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Association of vehicle emission charging zones with adult emergency hospital admissions: an interrupted time series analysis of the toxicity charge and Ultra Low Emission Zone in London, UK

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ABSTRACT

Background: The Toxicity Charge (T-charge) introduced in 2017 in central London, and Ultra-Low Emission Zone (ULEZ) which replaced it in 2019, are vehicle emission-based charging schemes introduced to reduce urban air pollution. There is growing evidence of health co-benefits of such interventions. We conducted a quasi-experimental analysis of these interventions' impact on the health of adult residents in London.

Methods: We used interrupted time-series (ITS) analyses to quantify associations of the T-charge/ULEZ with changes in rates of adult (≥15 years) emergency hospital admissions (cardiovascular diseases, respiratory diseases, and all-causes excluding external) in residents of the intervention and control (London, Bristol) areas, between 6 January 2014 and 1 March 2020. We conducted ITS analyses using segmented negative binomial regression to model age/sex-standardised admission rates, adjusting for temperature, relative humidity, month, and public holidays.

Findings: We found post-T-charge-introduction reductions in annual trends for cardiovascular (−8.1% [95% CI:−2.2,−13.6] per year), respiratory (−6.2% [−1.5, −10.7] per year), and all-cause (−3.1% [−0.9, −5.2] per year) emergency hospital admissions in the intervention area. After adjusting for concurrent changes in a London control area, reductions remained for cardiovascular (−9.3% [−3.4, −14.9]) and all-cause (−5.1% [−2.5, −7.5]), but not respiratory admissions (−2.7% [−7.6, 2.5]). The short ULEZ period prevented firm conclusions regarding ULEZ changes distinct from the T-charge.

Interpretation: We found evidence of an association between introduction of the T-charge/central ULEZ in London and reduced rates of emergency hospital admissions in adults. Ongoing research is needed to assess longer-term health effects.

1. Introduction

A large body of epidemiological evidence demonstrates adverse health effects of air pollution spanning the life course (Royal College of Physicians (RCP)), with short-term (hours or days) and long-term (years to decades) exposure both implicated. The health effects are wide-ranging; as well as established evidence of effects on respiratory and cardiovascular disease and mortality, (Royal College of Physicians (RCP)) there is developing evidence of associations with other conditions including Type 2 diabetes, (Joss et al., 2023) dementia, (Committee on the Medical Effects of Air Pollutants (COMEAP), 2022) and negative effects on birth outcomes. (Committee on the Medical

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Effects of Air Pollutants (COMEAP), 2025) Ambient air pollution is one of the leading environmental risk factors for premature death globally (Health Effects Institute (HEI), 2020).

Road traffic is a major contributor to urban air pollution (Greater London Authority (GLA), 2019), and growing numbers of policies are targeting road transport in efforts to reduce this (Campaign, 2022; Greater London Authority (GLA), 2018). These include vehicle emission-based charging schemes that ban or charge vehicles not meeting specified emission standards, aiming to reduce air pollution by promoting adoption of lower-emission vehicles or non-motorised transport modes (Holman et al., 2015). These are often referred to as Low Emission Zones (LEZ) or Clean Air Zones (CAZ).

London, United Kingdom, has a notable history of such policies. In 2003, the Congestion Charging Scheme (CCS) was implemented for all vehicles driving in the city's central zone, aiming to reduce congestion. In October 2017, a Toxicity Charge (T-charge) was introduced to target the most polluting vehicles by linking charges to emission standards. This was replaced by the more stringent Ultra-Low Emission Zone (ULEZ) in April 2019. Both the T-charge and ULEZ apply to most vehicle types, including cars. The Low Emission Zone (LEZ) was also implemented in this period, in a 2008–2012 phased introduction; this covers most of Greater London and applies to heavy diesel vehicles.

There is evidence that the T-charge and ULEZ have both increased the proportion of ULEZ-compliant vehicles in the area, (Greater London Authority (GLA), 2020) and improved air quality, particularly at roadside locations, (Greater London Authority (GLA), 2020; Prieto-Rodriguez et al., 2022; Beshir and Fichera, 2025) but evidence on the health impacts of the T-Charge/ULEZ is sparse. At present, there is one study of individuals working in the ULEZ area, which found evidence of improvements in general health after implementation of the T-charge/ULEZ. (Beshir and Fichera, 2025).

We evaluated the impact of the T-charge/ULEZ on health by quantifying changes in trends of emergency hospital admissions for cardiovascular diseases, respiratory diseases, and all-causes excluding 'external', among adult residents of the T-charge/ULEZ area, pre- vs post-implementation of these interventions, in January 2014 to March 2020. We then compared the trends observed in this area to changes in control areas to assess whether such changes could be explained by factors other than the T-charge/ULEZ.

2. Methods

2.1. Intervention

The T-charge began on 23 October 2017 and was replaced by the ULEZ on 8 April 2019. Both applied to a 21 km^2 area of central London (~190,000 residents) and placed emission-based restrictions on most vehicle types (including private cars) entering or driving within the area, with those not meeting specified emission standards required to pay a daily charge. The ULEZ stipulates more stringent emissions standards than the T-charge. The T-charge and 2019 ULEZ implementation are the focus of this analysis, rather than later 2021/2023 ULEZ expansions to most of inner/Greater London respectively. The older Congestion Charging Scheme (CCS) and Low Emission Zone (LEZ) also remain in operation. The appendix (p21) contains further details of these interventions.

2.2. Study areas

Fig. 1 shows the study areas. The intervention area was the aggregation of Lower-layer Super Output Areas (LSOAs, approximate population 1000–3000) with population-weighted centroids within the central ULEZ (n = 104), using 2011 LSOA boundaries. The control area was the aggregation of LSOAs (n = 2,792) whose population-weighted centroids were within Greater London but outside the boundary of the 2021 ULEZ expansion. In sensitivity analyses, the aggregation of LSOAs within the city of Bristol (ONS, 2016) (n = 333) was used as a non-London control area.

2.3. Health data

The study period was 6th January 2014 to 1st March 2020. Data on emergency (unplanned) hospital admissions of residents of the intervention and control areas were obtained from the Hospital Episode Statistics Admitted Patient Care database held by the UK Small Area Health Statistics Unit, provided by NHS England. Data were restricted to

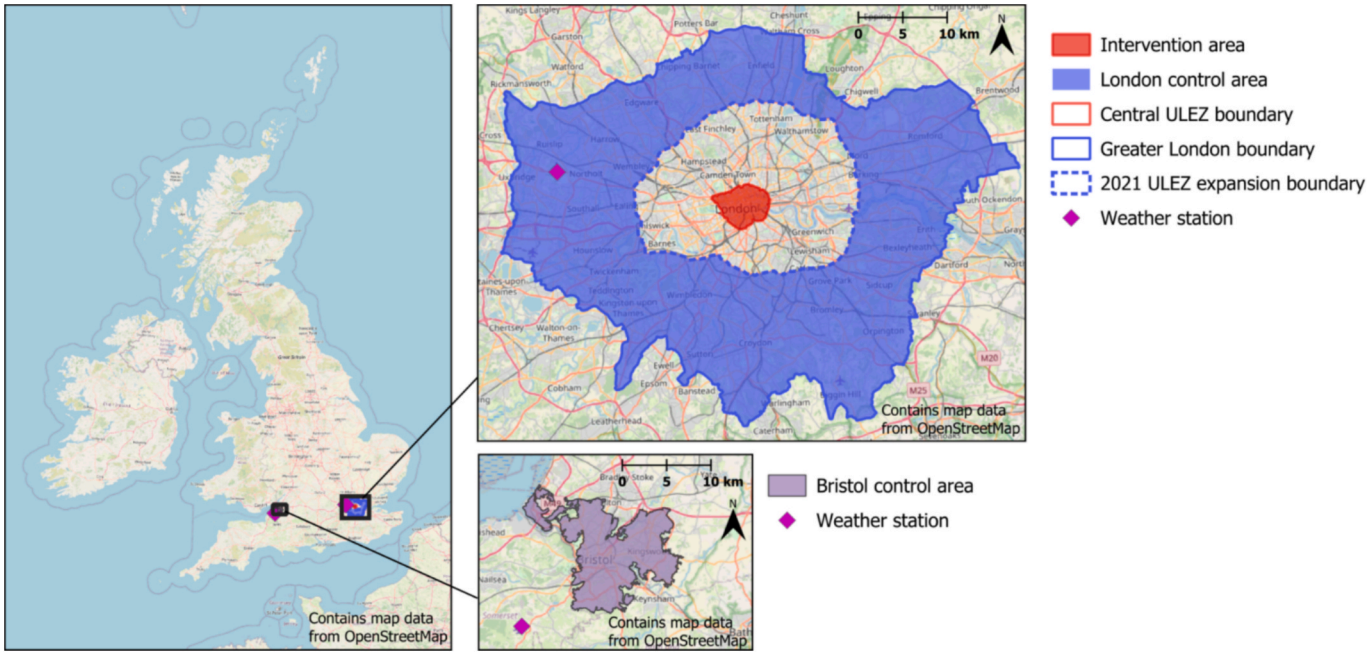

Fig. 1. Spatial extent of the intervention area, London control area, and Bristol control area. The weather stations used to derive meteorological covariates are also shown. Mapdata from.

admissions of patients aged ≥ 15 years, with a primary diagnosis code for cardiovascular disease (International Classification of Diseases-10th Revision (ICD-10) I00-I99), respiratory disease (J00-J99), or any non-external cause (A00-R99) (i.e., excluding injury, poisoning, certain other consequences of external causes, from here-on 'all-cause'). These broad categories of outcomes were used for comparability with previous studies considering similar interventions (Chamberlain et al., 2023; Pestel and Wozny, 2021; Margaryan, 2021; Yorifuji et al., 2011; Yorifuji et al., 2016). For each outcome, weekly counts were calculated for the intervention area and each control area, with weeks starting on Mondays.

Sex and 5-year age categories were extracted from the admission records, for age/sex-standardisation. Records with sex 'not specified' (n = 70, 0.003%) or 'not recorded' (n = 110, 0.004%) were excluded. LSOA-level mid-year population estimates by sex and 5-year age category were obtained from the Office for National Statistics (ONS) (ONS, 2021); aggregated for each study area, and weekly populations estimated by linear interpolation. Age- and sex-standardised outcome rates were calculated using direct standardisation (details in appendix p4), with the reference population being the combined populations of the intervention area, London control area, and the six most populous cities in England according to ONS 'major towns and cities' classification, for 2014 (ONS. Major towns and cities – methodological note and user guidance, 2016). From these age/sex-standardised rates, age/sex-adjusted populations (in person-years) were calculated. Weekly mean temperature and relative humidity were calculated from UK Meteorological Office hourly data from the closest weather stations to the study areas with ≥ 75% daily data coverage (appendix p4).

2.4. Statistical analyses

Interrupted time-series (ITS) analyses were conducted using segmented regression, with 'interruption' at the intervention timepoint. As hospital admissions are count data, we used negative binomial generalized linear models, which allow for over-dispersion. The outcomes were modelled as age/sex-standardised rates by using weekly counts as the outcome and age/sex-adjusted population as an offset. Models were adjusted for weekly mean temperature and relative humidity, modelled using natural cubic splines with three and two degrees of freedom respectively, and calendar month and public holiday indicators (appendix p4-5).

To account for temporal autocorrelation we calculated 95% confidence intervals (95% CI) using Newey-West standard errors (NW-SE), (Linden, 2018) derived using the *sandwich* R package and maximum lag of 5, determined by the rule of thumb of ¾ the third root of number of time-periods (321 weeks). (Stock and Watson, 2020) We also calculated p-values, and considered $p < 0.05$ to give evidence of an association.

The intervention area was first analysed in models without a control group (uITS), with separate models for each outcome. In primary analyses, the T-charge/ULEZ effect was modelled as a change in trend (i.e., change in slope) with a single intervention point at 23 October 2017 (T-charge start). This treats the T-charge and ULEZ as a combined intervention because there is evidence of ULEZ pre-compliance within the T-charge period (Greater London Authority (GLA), 2020) and therefore ULEZ-anticipation and T-charge-implementation effects are not easily separable. They are also conceptually and strategically linked, with the T-charge considered a ULEZ 'stepping stone' (Mayor of London, 2017) Previous analysis of air quality changes also modelled them as combined, and reported improvements beginning around the T-charge start. (Greater London Authority (GLA), 2020) The primary model assumes a gradual intervention effect based on observed air quality improvements. (Greater London Authority (GLA), 2020) Model fit was explored (appendix p7).

The trends were modelled as linear, on the log scale. The uITS with trend change at the T-charge start is defined by Eq. (1).

$$\begin{aligned} \boldsymbol{ln}(\mathrm{Y}_t) = &\boldsymbol{ln}(\text{std pop}_t) + \boldsymbol{\beta}_0 + \boldsymbol{\beta}_1 \boldsymbol{T}_t + \boldsymbol{\beta}_2 \boldsymbol{X}_t(\boldsymbol{T}_t - \boldsymbol{T}_{\boldsymbol{X}}) + \mathbf{f}_1(\boldsymbol{temp}_t) \\ &+ \mathbf{f}_2(\boldsymbol{rhum}_t) + \boldsymbol{\beta}_5 \boldsymbol{holiday}_t + \boldsymbol{\beta}_6 \boldsymbol{month}_t \cdots \boldsymbol{\beta}_{16} \boldsymbol{month}_t \end{aligned} \quad (1)$$

Here, Y_t is outcome count at week $\boldsymbol{t}$, std pop_t is age/sex-adjusted population at week $\boldsymbol{t}$, $\boldsymbol{T_t}$ is number of weeks since study start, $\boldsymbol{T_X}$ is $\boldsymbol{T_t}$ at the intervention timepoint (23 October 2017), $\boldsymbol{X_t}$ is a post-intervention indicator (1 if $\boldsymbol{T_t} > \boldsymbol{T_X}$, 0 otherwise), $\boldsymbol{temp_t}$ and $\boldsymbol{rhum_t}$ are mean temperature and relative humidity at week $\boldsymbol{t}$ (splines, described above), and $\boldsymbol{holiday_t}$ and $\boldsymbol{month_t}$ are the public holiday and calendar month indicators.

Therefore the $\boldsymbol{\beta}_1$ coefficient represents the pre-intervention weekly trend (i.e., change in the outcome associated with one week increase in $\boldsymbol{t}$). $\boldsymbol{\beta}_2$ gives the intervention effect, specifically the difference in post-intervention relative to pre-intervention weekly trend (i.e., the post- to pre-intervention difference in the change in outcome per one week increase in $\boldsymbol{t}$). $\boldsymbol{\beta}_0$ gives the baseline outcome rate (on log scale) at the study period start. To aid interpretation, the weekly trend and changes in weekly trend ($\boldsymbol{\beta}_1$ and $\boldsymbol{\beta}_2$) were converted to annual estimates by multiplying the coefficient estimate and confidence intervals by 52.179 (i.e., 365.25/7). All estimates were then exponentiated to give rate ratios (RR), which can be converted to percentage changes using 100*(RR-1) (further details in appendix p6).

The intervention area and London control area were then modelled together in ITS models with control group (cITS) (Eq. (2)).

$$\begin{aligned} \boldsymbol{ln}(\mathrm{Y}_t) = &\boldsymbol{ln}(\text{std pop}_t) + \boldsymbol{\beta}_0 + \boldsymbol{\beta}_1 \boldsymbol{T}_t + \boldsymbol{\beta}_2 \boldsymbol{X}_t(\boldsymbol{T}_t - \boldsymbol{T}_{\mathrm{X}}) + \boldsymbol{\beta}_3 \boldsymbol{Z} + \boldsymbol{\beta}_4 \boldsymbol{Z}\boldsymbol{T}_t \\ &+ \boldsymbol{\beta}_5 \boldsymbol{Z}\boldsymbol{X}_t(\boldsymbol{T}_t - \boldsymbol{T}_{\mathrm{X}}) + \mathbf{f}_1(\boldsymbol{temp}_t) + \mathbf{f}_2(\boldsymbol{rhum}_t) + \boldsymbol{\beta}_6 \boldsymbol{holiday}_t \\ &+ \boldsymbol{\beta}_7 \mathbf{month}_t \cdots \boldsymbol{\beta}_{17} \boldsymbol{month}_t \end{aligned} \quad (2)$$

Terms are as Eq. (1), with addition of Z as intervention area indicator. β_0, β_1,β_2 now refer to the control area, and β_3, β_4,β_5 to the intervention area. Most relevant is β_5, which gives the intervention area difference in post-intervention change in weekly trend relative to the control area (i.e., the intervention area post-intervention change in weekly trend, adjusted for concurrent change in the control area). As above, the estimates representing weekly trends and changes in weekly trend were converted to annual trends and changes in annual trend.

We calculated the difference ('contrasts') between the observed (i.e. $ZX_t = 1$) and cITS counterfactual ($ZX_t = 0$) marginal mean rates and weekly counts at one and two years post-T-charge, while averaging across the other predictors (holiday_t, month_t, temp_t, rhum_t) (details in appendix p9). The counterfactual predicts what intervention area rates would have been without the intervention, assuming the same trend change as the control area.

2.5. Sensitivity analyses

Several sensitivity analyses were conducted. We modelled an alternative impact model with a post-T-charge step change, in addition to the change in trend (Eq. (3)). Here, $\beta_6 X_t$ gives the step change in the control area and $\beta_7 Z X_t$ gives the differential step change in the intervention area relative to control.

$$\begin{aligned} ln(\mathrm{Y}_t) = &ln(\text{std pop}_t) + \beta_0 + \beta_1 T_t + \beta_2 X_t(T_t - T_{\mathrm{X}}) + \beta_3 Z + \beta_4 Z T_t \\ &+ \beta_5 Z X_t(T_t - T_{\mathrm{X}}) + \beta_6 X_t + \beta_7 Z X_t + \mathrm{f}_1(temp_t) + \mathrm{f}_2(rhum_t) \\ &+ \beta_8 holiday_t + \beta_9 \text{month}_t \cdots \beta_{19} month_t \end{aligned} \quad (3)$$

We restricted the respiratory outcome to COPD (J40-J44) and asthma (J45-J46) where evidence of associations with air pollution are particularly strong. (DeVries et al., 2017; Zheng et al., 2015) We conducted falsification analyses by moving the intervention timepoint to one and two years before the actual T-charge start. We conducted an analysis using the Bristol control, to preclude any effect of possible spillover/'contamination' from the intervention to Greater London control area. We also conducted a variation of the analysis using age- and sex-

stratified data rather than direct standardisation by age and sex (details in appendix p15). Finally, we modelled separate post-ULEZ (8th April 2019) changes (step and/or change in trend, equations in appendix p18) in addition to the post-T-charge trend change. These correspond to changes relative to the post-T-charge trend.

Data preparation and analyses were conducted using R v.4.1.1. (R Core Team. R, 2021).

3. Role of funding source

Funders had no involvement in study design, collection, analysis, or interpretation of the data, writing the report, or decision to submit for publication.

4. Results

Area-level characteristics of the intervention and control areas (appendix p3) show differences, including in age, socio-economic deprivation and proportion of residents from ethnic minority backgrounds.

The uITS model (Fig. 2) shows a moderate positive trend in pre-T-charge rates for all three outcomes in the intervention area, with annual increase of 3.1% [95% CI: 0.6, 5.7] per year in age/sex-standardised rate for cardiovascular, 3.7% [95% CI: 1.3, 6.0] per year for respiratory, and 2.6% [95% CI: 1.7, 3.5] per year for all-cause hospital admissions. After T-charge implementation we observed reductions in annual trends of − 8.1% [95% CI: −2.2, −13.6] per year ($p = 0.007$) for cardiovascular, −6.2% [95% CI: −1.5, −10.7] per year ($p = 0.011$) for respiratory, and − 3.1% [95% CI: −0.9, −5.2] ($p = 0.006$) per year for all-cause admissions (Table 1). Overall, the post-intervention trend becomes negative, with a 5.3%, 2.7%, 0.6% reduction in annual rates for cardiovascular, respiratory, and all-cause admissions respectively.

In cITS models with the London control area (Fig. 3), the pre-intervention baseline weekly admission rates were − 9.0% [95% CI: −6.9, −11.0] ($p < 0.001$) lower in the intervention area than control area for the all-cause outcome but with no apparent differences between

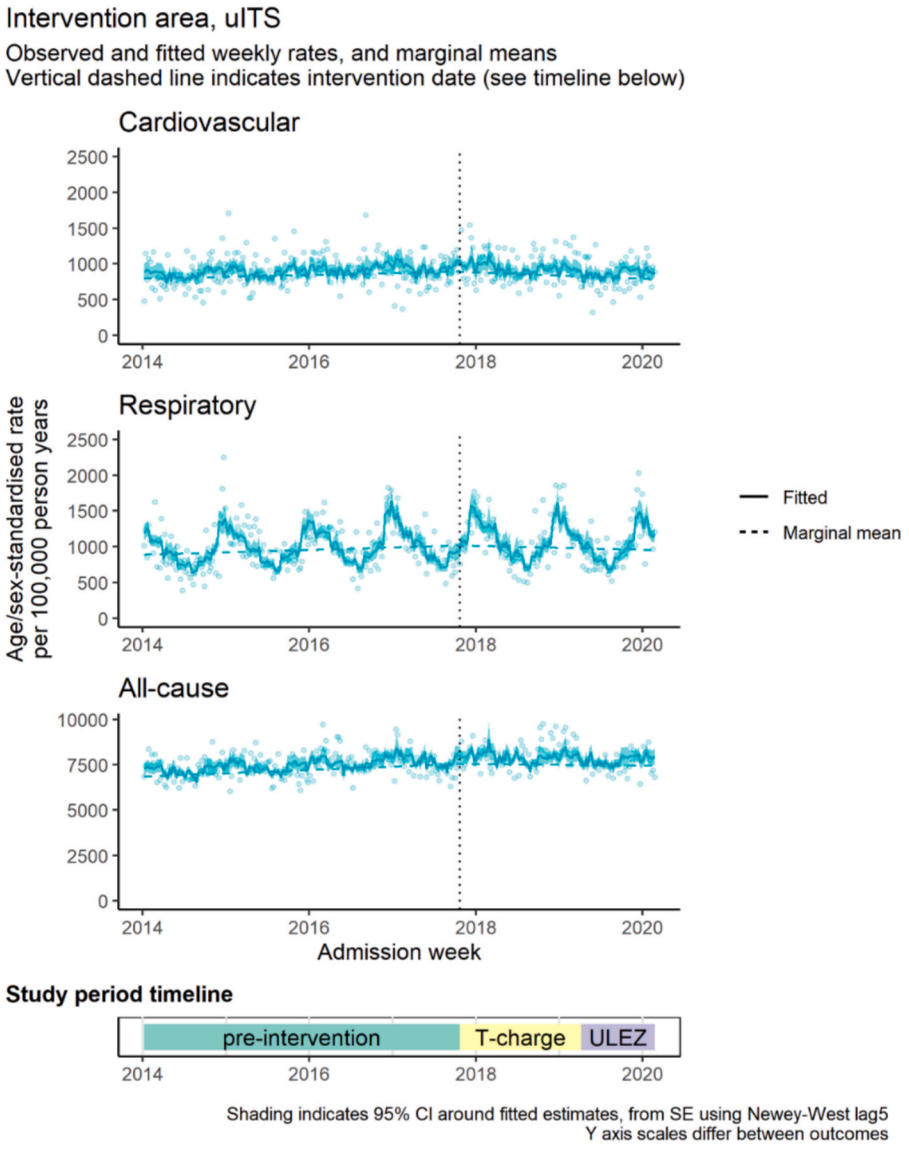

Fig. 2. Weekly age- and sex-standardised rates, fitted uITS model estimates, and marginal means for cardiovascular, respiratory, and all-cause emergency hospital admissions, in the intervention area. Vertical dashed line indicates the start date of the T-charge (23 October 2017). Marginal means are calculated with averaging over the levels of the $holiday_t$ and $month_t$ terms and using the average weekly mean temperature and relative humidity for the study period (full details in appendix p9). Confidence intervals use NW-SE, with maximum lag of 5. NW-SE, Newey-West standard errors; T-charge, toxicity charge; uITS, interrupted time series analysis without a control group; ULEZ, Ultra Low Emission Zone.

Table 1
Estimates for post-T-charge change in annual trend for primary and sensitivity analyses.

Outcome	uITS or cITS	Control area	Estimand	RR (95% CI)	p-value
Cardiovascular	uITS	*NA*	Post-T-charge change in annual trend	0.919 (0.864–0.978)	0.007
Cardiovascular	cITS	London control	Post-T-charge change in annual trend	0.907 (0.851–0.966) *	0.003
Cardiovascular	*cITS*	*Bristol*	*Post-T-charge change in annual trend*	*0.852 (0.795–0.914)* *	*<0.001*
Respiratory	uITS	*NA*	Post-T-charge change in annual trend	0.938 (0.893–0.985)	0.011
Respiratory	cITS	London control	Post-T-charge change in annual trend	0.973 (0.924–1.025) *	0.300
Respiratory	*cITS*	*Bristol*	*Post-T-charge change in annual trend*	*0.954 (0.900–1.011)* *	*0.111*
All-cause	uITS	*NA*	Post-T-charge change in annual trend	0.969 (0.948–0.991)	0.006
All-cause	cITS	London control	Post-T-charge change in annual trend	0.949 (0.925–0.975) *	<0.001
All-cause	*cITS*	*Bristol*	*Post-T-charge change in annual trend*	*0.910 (0.887–0.933)* *	*<0.001*

Sensitivity analyses are in italics. Estimates of other regression coefficients (e.g., pre-intervention trend) and Z-test statistics are in the appendix (p8,16). Model uses age and sex-standardised rates, adjusted for mean weekly temperature and relative humidity, calendar month and public holidays. Confidence intervals use NW-SE, with maximum lag of 5.
*Estimate *adjusted* for concurrent change in control area.
cITS, interrupted time series analysis with a control group; NA, not applicable; NW-SE, Newey-West standard errors; RR, relative risk; T-charge, toxicity-charge; uITS, Interrupted time series analysis without a control group.

the cardiovascular (p = 0.087) or respiratory (p = 0.517) outcomes (appendix p8). Pre-intervention trends (i.e. slopes) did not show differences between areas for any outcome (all p > 0.150). With the adjustment for the concurrent changes in the control area, we observed post-T-charge reductions in the annual trend in intervention area admission rates (Table 1) for cardiovascular (−9.3% [95% CI: −3.4, −14.9] per year, p = 0.003) and all-cause admissions (−5.1% [95% CI: −2.5, −7.5] per year, p < 0.001), but smaller effect for respiratory admissions with 95% CI spanning one (−2.7% [95% CI: −7.6, 2.5%] per year, p = 0.300). However, for respiratory admissions there was evidence of a concurrent post-T-charge reduction in annual trend in the control area of − 3.6% [95% CI: −0.5, −6.5] per year, (p = 0.021) in contrast to cardiovascular (1.2% [95% CI: −0.1, 2.5] per year, p = 0.068) and all-cause admissions (1.9% [95% CI: 0.7, 3.0], per year, p = 0.001) in the control area (appendix p8).

Two years post-T-charge start, the modelled marginal mean rates of admissions in the intervention area (on the absolute scale) were lower in the observed scenario than counterfactual by − 187 per 100,000 person-years [95% CI: −59, −315] for cardiovascular admissions and − 842 per 100,000 person-years [95% CI: −403, −1282] for all-cause (appendix p12). The respiratory two-year contrast was − 57 per 100,000 person-years [95% CI: −166, 52]. Plots of marginal means for observed and counterfactual scenarios, and their contrasts, are in the appendix (p10-11).

With conversion to weekly counts (appendix p12), at two years post-T-charge the marginal mean of weekly cardiovascular admissions of intervention area residents was estimated as − 4.6 [95% CI: −1.5, −7.7) compared to the counterfactual (25.8 [95% CI: 23.2, 28.5]), i.e, 4.6 fewer admissions equivalent to 18% reduction. For all-cause admissions, the corresponding estimate after two years was − 22.8 [95% CI: −10.9, −34.7] compared to the counterfactual (231.7 [95% CI: 221.0, 242.9]), approximately a 10% reduction. For respiratory admissions the contrast after two years was − 1.5 [95% CI: −4.1, 1.3] compared to the counterfactual (27.2 [95% CI: 24.9, 29.7]).

In the sensitivity analysis with a post-T-charge step change in addition to trend change, the trend change dominated, with similar magnitude effect estimates to the main analysis (appendix p13). Likewise, results of the sensitivity analysis of the respiratory outcome restricted to asthma/COPD were similar to those for the broader outcome (appendix p13). In falsification analyses (appendix p14), when the intervention was modelled at two years prior, the estimated change in annual trend (cITS) diminished for the cardiovascular outcome to − 2.9% [95% CI: −6.1, 0.5] and for all-cause admissions to − 2.0% [95% CI: −0.4, −3.6]; the respiratory uITS estimate also diminished here.

The sensitivity analysis with the Bristol control (Table 1, and appendix p16) again shows reductions in annual trend post-T-charge for cardiovascular and all-cause admissions, though with larger point estimates than for the London control cITS. The sensitivity analysis using age- and sex-stratified data rather than direct standardisation by age and sex (appendix p15) gives comparable results to the primary analysis, with slightly smaller effect sizes and larger confidence intervals.

Results from modelling an additional post-ULEZ change (appendix p18) are complex, with some models implying combinations of positive and negative step and trend changes.

5. Discussion

In a quasi-experimental analysis using ITS, we found evidence of a post-T-charge reduction in emergency hospital admissions for cardiovascular diseases and all-causes (excluding 'external') in the intervention area, adjusted for concurrent changes in control areas. The results for respiratory disease hospital admissions were inconclusive.

Falsification analyses are consistent with the observed trend reductions in cardiovascular and all-cause admissions being a real effect beginning at the T-charge start, with estimates' magnitudes attenuating as the intervention point is falsely moved earlier. Sensitivity analyses modelling an additional post-ULEZ change show complex results and highlight the challenges in identifying if there is a ULEZ-attributable change separate to the T-charge.

5.1. Comparisons to other studies

Our findings are consistent with our earlier systematic review (Chamberlain et al., 2023) of global evidence, which found that LEZs can reduce some air pollution related health outcomes, with strongest evidence for cardiovascular outcomes, although the strength of evidence in the review was limited by the relatively small number of studies. Since this review, an analysis from Bradford, UK, of a set of interventions including a LEZ, found evidence of post-implementation reductions in respiratory and cardiovascular primary care visits, as well as some evidence for reductions in respiratory emergency department visits, though

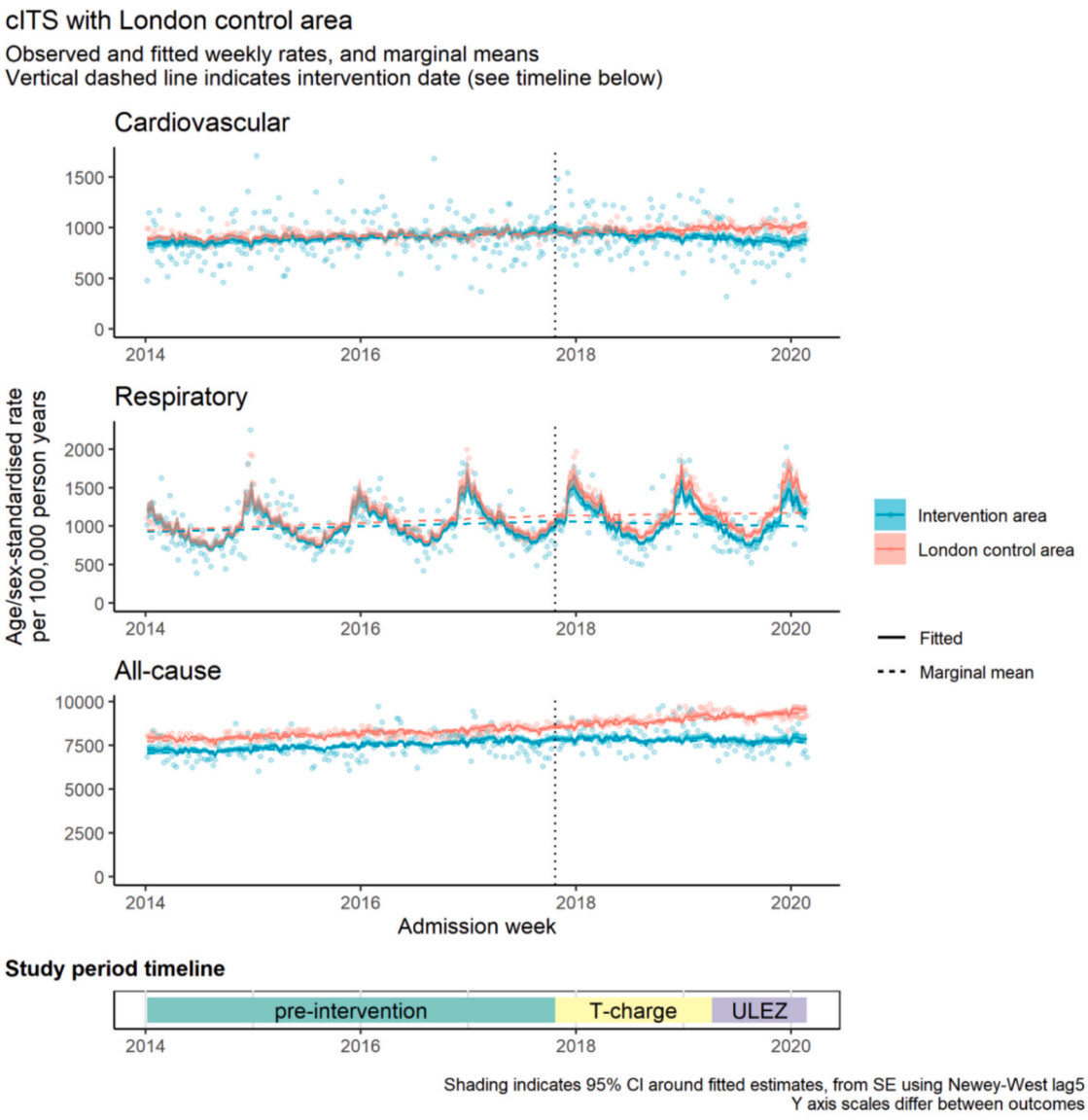

Fig. 3. Weekly age and sex-standardised rates fitted cITS model estimates, and marginal means of cardiovascular, respiratory, and all-cause emergency hospital admissions, in the London control area (orange) alongside the intervention area (blue). Vertical dashed lines indicate the start date of the T-charge (23 October 2017). Marginal means are calculated with averaging over the levels of the $holiday_t$ and $month_t$ terms and using the average weekly mean temperature and relative humidity for the study period. Confidence intervals use NW-SE, with maximum lag of 5. cITS, Interrupted time series analysis with a control group; NW-SE, Newey-West standard errors; T-charge, toxicity charge; ULEZ, Ultra-Low Emission Zone.

not cardiovascular (Mebrahtu et al., 2025). Their analysis of the primary care outcome is complementary to our analysis of hospital admissions and may indicate that less severe episodes of respiratory and cardiovascular illness are reduced by LEZs, as well as the more serious cases resulting in hospital admission (at least for cardiovascular, in our analysis). The contrast of their finding of reductions in respiratory primary care visits, in comparison to our inconclusive analysis of respiratory hospital admissions, may point to a more apparent effect on less severe disease, or it could be an effect specific to the Bradford implementation.

Some London-specific comparisons can also be made to work by Beshir & Fichera, who found evidence of improvements in general health after T-charge/ULEZ implementation, for workers in the area, using self-reported data. (Beshir and Fichera, 2025) Triangulating both studies' results increases confidence that the T-charge/ULEZ are associated with positive health effects.

5.2. *Considerations for interpretation*

There are several considerations to bear in mind when interpreting our analysis and considering the public health implications of the findings. Firstly, our analysis makes 'reduced form' estimates of the total association of the interventions with the health outcomes, without decomposing the contribution of mediating factors. This is informative for public health assessment of the overall benefits, but is of limited use for assessing how the intervention is working (i.e., process evaluation). Therefore while there is prior evidence of air pollution reductions associated with the T-charge/ULEZ, (Greater London Authority (GLA), 2020; Prieto-Rodriguez et al., 2022; Beshir and Fichera, 2025) and reason to anticipate resulting positive health effects, our study is not able to conclusively attribute our observed T-charge/ULEZ-associated changes in cardiovascular and all-cause admissions to this change in air quality, or to any other specific mediating factors. Observed increases in physical activity from active travel (Xiao et al., 2024), and hypothetical decreases in noise pollution are also potential mediators which would be

expected to have beneficial health impacts. Although unlikely, mediation by changes in healthcare-seeking behaviours, if the T-charge/ULEZ deterred people from seeking care if their private transport was non-compliant, is also hypothetically possible, but we are not aware of evidence of this.

Secondly, the finding of reductions in all outcomes in the models considering the intervention area alone, but confidence intervals for the respiratory outcome spanning the null after adjusting for concurrent changes in either control area (see Table 1), indicates no evidence of a further reduction in respiratory admissions in the intervention area relative to the reduction in the control areas. This could indicate reductions in respiratory admissions (but not cardiovascular or all-cause admissions) in the control areas independent of the interventions, but whether this is the case, and why, remains to be established. While this means only that a conclusion could not be made for the respiratory outcome, and not that the analysis shows that there is no T-charge/ULEZ-associated change, it is worth considering possible reasons why a relative change may not have occurred, or been too small to be detected, in this outcome specifically. One potential factor is that the intervention area has a relatively low number of elderly individuals, who are the age group most susceptible to air pollution effects on respiratory health in adults. Additionally, hospital admissions may not be the most appropriate measure for the effect on respiratory disease, as many acute respiratory episodes, such as acute exacerbations of COPD, are managed in the community (i.e. outside hospital).

Further points to consider when interpreting these findings include the fact that the ecological nature of the study means conclusions about individual-level health should not be drawn. However, population-level health effects are our primary interest, and the study design is appropriate for this. Additionally, we estimate effects occurring within a relatively short time of T-charge/ULEZ implementation, but the full impact of the air pollution-mediated effects on health will only be evident after prolonged exposure changes and effects on chronic disease development. (Webber et al., 2020) It should also not be assumed that the trend of decreasing admissions continues similarly beyond the study period because once all vehicles are ULEZ-compliant, further reductions would not be expected without changing the standards.

The fact that the intervention area is unusual in its socio-demographic and environmental characteristics may limit this study's generalisability to other settings. Another consideration for generalisability to future LEZ implementations is that as vehicle fleets become cleaner due to emission standards imposed at point of manufacture, LEZ may need to specify more stringent standards to continue accelerating improvements in air quality and associated health outcomes.

5.3. Strengths and limitations

We analysed outcomes from administrative health data which reduces potential information bias compared to self-reported outcomes, and also limits selection bias. We conducted several sensitivity analyses, and falsification analyses, which both increase confidence in our findings. By combining the T-charge and ULEZ periods we captured ULEZ pre-compliance, and by restricting our analysis to a period starting two years after implementation of London's first LEZ and before COVID-19 pandemic onset we reduce risk of contamination from other major influences on air quality and hospital admissions.

It was not possible to identify control areas with the same environmental and demographic attributes as the intervention area due to the unusual nature of central London. The within-group comparisons in ITS mean these differences should not affect the effect estimates via confounding, assuming they are time-invariant or change only gradually (Lopez Bernal et al., 2017). However, if these factors affect susceptibility to the intervention, imbalance between intervention and control areas could introduce bias in cITS (Lopez Bernal et al., 2018). For socio-economic deprivation (specifically, considering proportions of LSOAs in the most socio-economically deprived 20% in England), while the London control area was less deprived than the intervention area, the Bristol control area was more deprived, reducing the likelihood that bias from this imbalance could explain the cITS results. Plausibly, there could also be differences between intervention and control areas in unmeasured factors potentially affecting susceptibility to the intervention, such as rates of chronic cardiovascular or respiratory disease, which could bias the cITS. Time-varying disease risk factors such as smoking or obesity prevalence were not included in the model due to data unavailability, but these would only confound the intervention-outcome relationship if they change over time in a way that departs from the underlying trend and differs between intervention and control areas, and if that change occurs near the T-charge start and we have no reason to suspect this.

It is not possible to entirely disentangle the T-charge/ULEZ impacts from those of other changes within the study period, such as most private-hire vehicles losing their CCS exemption (same day as ULEZ start) (Greater London Authority (GLA), 2019) the shift towards more electric buses, and emissions-related changes in taxi licensing. However, the fact that these did not coincide with the start of the T-charge reduces any potential impact on our analyses. Furthermore, some of the changes may be partly driven by needing to comply with T-charge/ULEZ standards, and therefore also part of causal pathways between the T-charge/ULEZ and health outcomes. In addition, plans were announced in 2018 for a tightening of LEZ standards in 2021. (Mayor of London, 2021) While this makes anticipatory effects possible in the period between the T-charge and ULEZ, the potential for effects on the intervention area is reduced by the new LEZ standards being the same as the ULEZ standards.

The sensitivity analyses with an additional ULEZ intervention point highlight the complexity of determining whether there is a specific post-ULEZ change. We consider the positive trends combined with step change reductions (and vice versa) seen in some model specifications to be implausible and probably due to model over-fitting because of the shortness of the periods post-ULEZ, and between T-charge and ULEZ, and the flexibility allowed by increased model complexity.

5.4. Future work

Further research avenues include analysis of effects on childhood respiratory health, milder disease outcomes such as primary care visits, potential mediators (air pollution, active travel, noise), differential effects by socio-economic status and ethnicity, and health effects for people living outside the intervention area. Additionally, evaluation after a longer period is needed to capture longer-term effects of changes in air pollution exposure, as well as to better assess the specific post-ULEZ effect, although this would be complicated by the COVID-19 pandemic.

6. Conclusions

Our analysis indicates that implementation of the T-charge and ULEZ in central London was associated with positive changes in population health. This is complementary to evidence that these interventions have reduced air pollution, although we cannot directly attribute the reductions in admissions to air pollution. Generalisability may be limited due to the unusual characteristics of central London, but when taken together with work in other settings (Chamberlain et al., 2023; Mebrahtu et al., 2025) these findings are cautiously supportive of the potential to expect positive health impacts from similar interventions in other cities.

7. Data sharing

The health data used in this analysis cannot be shared.

Ethical approval

The study was covered by national research ethics approval from the London-South East Research Ethics Committee (22/LO/0256). Data access to confidential patient information without consent was covered by the Health Research Authority – Confidentiality Advisory Group under Regulation 5 of the Health Service (Control of Patient Information) Regulations 2002 ('section 251 support') (20/CAG/0028). This work uses data provided by patients and collected by the NHS as part of their care and support. Hospital Episode Statistics data are copyright © 2025, re-used with the permission of NHS England. All rights reserved.

The temperature and relative humidity hourly observations data were provided by the UK Meteorological Office through the National Centre for Atmospheric Science British Atmospheric Data Centre. Population estimates are from the Office for National Statistics, licensed under the Open Government License v3.0. These are ONS Crown Copyright Reserved. Geographical digital boundaries and look-ups were obtained from the Office for National Statistics, under the Open Government License v3.0, and containing OS data © Crown copyright and database right 2025.

CRediT authorship contribution statement

Rosemary C. Chamberlain: Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **John Hodsoll:** Writing – review & editing, Supervision, Methodology. **Chang Cai:** Writing – review & editing, Methodology, Formal analysis. **Chris Griffiths:** Writing – review & editing, Funding acquisition. **Frank Kelly:** Writing – review & editing, Funding acquisition. **Sean Beevers:** Writing – review & editing, Funding acquisition. **Gregor Stewart:** Writing – review & editing, Conceptualization. **Marta Blangiardo:** Writing – review & editing, Funding acquisition. **Paul Elliott:** Writing – review & editing, Funding acquisition. **Bethan Davies:** Writing – review & editing, Supervision, Methodology, Funding acquisition, Conceptualization. **Daniela Fecht:** Writing – review & editing, Validation, Supervision, Methodology, Funding acquisition, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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This work was conducted as part of a PhD at Imperial College London. Although RC is now working for UKHSA, this is not a UKHSA study.

The views expressed in this publication are those of the authors and not necessarily those of the MRC, the NIHR, HDRUK, the UKHSA, the Department of Health and Social Care, or Imperial College London.

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Appendix A. Supplementary data

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Data availability

The authors do not have permission to share data.

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