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Evaluation of low traffic neighbourhood (LTN) impacts on NO_2 and traffic

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ABSTRACT

Traffic restriction measures may create safer and healthier places for community members but may also displace traffic and air pollution to surrounding streets. Effective urban planning depends on understanding the magnitude of changes resulting from policy measures, both within and surrounding intervention areas; these are largely unstudied in the case of Low traffic Neighbourhoods (LTN). We evaluated impacts of three LTNs in the London Borough of Islington, UK, on air pollution and traffic flows in and around intervention areas, based on monthly Nitrogen Dioxide (NO_2) and traffic volume data provided by the local authority. We identified pre- and post-intervention monitoring periods and intervention, boundary and control sites. We then adapted the generalised difference in differences approach to evaluate the effects within LTNs and at their boundary. We found that LTNs have the potential to substantially reduce air pollution and traffic in target areas, without increasing air pollution or traffic volumes in surrounding streets. These results provide sound arguments in favour of LTNs to promote health and wellbeing in urban communities.

1. Introduction

Air pollution is the greatest environmental risk and fourth highest major risk factor contributing to global disease and mortality (GBD 2019 Risk Factors Collaborators, 2020). There is now a large consensus in the scientific, medical and public health communities of benefits of reducing annual ambient concentration to levels as low as 10 μg/m^3 for nitrogen dioxide (NO_2) (Hoffmann et al., 2021). In the UK in particular, there are approximately 40,000 deaths and up to £71.3 billion costs associated with air pollution each year (Brand and Hunt, 2018; RCP, 2016). In London, all boroughs have declared Air Quality Management Areas (AQMAs) because of exceedances in NO_2 annual limit values (regulatory standards) which are currently set at a lax level of 40 μg/m^3 (DEFRA, 2020).

Traffic typically contributes the largest share of ambient concentrations in urban areas, representing on average 27 % of PM2.5 in cities globally (Heydari et al., 2020). For NO_2, traffic contributions tend to be higher – for example 49 % of concentrations in London (DEFRA, 2020). Measures to reduce traffic emissions are thus naturally seen as prime candidates to improve urban health (Vardoulakis et al., 2018). Traffic restriction measures, however, are both considered difficult to implement due to their perceived unpopularity

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(Kuss and Nicholas, 2022), and hard to evaluate due to the complexity of real world conditions and likely small effects of each individual measures (Vardoulakis et al., 2018; Burns et al., 2020; Bigazzi and Rouleau, 2017). Even city-wide restrictions such as those seen during Olympic games struggle to indicate clear changes (Burns et al., 2020).

Low Traffic Neighbourhoods (LTNs) are small scale area-based interventions that use low-cost modal filters (planters, bollards or camera gates) to restrict through-traffic and limit vehicle speeds on residential streets (Aldred and Goodman, 2021). They had been increasingly implemented since 2014 across the UK following the announcement of a funding programme, with a sharp increase during Covid lockdown restrictions and amid concerns about virus transmission via public transport (Aldred and Goodman, 2021; Aldred and Verlinghieri, 2020). LTNs have been primarily implemented to increase healthy walking and cycling habits, and also to reduce car use (Aldred and Goodman, 2021), and thus to reap potential benefits from reductions in hazards such as air pollution, noise, and traffic

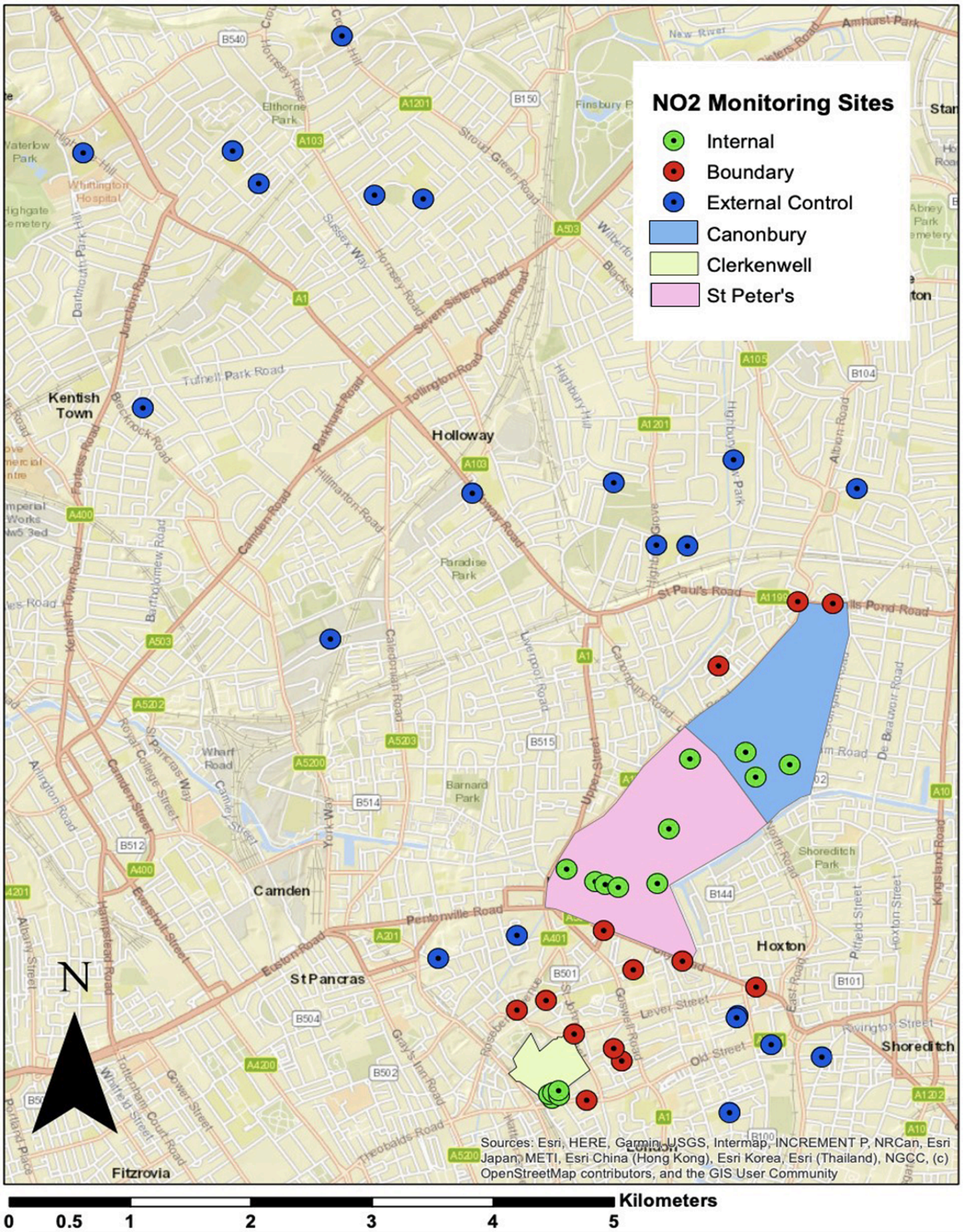

Fig. 1. Map location of NO_2 monitoring sites (from Hough 2021).

injuries while increasing physical activity (Dajnak et al., 2018; Fuks et al., 2014, 2017; Weichenthal et al., 2014; Louwies et al. 2015; Kaufman et al., 2016; Magalhaes et al., 2018; Yang et al., 2018; Aldred et al., 2021). LTNs, however, following the fate of many policies aimed at encouraging modal shifts from cars to walking and cycling, tend to be controversial (Aldred 2019). Opposition to their implementation has led some local authorities to remove LTNs, in part due to stakeholders expressing concerns about traffic and air pollution displacement to surrounding areas (Aldred and Verlinghieri, 2020). Robust evaluations are crucial for evidence-based approaches to policy making (Bigazzi and Rouleau, 2017), in particular to lend support, or not, to further implementation or maintenance of LTNs. So far LTNs have been robustly evaluated for their benefits on active travel, traffic injuries, and equity, (Aldred et al., 2021;

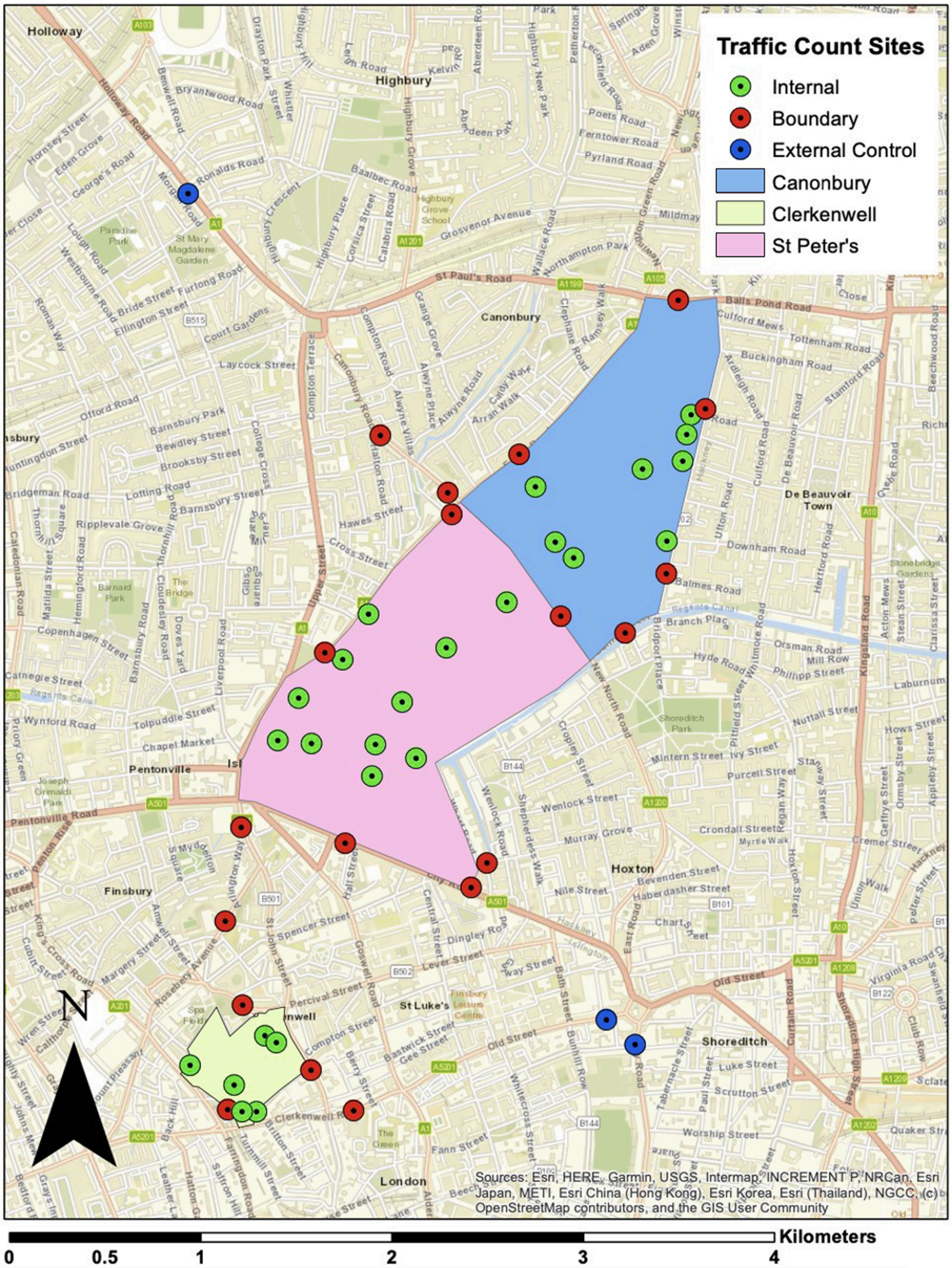

Fig. 2. Map location of traffic count sites (from Hough 2021).

Goodman et al. 2021; Laverty et al., 2021), but not for their effects on air pollution or traffic volumes (Aldred et al., 2021).

In 2021, Islington Council implemented LTNs installed on an 18-month trial basis. The London Borough of Islington (Fig. 1) is the third smallest borough in London covering 15 km^2; it has the second lowest proportion of greenspace coverage (13 %), is the most densely populated, and is the 6th most deprived local authority of any local authority in England and Wales (16,321 people per km^2). The Council released reports evaluating the impact on traffic flows and NO_2 concentrations of three LTNs (Canonbury East, Clerkenwell and St Peter's, Fig. 1) twelve months after their implementation (Islington Council, 2021a,b,c). They found that traffic volumes decreased respectively by 80 %, 11 % and 56 % in roads internal to these three LTNs, respectively, with mixed results for boundary roads. Mixed results with changes ranging from a 3 % decrease to a 7 % increase were also found when comparing trends in long term NO_2 monitoring in the borough and changes between NO_2 concentrations measured in and around intervention areas during the 6 months following LTN installation in 2021 and in their equivalent time periods in 2019 and 2020. Islington Council, however, did not make full use of all data available for their analyses, nor did they evaluate changes in traffic volumes and NO_2 concentrations attributable to the interventions in a robust statistical framework.

The goal of this study is to assess the causal effect of the three Islington LTNs through a pre and post analysis of changes in NO_2 and traffic volumes to draw robust conclusions about changes attributable to LTN implementation.

2. Overview of data and methods

2.1. Data collection and visualisation

Air quality and traffic volume data were provided by Islington Council for locations in and surrounding three LTNs: Canonbury East, Clerkenwell and St Peter's (Fig. 1 and Fig. 2). The air quality data contains monthly NO_2 ($\mu g/m^3$) concentrations measured by passive diffusion tubes at 93 sites in the borough between January 2018 and February 2021. To assess measurement precision and accuracy, Islington Council applied a bias adjustment factor to the diffusion tube data based on co-location of three tubes with reference monitors. Islington follows regulatory guidance processes for Quality Assurance and Quality Control, as detailed in their annual reports (London Borough of Islington, 2019, 2020, 2021). The start date for NO_2 analysis for this study is July 2019, when initial monitoring started at some sites to prepare for LTN installation. The council also provided 4 or 5 months of post-LTN data for each LTN. Traffic volume counts were undertaken at 42 sites using automatic traffic counters for one week during the month prior to LTN installations and for one week approximately-six months after the installations. In addition, the council also provided traffic volume data from 13 TfL count sites in Islington that were active in the same time period as the LTN automatic traffic counter sites. Islington uses pneumatic automatic traffic counter and TfL sites use radar counts; both run continuously and are considered approximately 98 % reliable by the supplier. Table 1 gives the timeframe of data availability for each LTN. Due to the different implementation dates for each LTN, the time periods for pre and post LTN installation varied accordingly.

For both NO_2 and traffic volume data, we assigned 'LTN situation' to be internal if a site was located within an LTN, boundary if it was in the boundary area, and external if the site was an external control site used for comparison. Fig. 1 and Fig. 2 show the locations of NO_2 monitoring sites and traffic count sites, respectively. For the purposes of this study, the boundary area was defined to be between 0 and 0.5 km via road network from an LTN, starting from and including LTN border roads. This was ascertained through mapping of sites relative to LTN road closures. External control sites were designated as such if they were located more than 0.5 km by road network from any traffic restriction implemented in the pre and post LTN time period. An additional qualitative check was used to ensure external sites would not be affected by traffic detours or diversions, but in the end the 0.5 km criteria was met for all sites.

Following DEFRA (2021) guidance, the location of an NO_2 monitoring site was further classified as background if it was located away from major pollution sources (and hence considered to be representative of pollutant concentrations in urban residential areas), and roadside if located within 5 m of a busy road's kerb. Similarly, in the traffic count data, following Department for Transport guidance, sites located on A roads were classified as major, and minor otherwise (DfT, 2012).

Before estimating the causal impact of the LTN implementation with our difference in differences framework, we compared mean NO_2 concentrations and traffic volumes before and after the installation between external, boundary and control sites.

2.2. Generalised difference in differences approach

For a robust evaluation of the impact of LTN, a generalised difference in differences (DID) methodology was adopted. This approach is meant to attribute change in an outcome as the result of an intervention (or "treatment") by comparing the difference in outcomes in "treatment" sites (i.e. places receiving the intervention) to control sites, over the same period of time after confounding adjustment. The DID is considered a quasi-experimental alternative to a randomised control trial - the gold standard for evaluating the effectiveness

Table 1
Data availability for the three LTNs.

LTN	LTN start date	NO_2		Traffic volume	
		Pre LTN	Post LTN	Pre LTN	Post LTN
St Peter's	2020-07-03	2019-07 - 2020-06	2020-07 - 2020-10	2020-06	2020-10
Canonbury East	2020-08-03	2019-07 - 2020-07	2020-08 - 2020-11	2020-07	2021-02
Clerkenwell	2020-09-07	2019-07 - 2020-08	2020-09 - 2021-02	2020-08	2021-02

of treatment (intervention) and outcome (Hariton and Locascio, 2018; Wing et al., 2018). This design is also able to account for trends that would be happening across the region, for example, the impact of the covid-related lockdowns as was clearly the case for us in our study.

Suppose a study containing an outcome variable Y_{gt} and a treatment variable D_{gt}, with $g = 1, \ldots, G$ groups (or sites in our case), where each group is measured for $t = 1, \ldots, T$ time periods. In a case where $G = 2$, i.e. one is interested in outcomes with only two treatment conditions: the treatment and the control, then $D_{gt} = 1$ if group g is exposed to treatment at time t, and $D_{gt} = 0$ if group g is exposed to the control condition at time t. To find causal effects of the treatment (i.e. the LTN intervention) on the outcomes (i.e. NO_2 or traffic volumes) for each group (i.e. site) and time period under alternative treatment conditions, we need to define potential outcomes that represents the same group but under different (hypothetical) treatment conditions (Wing et al., 2018). Hence, let $Y(1)_{gt}$ be the outcome for group g at time t under the assumption that the treatment was active in g at t, and $Y(0)_{gt}$ the outcome for the same group and time but under an alternative assumption that no treatment was active (e.g. control condition was active) in g at t. Then, the treatment effect for this group g at t would be $\tau = Y(1)_{gt} - Y(0)_{gt}$. However, this calculation could not be easily done because, in an observational study, each group is exposed to only one treatment condition at a certain time period, and consequently, we are only able to observe the corresponding outcome (i.e. $Y_{gt} = Y(0)_{gt} + \left[Y(1)_{gt} - Y(0)_{gt}\right]D_{gt}$). One remedy for this issue is the DID approach.

The simplest DID design (for two groups and two periods) is intuitive but does not accommodate the circumstance in which exposures to the treatment include multiple groups and multiple time periods (as in our study). When groups $G \geq 2$ and time periods $T \geq 2$, then $D_{gt} = 1$ if the treatment group g is exposed to treatment at time t and 0 otherwise. The model for the outcome with no treatment is defined as $Y(0)_{gt} = a_g + b_t + \epsilon_{gt}$, where a_g denotes the group-fixed effects (i.e., the effects of time-invariant characteristics of group g), whereas b_t denotes the time-fixed effect (i.e., the effects of time-varying but group-invariant characteristics of time periods t).

Suppose the outcome with treatment is a shifted version of the outcome without treatment, i.e., $Y(1)_{gt} = Y(0)_{gt} + \delta$. By substituting the fixed structures of $Y(1)_{gt}$ and $Y(0)_{gt}$ to the equation of the observed outcome Y_{gt} introduced above, we have

$$Y_{gt} = Y(0)_{gt} + [Y(1)_{gt} - Y(0)_{gt}]D_{gt} = a_g + b_t + \epsilon_{gt} + [Y(0)_{gt} + \delta - Y(0)_{gt}]D_{gt} = a_g + b_t + \delta D_{gt} + \epsilon_{gt} \quad (1)$$

This implies that the parameter δ would be our estimated treatment effect (i.e. the intervention effect of LTN). One important underlying assumption of the DID model is the common trend assumption. One practical way to validate the assumption is to check whether the lines appear to be approximately parallel after plotting the mean outcomes by group and time periods (Akosa Antwi et al., 2013). A formal test which interacts the group with time periods can also be used to check if the parallelism between different groups exists.

In our application of the DID framework, we denote boundary and internal sites as two different treatment groups, whereas external sites are the control group. Group- and time-fixed effects are measured by including the site and time period covariates. Time fixed effects were particularly relevant in our design to account for strong effect of covid lockdown periods in reducing traffic across our study region. Other covariates such as type of location of the monitoring site (background vs roadside) and road type (major vs minor) are considered for adjusting potential confounding. We carry out the analyses separately with two outcomes: log(NO_2) and log(traffic volume), log-transforming the data to match more closely normal distributions.

Table 2
Changes in average NO_2 for each LTN situation pre and post LTN.

LTN (number of observations)		Average NO_2		Change (%)
		Pre LTN	Post LTN	
St Peter's (129)	External	25.13	25.60	+0.47 (1.9 %)
	Boundary	27.60	26.80	−0.80 (−2.9 %)
	Internal	23.81	20.23	−3.58 (−15 %)
Canonbury East (59)	External	24.52	27.22	+2.70 (11 %)
	Boundary	34.06	35.11	+1.05 (3.1 %)
	Internal	24.25	23.03	−1.22 (−5%)
Clerkenwell (122)	External	24.41	28.20	+3.79 (15.5 %)
	Boundary	28.33	29.07	+0.74 (2.6 %)
	Internal	27.16	25.91	−1.25 (−5%)

3. Results

3.1. Comparisons in simple averages

By comparing the average NO_2 for the three LTN situation before and after the installation as shown in Table 2, we found that the average NO_2 concentration reduced at LTN boundary and internal sites for the St Peter's intervention when compared to external control sites. However, for both Canonbury East and Clerkenwell, we only found a decrease at internal sites but not at boundary sites. In all three LTNs, we found that average traffic volume reduced significantly at the internal sites and decreased to a lesser extent at boundary sites compared to external sites (Table 3).

3.2. Generalised DID analysis on NO_2

Before building the generalised DID model, we need to check the common trend assumption as explained in Section 2.2. Fig. 3 shows the average NO_2 for the two different treatment groups (boundary and internal sites) and the control group (external sites) at different times. We notice that before the interventions, the average NO_2 in different groups approximately follow the same trend, implying the common trend assumption is valid and we can then access the effect of the two treatment conditions on NO_2. This is supported by the formal test showing that none of the coefficients of the interaction between groups and pre-LTN time periods are statically significant (Appendix Table S2-1).

In the DID model with the response being $\log(NO_2)$, all covariates – except the LTN boundary sites– are significant at the $p < 0.05$ level (Table 4). Although LTN boundary and internal sites lead to higher $\log(NO_2)$ compared with control site, the $\log(NO_2)$ in boundary and internal sites appears to decrease after the LTN policy went into effect as shown by the negative post intervention effects variables (−0.093 and −0.059, respectively). When reverting these coefficients to a normal scale for ease of interpretation, we find that the LTN intervention reduced concentrations within the LTN areas by 5.7 % (95 % CI: [0.1 %, 11.0 %]) and in boundary areas by 8.9 % (95 % CI: [0.2 %, 15.7 %]) (using the formulae $exp(-0.093) - exp(0)$ and $exp(-0.059) - exp(0)$, respectively).

We observe, as expected, that the time effects are negative during the period of first national lockdown (from 2020-03 to 2020-07), indicating that NO_2 decreased during those months. The table also indicates that roadside sites (i.e. by the road) lead to higher log (NO_2) than background sites (i.e. on residential streets or in parks), as expected.

3.3. Generalised DID analysis on traffic volumes

Following the same approach, we first check for the common trends, and conclude that the assumption is met because trends in traffic volumes follow roughly the same pattern before each intervention for different site types over the measured time period (Fig. 4). This is supported by the lack of significant interactions found between sites and pre-LTN periods (Appendix Table S2-2). The corresponding result of the DID model in explaining the log of traffic volume is presented in Table 5. We find that, after accounting for measured confounding such as time and site location effect, internal sites appear to have a decreasing but statistically insignificant effect on traffic volumes compared to control sites, whereas boundary sites have the opposite but also statistically insignificant effect. Both internal and boundary sites have negative post intervention effects (−0.144 and −0.874), but only the effect for internal sites is statistically significant. By transforming these estimated post intervention effects back to a normal scale, we find that average traffic volume reduced by 13.4 % at boundary sites (not statistically significant) and 58.2 % at internal sites (statistically significant at the $p < 0.1$ level), after LTN implementation and relative to external control sites. The table also shows that major roads have significantly

Table 3
Changes in average traffic volume for each LTN situation pre and post LTN.

LTN (number of observations)		Average Traffic Volume		Change (%)
		Pre LTN	Post LTN	
St Peter's (42)	External	5573	5769	+196 (3.5 %)
	Boundary	8703	8344	−359 (−4.1 %)
	Internal	2175	868	−1307 (−60.1 %)
Canonbury East (38)	External	5735	5762	+27 (0.5 %)
	Boundary	11,931	9357	−2574 (−21.6 %)
	Internal	2317	606	−1711 (−73.8 %)
Clerkenwell (28)	External	6249	5748	−501 (−8.0 %)
	Boundary	4988	4104	−884 (−17.7 %)
	Internal	473	250	−223 (−47.1 %)

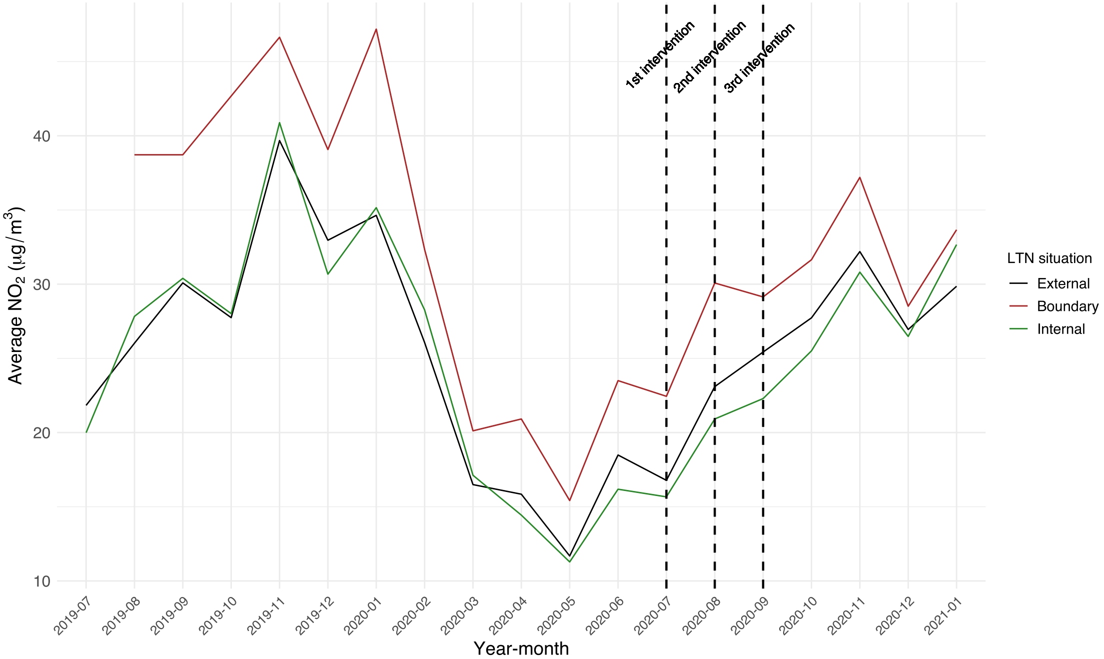

Fig. 3. Average NO_2 in two treatment groups (boundary and internal sites) and control group (external sites) at different time.

Table 4
Estimated coefficients with their 95% confidence intervals from the DID model explaining log(NO_2).

	Estimated coefficient (95 % CIs)
Intercept	2.968 (2.905, 3.030)***
Site (reference level: External)	
Boundary	0.037 (−0.024, 0.097)
Internal	0.064 (0.031, 0.097)***
Time period (reference level: 2019-07)	
2019-08	0.221 (0.136, 0.307)***
2019-09	0.354 (0.272, 0.436)***
2019-10	0.276 (0.192, 0.361)***
2019-11	0.640 (0.559, 0.722)***
2019-12	0.415 (0.335, 0.495)***
2020-01	0.503 (0.423, 0.584)***
2020-02	0.243 (0.163, 0.322)***
2020-03	−0.240 (−0.319, −0.160)***
2020-04	−0.316 (−0.396, −0.235)***
2020-05	−0.627 (−0.707, −0.547)***
2020-06	−0.191 (−0.272, −0.112)***
2020-07	−0.239 (−0.318, −0.159)***
2020-08	0.087 (0.007, 0.168)**
2020-09	0.156 (0.073, 0.238)***
2020-10	0.261 (0.179, 0.343)***
2020-01	0.433 (0.349, 0.517)***
2020-12	0.255 (0.169, 0.342)***
2021-01	0.395 (0.290, 0.450)**
Post intervention (reference level: External)	
Boundary	−0.093 (−0.171, −0.015)***
Internal	−0.059 (−0.117, −0.001)***
Location (reference level: Background)	
Roadside	0.292 (0.253, 0.331)***
Observations	697
R^2/ R^2 adjusted	0.817 / 0.810

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

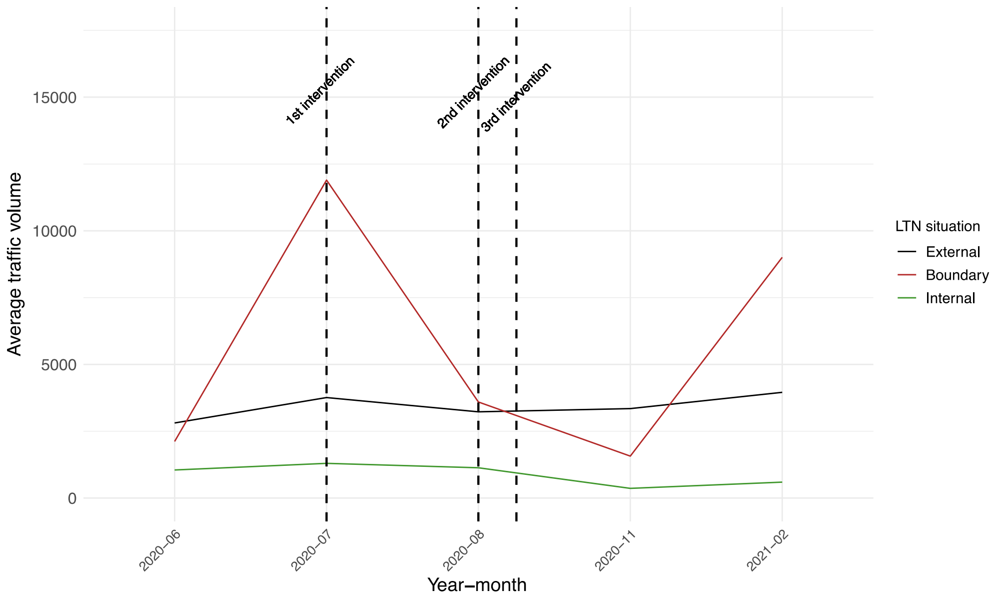

Fig. 4. Average traffic volume in two treatment groups (boundary and internal sites) and control group (external sites) at different time.

Table 5
Estimated coefficients with their 95% confidence intervals from the DID model explaining the log of traffic volume.

	Estimated coefficient (95 % CIs)
Intercept	7.848 (7.010, 8.685)***
Site (reference level: External)	
Boundary	0.580 (−0.113, 1.272)
Internal	−0.823 (−1.657, 0.012)
Time period (reference level: 2020-06)	
2020-07	0.298 (−0.222, 0.818)
2020-08	−0.661 (−1.231, −0.092)
2020-11	−0.021 (−0.905, 0.864)
2021-02	−0.166 (−1.10, 0.679)
Post intervention (reference level: External)	
Boundary	−0.144 (−1.073, 0.785)
Internal	−0.874 (−1.776, 0.028)*
Type of road (reference level: Minor)	
Major	0.836 (0.300, 1.369)**
Observations	*108*
R^2/ R^2 adjusted	*0.704 / 0.676*

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

higher impact on traffic volume than minor roads, as expected.

4. Discussions

4.1. Summary of the results

We evaluated the causal impact of LTN implementation on NO_2 and traffic volume using data provided by Islington Council for a period lasting from July 2019 to February 2021. We used a DID approach to estimate effects of the LTN intervention while simultaneously accounting for potential group- and time-fixed confounding effects and other confounding effects such as site location. Based on the estimated post-treatment effects, we found that LTN implementation leads to a statistically significant reduction in average NO_2 across boundary and internal sites by 8.9 % and 5.7 % in comparison of external control sites. Similar effect of LTN implementation was found on average traffic volume for internal and boundary sites. A statistically significant decrease of 58.2 % at internal sites and a decrease of 13.4 % but without statistical significance at boundary sites were detected when compared to external control sites.

Our study provides evidence that LTN implementation can reduce NO_2 and traffic volumes both within LTN boundaries, and also on LTN boundary roads. Thus, in the LTNs and LTN periods studied here, there was no evidence of displacement of traffic leading to increased traffic volumes and air pollution on the roads surrounding them.

4.2. Comparison with previous studies

Given the contribution of vehicle emissions to air pollution, traffic restrictions – including spatial and temporal vehicular access restrictions, traffic calming measures and low emission zones – are likely to have an impact on air quality. Real world interventions, however, are complex to evaluate, and many previously published studies have not been able to use methodologically robust causal evaluation methods as we have in this study. Ex-post evaluation designs are largely missing, and confounding factors, in particular temporally-sensitive factors such as meteorological conditions and general vehicle fleet turnover are typically insufficiently accounted for (Bigazzi and Rouleau, 2017). Despite methodological drawbacks in earlier studies, they still provide indicative results which are largely in line with our own findings, showing benefits of traffic restriction measures. For example, similar to an LTN intervention but at a larger scale, Lebrusán and Toutouh (2020) reported in a conference paper on the implementation of a vehicular access restriction zone in Central Madrid, Spain. They found no boundary effect and significant decreases in air pollution concentrations within the zone; however, the lack of control sites and clear definition of boundary limits the interpretability of the evaluation. Burns et al. (2020) conducted a systematic review of 38 types of air quality intervention across 42 studies and found evidence suggesting that traffic restrictions are associated with improvements in air quality with very little evidence suggesting such interventions were harmful. The authors, however, also acknowledged the difficulty in deriving overall conclusions regarding the effectiveness of traffic restriction measures in terms of improved air quality because of the heterogeneity across interventions, outcomes, and methods. For example, using a suite of traffic and air pollution models in an ex-ante evaluation framework, Ghafghazi and Hatzopoulou (2015) estimated that speed bumps could increase NO_2 concentrations by 0 % to 10 % in a dense borough of Montreal, Canada. Owen (2005), on the other hand, using an ex-post framework concluded that no changes in measured NO_2 concentrations could be attributed to the implementation of 20 mph zones, which included speed bumps and traffic signs, in six urban areas of North West England.

Ex-post evaluations are widely called for to provide ground-truthing given the complexity of real-world interventions (Bigazzi and Rouleau, 2017). Planning for such evaluations, however, is often uncertain and costly, and interventions are typically implemented with little or no purposeful monitoring campaign designed for evaluation. Monitoring at control sites is particularly important to capture any changes that may be happening across the region and affecting general trends, such as weather patterns which play an essential role in governing fluctuations in pollutant concentrations. There may also be other trends and multiple policies implemented across the study area that could confound any impacts of an intervention under study. This is clear for example from our analysis, where we see how covid restrictions significantly reduced traffic and air pollution in our study area. This is also seen in previous studies, for example, in Viard and Fu (2015) who reported a 21 % improvement in AQI with an odd–even number plate restriction scheme implemented in Beijing, China, or in Chen et al. (2013)'s findings of significant improvements in AQI correlated with the timing and location of power plant closures and traffic restrictions. Both studies concluded that multiple concurrent policies were responsible for observed air quality improvements. This highlights the difficulty of isolating effects of single policies, which our study design attempted to do.

Traffic interventions are also often difficult to evaluate due to their relatively small influence on local air pollution compared to the multitude of other influencing factors, from non-traffic sources to meteorology (Bigazzi and Rouleau, 2017). Finding statistically significant impacts can thus be challenging and requires sufficient sample sizes. This can be especially problematic when using relatively simplistic statistical frameworks. For instance, Owens (2005) included control site to account for exogenous factors, however the analysis relied on simple comparisons of means in each site rather than using a causal framework that could jointly analyse all sites while accounting for potential confounders. One of the strengths of our approach is to make use of all available data, whether or not purposefully and systematically collected at time periods before and after interventions at control and treatment sites for an intervention evaluation. This framework is particularly relevant in the context of disparate and disjointed data collection efforts carried out by Local Authorities; it makes use of a large amount of data otherwise unusable. It does require, however, a careful categorization of monitoring sites, and thus local knowledge to ascertain whether control or intervention sites might have been influenced by local changes impacting specific locations (and not the region as a whole).

4.3. Limitations

Our study thus brings significant improvements to previous work as it attempts to establish causation through the difference in differences approach to estimate post intervention effects with the availability of control sites. It does, however, have several potential limitations. One is with regards to the site selections as control or intervention sites. We carefully selected monitoring sites in partnership with the local authority to remove any interference with any other potential local interventions that might have affected either control or intervention sites. We cannot discard, however, the potential for site specific activities, such as building construction, that might have interfered. It is unlikely, nevertheless, for such activities to have had systematic occurrences across our three intervention sites.

Another potential limitation is the timeframe of the evaluation. Although there is no standardised or widely agreed timespans over which data should be analysed to assess the impact of interventions on air quality, Gallego et al. (2013) asserted that some policies can take up to 3 years to reach their full impacts so that short-term data analysis would inhibit the detection of such impacts.

Also, while we made maximal use of available data, we were still limited by the relatively low numbers of observation points, particularly for traffic volumes. In particular, none of the external control sites included minor roads and only one month of data were collected prior to the intervention at control sites. There were by design no internal major road sites given the nature of LTNs, but we did use major roads for external and boundary sites.

Finally, further adjustments by confounding factors such as local wind patterns and street geometry may have further reduced any bias in model estimates. We were able, however, to adjust for confounding effects due to time, group and location of the monitoring sites, and important regional weather patterns would have been accounted for by the DID design of our analytical framework.

4.4. Future research

There are currently over 100 LTNs installed in London, and more across the UK, hence equivalent analysis of their impact on air pollution would give greater context for the findings of this study (Aldred et al., 2021). With a larger dataset and further resources, further confounding analysis might include sources of local variation in air pollution including local land use patterns and nearby sources. Further work could also investigate on a larger scale the effect of choice of boundary limits. Since the data provided is unbalanced, to obtain more accurate estimation of the LTN effect, propensity score based methods providing balance across treatment and control groups should be considered. Also, given the diversity of research methods used in previous studies evaluating the air quality impact of traffic restrictions, such as Regression Discontinuity (Viard and Fu, 2015), multilevel *meta*-regression (Heydari et al., 2020), it would also be valuable to test the impact of different methodologies on study findings for further research.

4.5. Conclusion

LTNs have the potential to reduce air pollution and traffic volumes within their boundaries and immediate surroundings. While effect sizes are relatively small (6 to 9 %), even small changes in air quality can bring large benefits to population health (Hoffmann et al., 2021). The fear of displacement of traffic and air pollution to surrounding streets is often invoked in opposition to LTNs. Our findings add to a growing and convincing body of literature making the case for cities to adopt traffic restriction strategies such as LTNs, with minimal risk of unintended and unequal consequences. Air pollution may thus be considered as one of the multiple benefits of traffic reduction strategies, alongside previously documented accounts of improvement in public realm making streets safer from traffic collisions, better suited for social interactions and physical activity, and generally supportive of greater wellbeing.

CRediT authorship contribution statement

Xiuleng Yang: Conceptualization, Methodology, Formal analysis, Writing – original draft, Writing – review & editing. **Emma McCoy:** Methodology, Conceptualization, Formal analysis, Writing – original draft, Writing – review & editing. **Katherine Hough:** Data curation, Investigation, Writing – review & editing. **Audrey de Nazelle:** Conceptualization, Methodology, Data curation, Formal analysis, Writing – original draft, Writing – review & editing, Investigation, Supervision.

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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Appendix A. Supplementary material

Supplementary data to this article can be found online at https://doi.org/10.1016/j.trd.2022.103536.

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