Did the London Congestion Charge Reduce Pollution?

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Abstract

Recent vehicle charging schemes aim to reduce pollution and other congestion externalities. We reexamine the London congestion charge introduced in 2003 and demonstrate significant reductions in several pollutants relative to controls. We even find evidence of reductions per mile driven suggesting amelioration of a congestion externality. Yet, we find a more robust countervailing increase in harmful NO_2 likely reflecting the disproportionate share of diesel vehicles exempt from the congestion charge. This unintended consequence informs on-going concern about pollution from diesel-based vehicles and provides a cautionary note regarding substitution effects implicit in many congestion charging schemes.

KEYWORDS: Pollution; Traffic; Congestion Charging.

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I. Introduction

Starting in 2003 the Greater London Authority imposed a charge for driving during prime hours on the roads in its central district. Supporters championed this congestion charge as a tool to battle the incredibly slow speeds and gridlocked traffic of the UK capital. These same supporters saw a "secondary benefit" of reduced air pollution (Transport for London, 2004). Whether or not this secondary benefit came to fruition has taken on increasing importance as a British Parliament select committee recently declared London air pollution a "public health emergency" (Carrington, 2016) and argued for new charges within the congestion zone specifically designed to combat vehicle emissions.[1] With as many as 50 thousand premature deaths in the UK due to air pollution and with automobile exhaust the single most rapidly rising source of deaths world-wide (Lim *et al*., 2012), the time is ripe for greater understanding the consequences of the original London congestion charge on air pollution.

This paper reexamines the introduction of the London Congestion Charge Zone (CCZ) in 2003 focusing on three objectives. The first objective is to test whether the CCZ reduced harmful pollutants associated with motor vehicles. This has been the subject of previous research as we will summarize. In practice this research often lacks an identification strategy likely to provide credibly causal estimates. We adopt such a strategy by relying on comparisons with other urban areas. The second objective, simply not previously examined, recognizes that while pollution itself may evidence an externality, it may be made worse by the underlying congestion externality. Thus, we test whether or not the congestion charge reduced pollutants beyond the underlying reduction in traffic flows. A reduction in pollution per mile driven likely reflects

[1] Slated to start in September 2020, the Ultra Low Emission Zone requires cars, motorcycles, vans, minibuses, buses, coaches and heavy goods vehicles to either meet far tighter exhaust emission standards or pay a daily charge to travel. The charge will apply inside the current Congestion Charging Zone (CCZ) and will be in addition to the existing congestion charge (Transport for London, 2015).

alleviating the congestion externality and improving road speeds. The third objective, not sufficiently explored, tests for substitution effects implicit in the details of the congestion charge. In common with other congestion charge pollution schemes, the London CCZ incorporated both a range of exemptions by vehicle type and, at the same time, active promotion of alternatives to personal car travel. We show an influence on the mix of pollutants consistent with such substitution.[2]

Air pollution stands as a textbook negative externality 'inherently' not priced into individual decisions (Walters, 1961; Vickery, 1963). While governmental action is not new (e.g. the UK Clean Air Act of 1956 responding to the 1952 'great smog of London'), attention has been renewed in major cities where air pollution, largely due to exhaust, frequently exceeds harmful levels. Indeed, London has remained in violation of governmental standards since 2010 and lost a critical Supreme Court decision for its failure to meet standards in 2015 (Harvey, 2015). The idea that reducing congestion can improve air quality and improve health seems sensible and has received support. Currie and Walker (2011) show that increased speed and eliminating the traffic congestion associated with toll road booths contributes significantly to improved health among infants.[3] Knittel *et al*. (2016) use shocks in traffic interacted with weather to show that reduced automobile congestion reduces ambient air pollution and lowers infant mortality in California. Wolff (2014) examines German cities implementing the European Commission's 2005 Clean Air Directive by prohibiting entry by high polluting vehicles. He demonstrates marked reductions in pollution with no offsetting pollution increase in non-policy areas.

[2]Davis (2008) found that driving restrictions in Mexico City based on license plate numbers changed the mix of vehicles toward those with higher emissions.
[3] Relatedly, Fu and Gu (2017) show that a temporary elimination of road pricing between major Chinese cities increased congestion and pollution.

While a range of potential policy interventions might be implemented to address the externality of pollution, the efficient pricing of auto exhaust remains difficult. The determination of proper Pigouvian taxes depends on understanding the associated damages (Vickery, 1963). These vary by type and vintage of vehicle, the number of other drivers on the road at the same time, the concentration of drivers nearby and the number of other non-driving citizens in close proximity (Newberry, 1990). This variation means that second best uniform taxes like the gasoline tax perform very poorly in eliminating the deadweight burden associated with auto exhaust (Knittel and Sander, 2013). This has led to somewhat more targeted approaches with urban driving charges among the leading candidates. Yet, optimal road pricing also remains complicated and should also vary with the type of vehicle and time of travel. Pricing is further complicated by interactions with parking provision and costs (Fogerau and De Palma, 2013) and the endogenous choice of speed by drivers (Verhoef and Rouwendal, 2004).

In contrast to such optimal pricing, actual congestion charges are blunt instruments. Nonetheless, London, Stockholm, Singapore and Milan, have each adopted congestion charges within the last 20 years. Over the same period New York City, Hong Kong, Manchester and Edinburgh have rejected explicit bids for such charging. Such rejections often reflect political resistance to a charge not explicitly designed to pay for infrastructure (Hårsman and Quigley, 2010). The adopting cities vary in their emphasis on increasing traffic speeds (London) and on reducing pollution (Milan). They also differ in earmarking the revenue from the congestion charge. London earmarked mass transit improvements while Stockholm earmarked road construction. Yet, insofar as these charges successfully reduce traffic flows, the schemes have the potential to reduce motor vehicle pollution in settings where the density of living and foot traffic is high and so where the damage from pollution is likely to be substantial.[4]

While some trips to the city center simply may not take place, congestion charging policies seem more likely to change the method of transit. Driving becomes more expensive and, at least in London, mass transit was improved, especially the bus service. In addition, certain forms of transit are exempt from the London congestion charge. These include bikes, motorcycles, taxis and mass transit.[5] As might be anticipated, these exemptions meant that more travelers used buses and taxis in central London (Transport for London, 2005). In both cases, this causes a move away from predominantly petroleum-based transportation (private vehicles), towards a diesel-based transportation (black cabs and buses). In fact, a key component of the congestion charge introduction was an increase in bus frequency and expansion in bus routes (Transport for London, 2006)

This raises questions regarding the effect of the policy on pollution levels, and the pollution mix. While many forms of air pollution can hurt health (see Lagravinese et al. 2014), there are mounting concerns regarding the dramatically negative effects of diesel-based pollution in urban settings. Indeed, the UK Department of Energy and Climate Change concludes that fumes from diesel are significantly more harmful than those from petrol engines and the World Health Organization lists diesel but not petrol fumes as a Group 1 carcinogen (Vidal, 2013). Historically, these concerns in part reflected the fact that diesel engines emitted higher levels of particulate matter than petrol engines. However, changes in modern diesel engines over the last several

[4] Beyond the London studies which we will review, Gibson and Carnovale (2015) examine the Milan congestion charge demonstrating marked reductions in CO and particulate matter. The fees in Stockholm reduced ambient air pollution which, in turn, reduced acute asthma attacks among young children (Simeonova *et al*., 2018).

[5] Road safety initiatives together with the exemption on bikes resulted in a huge increase in cyclists in London with controversy surrounding the increase in cyclist injuries and in their breathing of exhaust (Green *et al*., 2016).

decades first closed this gap and recent diesel engines actually emit less particular matter than comparable petrol-based engines (Platt *et al*., 2017). Diesel combustion continues to still produce higher levels of NO and NO_2 emissions. Yet, NO_2 emissions are much greater with diesel use while NO is only modesty greater (UCAR 2017; Air Quality Expert Group, 2004; Khalek *et al*., 2009). Thus, if sufficiently many petrol cars are taken off the road, NO could decrease while NO_2 increases.[6] This is critical because NO_2 is linked to a range of particularly adverse health outcomes including severe lung and respiratory problems (see for instance Guerriero *et al*., 2016). Moreover, the scientific consensus increasingly regards the association between respiratory morbidity and NO_2 to be causal and not just a function of other associated pollutants (Committee on the Medical Effects of Air Pollution, 2015).

As mentioned, we are not the first to test whether the London congestion charge reduced pollution. However, this previous literature has often suffered from a lack of suitable comparison areas. Tonne *et al*. (2008) find modest reduction in pollutions, approximate 1% reductions in NO_2 and PM_{10}, simply looking before and after without control jurisdictions. This assumes both that the vehicle fleet remained constant (highly unlikely given the exemptions) and that pollution would have remained entirely unchanged conditional on observables in the absence of the charge. Atkinson *et al*. (2009) use jurisdictions within London as controls finding mixed results depending on pollutant and methodology. Jurisdictions within London seem an unsuitable control. First, there exists potential substitution in travel behavior and pollution in areas surrounding the charge area (Wolff, 2014; Green *et al*., 2016). Second, the potential for common shocks to regional pollution seems unlikely to be adequately captured by control variables.

[6] Such predictions are difficult because after emission, NO further oxidizes and becomes NO_2 (Air Quality Expert Group 2004 and UCAR 2017).

As this review suggests, it can be problematic to find suitable comparison areas for policies aimed at reducing congestion externalities. By their very nature, such policies typically find support and are implemented in the densest and most congested urban areas. As made clear in the economics literature on urban form, the densest urban areas bring higher wages, greater innovation and more amenities but also the major cost of greater pollution (see Ahlfeldt and Pietrostefani, 2019, and sources therein). In response, we adopt the approach of Green *et al*. (2016) who examined the influence of the London congestion charge on traffic accidents. We take observations from the 20 other largest UK cities (in terms of population) as comparisons. This, as we discuss in the data section, brings data quality advantages. It avoids making comparisons within the same dense urban area of London in which pollution can easily travel or be generated outside the congestion zone in response to the policy.[7] Yet, the number 20 remains arbitrary so we examine robustness using narrower comparisons (10 largest and 5 largest) and let model-based methods determine the appropriate comparison.

We implement two obvious model-based candidates. First, we use propensity score matching (PSM) to select comparison groups most similar to the treated area. In practice, we are limited by a lack of cross-sectional variation. We have only 2 treatment cross-sectional units and adopting two alternative comparisons over all periods seems very limiting. Thus, we adopt a form of PSM that selects the best matched observations to the treated units among the potential control area but allow these to vary over time. Our propensity score measure is estimated through a probit specification on the basis of observable co-variates such as vehicle miles travelled, population size, unemployment rates and weather measures that are collected annually at the local authority level. Second, we adopt a synthetic control approach which matches a weighted

[7] We recognize that in contexts other than pollution the edge of the congestion zone can be critical. For example, see Tang (2018) who examines house prices on either side of the boundary.

average of the potential controls to best mimic pre-intervention trends of the treatment. Thus, we adopt a range of approaches aimed at examining the robustness (or not) of the main policy effects.

In addition to issues of a suitable control, previous work does not examine the relationship between pollution and miles driven and so does not address the congestion externality at the heart of the charging scheme. Instead, they may simply reflect that reductions in miles driven reduced pollution. We examine whether the alleviation of congestion was sufficient that reduced standstills and faster commutes reduced pollution per mile.

To summarize our findings, we demonstrate varied but substantial reductions in three pollutants but a sharp increase in NO_2. The reduction of the first three pollutants can credibly be linked to the reduction in petrol-based and overall motor-vehicle transportation. We argue the NO_2 increase likely reflects the unintended incentives that the charging scheme provided to shift towards diesel-based transportation. These findings differ from previous literature which, as highlighted before, often found mixed effects of the congestion charge on pollution and where the findings of an increase in NO_2 are simply absent or not robust.[8] We also show that the reduction in the three basic pollutants exceeds that expected from the reduction in traffic flows alone. As such, it provides evidence of these pollutants (but obviously not NO_2) potentially being reduced because of ameliorating a congestion externality beyond simply reducing miles driven. In addition, we further examine statistical inference by adjusting in various ways for the small number of treated jurisdictions. This reveals that the increase in NO_2 stands as a far more robust result than the reduction in the other three pollutants.

[8] Thus, Atkinson *et al*. (2009) find that based on roadside monitors it was not possible to identify any relative changes in pollution concentrations in the CCZ relative to other London controls. Yet, when they examine monitors not on roadside (background) monitors they find no relative change for PM10 and CO but a decline for NO and an increase for NO_2.

The finding that substitution may thwart the objective of reducing pollution fits other evidence such as licensed plate-based driving restrictions in Colombia where pollution reductions were undermined by both the purchase of a second car and the use of alternative dirtier modes of transportation (Zhang *et al*., 2017). It argues that aggregate reductions need not be beneficial (Borck 2019) as it provides a cautionary tale about exemptions that altered the fuel mix in an unhealthy manner. These exemptions resulted from the desire to encourage mass transit (buses) and to protect entrenched interests (taxis). Ultimately, we are unable to definitively identify the relative contribution of increased bus and taxi use. However, a substantial role for polluting diesel buses would fit with efforts to replace the London fleet with newer hybrid buses.

The remainder of the paper is structured as follows. The next section provides background information on the introduction of the CCZ. Section 3 sets out the data sources and empirical methodology. Section 4 provides the results, while section 5 concludes.

2. Background on the Congestion Charge

Central London has long been among the most congested of western cities. Traffic speeds decreased and vehicle counts increased continuously over the second half of the 20th century. Just prior to imposing the congestion charge, all-day average speeds averaged a low 8.6 mph and more than 1/3 of all travel time was spent at a complete standstill (Transport for London, 2003).

The London congestion charge was first imposed on the 17th of February 2003. The initial charge was £5 for entering the congestion zone between 7 a.m. and 6:30 p.m. on weekdays. Despite subsequent increases in fees (£8 in 2005, £10 in 2011 and £11.50 in 2014), and charging times (reduced to 6pm in February 2007), the charge still exists largely in its original form. Passes can be purchased on-line and enforcement relies on a series of video cameras at every entry point to the zone and on mobile units within the zone. A sophisticated license plate recognition system matches against daily purchases and violators are sent penalty notices for escalating fines that average 20 to 30 times the daily charge. The day pass allows travel in and around the congestion zone of Central London. This eight square mile zone includes tourist sites, the City (London's financial district), Parliament, major government offices and prime business locations (see Figure 1). This zone was extended in February 17th 2007 to take in areas immediately west of the initial congestion zone (the so-called 'Western Extension') but this extension was subsequently removed in December 24th 2010.[9] As discussed later, this timing ultimately influences our policy window.

< INSERT FIGURE 1 >

The charge applies to private and commercial vehicles entering the congestion zone during the charging hours, but motorcycles, bicycles, buses and taxis are exempt. There are exemptions for vehicles belonging to those who live within the zone but keep their vehicles off the street during the charging hours. When these residents do travel during the charging hours, they pay a highly discounted charge of 10 percent of the full charge.

Revenue raised from the charge program is earmarked primarily for mass transit improvements, along with smaller expenditures on road safety and bike/walking initiatives.[10] Santos (2005) identifies that a key part of the mass transit initiative was an expansion of the bus

[9] It is also worth noting that in February 2008 the Low Emission Zone (LEZ) policy was introduced which charged certain high emission vehicles for driving in the Greater London area.
[10] Note that the well-known London Bike rental programme, colloquially known as Boris Bikes, did not start until 2010 and is separate from the Congestion Charge initiative.

transit network within the zone and across London. Santos and Schaffer (2004) and Leape (2006) report initial changes in traffic flows after the introduction of the congestion charge. Notably, while overall traffic volume decreased, bus travel flows increased by 22% and Taxi flows increased by 21%. These increases persisted for many years as shown by Santos (2008). As emphasized, this raises potentially unintended consequences as, in London, these two types of vehicle are exclusively diesel powered.

3. Data and Methodology

The data used in this paper come from several administrative sources. We draw pollution data for both the CCZ and control cities from fixed location monitoring stations within the UK. We focus on a set of pollutants related to vehicular traffic for which we have consistent data across our period of interest: CO, NO, PM10 and NO_2. We collect pollution data from stations within the congestion zone area and from other urban areas of Britain. The concentrations of the specific pollutants are reported hourly from each station. We chose as our comparison groups the twenty largest UK cities, in terms of population, that had pollution monitoring stations in a fixed location over the time period being examined. The set of comparison cities are Aberdeen, Belfast, Birmingham, Brighton, Bristol, Cardiff, Glasgow, Hull, Leeds, Leicester, Manchester, Newcastle, Nottingham, Plymouth, Portsmouth, Sheffield, Southampton, Stoke, Swansea and Wolverhampton. When more than one station exists within a local authority, we adopt the most central location in order to best mimic the CCZ. We have two stations within the CCZ each within their own local authority. Thus, we have a single reporting station per local authority in both our treatment and control.

We explicitly exclude all monitoring stations for London and the greater London area outside the congestion zone. This exclusion is crucial as past work has shown that the congestion charge influences urban area traffic flows outside the CCZ.[11] Thus, there is a high potential for pollution spillover effects from the congestion charge when other London jurisdictions have been chosen as controls. Moreover, it seems sensible that there also exist common unobserved factors that influence pollution, or traffic flows, in both the CCZ and other parts of London threatening independence of treatment and control. Our emphasis on using controls outside the urban area is unique and causes us not to use, for instance, the Kings College data used by several of the earlier studies. In extensions however, and in the interest of understanding the total effect of the congestion charge zone on pollution, we examine the effect on areas of London surrounding the charge zone.

We restrict our data to the period from 2000 to 2007 for several reasons. First, pollution data before 2000 is simply less reliable. There are fewer air quality reporting stations and many more problems with missing data. Second, we attempt to achieve consistency by roughly balancing the time before and after the introduction of the CCZ. Third, we stop at the end of 2007 as the Low Emission Zone (LEZ) introduced in early 2008 seems a potentially important confounding factor.[12] This also means that we are examining the effect of the CCZ before the also potentially confounding introduction of the western extension. The pollution effects of this extension are interesting, but we cannot easily disentangle this from any effects of the LEZ which covered much of greater London.

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[11]Green *et al*. (2016) demonstrate that the introduction of the charge reduced traffic flows in areas outside the CCZ.
[12] This was a more modest version of the Ultra Low Emission Zone described in the introduction. It did charge certain vehicles if they had extremely high emissions.

In addition to this data we utilize weather data drawn from the Met Office-MIDAS Land Surface Station Data Source. We match weather and pollution stations geographically and use daily weather information. We ultimately match our pollution data to traffic flows data available from the Traffic Count Data Source collected by the Department of Transport. This data is available at the road level and aggregated to the local authority level. Thus, we match to each monitoring station, local authority population, vehicle miles driven and unemployment rates and weather from the closest weather station to the monitoring station. These are matched with comparable data for the two stations within the CCZ, each within its own separate local authority.

The traffic flows data has two additional complications. First, it is annual providing fewer observations. Second, the disaggregation by vehicle type is limited by the underlying mechanics of the surveying technology. Thus, we cannot distinguish between private cars and taxis in the flow data. This is important as the taxis are exempt from the charge. We discuss our approaches to using this data in more detail later when discussing our pollution rate estimates.

Our basic approach is to estimate variants of the following:

$$P_{it} = \varphi + \delta CCZ_i + \alpha Policy_t + \beta(CCZ_i * Policy_t) + \gamma X_{it} + \tau T_t + \varepsilon_{it} \quad (1)$$

The underlying observation is the local authority *i* at time *t* with the dependent variable an hourly pollution reading. The core estimates are limited to the hours of the congestion charge. The coefficient β provides a difference-in-difference estimate of the effect of the introduction of the CCZ on pollutant *P*. We observe two stations within the zone (Bloomsbury and Westminster) and our main approach is to use both.[13] In robustness checks we estimate (1) using each station

[13] We cannot identify PM10 within the charged time for the Westminster station as it provides only daily measures for PM10. As a result, our main estimates for PM10 are for only the Bloomsbury station. In unreported results, we estimate *daily* observation models of PM10 using both Bloomsbury and Westminster together. These are available

in turn. *Policy* is an indicator variable for an observation from the 17th of February 2003 onwards and *X* is a vector of controls. We adopt a range of approaches to specifying the time dimension. We routinely allow for differential trends by treatment and control and show the results are robustly allowing for even station specific time trends. Similarly, experimenting with a variety of time specific dummies does not greatly alter the estimates.

We estimate (1) separately for each of the 4 pollutants. As emphasized, the substitution to diesel transport suggests that β may be differently signed according to pollutant type. Our basic estimates cluster standard errors at the level of the local jurisdiction. Recognizing the potential for problems we examine the robustness of our inference to approaches aimed at estimating correct standard errors in the presence of small numbers of clusters.

3.1 Initial Description of the Data

As an initial description, Figures 2 and 3 present information on monthly average pollutant levels before and after the introduction of the CCZ for the treatment and control local authorities. In Figure 2 we present the first hint that the response of CO, PM10 and NO differs from that of NO_2. In the left panel we aggregate the first three pollutants by transforming each observation to its relevant z-score and then averaging across pollutants.[14] The trend lines clearly show a *decline* after the CCZ relative to the comparison. This contrasts with the right panel which shows declining NO_2 that was declining more steeply in the treated area prior to the CCZ. The CCZ itself is associated with a huge *increase* of more than a standard deviation in NO_2 relative to the control. Figure 3 shows the underlying monthly pollution averages for each of the four

upon request, but the resultant policy estimates remain essentially unchanged. Likewise, our estimates are not sensitive to using only one of the two stations in our main results.

[14] Z-scores are obtained by normalizing each pollution observation by subtracting the mean of each pollutant and dividing it by the pollutant specific standard deviation.

pollutants measured in the concentration levels relevant for each rather than in z-scores. The visual pattern remains evident.

<INSERT FIGURE 2 and 3>

At issue is whether the apparent changes associated with the initiation of the CCZ remain after examining the disaggregated hourly data (not monthly averages) and after accounting for controls and proper inference. All figures raise concerns regarding the violation of the parallel trends assumption. These concerns are not, for instance, mitigated in unreported figures plotting these trends after controlling for monthly fixed effects and weather. Thus, our approach, as discussed above, includes treatment specific trends (the trend plus a treatment times the trend) and tests using even individual local authority trends to evaluate the robustness of our results. We also experiment with a wide variety of time fixed effects including year, month and hour among others. We now turn to those results.

4. Results

Table 1 provides estimates of the impact of the introduction of the congestion charge on the four different pollutants from 2000-2007. All models include controls that capture daily local authority weather variation (see Table 1 notes for more detail). Weather conditions profoundly influence pollutant concentrations and the concern is that weather differences over time and between treatment and control may influence the results. In the final column of Table 1 we additionally include all weather controls in quadratic form to explore the robustness of our results to potential non-linearities in the impact of weather. Note that each table, including Table 1, reports pretreatment means for the congestion zone to aid in interpretation.

<INSERT TABLE 1>

Table 1 suggests that the introduction of the CCZ reduced the levels of CO, PM_{10} and NO. In the case of the latter two pollutants, these estimated reductions are largely unaffected by different approaches to controlling for time variation through temporal fixed effects.[15] These include Estimated reductions in PM_{10} range from -5.6 to -7.7, while for NO the reduction ranges from -7.2 to -9.5. These are large effects of around 20% reductions when compared to the pretreatment means for the congestion zone. These are important effects of the policy insofar as these pollutants are associated with a range of negative health outcomes. At the same time, the reductions in CO are small between 6% to 9% and the statistical significance of these fade with some of the controls for time variation.

The results for NO_2 differ starkly. The key estimate of interest remains positive across all specifications. The effect magnitudes are large at 14% to 17% depending on the specification. This provides initial evidence of substitution effects as a result of the congestion charge. Three pollutants decrease while NO_2 increases dramatically.

A concern is that identification of the key parameters come from a change in policy for only two local authorities in a relatively small number of overall local authorities. Clustering at the local authority level in this case can cause the reported standard errors to be misleadingly small (Bertrand *et al*., 2004). In response we implement the Wild bootstrap procedure from Cameron *et al*. (2008). This reduces the high type I error rates common in the presence of clustering on a small number of groups. The procedure replicates the within group correlation in

[15] Note that in robustness checks we add day of the week and week of the year fixed effects to each of the estimates in Table 1. The pattern shown in Table 1 remains essentially unchanged. The estimates are available upon request.

errors when generating new estimates (Cameron and Miller, 2015). Under the null hypothesis of no difference in difference effect, the Wild bootstrap p-values clustered at a local authority level with 1000 replications were performed for every estimate in Table 1. In no case in Table 1 does the bootstrapping reverse the claims of statistical significance.

As an alternative, we follow Simeonova *et al*. (2018) and conduct a permutation test where equation (1) is estimated using 1000 Monte Carlo simulations in which treatment is randomly assigned across monitors and time. We then examine the fraction of times that the coefficient estimate exceeds the estimated value when the CCZ is correctly assigned. Our results always yield p-values of nearly zero suggesting that virtually none of the permutation coefficients exceeded the actual coefficient. This simply reinforces the Wild bootstrapping exercise.[16]

As discussed earlier, previous literature on the pollution effects of the London Congestion Charge may not have adopted the appropriate comparison groups. This is likely inherent in the analysis of policies aimed at reducing urban traffic in uniquely congested cities. We have argued that it is unlikely that areas near the treated ones are sensible controls and so focus on other distant major cities that are less likely to exhibit confound effects. Yet, within this broad approach we investigate the stability of our main estimates to a range of alternative methods of constructing comparison groups. These are summarized in Table 2.

< INSERT TABLE 2 >

[16] One might be concerned that extreme cases of pollution generate the patterns evident in Table 1. To examine this, we truncated the largest 1 percent of observations and the largest 5 percent of observations. The pattern of results remains plainly evident and these two replications of Table 1 are available upon request.

The first two columns report variants of our main approach by restricting our comparison groups to, successively, the 10 and the 5 most populous cities in the UK. Restricting to the 10 most populous cities leaves the estimates essentially unaffected for PM_{10}, NO and NO_2, while the estimates for CO become more negative. Moving to the 5 largest cities has more deleterious effects on precision, particularly PM_{10}, which when compared to a reduction in the size of the estimate means we can no longer detect a negative statistically significant effect on this pollutant. The effects for the other pollutants are essentially unchanged. Thus, narrower comparison groups leave the estimates of the effect of NO and NO_2, while those for CO and PM_{10} fade.

The next two columns adopt approaches based on model selection of appropriate comparison groups through propensity score matching. We build a propensity score to select control observations most similar to the treatment observations in terms of observed characteristics including local authority population size, unemployment rates, vehicle miles travelled and weather controls. Given the propensity score, we conduct two estimates. The first uses the overlapped region to compare our treatment with the controls having very similar observed characteristics. Estimates remain robust in terms of direction and significance. In an alternative estimate (Column IV of Table 2), we use the inverse of our propensity score to weight our regression. Again, the main estimates are, in the whole, robust to this approach and essentially follow the patterns reported in Table 1.

Finally, we adopt a synthetic control approach as set out by Abadie and Gardeazabal (2003). This generates a unique control group time series of pollution levels as a weighted average of pollution levels in the different potential control local authorities. This is generated to optimally match pre-intervention trends of the treatment group in terms of not only pollution levels but also observed characteristics such as population, unemployment rates, vehicle miles travelled and weather controls (as we were doing in the PSM approach). One difficulty implementing this method is occasional missing data as reporting stations temporarily malfunction or are replaced. The synthetic control approach requires no missing data, this leads us to move away from hourly data and instead aggregate data from each local authority to an average monthly level. When examining NO_2, as an example, the mean squared prediction error between the CCZ and the control was reduced from 359 over all equally weighted control cities to only 12.2 with the optimal city weighting. The procedure routinely gave greatest weight to Hull and Manchester.

The resulting estimates are reported in the column V and corresponding figures reported as figure 4. The estimates appear somewhat larger and less precise. Yet, the general pattern remains. The differences from earlier results may flow from the aggregation rather than from identifying a synthetic cohort. Thus, in column VI we ignore the synthetic cohort procedure and simply provide estimates with data aggregated for each local authority to the monthly level. These estimates tend to look broadly like those using the synthetic cohort. In summary, the results in Table 2 suggest that once we have decided to use major cities outside London as our control, the exact choice of how we use them is not crucial.[17] With this in mind, we revert to using the 20 largest cities in the UK as the comparison group for the remainder of the paper.

<INSERT FIGURE 4>

We recognize that there may exist unmeasured differences in local authorities that influence pollution measurement. These differences may influence the key measurements in the treatment area and across our controls. As many of these differences seem unlikely to vary

[17] As with Table 1, neither Wild Bootstrapping nor permutation tests reverse any claims of achieving statistical significance.

systematically over time, we re-estimate our main models introducing local authority fixed effects. Such estimates remove the effect of time invariant local authority specific factors that might bias the estimated influence of the policy. Table 3 reports these estimates replicating the alternative temporal controls from Table 1.[18]

< INSERT TABLE 3 >

The point estimates associated with the first three pollutants do not differ dramatically across specification in Table 3 or from those presented in Table 1. Even when we allow local authority specific trends (column VI), the pattern remains.[19] While the permutation tests continue to reinforce the pattern of significance, for the first time, the Wild bootstrap reverses a claim of significance. Using the bootstrap inference, the reductions in CO are not statistically different from zero at reasonable levels even as the magnitudes remain as in Table 1. The bootstrap inference continues to support significant declines in both PM10 and NO.

The point estimates for NO_2 suggest an increase in concentration of 5 or 6 which is smaller than the results in Table 1 without local authority fixed effects. Those earlier results suggested an increase of 8 or 9. Nonetheless, there is no reversal in significance as the bootstrap inference continues to easily indicate statistical significance. Thus, allowing for local authority fixed effects and specific trends leaves in place the general view that while the levels of some pollutants decrease, that of NO_2 has clearly increased.

Table A3 aims to provide evidence on spatial heterogeneity across areas in London surrounding but outside the charge zone. In order to do this, we identify 3 London areas each

[18] Again, in robustness checks we add day of the week and week of the year fixed effects to each of the estimates in Table 3. The pattern shown in Table 3 remains essentially unchanged. The estimates are available upon request.
[19] Table A2 provides a corresponding version of Table 2 where we include local authority fixed effects.

farther away from the congestion zone as different potential treatment groups. This allows us to see whether the effects we identify attenuate as we move away from the congestion zone area or, on the contrary, reverse or become stronger. The latter could happen if, for example, traffic is displaced outside the congestion charge zone. Our estimating approach is to vary equation (1) by additionally including dummy variables for these areas and the interaction between these and the policy. We do this for different combinations of areas in greater London but always including the main congestion zone areas. Table A3 reports these area specific, difference in difference, estimates.

In all cases in Table A3, the significant pattern of CCZ results remain. When we compare columns I, IV, VII, X with columns II, V, VIII and XI we observe that the point estimate is very similar for the CCZ difference-in-difference regardless of whether surrounding jurisdictions are treated. There are, however, significant coefficients within some of the surrounding areas for some of the pollutants. Given previous evidence that traffic flows in surrounding areas were influenced by the CCZ this is not surprising (Green *et al*., 2016). Indeed, it supports our basic motivation that controls within London may be unsuitable as they will be variously influenced by the congestion charge. Similarly, we urge some caution in interpreting coefficients for the London areas outside of the CCZ due to the potential for the comparison cities to be less suited as controls for the areas of London away from the central city.

The coefficients on the Panel B of Table A3 in the Appendix shows how the charge affected air quality in the charge zone outside charged hours. If anything, these suggest that the focus on charged hours understates the effect of the congestion charge on pollution. This result may reflect the well-known tendency for pollution to "linger" or that the congestion charge altered miles driven even outside the charged hours (Green *et al*., 2016). We also add a note of caution that part of the congestion zone policy was an increase in public transport and that late night public transport may be substantially more limited in other cities in the UK.

4.1 Rates of Pollution

To this point we have demonstrated reasonably robust reductions in pollutants associated with petrol-based vehicles, and an increase in NO_2 pollution closely linked to diesel based vehicles. The original hope for the CCZ was that it would improve speeds and reduce gridlock. Santos and Shaffer (2004), Leape (2006) and Green *et al*. (2016) suggest that this happened. This, in turn, raises the possibility that reductions in pollution might reflect not just that there was less driving but that a congestion externality was ameliorated. In the case of the three pollutants, if they were reduced simply in proportion to the miles driven, the pollution externality itself might be reduced but there would be no evidence of an improved congestion externality. Evidence in favor of ameliorating the congestion externality would be suggested if each mile driven by a charged vehicle into London was associated with less pollution.

This suggestion mirrors Edlin and Karaca-Mandic (2006) who argue that only a reduction in traffic accidents per mile driven is evidence of ameliorating a congestion externality. We recognize that measured pollution need not always be linearly related to miles driven due to absorptive capacity of the environment, wind patterns and terrain. Yet, traffic congestion is empirically associated on average with sharply deteriorating ambient air quality (see Zheng and Batterman 2013 and cites therein). It is this non-linear relationship that makes a reduction in pollution relative to miles driven suggestive evidence of reducing a congestion externality. Thus, less time spent in slow or stalled traffic (less congestion) would improve ambient air quality more than indicated by the reduction in total miles driven. At minimum, a reduction relative to miles driven stands as a far stricter standard and as, at least suggestive of reduced congestion. We now turn to this question: did the introduction of the congestion charge influence pollution per mile for charged vehicles?

We examine this by combining our earlier pollution data with traffic flow data sourced from the Department of Transport for each local authority. This data is only available at an annual level and as a result we aggregate our pollution data up to annual data. We compute the average pollution across the year within the charged time for each local authority. The dependent variable then becomes this average charge time pollution in the year divided by the millions of miles driven in the authority in the year.

As mentioned, a complication is that the surveying technology cannot distinguish between private cars and taxis, when ideally we would like to completely disentangle flows by charging status. In our preferred measure we divide the pollution levels by all the closest proxy of charged mileage available. For our combined pollutants this is cars. We estimate analogous models to (1) that include local authority and year fixed effects. These results are included as Table 4.

< INSERT TABLE 4 >

Interpreting these results requires a recognition that a zero estimate would imply that the earlier estimates entirely reflect changes in traffic flows. In other words, the decline in pollution merely reflected a decline in miles driven. Yet, this is not the case. There is a marked reduction in PM_{10} and NO. These do not markedly vary when using an alternative flow of all miles driven rather than our proxy for charged miles. These results suggest that the introduction of the charge reduced pollution beyond what would have been expected from the reduction in traffic flows itself. Thus, part of the reduction in the pollutants came from reduced congestion indicating that not only the pollution externality was improved but that a congestion externality may also have improved. The reductions in the rates of PM10 and NO appear roughly of the same order of magnitude as the estimates without rates but this is only an artifact resulting from dividing by millions of miles in the jurisdiction and that average number of miles is not far from a one million miles (see appendix).

The evidence on NO_2 shows a very large increase in the rate of pollution. This increase of over 20 in the concentration per million miles falls when dividing by total miles rather than charged miles. The fall in charged miles is more marked and so the increase in NO_2 looks somewhat more dramatic. Nonetheless, the increase when using total miles remains over 14 in the concentration and highly significant (also available upon request). This unique sensitivity of the NO_2 to the choice of miles (charged vs. total) continues to argue that it is associated with uncharged vehicles.

The vehicle flows that underlie these estimates are of interest in their own right. They allow us to expand on this point and show how the composition of vehicle miles driven changed as a result of the congestion charge. Table 5 estimates the difference in difference on the annual data for total miles driven and for miles driven by each type of vehicle that is given in our traffic flow data. The results show the large decline in total miles driven. Yet, counteracting this general movement is an increase in uncharged miles by buses, motorcycles and bicycles. The results also show the decline in the charged miles by heavy vehicles, light vehicles and cars (which unfortunately include the uncharged taxis). We use these estimates to make a back of the envelope calculation to suggest the increase in the miles driven by diesel powered vehicles.

< INSERT TABLE 5 >

While it is easy to observe the miles driven by diesel buses and transport vehicles (light and heavy), it requires sensible assumptions to imagine what happens to diesel powered taxis. We know from our data that the average annual miles driven in the CCZ prior to the charge by cars and taxis together is 478.70 million miles. From Leape (2006) we know that prior to the charge 24.9 percent of all taxi and car miles in the CCZ were from taxis or 119.2 million miles. Thus, we imagine that these taxi miles increased by the same percent as did the miles of other uncharged vehicles. From Table 5 the increase in the three uncharged categories is 15.45 million miles (the sum of the three coefficients). This happens on a base prior to the CCZ of 181.56 for an increase in miles driven by uncharged vehicle categories is 8.5%. If taxi miles increased by this same percent, the increase in diesel powered taxi miles would be 10.13. This should be combined with the decrease in diesel-powered heavy transport and the increase in diesel powered buses for a net increase of 8.49 million miles per year. Thus, it appears that increases in diesel miles driven stands as a crucial indicator behind the substantial increase in NO_2 generated by the congestion charge.

We recognize that our estimated increase in miles driven by buses in Table 5 of 3.6 million miles is smaller than our imputed increase in the miles driven by taxis of 10.1 million miles. This reflects the basic fact that many more miles are driven per year by taxis than buses in the CCZ (Leap 2006). This might argue for taxis being included in the charge scheme if perhaps with a smaller charge. Yet, as a caveat, both the pollution and the passengers per mile associated with buses exceed that from taxis. Thus, in the end we must leave the exact role of buses versus taxis in doubt but emphasize that both contribute substantially to the increase in NO_2.

4.2 Robustness: Additional Concern with Inference

We have presented a cautionary tale about the changing composition of pollution and isolated the increasing concentration of dangerous NO_2 even in the face of fewer driven miles and lower concentrations of other pollutants. We now turn to additional exercises to test the sensitivity of this conclusion. We follow the randomization inference procedure of Conley and Taber (2011) which is based on estimated coefficients (or where treatment point estimators can be used as test statistics). Using test statistic inversion, we construct confidence intervals in order to identify the key parameter when its identification arises from changes in policy by a small number of groups.

Table 6 presents the results for the full set of estimates from Table 3. These routinely include local authority fixed effects. The individual pollutants provide inconsistent and heterogeneous results. There is weak or no support for inferring a decline in the first three pollutants. The NO_2 results present a more consistent picture. The levels of NO_2 in the CCZ increased as a result of the congestion charge. The confidence bands rule out negative estimates indicating statistical significance. These results provide added emphasis to our concern that the single strongest inference is the unanticipated increase in the NO_2 associated with diesel.

<INSERT TABLE 6>

With this robust effect on NO_2 in mind, we further explore of influence of the CCZ on NO_2 levels by estimating a time-trend DD model following Ahlfeldt *et al*. (2017) and Vuuren *et al*. (2019). This is reported in Figure 5 and illustrates two related points. First, it demonstrates a sharp increase at the discontinuity which fits with the evidence throughout this paper. Second, it provides point estimates of the policy for leads and lags allowing us to be confident that our main policy effects do not reflect, for instance, mean reversion in NO2 pollution levels.

<INSERT FIGURE 5>

5. Conclusion

Air pollution in central cities has been a source of increasing concern. As vehicle exhaust represents a huge share of urban pollution, congestion charging offers a method of reducing total travel miles and standstills (congestion) and so reducing pollution. This paper reexamines the effect of the London Congestion Charge introduced in 2003 on a range of pollutants. It differs from past studies by using other major urban areas as controls. We demonstrate significant reductions across a range of pollutants in comparison to control cities in the same period. Moreover, these reductions are substantially larger than what would be expected from the reduction in traffic flows by itself. Thus, the charging scheme not only internalized a pollution externality, but had additional social benefits through the reduction of the congestion externality. The reduction in standstills and the speeding up travel time reduced pollution per mile.

At the same time, we focus on NO_2 closely linked to diesel powered motor vehicles. Exempting buses and taxis, and increasing the provision of bus services, meant that these diesel vehicles drove many more miles as a result of the congestion charge as commuters transferred out of personal cars into these forms of public transport. This reflected an explicit policy to expand public transport provision in the zone. Thus, the fuel mix of vehicles in the zone moved toward diesel to such an extent that we show that diesel miles increased.

The reduction in other pollutants must be weighed against the particularly negative health effects associated with a marked increase in NO_2. Our experimentation with alternative inference methods makes clear that the increase in NO_2 remains the most robust of the results we present. This provides a cautionary note regarding substitution effects implicit in congestion charging schemes. Reducing congestion and reducing the harms of air pollution may be related but are certainly not identical as our study shows. Indeed, the concern with diesel in Europe continues to grow with Dusseldorf and Stuttgart moving toward simply banning older diesel fueled vehicles. These and related moves now seem legally allowed by a recent German court ruling (Connolly, 2018). Indeed, Norway will ban diesel fueled vehicles as of 2025. London continues to have exemptions to the congestion charge that we have argued may be harmful but at the same time it has begun to increasingly rely on alternative charges (such as the LEZ) and even limiting some corridors to only electric and hybrid vehicles. The overall influence of these seemingly contradictory policies has yet to be observed.

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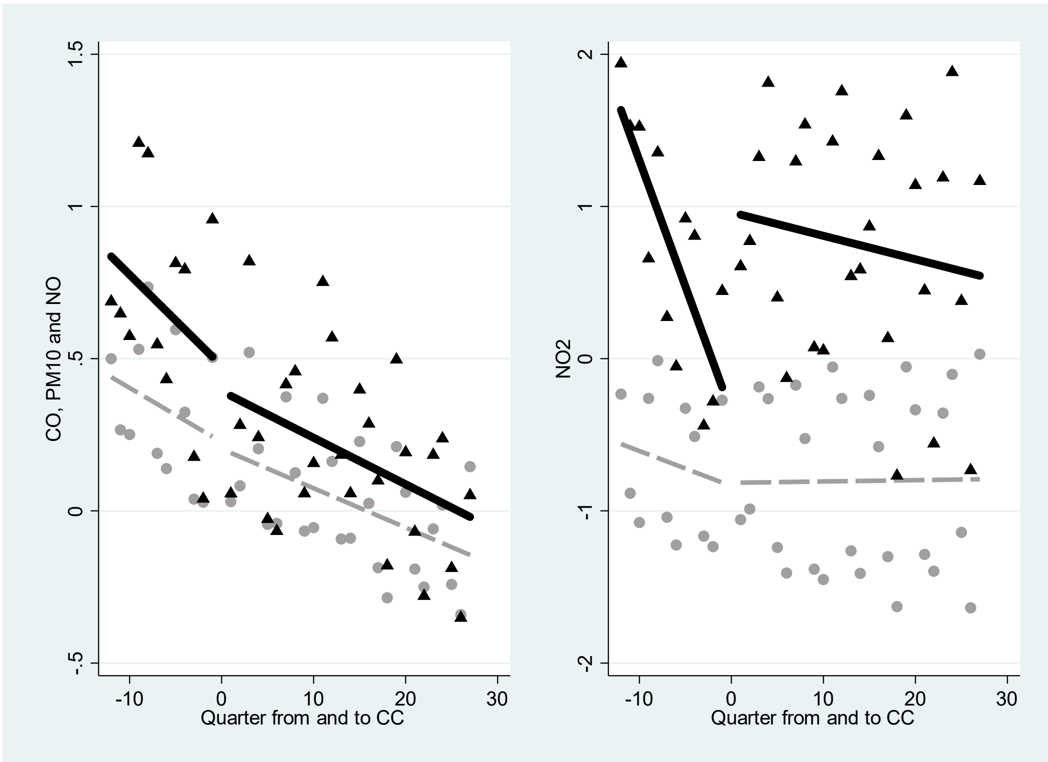
Figure 1: The original London Congestion Charge Zone

Source: Transport for London (2004 p.8)

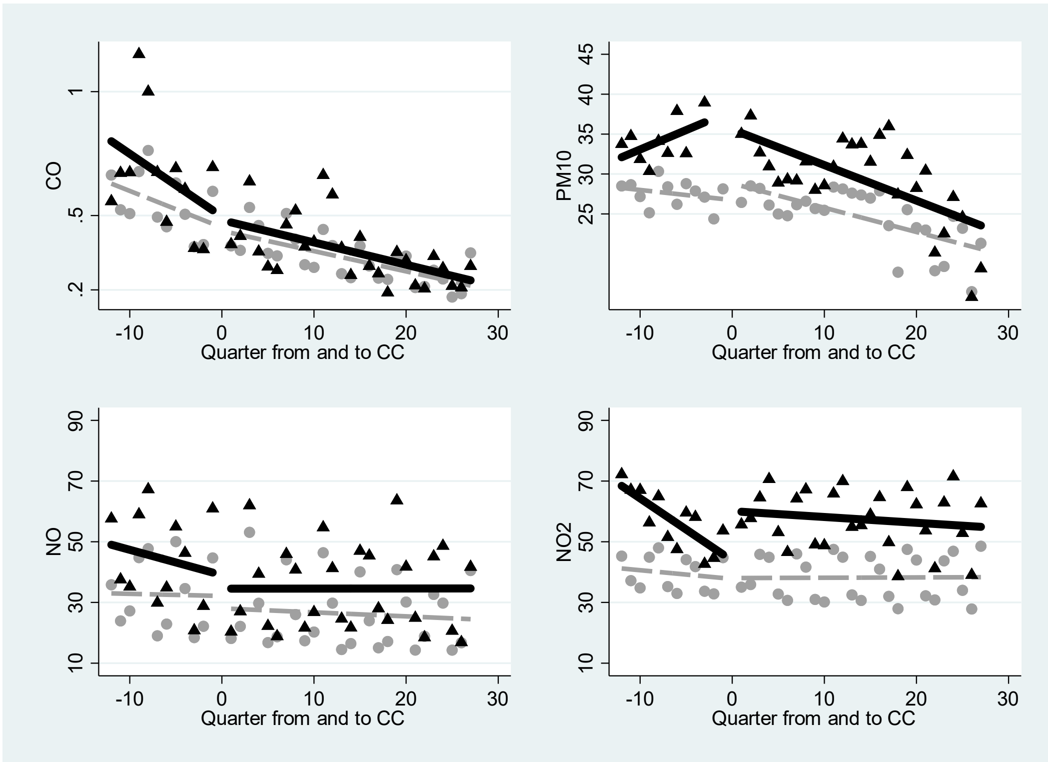
Figure 2: CO, PM_{10} and NO (averaged z-scores) and NO_2 (z-scores) Pre and Post Introduction of the Congestion Charge Zone



Legend: Solid upper lines – Congestion Charge Zone pre and post linear trend
Dashed lower lines – Comparison Group pre and post linear trend

Note: The dots correspond to quarterly pollution averages for treatment (dark triangles) and comparison (light circles) areas.

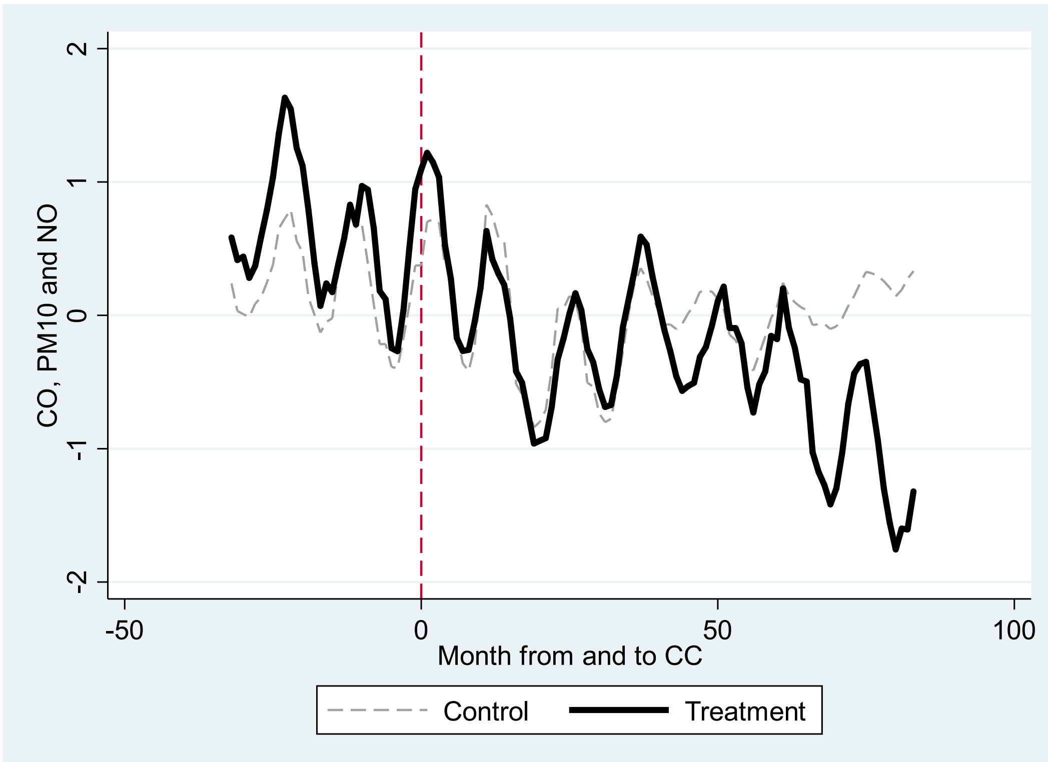
FIGURE 3: Each Polutant (measure in underlying concentrations) Pre and Post Introduction of the Congestion Charge Zone



Legend: Solid upper lines – Congestion Charge Zone pre and post linear trend
Dashed lower lines – Comparison Group pre and post linear trend

Note: The dots correspond to quarterly pollution averages for treatment (dark triangles) and comparison (light circles) areas.

FIGURE 4: Synthetic Cohort Estimates of Congestion Charge Effects on Pollution.
Panel A – Combined 3 Pollutants, Z-Scores



Panel B – Individual Pollutants, Levels

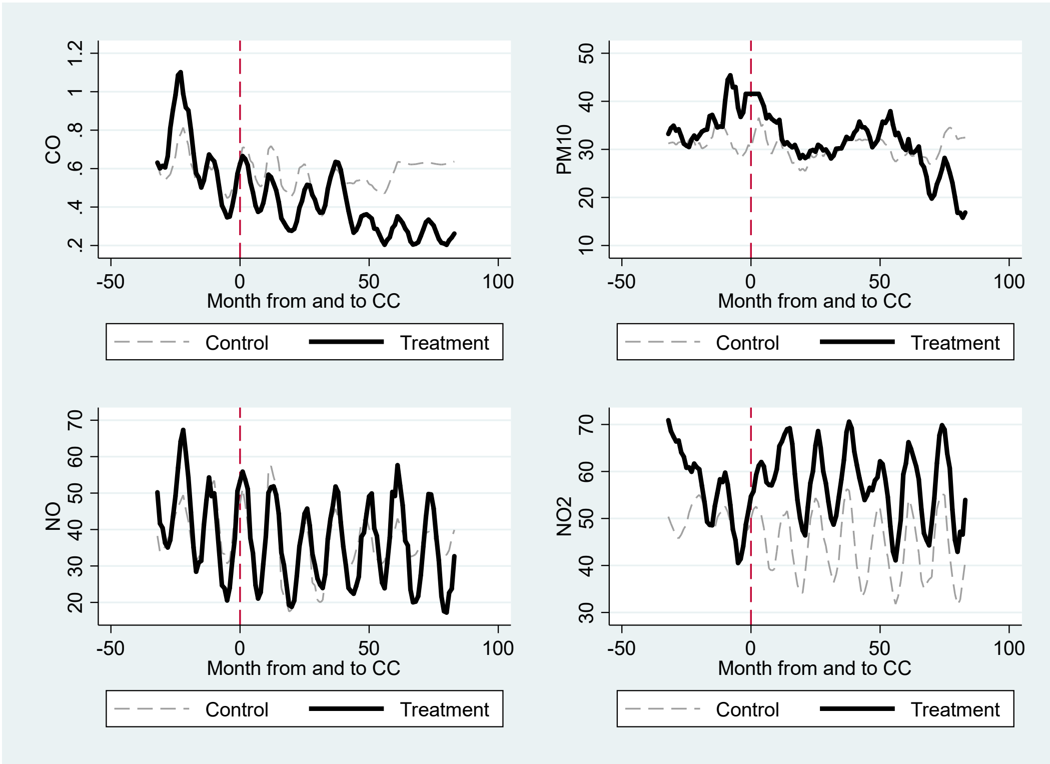
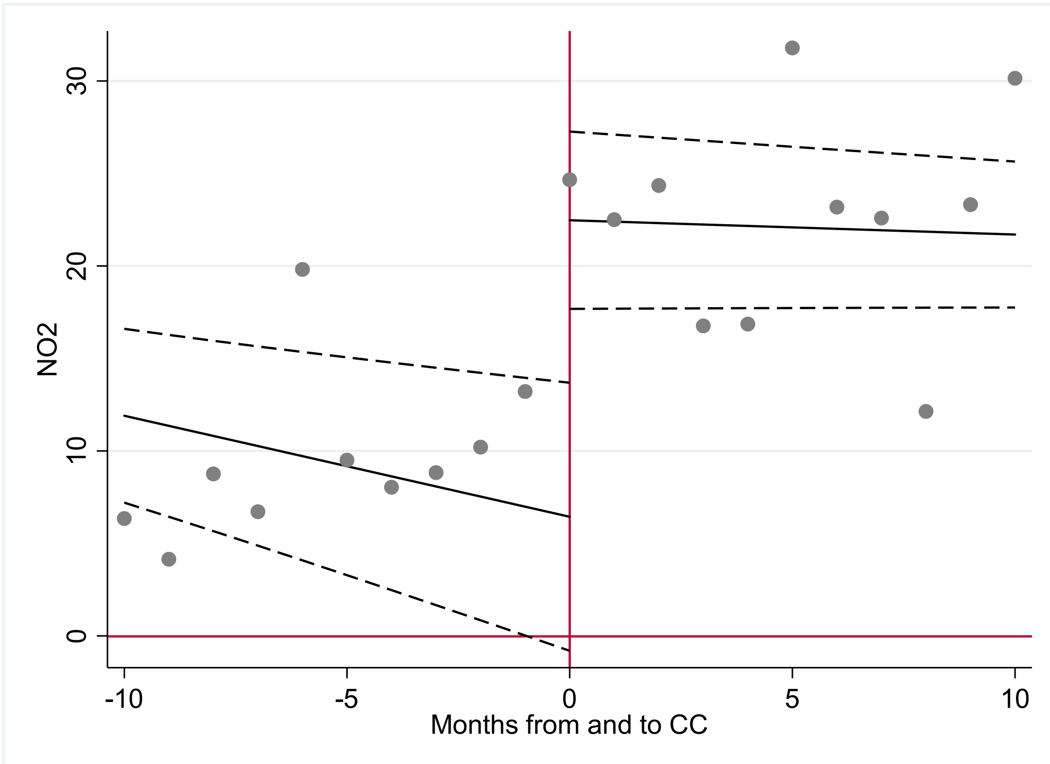


FIGURE 5 Estimated Effect of the Congestion Charge on NO_2 Levels



Note: Dashed lines correspond to the 95% pointwise confident intervals, using the Delta method so as to calculate standard errors.

TABLE 1 The Effect of the Introduction of the Congestion Charge on Hourly Pollution Levels during Charge Time 2000-2007

	(I)	(II)	(III)	(IV)	(V)
CO	-0.060**	-0.040	-0.040	-0.060**	-0.060**
	(0.025)	(0.025)	(0.030)	(0.025)	(0.025)
p-value (Wildbootstrap)	0.036	0.144	0.238	0.036	0.028
p-value (Permute)	0.000	0.000	0.000	0.000	0.000
Mean	0.618	0.618	0.618	0.618	0.618
Observations	444,430	444,430	444,430	444,430	444,430
R-squared	0.205	0.217	0.229	0.223	0.220
PM10	-7.690***	-7.168***	-5.664***	-7.691***	-8.279***
	(1.299)	(1.311)	(1.417)	(1.298)	(1.272)
p-value (Wildbootstrap)	0.002	0.002	0.002	0.002	0.002
p-value (Permute)	0.000	0.000	0.000	0.000	0.000
Mean	34.890	34.890	34.890	34.890	34.890
Observations	421,758	421,758	421,758	421,758	421,758
R-squared	0.084	0.096	0.112	0.092	0.103
NO	-9.543***	-7.268**	-8.030***	-9.607***	-9.625***
	(2.520)	(2.742)	(2.563)	(2.506)	(2.868)
p-value (Wildbootstrap)	0.002	0.014	0.004	0.002	0.004
p-value (Permute)	0.000	0.000	0.000	0.000	0.000
Mean	44.214	44.214	44.214	44.214	44.214
Observations	457,465	457,465	457,465	457,465	457,465
R-squared	0.168	0.182	0.191	0.194	0.197
NO_2	7.940***	9.442***	9.436***	7.893***	7.838***
	(1.287)	(1.288)	(1.231)	(1.279)	(1.372)
p-value (Wildbootstrap)	0.000	0.000	0.000	0.000	0.000
p-value (Permute)	0.000	0.000	0.000	0.000	0.000
Mean	56.700	56.700	56.700	56.700	56.700
Observations	450,310	450,310	450,310	450,310	450,310
R-squared	0.260	0.297	0.304	0.286	0.273
Treatment Specific Trends	X	X	X	X	X
Year FE	X	X		X	X
Month FE		X			
Year-by-month FE			X		
Hour-FE				X	
Quadratic Weather Controls					X

The table reports the difference in difference estimate of the introduction of the congestion charge on levels of each pollutant. Standard Errors clustered at the local authority level in parentheses (). ***, **, * indicate statistical significance at the 1%, 5% and 10% level, respectively. All models include daily, local authority level, controls for average temperature, precipitation, average wind speed and average wind direction. All columns allow for treatment specific trends. Column I provides our baseline specification, Columns II and III show robustness of our estimates to including month fixed effects and year by month fixed effects, respectively. Columns IV and V add hour fixed effects and a second order polynomial of weather controls to our baseline specification, respectively.

TABLE 2 The Effect of the Introduction of the Congestion Charge on Hourly Pollution Levels during Charge Time. 2000-2007, Alternative Comparison Groups

	(I) 10 Largest Cities	(II) 5 Largest Cities	(III) PSM trimmed	(IV) PSM weighted	(V) Synthetic Control	(VI) Collapsed
CO	-0.089**	-0.119**	-0.063**	-0.046	-0.112	-0.071***
	(0.030)	(0.040)	(0.027)	(0.032)	(0.076)	(0.023)
p-value (Wildbootstrap)	0.018	0.082	0.046	0.212		
p-value (Permute)	0.000	0.000	0.000			
Mean	0.618	0.618	0.618	0.618	0.654	0.657
Observations	249,486	145,532	417,674	417,674	192	1,792
R-squared	0.228	0.236	0.209	0.207	0.395	0.434
PM10	-5.880**	-3.523	-7.556***	-6.148***	-5.349**	-8.467***
	(2.006)	(3.151)	(1.365)	(2.098)	(2.322)	(1.221)
p-value (Wildbootstrap)	0.010	0.370	0.002	0.004		
p-value (Permute)	0.000	0.000	0.000			
Mean	34.890	34.890	34.890	34.890	36.464	35.107
Observations	236,261	132,368	393,911	393,911	192	1,723
R-squared	0.084	0.096	0.085	0.089	0.317	0.165
NO	-7.259**	-5.709**	-9.115***	-7.632**	-11.906*	-7.728***
	(2.466)	(2.324)	(2.607)	(2.745)	(6.340)	(1.459)
p-value (Wildbootstrap)	0.004	0.042	0.002	0.036		
p-value (Permute)	0.000	0.000	0.000			
Mean	44.214	44.214	44.214	44.214	44.143	44.384
Observations	252,266	144,890	430,102	430,102	192	1,832
R-squared	0.195	0.210	0.171	0.189	0.188	0.436
NO_2	8.831***	7.053***	7.975***	7.492***	11.026**	11.591***
	(1.256)	(1.856)	(1.323)	(1.909)	(4.650)	(0.507)
p-value (Wildbootstrap)	0.000	0.016	0.000	0.000		
p-value (Permute)	0.000	0.000	0.000			
Mean	56.700	56.700	56.700	56.700	57.186	57.290
Observations	245,273	138,993	422,947	422,947	192	1,814
R-squared	0.281	0.294	0.266	0.264	0.366	0.506
Treatment Specific Trends	X	X	X	X	X	X
Year FE	X	X	X	X	X	X

The table reports the difference in difference estimate of the introduction of the congestion charge on levels of each pollutant experimenting with most suitable control groups. Standard Errors clustered at the local authority level in parentheses (). ***, **, * indicate statistical significance at the 1%, 5% and 10% level, respectively. All models include daily local authority controls for average temperature, precipitation, average wind speed and average wind direction. All columns allow for treatment specific trends and year fixed effects. Columns I and II provide the effect of the charge on pollution using as control group the 10 and 5 most populated cities in the UK, respectively. Column III shows estimates for the sample of treatment and control observations which fall in the overlapping region for Propensity Scores resulting from matching our treatment with controls in terms of observable characteristics including local authority population size, unemployment rates, vehicle miles travelled and weather controls. Column IV shows the effect of the charge weighting our estimates by the inverse of the derived propensity score. Column V show estimates from a synthetic cohort procedure that matches pre-trends on pollution, weather, population, unemployment rates and vehicle miles travelled measures. Since our synthetic cohort estimation is implemented at the month and year level, Column V aggregates the time dimension of our data at the month-year level in order for Columns V and VI to be comparable.

TABLE 3 The Effect of the Introduction of the Congestion Charge on Hourly Pollution levels during Charge Time. 2000-2007 Local Authority Fixed Effects and Specific Trends

VARIABLES	(I)	(II)	(III)	(IV)	(V)	(VI)
CO	-0.058**	-0.038	-0.038	-0.058**	-0.058**	-0.062**
	(0.026)	(0.026)	(0.031)	(0.026)	(0.025)	(0.027)
p-value (Wildbootstrap)	0.178	0.448	0.61	0.174	0.232	0.154
p-value (Permute)	0.000	0.000	0.000	0.000	0.000	0.000
Mean	0.618	0.618	0.618	0.618	0.618	0.618
Observations	444,430	444,430	444,430	444,430	444,430	444,430
R-squared	0.212	0.226	0.237	0.233	0.229	0.305
PM10	-7.742***	-7.322***	-5.810***	-7.743***	-8.354***	-7.418***
	(1.273)	(1.291)	(1.384)	(1.272)	(1.233)	(1.339)
p-value (Wildbootstrap)	0.002	0.004	0.068	0.002	0.002	0.008
p-value (Permute)	0.000	0.000	0.000	0.000	0.000	0.000
Mean	34.890	34.890	34.890	34.890	34.890	34.890
Observations	421,758	421,758	421,758	421,758	421,758	421,758
R-squared	0.080	0.091	0.107	0.089	0.101	0.126
NO	-10.711***	-8.379***	-9.087***	-10.764***	-10.814***	-10.534***
	(2.369)	(2.631)	(2.347)	(2.352)	(2.747)	(2.437)
p-value (Wildbootstrap)	0.006	0.068	0.020	0.006	0.012	0.008
p-value (Permute)	0.000	0.000	0.000	0.000	0.000	0.000
Mean	44.214	44.214	44.214	44.214	44.214	44.214
Observations	457,465	457,465	457,465	457,465	457,465	457,465
R-squared	0.178	0.192	0.201	0.205	0.206	0.217
NO2	4.845***	6.474***	6.499***	4.802***	4.745***	5.310***
	(1.578)	(1.568)	(1.495)	(1.569)	(1.661)	(1.518)
p-value (Wildbootstrap)	0.006	0.000	0.000	0.006	0.012	0.008
p-value (Permute)	0.000	0.000	0.000	0.000	0.000	0.000
Mean	56.700	56.700	56.700	56.700	56.700	56.700
Observations	450,310	450,310	450,310	450,310	450,310	450,310
R-squared	0.216	0.256	0.264	0.247	0.231	0.335
Local Authority FE	X	X	X	X	X	X
Treatment Specific Trends	X	X	X	X	X	
Year FE	X	X		X	X	X
Month FE		X				
Year-month FE			X			
Hour-FE				X		
Quadratic Weather Controls					X	
Local Authority Specific Trends						X

The table reports the difference in difference estimate of the introduction of the congestion charge on levels of each pollutant with local authority fixed effects. Standard Errors clustered at the local authority level in parentheses (). ***, **, * indicate statistical significance at the 1%, 5% and 10% level, respectively. All models include daily local authority controls for average temperature, precipitation, average wind speed and average wind direction. Column I presents our baseline specification. Columns II and III show robustness to including month fixed effects and year by month fixed effects, respectively. Columns IV and V add hour fixed effects and a second order polynomial of weather controls to our baseline specification. Column VI allows for local authority specific trends.

TABLE 4. The Effect of the Congestion Charge on the *Rate* of Annual Pollution during Charge Time 2000-2007. Charged Miles (Millions).

	CO	PM10	NO	NO2
DD	-0.029 (0.072)	-12.969*** (2.157)	-9.800*** (3.293)	20.888*** (1.810)
p-value (Wildbootstrap)	0.6720	0.0020	0.0020	0.0000
p-value (Permute)	0.8099	0.0000	0.3333	0.0000
Mean	1.105	63.239	76.274	97.935
Treatment Specific Trends	X	X	X	X
Year FE	X	X	X	X
Local Authority FE	X	X	X	X
Observations	152	146	154	154
R^2	0.537	0.222	0.333	0.242

The table reports the difference in difference estimate of the introduction of the congestion charge on the annual rates of each pollutant. ***, **, * indicate statistical significance at the 1%, 5% and 10% level, respectively. All models include annual local authority level controls for average temperature, precipitation, average wind speed and average wind direction.

TABLE 5: The Congestion Charge and Annual Vehicle Miles Driven, 2000-2007 (in millions)

	Total	Cars/Taxis	Light Goods	Heavy Goods	Bicycles	Motor Cycles	Buses
			CHARGED			UNCHARGED	
DD	-42.733***	-38.513***	-13.613***	-5.216***	5.581***	6.291***	3.575***
	(4.035)	(2.864)	(0.000)	(0.951)	(0.101)	(0.105)	(0.326)
p-value (Wildbootstrap)	0.0020	0.0020	1	0.0020	0.0000	0.0000	0.0000
p-value (Permute)	0.0270	0.0150	0.0150	0.0270	0.0420	0.0150	0.0150
Mean	673.400	478.704	79.404	23.673	23.691	43.983	24.031
Treatment Specific Trends	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Local Authority FE	X	X	X	X	X	X	X
Observations	176	176	176	176	176	176	176
R^2	0.002	0.007	0.996	0.008	0.946	0.926	0.304

The table reports the difference in difference estimate of the introduction of the congestion charge on the annual vehicle miles driven by transportation model.***, **, * indicate statistical significance at the 1%, 5% and 10% level, respectively. All models include annual local authority level controls for average temperature, precipitation, average wind sped and average wind direction.

TABLE 6: Alternative Inference as per Conley and Taber (2011)

	(I)	(II)	(III)	(IV)	(V)
CO					
CI (Conley and Taber)	[-0.289, 1.645]	[-0.218, 0.574]	[-0.216, 0.273]	[-0.289, 1.643]	[-0.303, 1.643]
PM10					
CI (Conley and Taber)	[-16.022, 2.701]	[-15.098, 7.619]	[-20.461, 33.519]	[-16.030, 2.699]	[-16.735, 2.300]
NO					
CI (Conley and Taber)	[-25.509, 3.112]	[-20.582, 1.280]	[-18.131, 0.940]	[-25.534, 3.099]	[-26.703, 1.849]
NO_2					
CI (Conley and Taber)	[6.902, 20.864]	[6.738, 18.204]	[6.678, 23.127]	[6.915, 20.742]	[6.149, 19.873]
Treatment Specific Trends	X	X	X	X	X
Year FE	X	X		X	X
Month FE		X			
Year-by-month FE			X		
Hour-FE				X	
Quadratic Weather Controls					X

The table reports the 95% confidence intervals for the effect of the congestion charge introduction on each of the pollutants.

APPENDIX TABLE A1: Descriptive Statistics, 2000-2007

Variables	Observations	Mean	Standard Dev	Min	Max
CO	444,430	0.435	0.358	0	15.1
PM10	421,758	27.3325	19.140	0.05	1097
NO	457,465	30.250	44.433	0	1454
NO2	450,310	40.526	20.451	0	397
Av Temperature	450,310	10.050	4.983	-7.05	25.9
Av Precipitation	450,310	2.549	5.473	0	107.2
Mean Wind direction	450,310	202.135	70.742	0	608.667
Mean Wind speed	450,310	8.201	4.360	0	45.125
CO rate Total Miles	152	0.839	0.536	0.089	2.642
CO rate Miles Charged	152	0.975	0.624	0.105	3.043
PM10 rate Total Miles	146	49.478	23.905	9.518	129.393
PM10 rate Miles Charged	146	57.571	28.319	11.119	153.827
NO rate Total Miles	154	53.729	28.255	11.251	159.087
NO rate Miles Charged	154	62.482	33.051	12.894	183.225
NO2 rate Total Miles	154	73.033	32.256	14.729	174.414
NO2 rate Miles Charged	154	85.018	38.203	17.206	207.350
Total Miles (in millions)	154	0.738	0.556	0.263	2.711
Miles in Charge period	154	0.637	0.479	0.220	2.298

APPENDIX TABLE A2, Alternative Control Groups with Local Authority Fixed Effects

	10 controls	5 controls	PSM Trimmed	PSM weighted
CO	-0.079**	-0.117**	-0.063**	-0.045
	(0.032)	(0.042)	(0.027)	(0.032)
Observations	249,486	145,532	417,674	417,674
R-squared	0.232	0.239	0.217	0.279
PM10	-5.886**	-3.408	-7.716***	-6.528***
	(2.039)	(3.193)	(1.347)	(1.873)
Observations	236,261	132,368	393,911	393,911
R-squared	0.087	0.091	0.082	0.130
NO	-7.956***	-7.757**	-10.507***	-9.669***
	(2.103)	(3.024)	(2.361)	(2.529)
Observations	252,266	144,890	430,102	430,102
R-squared	0.212	0.225	0.182	0.234
NO2	5.997***	4.034*	4.658***	4.045*
	(1.552)	(2.059)	(1.581)	(2.012)
Observations	245,273	138,993	422,947	422,947
R-squared	0.236	0.245	0.220	0.335
Treatment specific trends	X	X	X	X
Year FE	X	X	X	X

TABLE A3. The Effect of the Congestion Charge Across Different Areas of London and Outside of Charge Time, Hourly Pollution Levels 2000-2007.

	(I) CO	(II) CO	(III) CO	(IV) PM10	(V) PM10	(VI) PM10	(VII) NO	(VIII) NO	(IX) NO	(X) NO2	(XI) NO2	(XII) NO2
PANEL A – Alternative Treatment Areas												
DD (CCZ)	-0.075***	-0.078***	-0.079***	-7.539***	-6.952***	-7.077***	-7.785*	-11.270**	-11.572**	7.339***	6.842***	6.748***
	(0.023)	(0.025)	(0.026)	(1.105)	(1.268)	(1.315)	(3.954)	(4.171)	(4.625)	(1.955)	(2.054)	(2.288)
DD (A)		0.027	0.027		4.927***	4.780***		-27.353***	-27.577***		-6.262*	-6.347*
		(0.047)	(0.047)		(0.863)	(0.873)		(7.252)	(7.307)		(3.596)	(3.665)
DD (B)					1.680	1.540		-1.242	-1.168		2.876**	2.936**
					(1.818)	(1.828)		(3.253)	(3.302)		(1.280)	(1.296)
DD (C)		-0.033			3.993*			-6.518			0.751	
		(0.035)			(2.098)			(4.801)			(2.722)	
Observations	660,489	660,489	592,828	565,612	565,612	504,791	551,589	551,589	505,721	667,963	667,963	600,228
R-squared	0.022	0.165	0.180	0.094	0.112	0.107	0.010	0.037	0.038	0.018	0.103	0.110
PANEL B – Outside of Charged Times												
DD (CCZ)	-0.040*			-2.550***			-3.758*			11.837***		
	(0.021)			(0.510)			(2.142)			(0.771)		
Observations	450,363			427,287			457,985			449,145		
R-squared	0.159			0.090			0.128			0.249		
Treatment Specific Trends	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X

The table reports the difference in difference estimate of the introduction of the congestion charge on levels of each pollutant. Standard Errors clustered at the local authority level in parentheses (). ***, **, * indicate statistical significance at the 1%, 5% and 10% level, respectively. All columns allow for treatment specific trends. Columns I, IV, VII and X of Panel A constitutes our baseline specification. Columns II, V, VIII and XI show robustness of our estimates to including effects of the congestion charge policy on closer areas in London surrounding the charge zone such as A (Camden, Tower Hamlets and Southwark), more distant areas such as B (Lewisham, Greenwich and Haringey) and still more distant areas C (Bromley, Bexley and Hillingdon). Columns III, VI, IX and XII show robustness of our estimates to including effects of the congestion charge policy on areas in London surrounding the charge zone such as A (Camden, Tower Hamlets and Southwark) and B (Lewisham, Greenwich and Haringey). Panel B shows the difference in difference estimate of the introduction of the congestion charge on levels of each pollutant for non-charged hours.