
WEZ	0.0760***	0.0818***	0.0826***	0.0747***	0.0550***	0.0895***
	(0.0111)	(0.0111)	(0.0100)	(0.0100)	(0.0112)	(0.0135)
N	102708	102708	102708	102708	102708	102708
R^2	0.735	0.736	0.805	0.806	0.806	0.816
RemWEZ	0.0457***	0.0468***	0.0363***	0.0346***	0.0237**	0.0511***
	(0.0109)	(0.0110)	(0.0102)	(0.00998)	(0.0109)	(0.0145)
N	130166	130166	130166	130166	130166	130166
R^2	0.754	0.755	0.820	0.820	0.820	0.829
Postal Fixed Effects	✓	✓	✓	✓	✓	✓
Wedge Fixed Effects-by-year		✓	✓	✓	✓	✓
Housing characteristics			✓	✓	✓	✓
Census OA characteristics-by-year				✓	✓	✓
Location characteristics-by-year					✓	✓
Inverse Boundary Distance weights						✓

Note: Dependent variable is the natural logarithm of transacted prices. All regressions are estimated with post code fixed effects for transactions within **5 kilometres** from CCZ/WEZ boundary. In column (2), housing characteristics, including 1. a dummy denoting whether the property is leasehold 2. a dummy denoting whether the property is new build 3. a dummy denoting whether the property is terrace housing, are controlled for. In column (3), addition census variables, including the percentage of residents 1. with no education qualification, 2. of minority race, 3. unemployed and 4. percentage of lone parent households, are accounted for. In column (4), addition location characteristics such as whether the property has a Thames river view, the counts of heritage buildings within the 200 metres from the property and the distance of the property from the nearest Grade 1 park are controlled for. In column (5), wedge fixed effects-by-year is included. For the sample window of each of the CC event, refer to figure 4 for more information.Robust standard errors clustered at 300 cross-boundaries wedges reported in the parenthesis.

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

Table 5: The Effects of CCZ/WEZ on Residential Property Prices: Proximate Transactions

	Panel A:Implementation					**Panel B:Announcement**				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	5km	4km	3km	2km	1km	5km	4km	3km	2km	1km
CCZ2003_2005	-0.0148	-0.0140	-0.0140	-0.00868	-0.00220	-0.00761	0.00275	0.0176	0.0148	-0.00766
	(0.0126)	(0.0125)	(0.0125)	(0.0126)	(0.0133)	(0.0161)	(0.0168)	(0.0166)	(0.0182)	(0.0214)
N	193663	153383	110719	70737	40039	118989	94167	68340	43372	24616
R^2	0.781	0.774	0.776	0.770	0.756	0.782	0.775	0.776	0.768	0.757
CCZ2005_2011	-0.00860	-0.00412	-0.000345	0.00559	0.0148	-	-	-	-	-
	(0.0142)	(0.0141)	(0.0142)	(0.0170)	(0.0193)	-	-	-	-	-
N	98630	75477	52898	34031	19162	-	-	-	-	-
R^2	0.803	0.791	0.792	0.803	0.780	-	-	-	-	-
CCZ2011_2014	-0.0307	-0.0247	-0.0101	-0.0184	0.0216	-	-	-	-	-
	(0.0196)	(0.0209)	(0.0216)	(0.0265)	(0.0245)	–	–	–	–	–
N	90293	71455	51575	34342	18964	-	-	-	-	-
R^2	0.811	0.802	0.800	0.809	0.795	-	-	-	-	–
CCZ_2014	-0.0266	-0.0347	-0.0341	-0.0232	-0.0244	-	-	-	-	-
	(0.0228)	(0.0221)	(0.0237)	(0.0261)	(0.0266)	-	-	-	-	-
N	76572	57263	39424	24992	14320	-	-	-	-	-
R^2	0.815	0.803	0.810	0.820	0.801	-	-	-	-	-
WEZ	0.0895***	0.0757***	0.0628***	0.0531***	0.0361**	0.0518***	0.0370***	0.0408***	0.0333**	0.0198
	(0.0135)	(0.0134)	(0.0141)	(0.0154)	(0.0161)	(0.0140)	(0.0142)	(0.0154)	(0.0169)	(0.0179)
N	102708	82001	62939	44172	26032	91360	73083	56041	39515	23643
R^2	0.816	0.811	0.803	0.801	0.766	0.810	0.807	0.801	0.801	0.763
RemWEZ	0.0511***	0.0388***	0.0257*	0.0226	0.0164	-	-	-	-	-
	(0.0145)	(0.0147)	(0.0154)	(0.0158)	(0.0160)	-	-	-	-	-
N	130166	104077	80896	56948	34399	-	-	-	-	-
R^2	0.829	0.826	0.818	0.819	0.786	-	-	-	-	-

Note: Postal code fixed effects estimation for transactions within 5 kilometres to 1 kilometres from the CCZ/WEZ boundary. Dependent variable is natural logarithm of transacted prices. Specification is similar to that in column 6 of Table 4. Panel A (B) summarizes the findings associated with implementation (announcement) of CCZ/WEZ. For announcement, treatment period begins the day the CC event is announced officially by TfL and ends the day the CC event is implemented. Robust standard errors clustered at 300 cross-boundaries wedges reported in the parenthesis.

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

Table 6: The Effects of CCZ/WEZ on Residential Property Prices: Robustness Test

	CCZ2003_05	CCZ2005_11	CCZ2011_14	CCZ2014	WEZ	RemWEZ
Panel A. Matched Pair Analysis						
Treatment	-0.0106	-0.0269	0.00117	0.0460	0.127**	-0.115
	(0.0364)	(0.0343)	(0.0442)	(0.0507)	(0.0585)	(0.0778)
N	25256	12437	8142	7306	11006	11468
R^2	0.444	0.512	0.505	0.548	0.567	0.508
Panel B. Shrunk CCZ/WEZ						
Treatment	-0.0308	-0.0181	-0.0379	-0.0366	0.0310	-0.00243
	(0.0237)	(0.0249)	(0.0480)	(0.0492)	(0.0401)	(0.0379)
N	19204	10509	8793	7612	16679	19444
R^2	0.721	0.757	0.773	0.763	0.737	0.748
Panel C. Expanded CCZ/WEZ						
Treatment	-0.0143	-0.00467	0.0159	0.00908	0.00994	-0.00479
	(0.0124)	(0.0123)	(0.0143)	(0.0228)	(0.0110)	(0.0132)
N	51814	24343	25713	17505	30536	37575
R^2	0.769	0.795	0.799	0.807	0.772	0.792
Panel D. North of Thames River Only						
Treatment	-0.0256*	0.0153	-0.00429	-0.0261	0.0444***	0.0157
	(0.0140)	(0.0203)	(0.0295)	(0.0328)	(0.0169)	(0.0174)
N	80599	34204	35338	26115	45252	58971
R^2	0.755	0.777	0.783	0.794	0.800	0.816
Panel E. Additional Variables						
1. Distance to CBD polynomials-by-year						
	CCZ2003_05	**CCZ2005_11**	**CCZ2011_14**	**CCZ2014**	**WEZ**	**RemWEZ**
Treatment	0.0163	-0.0104	0.0256	-0.0187	0.0505***	0.0346**
	(0.0168)	(0.0179)	(0.0375)	(0.0295)	(0.0147)	(0.0172)
N	110719	52898	51575	39424	62939	80896
R^2	0.776	0.792	0.800	0.810	0.803	0.818
2. Transport Characteristics-by-year						
Treatment	-0.00320	-0.00288	-0.00116	-0.0324	0.0569***	0.0289*
	(0.0123)	(0.0144)	(0.0215)	(0.0235)	(0.0142)	(0.0154)
N	110719	52898	51575	39424	62939	80896
R^2	0.776	0.792	0.800	0.810	0.803	0.818
3. 300 Cross Boundary Wedge Fixed Effects x Year Trends						
Treatment	-0.0216	0.00440	-0.0153	-0.0352	0.0587***	0.0230
	(0.0137)	(0.0150)	(0.0235)	(0.0242)	(0.0137)	(0.0166)
N	110719	52898	51576	39424	62952	80901
R^2	1.000	1.000	1.000	1.000	1.000	1.000

Note: In Panel A, transactions within the CCZ are paired with the transactions according to distance and transaction dates for housing sales of similar property type, tenure and new build status. Analysis is conducted with pair-fixed-effects and estimates are inversely weighted by distance between the pairs. In Panel B (C), the original CCZ/WEZ is shrunk (expanded) by 1 kilometre. For shrunken CCZ/WEZ, transactions that are less than 1 kilometre from the CC boundary but inside the cordoned charge area are now in the control group while buildings that are more than 1 kilometre into the CC zone are allocated in the treatment group. For expanded CCZ/WEZ, transactions that are less than or equal to 1 kilometre from treatment boundary outside the cordoned charge area are now classified as treated units. Counterfactuals are transactions between 1 and 2 kilometres outside the CC boundary. Transactions in the original CCZ/WEZ now omitted from the analysis. In Panel D, analysis is conducted for transactions located at the north of Thames River. In Panel E(1), distance to CBD and the subsequent polynomials (to the power of 4)-by-year are controlled for. In Panel E(2), distance to tube station-by-year and number of buslines-by-year are included. In Panel E(3), 300 cross boundary wedge fixed effects interacted with year trends are included. All regressions in Panel D and E include transactions within 3 kilometres from the treatment boundary. Robust standard errors clustered at 300 cross-boundaries wedges reported in the parenthesis.

Table 7: Effects of CCZ/WEZ on Traffic Count

	(1) All	(2) 5km	(3) 4km	(4) 3km	(5) 2km	(6) 1km
CCZ2003_2005	-0.0998***	-0.0633***	-0.0610**	-0.0589**	-0.0613**	-0.0941**
	(0.0236)	(0.0242)	(0.0244)	(0.0250)	(0.0266)	(0.0349)
N	10809	3916	3466	2802	2280	1424
R^2	0.989	0.982	0.981	0.981	0.979	0.981
CCZ2005_2011	-0.0522**	-0.0631**	-0.0618**	-0.0626**	-0.0677**	-0.0866**
	(0.0251)	(0.0255)	(0.0255)	(0.0257)	(0.0259)	(0.0390)
CCZ_to_WEZ	0.0525	0.0522	0.0536	0.0562	0.0575	0.0439
	(0.0367)	(0.0377)	(0.0380)	(0.0386)	(0.0398)	(0.0443)
N	11706	4066	3596	2941	2379	1430
R^2	0.991	0.984	0.983	0.981	0.981	0.978
CCZ2011_2014	0.0101	0.0489	0.0434	0.0520	0.0511	0.0413
	(0.0438)	(0.0443)	(0.0446)	(0.0450)	(0.0455)	(0.0506)
N	8761	2160	1760	1403	887	583
R^2	0.990	0.988	0.989	0.990	0.989	0.988
WEZ	-0.0422**	-0.0561***	-0.0562***	-0.0579***	-0.0400*	-0.0352*
	(0.0180)	(0.0189)	(0.0188)	(0.0193)	(0.0207)	(0.0207)
WEZ_to_CCZ	0.0178	0.0177	0.0177	0.0177	0.0177	0.0321
	(0.0337)	(0.0334)	(0.0335)	(0.0335)	(0.0336)	(0.0320)
N	7237	1767	1456	1152	738	466
R^2	0.994	0.992	0.992	0.992	0.995	0.996
RemWEZ	0.00633	0.0470	0.0427	0.0458	0.0445	0.0364
	(0.0400)	(0.0406)	(0.0409)	(0.0410)	(0.0413)	(0.0466)
N	9717	2370	1965	1558	1012	678
R^2	0.990	0.988	0.988	0.990	0.990	0.989

Note: Fixed Traffic Count Point regression. Sample for the different events includes traffic count from year *t-2* to year *t+2* where *t* = the year when treatment is implemented. Refer to figure 4 for more information. Dependent variable is the logarithm of traffic count for 4 wheels or more drive. Additional control include year dummies. CCZ_to_WEZ (WEZ_to_CCZ) flags for roads that are carrying traffic from the CCZ to the WEZ (the WEZ to the CCZ). CCZ2014 is excluded as I do not have traffic count after 2014. Robust standard errors clustered at ward level reported in the parenthesis.

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

Table 8: The Effects of the CCZ/WEZ on Traffic Count:Robustness Tests

	Panel A: Announcement					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	5km	4km	3km	2km	1km
PlaceboCCZ2003	-0.0565***	-0.0374**	-0.0304*	-0.0250	-0.0145	0.00793
	(0.0167)	(0.0180)	(0.0178)	(0.0181)	(0.0189)	(0.0156)
N	5953	1908	1695	1373	1118	702
R^2	0.995	0.991	0.992	0.992	0.992	0.993
PlaceboWEZ	0.0285	0.0474**	0.0480**	0.0473*	0.0633*	0.116***
	(0.0203)	(0.0229)	(0.0232)	(0.0249)	(0.0340)	(0.0326)
N	8493	2677	2361	1905	1549	963
R^2	0.993	0.985	0.983	0.982	0.980	0.978
		Panel B: Shrunken CCZ/WEZ				
		CCZ2003_2005	CCZ2005_2011	CCZ2011_2014	WEZ	RemWEZ
Treatment		0.0618*	0.0494	-0.0111	-0.00735	0.0466
		(0.0350)	(0.0406)	(0.00885)	(0.0318)	(0.0591)
N		1043	1082	1063	177	234
R^2		0.974	0.963	0.986	0.996	0.984
		Panel C: Expanded CCZ/WEZ				
Treatment		0.0255	0.00484	0.0133	-0.00927	0.00658
		(0.0248)	(0.0186)	(0.0101)	(0.0284)	(0.0342)
N		1237	1301	1319	561	653
R^2		0.983	0.993	0.988	0.995	0.992
		Panel D: Distance to CBD polynomials-by-year				
Treatment		-0.0962**	-0.0542***	0.00713	-0.0525**	0.0727
		(0.0396)	(0.0176)	(0.0167)	(0.0203)	(0.0456)
N		2802	2941	2982	1152	1403
R^2		0.981	0.981	0.988	0.992	0.990

Note: Fixed Traffic Count Point regression. Sample for the different events includes traffic count from year *t-2* to year *t+2* where *t* = the year when treatment is implemented. Refer to figure 4 for more information. Dependent variable is the logarithm of traffic count for 4 wheels or more drive. Additional control include year dummies. In Panel A, announcement dates are used to define treatment period. In Panel B (C), I shrunk (expand) the treatment area accordingly to mitigate concerns that the treatment effects could be due to CBD. In Panel D, distance to CBD and the polynomials (up to 4) interacted with year for CPs within 3 kilometres from CC boundary. CCZ2014 is excluded as I do not have traffic count after 2014. Robust standard errors clustered at ward level reported in the parenthesis.

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

Table 9: Residential Property Prices and Traffic in the Congestion Charge Discount Zone

	(1)	(2)	(3)	(4)	(5)
Panel A: Property Prices					
	5km	4km	3km	2km	1km
Discount	0.0385*	0.0265	0.0111	0.00621	0.00995
	(0.0198)	(0.0212)	(0.0225)	(0.0227)	(0.0238)
WEZ	0.0893***	0.0753***	0.0622***	0.0520***	0.0340**
	(0.0135)	(0.0134)	(0.0141)	(0.0153)	(0.0162)
N	105830	85073	65984	47215	29062
R^2	0.816	0.811	0.803	0.800	0.765
Discount	0.0287	0.0160	-0.00966	-0.0255	-0.0250
	(0.0196)	(0.0196)	(0.0198)	(0.0210)	(0.0217)
RemWEZ	0.0514***	0.0388***	0.0250	0.0202	0.0108
	(0.0145)	(0.0147)	(0.0156)	(0.0163)	(0.0169)
N	130425	104195	80974	57020	34471
R^2	0.829	0.826	0.818	0.818	0.785
Panel B: Traffic					
	(1)	(2)	(3)	(4)	(5)
	5km	4km	3km	2km	1km
1. WEZ					
Discount	0.0111	0.0113	0.0119	0.0532	0.0677**
	(0.0146)	(0.0158)	(0.0186)	(0.0321)	(0.0282)
N	1767	1456	1152	738	466
R^2	0.992	0.992	0.992	0.995	0.996
2. RemWEZ					
Discount	0.0504*	0.0506*	0.0534*	0.0269	0.00191
	(0.0256)	(0.0265)	(0.0284)	(0.0331)	(0.0514)
N	2160	1760	1403	887	583
R^2	0.988	0.989	0.990	0.990	0.989

Note: Difference-in-difference-in-difference estimates are reported in this table. Key estimate reported is the binary variable for untreated buildings outside WEZ entitled to 90% CC discount interacted with the time period when Western Extension is implemented and removed ($Discount$). In Panel A, the dependent variable is the natural logarithm of the transacted property prices. Specification is similar to that of Column (6) in Table 4 other than the inclusion of $Discount$. In Panel B, dependent variable is the natural logarithm of traffic at CP. Specification is similar to that in Panel E of Table 7 other than the inclusion of $Discount$. Analysis is not conducted for original CCZ due to the small sample size. Robust standard errors clustered at 300 cross-boundary wedges are reported in parenthesis.

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

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