Journal of Public Economics 227 (2023) 105014

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

The air quality and well-being effects of low emission zones[☆]

Luis Sarmiento[a], Nicole Wägner[b,c,d,*], Aleksandar Zaklan[b,e]

[a] RFF-CMCC: European Institute on Economics and the Environment (EIEE), Centro Euro-Mediterraneo sui Cambiamenti Climatici (CMCC), Italy
[b] German Institute for Economic Research (DIW Berlin), Germany
[c] RWI – Leibniz Institute for Economic Research, Germany
[d] Norwegian School of Economics (NHH), Norway
[e] Federal Ministry for Economic Affairs and Climate Action, Germany

ARTICLE INFO

Dataset link: 10.17632/zgf9xz756j.1

JEL classification:
Q53
Q58
I31
I18

Keywords:
Low emission zones
Air quality
Well-being
Health
Difference-in-differences

ABSTRACT

This study provides the first evidence of the subjective well-being impacts of low emission zones (LEZs) while also undertaking a comprehensive analysis of their air quality effects. We identify causal impacts by exploiting the zones' introduction date with difference-in-differences designs robust to staggered implementations and time-varying treatment effects. Results show air quality improvements through reductions in traffic-related pollutants despite ground-level ozone increases and harmful spatial pollution spillovers. We further find that the zones cause transitory yet long-lasting reductions in individuals' life satisfaction despite health benefits, suggesting that the subjective well-being effects of restricting mobility potentially outweigh those of improved health.

1. Introduction

Air pollution is a well-known cause of economic losses, mainly through its impacts on health and human capital (e.g., Graff Zivin and Neidell, 2013; Simeonova et al., 2019; Sarmiento, 2022). Pollution is especially high in urban areas, where tailpipe emissions from motor vehicles are one of its primary sources (Davis, 2008; Gallego et al., 2013). Governments have responded to the mitigation challenge through various policy measures to reduce tailpipe emissions, such as exhaust standards, fuel taxes, alternate-day travel, or congestion tolls. One increasingly popular approach is low emission zones (LEZs), which restrict vehicles from entering specific geographical areas based on their emission intensity.

LEZs are one of the most widespread local mechanisms to regulate traffic-related air pollution. They are particularly common in Western and Northern Europe, with more than 100 LEZs scattered throughout Scandinavia, the Netherlands, Germany, Italy, France, and Spain (Holman et al., 2015). Other countries outside Europe have also implemented some variations of LEZs. For instance, Beijing restricts highly polluting heavy-duty vehicles (Zhang et al., 2018); Tokyo regulates the transit of diesel cars (Tokio Metropolitan Government, 2022); Hong Kong specifies operation areas for franchise buses (Ai et al., 2016); and New York City is planning to implement a combination of LEZs and congestion charges in Manhattan (Baghestani et al., 2020).

While the effects of LEZs on traffic-related pollution are well understood (e.g., Wolff, 2014; Gehrsitz, 2017; Pestel and Wozny, 2021; Margaryan, 2021), the current literature has yet to broaden its scope to a more general assessment of spatial spillovers and air quality effects. Examining the impact of the zones on overall air quality is necessary because air quality goes beyond traffic-related pollution by incorporating relevant secondary pollutants such as O_3, which are

☆ We thank Jana Hamdan and Shushanik Margaryan for helpful discussions and participants of the Verein für Socialpolitik Annual Conference, the EAERE Annual Conference, and the European Winter Meeting of the Econometric Society, and seminar members in Amsterdam, Berlin, and Milan for valuable comments. All remaining errors are our own. This publication has received funding from the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (grant agreement No 853487).

* Corresponding author.
E-mail addresses: luis.sarmiento@eiee.org (L. Sarmiento), nicole.waegner@nhh.no (N. Wägner), azaklan@diw.de (A. Zaklan).

[1] Primary (point source) pollutants are emitted from a source. Secondary (non-point source) pollutants form when other pollutants react in the atmosphere. Examples of primary pollutants are carbon monoxide (CO) and sulfur dioxide (SO_2). Examples of secondary pollutants are ozone (O_3) and coarse particulate matter (PM_{10}). It is also worth noting that nitrogen dioxide (NO_2) can be a primary and secondary pollutant.

https://doi.org/10.1016/j.jpubeco.2023.105014
Received 15 June 2022; Received in revised form 11 October 2023; Accepted 12 October 2023
Available online 25 October 2023

currently under-researched in the LEZ literature.[1] Looking at spatial spillovers is also relevant because behavioral adaptations of drivers or chemical interactions in the lower atmosphere can lead to unintended changes in air pollution outside the zones' borders, raising questions of environmental justice and policy effectiveness.

We further provide the first evidence of the life satisfaction effects of LEZs. Although current studies show that LEZs improve health outcomes (Margaryan, 2021; Pestel and Wozny, 2021), the literature is yet to give a fuller picture of their subjective well-being effects. A broader scope is warranted, as driving restriction policies do not only imply benefits in the form of improved health outcomes but also costs by restricting individual mobility, forcing changes in transportation modes, or imposing costly vehicle replacements or retrofits. Evaluating the impact of LEZs on self-reported life satisfaction allows us to understand whether the cost or the benefit side plays a more prominent role in determining the policy's impact on individuals' well-being. We estimate the effect of LEZs on individuals' life satisfaction and health outcomes using data from the German Socio-Economic Panel (SOEP).

LEZs feature a structure of cost and benefits typical of policies targeting urban air pollution. While the policy's costs affect individuals directly and immediately by imposing a new constraint on their behavior, its benefits are realized over time and through collective action. As such, we would expect the cost side of the policy to have a more prominent role concerning perceived life satisfaction in the short to mid-run. This general pattern may be even more pronounced in our setting because of stakeholders in the German motor industry claiming that LEZs restricted individual freedom with little actual effects on air pollution (e.g., Möller, 2007).

Like previous studies, we identify the causal effect of LEZs by exploiting the spatiotemporal variation arising from their staggered implementation using difference-in-differences (DD) designs. The identifying assumption is that conditional on observables, the implementation of LEZs is orthogonal to unobserved determinants of air pollution, health, and subjective well-being. Note that recent advances in the DD literature show that TWFE-DD estimates are potentially biased under staggered policy adoption and dynamic treatment effects (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). We confirm the existence of this bias in the case of LEZs by decomposing the TWFE-DD estimate with the method proposed by Goodman-Bacon (2021). To avoid this source of bias, we use the Callaway and Sant'Anna difference-in-differences (CS-DD) estimator, which allows us to calculate unbiased group and time-specific average treatment effects on the treated (ATTs) (Callaway and Sant'Anna, 2021).

Our results confirm previous studies on the effectiveness of LEZs at decreasing the concentrations of PM_{10} and NO_2 (Wolff, 2014; Gehrsitz, 2017; Pestel and Wozny, 2021; Margaryan, 2021). In line with evidence from the existing literature (Margaryan, 2021), the effectiveness of LEZs increases over time. This pattern of treatment effect dynamics is consistent with previous research showing that LEZs cause changes in fleet composition, as vehicles restricted from entering LEZs are phased out in favor of newer, cleaner, and unrestricted models (Wolff, 2014; Margaryan, 2021).

We also provide the first evidence of LEZ-related ozone increases resulting from titration in NO_x-saturated environments (Monks et al., 2015). This ozone increase is relevant, given the intrinsic relationship between ozone exposure and exacerbated morbidity and mortality. For instance, the 2019 Global Burden of Disease Study estimates 365,000 ozone-related deaths annually worldwide (DeLang et al., 2021). Nevertheless, even with the rise in O_3, we provide evidence of air quality improvements as captured by the zones' effect on the air quality index (AQI). Our results suggest that while the unintended increase in O_3 in our setting merits consideration with respect to policy refinement, LEZs have a clear positive net effect with respect to air quality. Concerning spatial spillovers, we document increases in the concentration of O_3 and CO outside the zones' borders and suggestive evidence of increases in PM_{10} and NO_2.

Examining the effect on individual-level outcomes, we find a significant decrease in self-reported levels of life satisfaction. Declines are especially pronounced for owners of diesel cars – the engine class facing the most stringent restrictions – and for working-age individuals. On average, the life satisfaction of individuals living inside a LEZ decreases by 0.2 points after policy adoption. This effect is substantive as it amounts to about 20% of the life satisfaction effect of becoming unemployed found in the literature (Kassenboehmer and Haisken-DeNew, 2009). Our analysis shows that LEZs can reduce self-reported well-being by introducing an added constraint on decision-making in the face of externalities. We show that the drop in life satisfaction is immediate, lasts for several years after policy implementation but is ultimately transient. We also offer suggestive evidence that higher public transport quality and better accessibility may mitigate the negative life satisfaction effects.

Additionally, we provide evidence of improvements in objective health outcomes. Focusing on the subset of individuals with available health data, we estimate a significant decrease in hypertension cases while confirming the negative impact on perceived life satisfaction for this smaller sample. Therefore, our results suggest that the costs of restricting mobility outweigh the well-being benefits of improved health outcomes, at least in the short run. Looking at heterogeneous effects by age groups reveals that the decrease in hypertension cases accrues mostly to people aged 60 or older. In contrast, the life satisfaction decrease is less pronounced within this age group.

Concerning policy spillovers to outside areas, we find similar-sized reductions in life satisfaction for individuals dwelling near a LEZ, which is not surprising given that individuals living outside the zone's borders likely need to enter the neighboring zone for leisure or work-related activities. However, health outcomes for these individuals show no significant changes after policy implementation, implying that people in the vicinity of LEZs bear the costs of restricted mobility but do not benefit from improvements in air quality.

The divergence between the positive environmental and health effects of LEZs and their negative impact on life satisfaction suggests that the cost of restricted mobility is more salient than its benefits for the first years after implementation. The dynamics of the effect of LEZs on individuals' subjective well-being further suggest that it is temporary, reverting to zero after some time. One interpretation is that the purchase of a cleaner and unrestricted vehicle leads to the removal of the mobility restrictions imposed by LEZs. Another explanation is that individuals learn about the policy's effectiveness over time from their experience, government agencies, or civil society discourse. These dynamics are intuitive, as the receding negative well-being impact over time goes hand-in-hand with escalating policy benefits concerning air quality. These results are in line with previous literature showing that biased beliefs on the costs and effectiveness of climate policies lower their acceptance (see Douenne and Fabre, 2022; Ewald et al., 2022).

From a policy perspective, our findings suggest that policymakers should consider refining the design of LEZs to reduce the impact of harmful spatial spillovers and the unintended effects on secondary air pollutants. Possible strategies could be to increase LEZs' coverage area while anticipating the impact on ozone for regions in NO_x-saturated environments. Other alternatives would be to restrict traffic only in the winter months when traffic-related pollution is more elevated or to impose more stringent restrictions for older vehicle vintages to accelerate fleet turnover towards greater pollution efficiency (Barahona et al., 2020).

Our results show that although LEZs have clear objective benefits, they generate negative impacts on perceived life satisfaction, at least temporarily. Policymakers should thus consider devoting more resources to improving the quality of alternative transportation modes, like public transport, which may decrease the negative effect on subjective well-being by lessening the burden of the constraint on individual behavior introduced by LEZs. Moreover, policymakers should consider more forceful information campaigns to increase the salience of the policy's air quality and health benefits to overcome the adverse subjective well-being effects happening either because of biased beliefs or due to a lack of general trust in governmental policies. This could mitigate the negative impact of LEZs on subjective well-being and better align the objectives of a policy aimed at maximizing overall welfare with its actual impacts on subjective well-being.

Related literature

This paper contributes to two distinct but related strands of the literature. First, we build on the literature examining the effects of LEZs on air pollution and morbidity by providing a detailed analysis of LEZs' effects on air pollution, the first assessment of spatial pollution spillover effects, and the first analysis of their overall air quality effects. Second, we build on the broader literature on the effects of policy interventions on perceived well-being with the first analysis of the subjective well-being effects of driving restriction policies.

Previous studies of the effects of LEZs on air pollution provide compelling evidence of a significant reduction in the concentration of traffic-related pollutants within the zones' borders. Wolff (2014) is an important earlier contribution estimating a significant PM_{10} decrease with TWFE-DD, a result later confirmed by Malina and Scheffler (2015) with fixed-effects panel regressions. Gehrsitz (2017) and Pestel and Wozny (2021) update the PM_{10} estimates from Wolff (2014) and provide additional evidence of a decrease for NO_2. Finally, Ellison et al. (2013) and Zhai and Wolff (2021) uncover similar PM_{10} effects for the London LEZ.

Regarding spillovers, although Wolff (2014) finds beneficial PM_{10} spillovers for the case of the Berlin LEZ, he obtains no significant estimate when looking at the average effect across Germany. Gehrsitz (2017) analyzes PM_{10} spillovers using a larger data set covering more LEZs over a longer period and finds only suggestive evidence of beneficial effects, with point estimates at outside stations being negative but insignificant.

Concerning the effects of the policy on objective health outcomes, Margaryan (2021) uses detailed register data on outpatient and inpatient health care to show that LEZs have clear health benefits. Pestel and Wozny (2021) provide evidence that LEZs decrease extreme health outcomes like hospitalizations due to cardiovascular and respiratory conditions. Rohlf et al. (2020) provide evidence of reductions in pharmaceutical expenses. And Gehrsitz (2017) deviates from this pattern of results by concluding that LEZs' pollution reductions are too small to affect infant health.

A related but distinct stream of the literature analyzes the effectiveness of the second dominant type of driving restriction, alternate-day travel policies. Our results suggest that LEZs are more effective than alternate-day travel at reducing traffic-related air pollutants, as the evidence on the effectiveness of alternate-day travel policies is mixed. For example, Davis (2008, 2017) analyzes the effect of driving restrictions in Mexico City and concludes that the program was ineffective and even counterproductive concerning pollution. Gallego et al. (2013) echo this finding for similar transport reforms in Santiago de Chile. However, evidence of similar instruments in China suggests greater effectiveness. For instance, Zhong et al. (2017) show that the alternate-day travel policy of Beijing reduces air pollution, Chen et al. (2013) provide evidence that Beijing temporarily improved air quality with alternate-day travel restrictions on the eve of the 2008 Olympic Games, and Viard and Fu (2015) study the same restriction policy and suggests that it is effective at reducing air pollution despite limited compliance (Wang et al., 2014).

Last but not least, our paper contributes to understanding the effects of environmental policy on subjective well-being. While the question of whether governments should maximize well-being goes back to Robbins (1932), and while empirical contributions in a range of fields consider the life satisfaction impacts of economic policy, there is a surprising dearth of papers on the life satisfaction impacts of environmental policy. One notable exception is Luechinger (2009), who analyzes the subjective well-being impacts of air pollution by exploiting quasi-experimental variation from the mandated installation of SO_2 scrubbers in Germany and finds a negative impact of air pollution on subjective well-being. Other contributions that directly evaluate the impact of air pollution on subjective well-being find that PM_{10} negatively affects life satisfaction (Levinson, 2012) and subjective mental health measures (Zhang et al., 2017).

Papers in related fields investigate the subjective well-being impacts of various policies. Odermatt and Stutzer (2015) analyze the effects of smoking bans on well-being by exploiting the staggered introduction of such bans and find that bans increase the life satisfaction of smokers who want to quit. McGuire et al. (2022) provide a meta-analysis of the subjective well-being effects of cash transfers, and Allcott et al. (2020) find that deactivating Facebook in a randomized experiment increases subjective well-being. To our knowledge, other than this paper, currently, no evidence exists on the subjective well-being effects of any type of driving restriction policy, including LEZs.

2. Background

2.1. LEZs

Policy makers in the European Union try to reduce air pollution's health risks by setting limit values to pollutant concentrations.[2] The limit values are legally binding. In the case of non-attainment, the member states must propose and implement action plans to reduce the risk or duration of future limit violations. If member states fail to implement sufficient measures to reduce pollution, repeated non-attainment results in financial penalties. Table 1 presents the current exposure limits in the European Union.

Germany implemented the 22nd Ordinance of the Federal Immission Control Act (Bundes-Immissionsschutzgesetz - BImSchG) to comply with EU legislation. This law made EU limit values binding as of January 2005. In the following years, many cities could not adhere to the limit values for NO_2 and PM_{10}. Between 2005 and 2007, 89 urban centers violated the daily PM_{10} limit of 50 $\mu g/m^3$ on more than 35 days in at least one year. Among these, 52 were large cities with more than 100,000 inhabitants or 65 percent of all large cities in Germany. Concerning NO_2, 54 cities exceeded the annual limit of 40 $\mu g/m^3$ for at least one year between 2005 and 2007. Consequently, German federal states and local administrations had to design clean air action plans (CAAPs) to improve air quality. These action plans targeted traffic exhaust-related pollutants and consisted of bundles of policy measures, commonly involving a low emission zone (LEZ).[3]

The Ordinance on the Marking of Vehicles (35th BImSchV) provides the legal basis for introducing low emission zones by giving state and local governments the right to restrict access to specific city areas for cars not complying with predefined emission standards. Germany enforces LEZs through colored stickers on cars' windshields: Only automobiles with a specifically colored sticker can enter the LEZ. Red stickers represent the highest emitting vehicles and green stickers the least emitting ones. The police and municipal authorities enforce the policy and infringement results in fines.[4] Table 2 lists details on the stringency of emission standards and stickers.

[2] Directive 1999/30/EC (EU, 1999) defines permissible concentrations for NO_2, SO_2 and PM_{10}, Directive 2000/69/EC (EU, 2000) set limits for carbon monoxide (CO), and Directive 2002/3/EC (EU, 2002) focuses on O_3. These legislations were revised in 2008 and unified into the single Directive 2008/50/EC (EU, 2008) that defines current limit values and detailed measurement procedures for all criteria pollutants.

[3] Other applied policy measures include procuring cleaner public transport vehicles, closing heavy traffic for commercial vehicles, building ring roads, and re-routing traffic.

[4] In 2008, the fine for entering a LEZ without the appropriate sticker amounted to €40 plus one penalty point at the driving license office. In 2014, authorities doubled fines and abolished the penalty point.

Table 1
EU air quality regulations.

Pollutant	Concentration	Avg. period	Legal nature	Permitted exceedance per year days
CO	10 mg/m^3	Max. daily 8 h mean	Limit value as of 1.1.2005	NA
NO_2	200 $\mu g/m^3$	1 h	Limit value as of 1.1.2010	18
NO_2	40 $\mu g/m^3$	1 year	Limit value as of 1.1.2010	NA
O_3	120 $\mu g/m^3$	Max. daily 8 h mean	Target value as of 1.1.2010	25 days averaged over 3 years
PM_{10}	50 $\mu g/m^3$	24 h	Limit value as of 1.1.2005	35
PM_{10}	40 $\mu g/m^3$	1 year	Limit value as of 1.1.2005	NA

Notes: Limit values for ambient air pollution levels defined in Directive 2008/50/EC (EU, 2008).

Table 2
Relevant emission standards for LEZ sticker categories.

	No sticker	Red	Yellow	Green
Diesel	Euro 1 or older	Euro 2/Euro 1 with particle filter	Euro 3/Euro 2 with particle filter	Euro 4 or better/Euro 3 with particle filter
Gasoline	Without catalytic converter	–	–	Euro 1 with catalytic converter or better

Notes: Relevant emission standards for LEZ sticker categories defined in the Ordinance on the marking of vehicles (35th BImSchV). The Euro standards represent the EU emission regulations for new light-duty vehicles based on Directive 70/220/EEC and its amendments.

2.2. Ground-level ozone

Discussing the particularities of ground-level ozone pollution is important to understanding how LEZs can affect its concentration. Ozone is a secondary pollutant whose concentration depends on complex photochemical reactions of volatile organic compounds (VOCs) in the presence of nitrogen oxides (NO_x) (Monks et al., 2015).[5] High levels of ozone levels are a common issue in large cities where exacerbated VOC and NO_x levels enhance its formation (Bon et al., 2011; Melkonyan and Kuttler, 2012).

The relationship between ozone and its precursors is complex and non-linear. While NO_x increase ozone in *NO_x-limited* environments with high ratios of VOCs to NO_x, the contrary occurs in NO_x-*saturated* areas with low VOCs-to-NOx ratios. NO_x saturation is typical of urban regions because of high traffic-related NO_x emissions (Kroll et al., 2020). Fig. 1 shows Germany's emissions of NO_x and VOCs in road transport. The left panel plots the emissions of all road sources (heavy-duty vehicles, light-duty vehicles, and passenger cars). The right panel only focuses on passenger cars more relevant to urban areas. The figure shows that the German car fleet has a higher emission of NO_x than of VOCs. This low VOCs-to-NOx ratio leads to *NO_x-saturated* conditions that in turn lead to NO_x titration, i.e., the removal of O_3 through its reaction with nitric oxide.

NO_x titration can lead to higher ozone levels during the weekends (de Foy et al., 2020; Murphy et al., 2007), in rural areas (Wilson et al., 2012), and during COVID lockdowns (Brancher, 2021; Grange et al., 2021). Note that the chemical behavior of ozone hinges on more than the VOCs-to-NO_x ratio. It also depends on its long-range transport, atmospheric deposition, weather conditions, and VOCs' reactivity (Monks et al., 2015). The literature on the tropospheric determinants of ozone is still trying to understand the behavior of this complex and dynamic system of chemical interactions (Monks et al., 2015).

One way to check for NO_x titration within our sample of measuring stations is to look at the ratio of weekday-to-weekend ozone measurements. A higher ratio indicates the presence of titration, since lower NO_x emissions during the weekend (due to less traffic) lead to higher ozone levels (Yarwood et al., 2003). Fig. 2 shows the ratio of weekend-to-weekday ozone concentrations for all LEZs in Germany (left panel) and monthly averages over all LEZs over the sample period (right panel).[6] We construct this figure by aggregating O_3 measuring station data from the German Environment Agency (see Section 3 for more information on the data). Fig. 2 indicates the presence of titration as the ratio of weekend-to-weekday ozone is higher than one across all LEZs and remains above unity for most of the sample period.[7] Because of titration in NO_x-saturated environments, we expect that policies aimed at reducing nitrogen oxides, like LEZs, would lead to increased ozone levels – a hypothesis we formally test in this paper. To prevent ozone increases due to policy-induced NO_x reductions, an effective control strategy would need to target both VOC and NO_x emissions (Zhang et al., 2022). However, designing an optimal policy is challenging because VOC and NO_x emissions often share several common sources, with varying emission ratios across sources, and the ozone formation process depends on complex interactions between atmospheric conditions and meteorological factors (Wang et al., 2022).

3. Data

We collect granular pollution measures from the German Environment Agency (UBA). The data contain daily average concentrations of CO, NO_2, O_3, and PM_{10} between 2005 and 2018 across 659 monitoring stations scattered throughout Germany. We also get hourly pollution readings to calculate the AQI according to the formula of the U.S. Environmental Protection Agency (EPA) (EPA, 2018). The AQI maps the concentration of all criteria pollutants into an index between zero and five hundred units. The higher the index, the worse the air quality conditions.[8]

Fig. 3 shows the spatial distribution of LEZs and air pollution measuring stations in Germany.[9] The location and implementation

[5] VOCs are compounds with high vapor pressure and low water solubility. Anthropogenic VOCs come from industrial processes for products like paints, fuels, and hydraulic fluids (EPA, 2022). NO_x are the mixture of nitric oxide and nitrogen dioxide ($NO_x = NO + NO_2$); they come from the combustion of fossil fuels and other naturally occurring processes (American Lung Association, 2022).

[6] The German Environment Agency does not collect nationwide data on VOCs. As a result, we cannot examine the effect of LEZs on the VOCs-to-NO_x ratio at the station level. Figure A.1 plots the location of VOC monitors and LEZs on a map of Germany.

[7] The difference in the ratio between winter and summer (i.e., higher values in the winter compared with the summer) occurs because winter enhances NO_x titration through less efficient NO_x transfers from the planetary boundary layer alongside lower photochemical O_3 production (Parrish et al., 1999).

[8] We also construct data sets for the European and German air quality indexes.

[9] Figure A.2 complements this panel by plotting the spatial distribution of weather stations and the ten largest cities in the country.

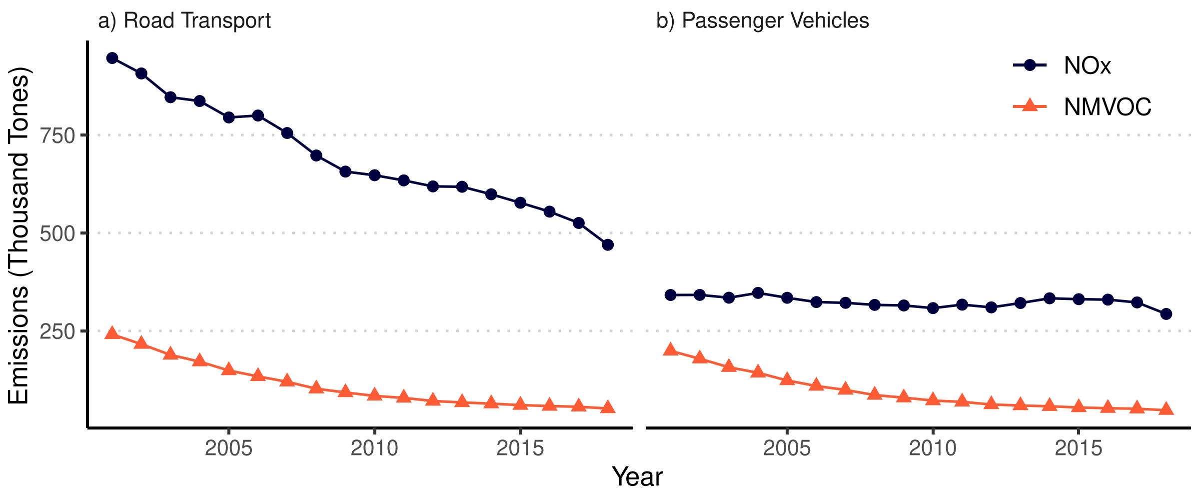


Fig. 1. Road-transport emissions of Nitrogen oxides (NOx) and Non-Methane Volatile Organic Compounds (NMVOC).
Notes: NOx and NMVOC emissions measured in thousand tonnes. The data are from the German emission inventories published by the European Environmental Agency.

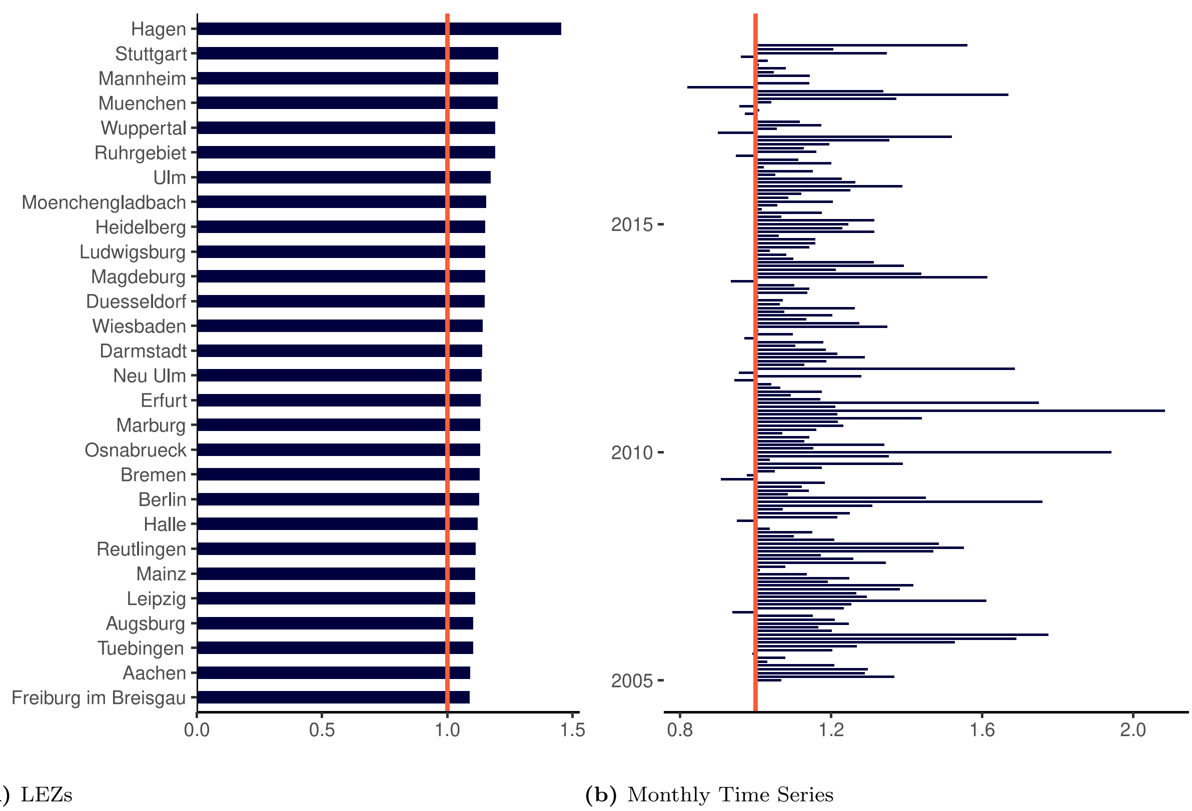
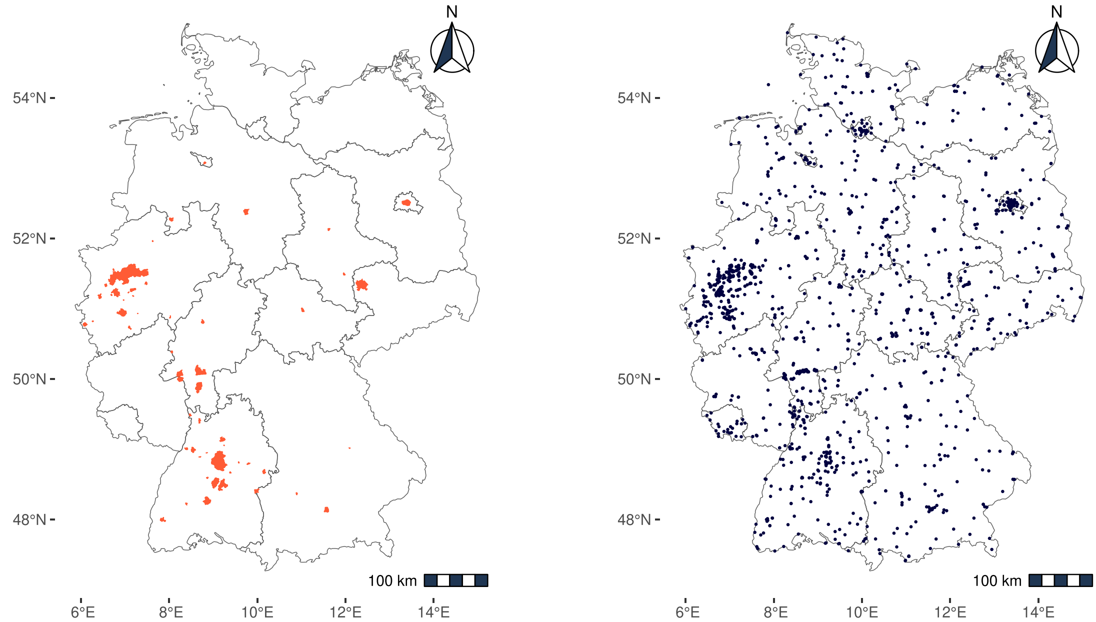


Fig. 2. Weekend to weekday ratio of ozone levels.
Notes: This figure contains the ratio of average ozone measures between weekdays and weekends for all LEZs that have ozone measuring stations. The left panel presents the average ratio at the LEZ level. The right panel presents the average ratio for all LEZs across each month of observation.

dates of LEZs come from UBA's website. We further collect data on the implementation of Clean Air Action Plans (CAAP), bundles of local air pollution mitigation policies introduced by city governments that can come with or without LEZs. In 2018, there were 58 active LEZs concentrated in the country's most populated urban areas around Berlin, Munich, Stuttgart, Frankfurt, Cologne, and the Ruhr area. In contrast, monitoring stations are more scattered.

Fig. 4 compares the evolution of CO, NO_2, O_3, and PM_{10} levels between treated and control stations inside and outside active LEZs. We account for the staggered introduction of LEZs by aggregating the values of all possible event-time combinations across treatment and control groups.[10] Table A.1 lists overall average pollutant concentrations for treatment and control stations. E.g., the value at $t = -1$ for the treated group is the average value one year before implementation across all treatment cohorts. We define treatment cohorts as the group of stations with the same implementation year. We avoid compositional changes in the treatment group by restricting the time window to three

[10] Figure A.3 shows the time series across all specifications of the control group we use in our empirical design.



(a) Low Emission Zones **(b)** Air pollution stations

Fig. 3. Spatial distribution of low emission zones and pollution monitors.
Notes: The left-hand panel depicts all low emission zone (LEZ) introduced between 2008 and 2018. the right panel shows all pollution monitors active during the study period between 2005 and 2018.

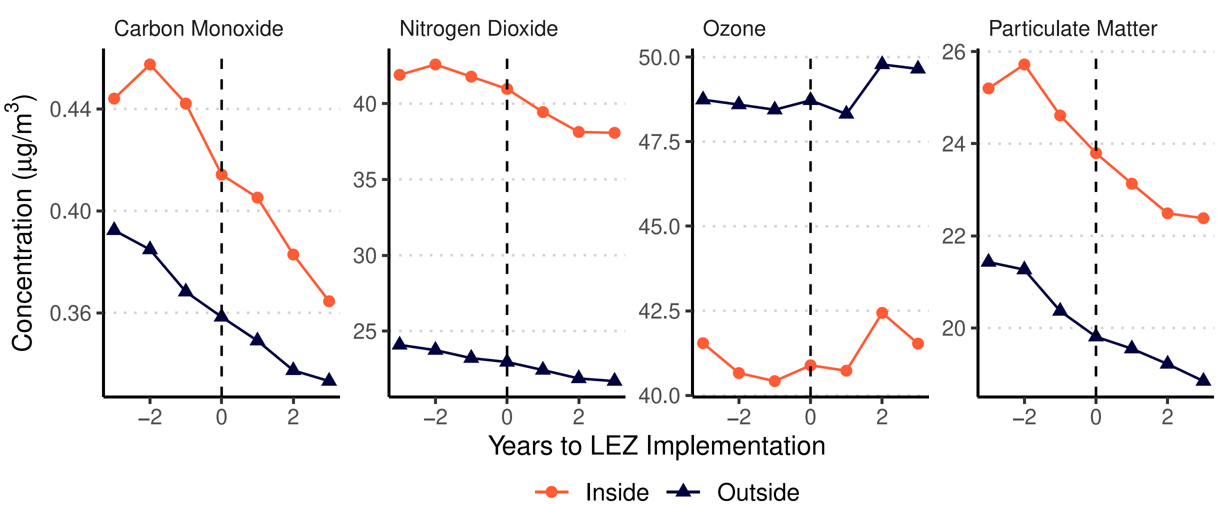


Fig. 4. Pre- and post-treatment averages for treated and control stations.
Notes: Inter-temporal comparison of average pollution between treated and control stations. The vertical axis contains the average of each criteria pollutant and the horizontal axis the time to treatment, i.e., years to the introduction of a LEZ. Each data point corresponds to the average value of all possible event-time combinations across treated and control units three years around policy implementation.

years around treatment. This restriction is necessary because the sample of treated stations changes with time, e.g., stations treated in 2016 would drop from the sample at $t = -4$.[11] We observe similar trends between treated and control units before the introduction of LEZs.

Figures A.4 and A.5 complement Fig. 4 by showing the yearly and monthly average values of inside and outside stations for each treatment cohort (we also show the trends for the different control groups we use in the main analysis). Average pre-treatment trends are similar between treated and control stations regarding CO, NO_2, O_3, and PM_{10}.

To assess the effect of LEZs on individual-level outcomes, we use data from the Socio-Economic Panel Study (SOEP) between 2005 and 2018. The SOEP is a representative longitudinal survey that collects information on persons and households in Germany since 1984. The data contain our primary outcome variable; self-rated individual life

[11] Specifically, the exposure value τ periods to the treatment date is: $\hat{Pol}_\tau^{Treated} \mid \hat{Pol}_\tau^{Control} = \frac{1}{N^\tau}\sum_{\tau=-15}^{6}\frac{1}{N^c}\sum_c Pol\tau^c \quad \forall\, \tau = (Y-G)$. Y indicates the year of the observation and G the treatment group, i.e., the year of LEZ implementation. N^τ is the number of times τ takes an specific value, e.g., for $\tau = -1$, there are six different combinations of Y and G; (2009–2010, 2010–2011, ….2016–2017). Finally, N^c refers to the number of stations.

satisfaction measured on an 11-point Likert scale,[12] as well as a wide range of health and socio-demographic characteristics. Importantly, we observe the geographic location of households at the street-block level and the exact interview date of individuals, allowing us to determine whether they live within LEZs and whether the SOEP interviewed them before or after the zone became active. For individuals outside of LEZs, we observe the distance to the closest LEZ and the corresponding implementation date.

Our treatment group consists of individuals who reside inside an active LEZ during our sample period.[13] The SOEP incorporated several enlargements and refreshment samples in recent years, such that individuals can already live in an active LEZ at the time of their first interview. We exclude these always-treated individuals because their pre-treatment outcomes are unobserved and do not contribute to identification. By dropping these cases, we avoid unnecessary compositional changes in our treatment group that hinder the causal interpretation of point estimates. For similar reasons, we also drop individuals surveyed for one single year and individuals outside of LEZs who were interviewed by SOEP after the closest LEZ came into effect. Additionally, we exclude individuals who changed their place of residence during our study period to avoid switches between treatment and controls, such that we identify the perceived life satisfaction effects on non-movers. However, results are never statistically different from those obtained on the sample with movers (see Table C.3).

Table 3 lists descriptive statistics on all treated and control individuals. We also show descriptives on two alternative control groups used in the empirical analysis in Table A.2. On average, individuals residing inside LEZs are more educated, have higher incomes, fewer children, and fewer motor vehicles. Regarding health variables, we observe, on average, one additional doctor visit per year among treated subjects, while hypertension and cancer shares are very similar in both groups. For treated individuals residing inside LEZs, we observe increases in age, income, and education after LEZ implementation. Although their average life satisfaction increases post-treatment due to an increasing linear trend across our sample period, averaging across the whole post-treatment period masks significant heterogeneity concerning treatment timing. Table A.3 shows treated individuals' average subjective well-being six years before and after LEZ implementation, illustrating that subjective well-being drops in the first year after LEZ adoption before increasing again in later years.

Panel A in Fig. 5 depicts the number of treated and control individuals per year. About 1000 treated persons are observed every year, whereas the number of untreated individuals is about ten times as large. The number of individuals in each group decreases towards the end of our sample period due to panel attrition. Panel B depicts the number of treated observations by treatment group. We define treatment groups as the first year after treatment, i.e., the year of the first SOEP interview after LEZ implementation. Since most cities implemented LEZs on January 1st, the treatment group of two-thirds of treated individuals corresponds to the calendar year of LEZ implementation. The SOEP interviewed the remaining individuals in the first calendar year after LEZ introduction, at a maximum of fourteen months after policy adoption. In total, 98% of all treated individuals were interviewed within the first 12 months after policy adoption. The fact that new persons are treated every year between 2008 and 2016 illustrates the variation in treatment timing induced by the staggered adoption of LEZs. Early LEZs affect around 42% of all treated persons, with large cities like Berlin and Munich introducing LEZs between 2008 and 2009.[14]

Table 3
Descriptive statistics on SOEP individuals.

	Inside LEZ			Outside LEZ
	Total	Before	After	Total
Life satisfaction [0–10]	7.09	7.07	7.11	7.10
	(1.74)	(1.75)	(1.73)	(1.76)
Age [years]	56.74	54.14	58.80	55.52
	(15.26)	(15.25)	(14.96)	(16.24)
Is female [%]	0.53	0.53	0.53	0.53
	(0.50)	(0.50)	(0.50)	(0.50)
Is employed [%]	0.54	0.54	0.54	0.55
	(0.50)	(0.50)	(0.50)	(0.50)
Income [Thsd Euro]	45.74	43.88	47.21	42.01
	(35.27)	(34.50)	(35.80)	(33.67)
Education [years]	12.85	12.68	12.99	12.25
	(3.07)	(3.03)	(3.10)	(2.64)
Number children	0.46	0.52	0.41	0.49
	(0.91)	(0.94)	(0.88)	(0.92)
Owns motor vehicle [%]	0.82	0.84	0.82	0.90
	(0.38)	(0.37)	(0.39)	(0.30)
Number motor vehicles	1.28	1.28	1.29	1.58
	(0.94)	(0.91)	(0.95)	(1.05)
Owns diesel car [%]	0.32	0.33	0.31	0.33
	(0.47)	(0.47)	(0.46)	(0.47)
Number doctor visits	11.40	11.54	11.28	10.23
	(15.31)	(15.65)	(15.04)	(15.20)
Has hypertension [%]	0.34	0.34	0.34	0.34
	(0.47)	(0.47)	(0.47)	(0.47)
Has cancer [%]	0.07	0.06	0.07	0.06
	(0.25)	(0.23)	(0.26)	(0.24)
Number of observations	11466	5067	6399	133212
Number of individuals	1215	1215	1215	18225

Notes: This table shows the average characteristics of treated and control SOEP individuals observed between 2005 and 2018. Treated individuals reside within the LEZs area and control individuals outside. We present overall, pre-treatment, and post-treatment averages for the treated sample. For control persons, we present overall averages.

Panel C shows the number of individuals in each year before and after LEZ implementation. We observe more than 50% of individuals for at least four years before and after policy adoption. Panel D depicts the annual averages of life satisfaction in the treatment and the control group centered around LEZ implementation dates, based on the same methodology as for the pollution outcomes.[15] In the six years before LEZ adoption, average life satisfaction develops in parallel for the treated and control samples. In the year of LEZ implementation, the average well-being of treated individuals drops visibly by about 0.08 standard deviations. Section 7.1 provides further evidence that the common trends assumption between treatment and control groups cannot be rejected.

4. Research design

4.1. Empirical methodology

Identifying the effect of LEZs is not trivial, as their implementation across Germany is not random (see Section 2.1). If we do not consider this systematic (non-random) difference between treated and control stations, it can lead to biased estimates of the true treatment effect. Like previous studies analyzing the impacts of LEZs, we address

[12] SOEP individuals rate how satisfied they are with their lives overall on a scale from 0 ("completely dissatisfied") to 10 ("completely satisfied").

[13] We acknowledge that people living near an LEZ are also potentially restricted in their mobility, e.g., if they commute by car into the LEZ. Unfortunately, our data do not contain information on the workplace of individuals, precluding us from directly testing the commuting hypothesis. Nevertheless, we show that LEZs' negative life satisfaction impacts spill over to neighboring residents in Section 7.3.

[14] We exclude LEZs introduced in 2017 and 2018 because they were all implemented in relatively small towns, where we observe fewer than 20 treated individuals per treatment group. The small number of units in these treatment cohorts hinders a reliable estimation of group-time ATTs.

[15] Figure A.6 shows a similar graph for all control samples used in the empirical analysis.

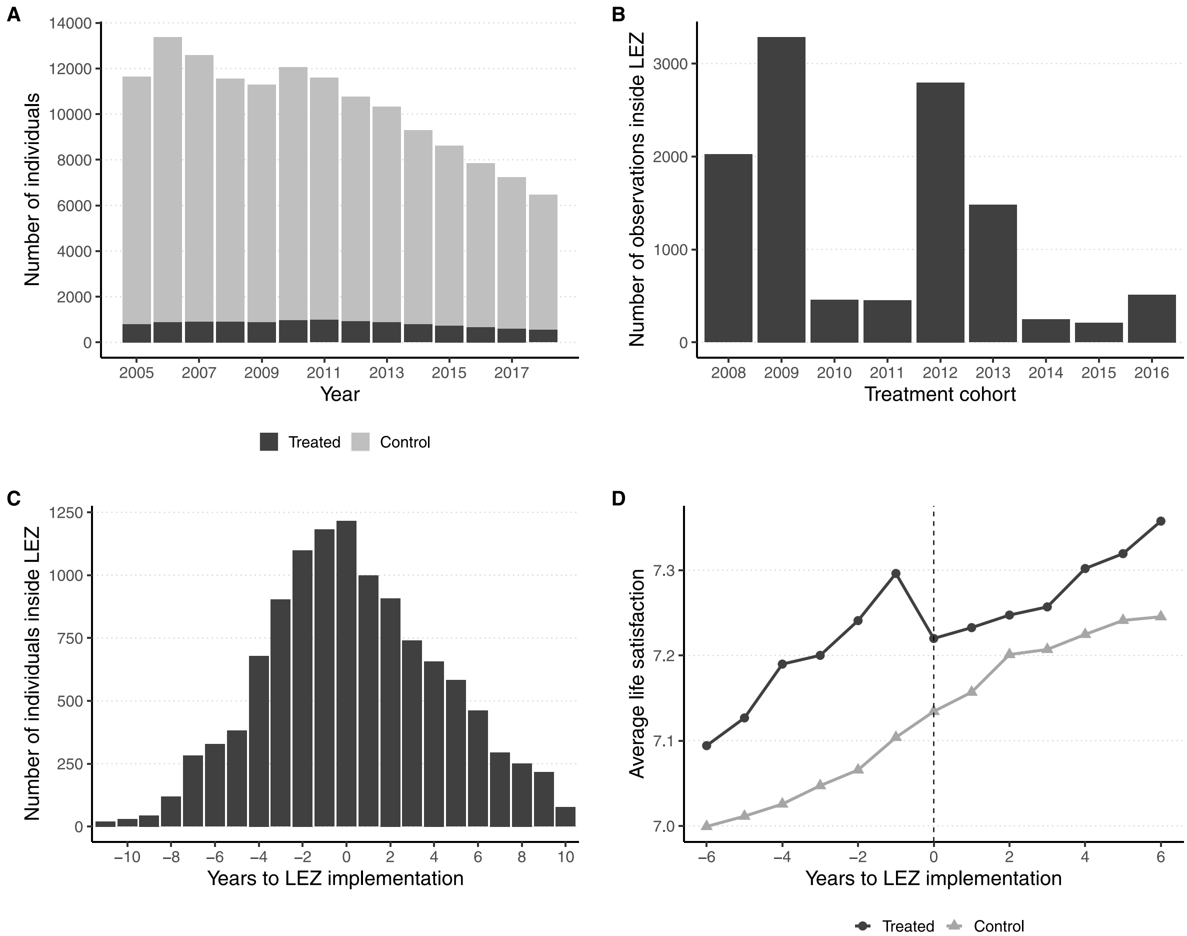


Fig. 5. Number of individuals per treatment group and average well-being of treated and control individuals.
Notes: Panel A depicts the number of individuals in the treatment and control groups per calendar year. Treated individuals reside within the LEZs area and control individuals outside. Panel B shows the number of observations per treatment cohort between 2008 and 2016. We define treatment cohorts as the first year we observe individuals after treatment. Panel C shows the number of treated individuals for each year before and after LEZ implementation. Panel D shows annual averages of life satisfaction for treated and control individuals. The vertical axis depicts the average of life satisfaction and the horizontal axis the time to treatment in years. Each data point corresponds to the average value of all possible event-time combinations between treatment and control groups six years around policy implementation. These averages are based on 9 treatment cohorts consisting of 39 LEZs.

the identification challenge by leveraging the spatiotemporal variation in the zones' implementation date using DD designs. However, different from previous studies, our research design explicitly accounts for the bias of TWFE-DD estimators in the presence of variation in treatment timing and dynamic treatment effects (De Chaisemartin and d'Haultfoeuille, 2020). For instance, Goodman-Bacon (2021) shows that the coefficient of the TWFE-DD estimator is the weighted average of all possible two-group two-period combinations of three different comparison groups: earlier vs. later treated, later vs. earlier treated, and treated vs. untreated.

Fig. 6 presents the Goodman-Bacon decomposition for the TWFE-DD estimates of LEZs' impact on PM_{10} across these three comparison groups. The red line represents the standard TWFE-DD estimate across all comparison groups, and the blue lines are weighted averages for each comparison group. Because already-treated stations ("Earlier Treated" in Fig. 6) experienced substantial decreases in PM_{10} after implementation, their results bias the TWFE-DD towards zero, resulting in an underestimation of LEZs' true impact. In our setting, the bias is so severe that for most Later vs. Earlier Treated comparisons, the sign of the coefficient reverses relative to other comparison groups.[16]

To obtain unbiased estimates under staggered policy adoption and time-varying treatment effects, we use the CS-DD estimator that allows us to estimate and flexibly aggregate group-time ATTs across multiple treatment groups and time periods (Callaway and Sant'Anna, 2021). Other advantages of CS-DD are that it lets us compute unbiased event-time estimates suitable to examine time-varying treatment effects and formally assess the common trends assumption while considering the effects of selective treatment timing. Eq. (1) shows the CS-DD empirical model.

$$y_{it} = \alpha_0^{gt} + \alpha_1^{gt}\mathbb{1}\{G_i = g\} + \alpha_2^{gt}\mathbb{1}\{T = t\} + \beta^{gt}\mathbb{1}\{G_i = g\}\mathbb{1}\{T = t\} + \epsilon_{it}^{gt} \quad (1)$$

Subscripts i, t, and g denote the unit of observation, year of observation, and treatment cohort, respectively. For pollution, y_{it} is the average pollution level of station i in year t. When studying individual-level outcomes, y_{it} is the subjective well-being of individual i in year t. The coefficient α_0^{gt} is the average outcome of never-treated units at t. α_1^{gt} are cohort g fixed effects and α_2^{gt} year fixed effects. The group-time specific ATT for units treated in group g in year t is β^{gt}.

To estimate dynamic treatment effects in the vein of event study designs, we aggregate β^{gt} according to Eq. (2). In it, β^{e} is the average

[16] On average, the 2 × 2 DD estimates for the Later vs. Earlier Treated comparisons amount to 0.49 micrograms per cubic meter (μg/m^3) for PM_{10}. These comparisons get a weight of 12 percent, indicating that this timing group is relatively influential for the overall TWFE-DD parameter.

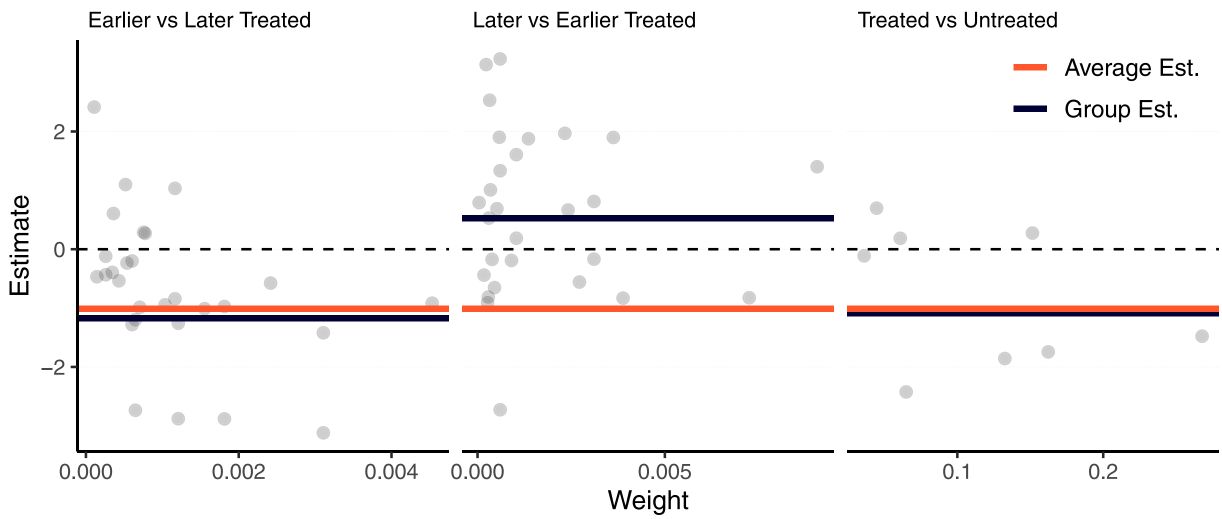


Fig. 6. Goodman-Bacon decomposition for the effect of LEZs on the concentration of coarse particulate matter (PM_{10}).
Notes: Goodman-Bacon (2021) decomposition for the TWFE-DD estimates of LEZs' impact on annual PM_{10} levels. Each dot represents a two-group two-period comparison with point estimates depicted on the y-axis and weights on the x-axis. The colored line is the standard TWFE-DD estimate. Solid horizontal lines represent weighted averages of two-by-two-period comparisons within each comparison group. The sample consists of a balanced panel of stations measuring PM_{10} between 2005 and 2018. Treated stations are located inside the zone and control stations are outside the zones' borders.

treatment effect e periods after treatment, and $P[G = g|G + e \leq T]$ the probability of being first treated in period g conditional on being observed e periods after treatment. As the composition of treatment cohorts can change with e because the treatment length varies across units, we provide estimates robust to this potential pitfall by balancing the groups on e, i.e., we calculate β^e based on a subset of units treated for at least e periods.

$$\beta^e = \sum_{g \in G} \omega_e^{gt} \beta^{gt}, \quad \text{with } \omega_e^{gt} = \mathbb{1}[g+e \leq T] \times \mathbb{1}[t-g = e] \times P[G = g|G+e \leq T] \quad (2)$$

Eq. (3) aggregates group and event-time estimates into the average treatment effect for all treatment cohorts and periods. In it, β is the weighted sum of β^{gt} with strictly positive weights and larger weights for larger group sizes.

$$\beta = \frac{1}{\kappa} \sum_{g \in G} \sum_{t=1}^{T} \omega^{gt} \beta^{gt}, \quad \text{with } \omega^{gt} = \mathbb{1}[t \geq g] \times P[G = g|G \leq T] \quad (3)$$

4.2. Specifying the control group

Fig. 7 illustrates the construction of our baseline control samples. The left panel shows the raw design's definition of treated and control units, with treated and controls inside and outside the LEZ. The right panel illustrates the two restriction areas we use to refine the econometric specification. In the buffer design, we exclude all controls within 25 km of LEZs to avoid spatial spillovers threatening the validity of SUTVA. Treated units are inside the zone, excluded ones are between the LEZ and 25 km from the zone's border, and controls are further away than 25 km. In the doughnut design, we increase the comparability of treatment and control units by restricting the outer edge of the control group to 75 km.[17]

We also provide estimates for a fourth specification where we restrict the control group to stations in cities with a CAAP but no LEZ. This final sample allows us to increase the similarity of treated and controls regarding pollution levels. We select the doughnut design as our preferred specification because it balances the threat of spillovers with a closer geographical match between treated and controls. However, all specifications are consistent with the common trend assumption and deliver similar estimates.[18]

5. Effects on air pollution

5.1. Average treatment effect on the treated

Table 4 shows the estimate for the raw, buffer, doughnut, and CAAP specifications.[19] CO coefficients are negative and statistically significant for all but the CAAP specification. To the best of our knowledge, this is the first study pointing to a decrease in CO because of the implementation of LEZs. For NO_2, estimates are negative and different from zero at the one percent level. They range from 2.74 to 4.07 $\mu g/m^3$ in the CAAP and doughnut specifications. This result confirms the findings of previous studies like Gehrsitz (2017) and Pestel and Wozny (2021), where Gehrsitz (2017) estimates a reduction of 0.6 $\mu g/m^3$ and Pestel and Wozny (2021) of 1.6 $\mu g/m^3$. The difference in magnitude between our estimates and previous studies likely comes from the inherent bias of the TWFE estimator when used in staggered settings (see Fig. 6).

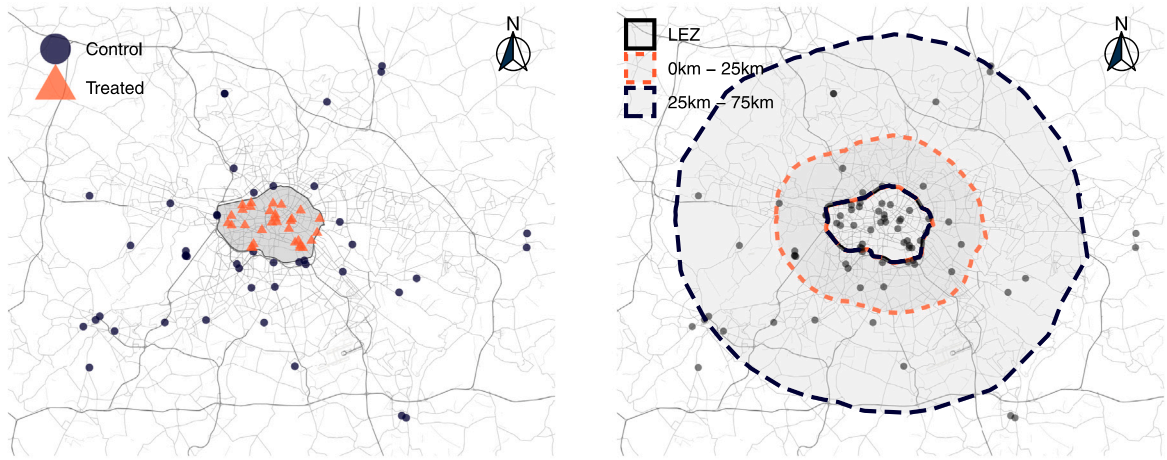
Our results also provide evidence of O_3 increases. In the doughnut design, we estimate that LEZs increase the concentration of O_3 by 1.24 $\mu g/m^3$. This result is in line with the presence of titration in *NO_x-saturated* environments like German urban centers (see Section 2.2). Concerning PM_{10}, our results align with previous TWFE-DD estimates, pointing to a significant PM_{10} reduction of between 1.56 and 2.06 $\mu g/m^3$. Specifically, Pestel and Wozny (2021), Gehrsitz (2017), and Wolff (2014) estimate respective reductions of 1.4, 0.7, and 2.1 $\mu g/m^3$.[20]

[17] Similar results hold at other distances, i.e., 100, 150, 200 km. They are available upon request.

[18] Our choice of the control group is in line with an alternative data-driven approach, in which we use regression discontinuity (RD) models with time as the running variable to estimate the local average treatment effect (LATE) of LEZs at pollution monitors outside the restriction area within a narrow time window around their implementation date. We refer the interested reader to the working paper version (Sarmiento et al., 2021) for further information.

[19] For the curious reader, Appendix B.1 provides results by type of measuring station. UBA divides stations into traffic and background. Traffic stations are next to major roads at street level. Background stations are on top of buildings in background areas.

[20] We obtain a reduction of 2.1 $\mu g/m^3$ by taking 9.1% of the 2007 average PM10 level of 23.1 reported by Wolff (2014) in Table 1. Note that this estimate



(a) Raw **(b)** Spatial Restrictions

Fig. 7. Definition of treated and control stations in the CS-DD design (Berlin low emission zone).
Notes: The left panel shows the raw design's definition of treated and control stations. The right panel illustrates the two restriction areas we use to refine the econometric specification. In the buffer design, we exclude all controls within 25 km of LEZs to avoid spatial spillovers threatening the validity of SUTVA. In the doughnut design, we increase the comparability of treatment and control units by restricting the outer edge of the control group to 75 km. The buffers in the map are on a scale of 1-to-4.

Table 4
Effect of the introduction of LEZs on air pollution.

(a) Raw

	CO	NO_2	O_3	PM_{10}
	-0.030^{*}	-3.715^{***}	0.975^{+}	-1.803^{***}
	(0.012)	(0.512)	(0.562)	(0.314)
N.Obs	1759	5798	3914	5050
N.Stations	255	555	365	507
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(b) Buffer

	CO	NO_2	O_3	PM_{10}
	-0.028^{+}	-3.755^{***}	1.326^{*}	-1.946^{***}
	(0.015)	(0.557)	(0.587)	(0.326)
N.Obs	1394	4503	2943	3848
N.Stations	197	424	271	382
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(c) Doughnut

	CO	NO_2	O_3	PM_{10}
	-0.034^{+}	-4.073^{***}	1.237^{*}	-2.059^{***}
	(0.020)	(0.790)	(0.565)	(0.376)
N.Obs	906	2831	1704	2501
N.Stations	134	265	157	247
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(d) CAAP

	CO	NO_2	O_3	PM_{10}
	−0.016	-2.738^{*}	1.165	-1.560^{**}
	(0.020)	(1.064)	(0.837)	(0.508)
N.Obs	1016	2587	1087	2162
N.Stations	131	243	106	219
N.Groups	8	8	8	8
N.Periods	14	14	14	14

Notes: CS-DD estimates for the impact of LEZs on annual air pollution concentrations across four different specifications of the control sample. The raw control group contains all stations outside LEZs. The buffer design restricts the raw sample to stations beyond 25 km from LEZs' borders. The doughnut sample further trims the control group by restricting its outer edge to 75 km. Finally, the CAAP control group contains only stations in cities with CAAP but no LEZ. Standard errors are clustered at the municipality level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{+}p < 0.10$. Ozone (O_3), nitrogen dioxide (NO_2), and coarse particulate matter (PM_{10}) reported in micrograms per cubic meter (μg/m^3) and carbon monoxide (CO) in milligrams per cubic meter (mg/m^3).

In Figure B.1, we test for the potential bias of TWFEs in our setting by comparing the results from the CS-DD and TWFE-DD estimators. In line with the Goodman-Bacon decomposition, results suggest that the TWFE-DD estimator underestimates the effect for NO_2, O_3, and PM_{10}. In the preferred doughnut specification, NO_2 decreases by 2.55 (vs. 4.07) and PM_{10} by 1.21 (vs. 2.06) μg/m^3. This reduction in PM_{10} of 1.2 μg/m^3 is within the range of estimates reported in previous studies. For CO, although it appears that the TWFE-DD estimator overestimates the effect of LEZs, point estimates are not statistically different from the CS-DD design.

Please note that we cannot distinguish whether the effects of LEZs on traffic-related pollutants (CO, NO_2, PM_{10}) arise because of reductions in traffic volumes or changes in the composition of the car fleet. The challenge in identifying the exact mechanism behind the decrease in traffic pollutants arises because the relationship between air pollution and both mechanisms is quite similar, e.g., one can decrease traffic-related pollution by reducing traffic or changing its composition. Although we cannot disentangle the two mechanisms due to a lack of micro-level traffic data, previous research has shown that LEZs induce changes in vehicle fleet composition. For instance, Wolff (2014) finds that drivers substantially increase the adoption of low-emission vehicles (mainly commercial vehicles) near a LEZ. However, he does not explore the number of miles driven and thus cannot determine the precise mechanism. Margaryan (2021) complements Wolff (2014) by running similar estimates with a larger data set spanning all LEZs introduced between 2008 and 2017. Her results suggest a decrease in cars in

does not suffer from the typical TWFE-DD bias because Wolff (2014) rules out comparisons between later vs. earlier treated units by focusing on LEZs introduced in early 2008 and by carefully selecting the pre- and post-treatment periods (Table 8).

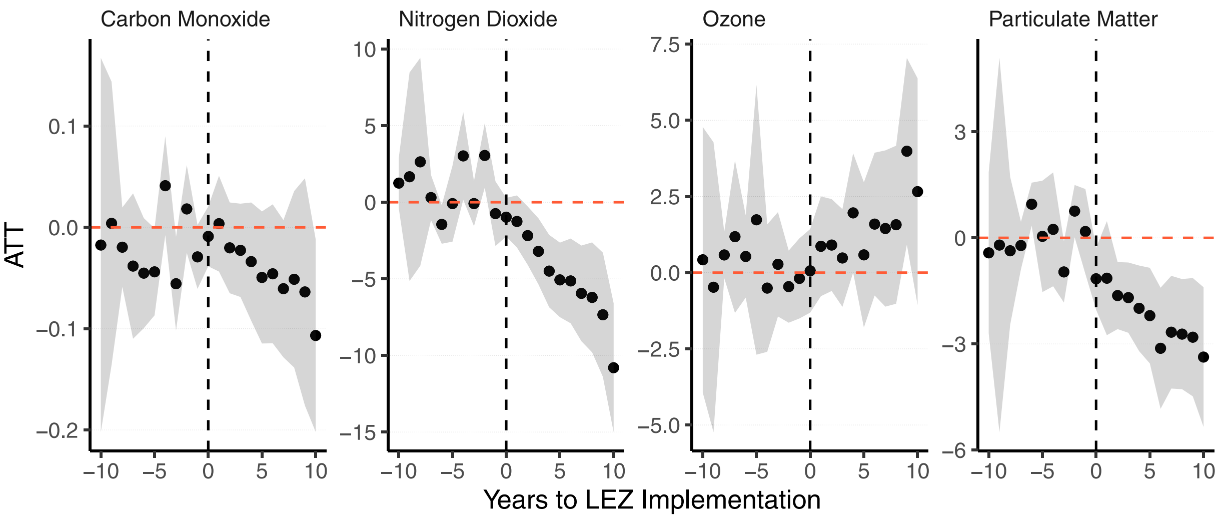


Fig. 8. Event-time ATTs.
Notes: Event-time CS-DD estimates for the impact of LEZs on annual air pollutant concentrations. Event time is measured in years before/after policy implementation. Treated stations are inside the zones. Control stations are between 25 and 75 km from the zones' borders. Gray ribbons represent 95% confidence intervals. We cluster standard errors at the municipality level. Ozone (O_3), nitrogen dioxide (NO_2), and coarse particulate matter (PM_{10}) reported in micrograms per cubic meter (μg/m^3) and carbon monoxide (CO) in milligrams per cubic meter (mg/m^3).

the most polluting vehicle class (Euro 1) after policy implementation, alongside an increase in less polluting and unrestricted Euro 4 vehicles. Although these two studies confirm that implementing LEZs leads to a cleaner car fleet, they do not provide information on the mileage of these vehicles. Pestel and Wozny (2021) is the only study looking at the effect of LEZs on traffic volumes with traffic-count data from the German Highway Research Institute (BASt). Although they find no impact of LEZs on traffic, the BASt data set may not represent intra-city traffic patterns as it covers highways and primary roads. Unfortunately, to the best of our knowledge, comprehensive data on inner-city traffic counts in Germany is not available, hindering researchers' ability to pinpoint the exact mechanism, i.e., the share of emissions attributable to the volume or the composition of urban traffic.

5.2. Dynamic treatment effects

Fig. 8 plots event-time ATTs for the preferred doughnut specification, which are similar for the raw, buffer, and CAAP samples (see Figure B.2). Each coefficient corresponds to the treatment effect at each period before and after treatment. The gray shaded areas represent 95% confidence intervals.

Concerning CO, although all post-treatment coefficients are negative, we find no significant effects after implementation. For NO_2, we observe statistically significant reductions from the second period onward, with pollutant levels decreasing by 10.3 μg/m^3 during the last time interval. O_3 point estimates are positive in every post-treatment period but with wide confidence bands. The event-time estimates for PM_{10} are negative and statistically different from zero from the first-period onward.[21]

In line with evidence from the existing literature (e.g., Margaryan, 2021), these results suggest that LEZs become more effective over time in reducing PM_{10} and NO_2. This increasing effectiveness is likely due to changes in fleet composition, as vehicles restricted from entering LEZs are phased out in favor of newer and cleaner unrestricted models (Wolff, 2014; Margaryan, 2021).

Much of the positive O_3 effect appears to happen because of the last two post-treatment periods. As a robustness check, we restrict the post-treatment period to five and seven years after treatment (Figure B.3). In line with previous research showing that the effects of LEZs on air pollution increase over time (e.g., Margaryan, 2021), the results show smaller (absolute) point estimates with restricted time windows. For NO_2 and PM_{10}, although estimates remain statistically significant across restrictions, they are up to 31% and 21% smaller. Concerning O_3, in line with the higher efficacy of LEZs in reducing NO_2 over time and NO_x titration, point estimates decrease by 23% and 35% in the seven- and five-year restriction samples, respectively. Although still statistically significant for the seven-year restriction, the decrease in the size of the O_3 coefficient renders it only borderline significant in the five-year restriction interval.

5.3. Heterogeneous effects

Working with annual averages can mask seasonal heterogeneity in the effectiveness of LEZs. For instance, O_3 is more predominant during the spring and summer months because of its interaction with solar radiation, while traffic-related pollution may be higher during the winter months because of the lower efficiency of internal combustion engines at low temperatures. Table 5 shows the seasonal results using the doughnut specification.[22]

Although we cannot statistically confirm significant differences in the effects of LEZs, the direction of the point estimates suggests some degree of seasonal heterogeneity. We estimate significant reductions in the concentrations of CO during the winter months of −0.05 milligrams per cubic meter (mg/m^3), but cannot detect significant effects during

[21] Note that compositional changes may affect the results of the CS-DD estimates. For robustness, we restrict the sample underlying the event-time ATTs to contain only stations with five post-treatment periods. Figure B.2d shows the balanced version of the doughnut specification. It confirms that compositional changes do not drive point estimates.

[22] Figure B.4 further contains point estimates for the other control group specifications.

Table 5
Seasonal effects of the introduction of LEZs on air pollution.

(a) Carbon monoxide (CO)

	Winter	Spring	Summer	Fall
	−0.049*	−0.051**	−0.025	−0.036+
	(0.021)	(0.016)	(0.015)	(0.021)
N.Obs	906	872	866	869
N.Stations	134	131	129	129
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(b) Nitrogen dioxide (NO_2)

	Winter	Spring	Summer	Fall
	−3.547***	−5.088***	−4.199***	−5.082***
	(0.794)	(0.764)	(0.632)	(0.790)
N.Obs	2831	2781	2783	2785
N.Stations	265	260	260	259
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(c) Ozone (O_3)

	Winter	Spring	Summer	Fall
	0.760	1.927+	1.957**	0.786+
	(0.642)	(1.170)	(0.681)	(0.474)
N.Obs	1703	1680	1670	1675
N.Stations	156	153	152	153
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(d) Coarse particulate matter (PM_{10})

	Winter	Spring	Summer	Fall
	−1.901***	−2.114***	−2.122***	−2.858***
	(0.504)	(0.456)	(0.535)	(0.468)
N.Obs	2501	2440	2443	2452
N.Stations	247	240	240	241
N.Groups	8	8	8	8
N.Periods	14	14	14	14

Notes: CS-DD estimates for the impact of LEZs on seasonal air pollution concentrations across four different seasons. Treated stations are inside the zones and control stations between 25 and 75 km from the zones' borders. We calculate the average exposure in each season by averaging daily pollution values. Standard errors clustered at the municipality level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{+}p < 0.10$. Ozone (O_3), nitrogen dioxide (NO_2), and coarse particulate matter (PM_{10}) reported in micrograms per cubic meter (μg/m^3) and carbon monoxide (CO) in milligrams per cubic meter (mg/m^3).

the summer months. Estimates for NO_2 and PM_{10} remain largely unchanged across seasons. In contrast, the coefficient for ozone increases in magnitude and becomes statistically significant in spring and summer, suggesting that LEZs increase O_3 concentrations by up to 1.96 μg/m^3 during these seasons. Based on the literature on the effectiveness of seasonally differentiated traffic restrictions (e.g., Rivera, 2021), the lower estimate for CO during the summer aligns with the higher efficiency of internal combustion engines at warm temperatures (Suarez-Bertoa and Astorga, 2018), which reduces the effectiveness of driving restriction policies. Concerning ozone, its different seasonal behavior vs. the other pollutants, with greater increases in the spring and summer, occurs because its formation hinges on photochemical reactions in the presence of solar radiation.

Next, we look at heterogeneous effects by day of the week. As LEZs target older vehicle models, we would expect smaller decreases in traffic-related pollutants during the weekend because of lower traffic volumes. To estimate the effects across the week, we change the unit of observation from yearly to daily averages and estimate the effects with standard TWFEs due to the computational burden of CS-DD with high temporal granularity.[23] However, note that even though we increase the precision of our point estimates by using TWFEs, these results are likely biased because of the staggered implementation of LEZs. Fig. 9 presents the estimates for each weekday. In line with traffic patterns, results suggest smaller reductions in traffic-related pollutants during the weekend. For ozone, the effect is less pronounced during the weekend because of NO_x titration and its inverse relationship with NO_2 in German LEZs (see Section 2.2).

We collect hourly measuring station data from the German Environment Agency to study the within-day variation in the effects of LEZs. The final data set is a panel of hourly pollutant measurements with more than 56 million observations. As in the previous exercise, we would expect higher reductions in traffic-related pollutants during peak traffic hours. Fig. 10 shows the hourly TWFE estimates of the effect of LEZs on average hourly air pollution.[24] The size of the coefficients is consistent with traffic patterns. While CO shows the largest reductions in the morning and afternoon peak hours, LEZs are most effective concerning NO_2 and PM_{10} during the morning rush hour. For O_3, although standard errors are larger, LEZs are associated with an increase at the start of the morning rush and a decrease after 7 PM because of higher NO_x emissions during the afternoon rush and lower solar radiation during the early evening.

5.4. Spillovers

We now estimate the spillover effects of LEZs by considering their impact on stations between 0 and 25 km from the zones' borders in Table 6.[25] As with the results for inside stations, we provide estimates for four different control samples: The raw sample contains all stations farther than 25 km from the zones' border; the buffer sample excludes controls in a 25 km buffer zone between treated and controls; the doughnut sample further excludes all stations more than 100 km away from LEZs; and the CAAP sample only considers controls in CAAP cities.

Although the coefficients for CO suggest harmful pollution spillovers likely due to a displacement of traffic flows around LEZs, we find insignificant coefficients for NO_2 and PM_{10}. These insignificant effects for NO_2 and PM_{10} are in line with other studies examining the spillover effects of German LEZs (see Wolff, 2014; Pestel and Wozny, 2021). For O_3, we find evidence of harmful spillovers in adjacent areas. The increase is similar in magnitude to those obtained inside LEZs and is compatible with the observation that O_3 travels over longer distances than other pollutants (Hov et al., 1978).

The significant spillovers for O_3 vis-à-vis insignificant coefficients for NO_2 may seem counter-intuitive given the close chemical relationship between the two pollutants. That is, should increases in O_3 always go hand in hand with decreases in NO_2? In the case of spillovers and

[23] Applying the CS-DD approach to daily averages would imply estimating separate group-time specific coefficients for each daily difference between the implementation date and each observation date. We decrease the computational burden by estimating the CS-DD with annual averages across weekdays. Table B.1 shows the results of this exercise. Although the standard errors are significantly larger because of aggregation, the coefficients point in the same direction as the TWFEs design.

[24] Figure B.6 presents the results with the CS-DD estimator based on yearly averages. Although the statistical precision of the coefficients decreases, there are similar patterns between the TWFEs model with hourly data and the CS-DD with yearly averages.

[25] Results are qualitatively similar for rings of other thickness, i.e., (0–1), (0–5), and (0–10) km. They are available upon request.

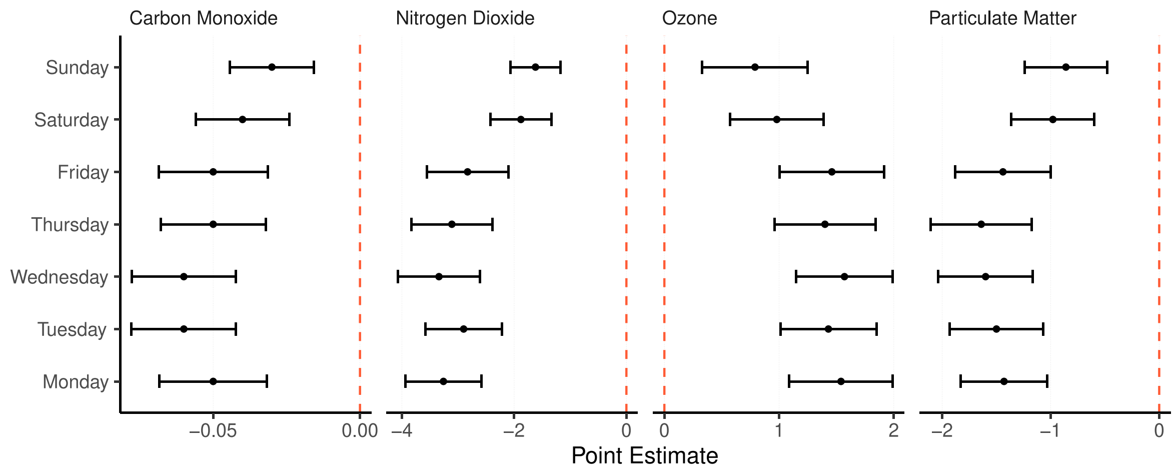


Fig. 9. Effects by day of the week (TWFEs).
Notes: TWFE-DD estimates of the impact of LEZs on daily air pollution concentrations by day of the week. Treated stations are inside the zones and control stations are between 25 and 75 km from the zones' borders. The TWFE-DD model controls for station, year, and month fixed effects alongside temperature (linear and quadratic), precipitation, and sunshine as weather controls. Standard errors clustered at the municipality-by-year level; 95% confidence intervals depicted. Ozone (O_3), nitrogen dioxide (NO_2), and coarse particulate matter (PM_{10}) reported in micrograms per cubic meter (μg/m^3) and carbon monoxide (CO) in milligrams per cubic meter (mg/m^3).

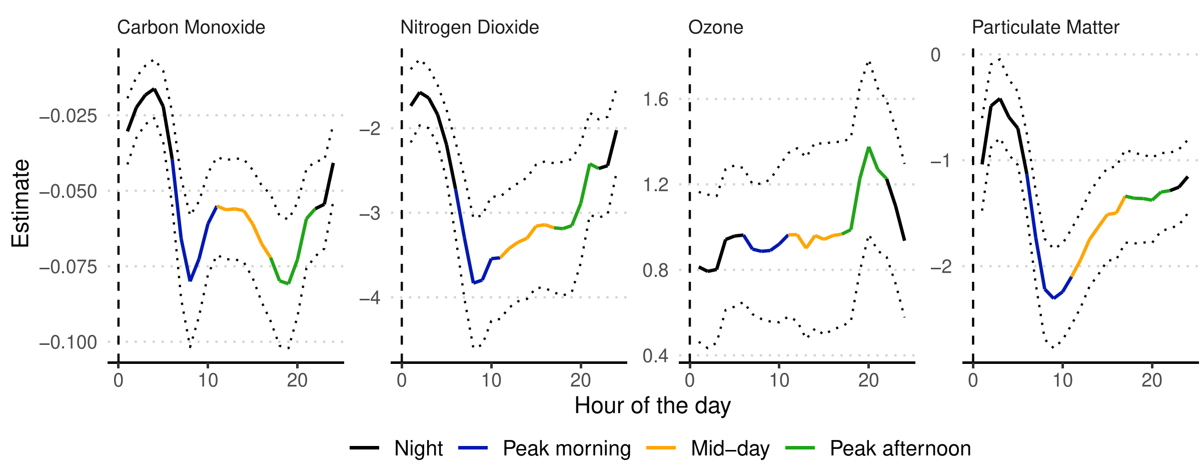


Fig. 10. Effects by the hour of the day (TWFEs).
Notes: TWFE-DD estimates of the impact of LEZs on daily air pollution concentrations by an hour of the day. The treated group is all measuring stations inside LEZs. Treated stations are inside the zones and control stations are between 25 and 75 km from the zones' borders. The TWFE-DD model controls for the station, year, month, week-of-the-year, and weekday fixed effects alongside temperature (linear and quadratic), precipitation, and sunshine as weather controls. Standard errors clustered at the municipality-by-year level; 95% confidence intervals depicted. Ozone (O_3), nitrogen dioxide (NO_2), and coarse particulate matter (PM_{10}) reported in micrograms per cubic meter (μg/m^3) and carbon monoxide (CO) in milligrams per cubic meter (mg/m^3).

different samples, this is not necessarily the case because of heterogeneous dispersion patterns.[26] Specifically, while the effects of LEZs on O_3 can affect stations far away from the restriction area, the impacts on NO_2 are only valid locally. For example, Querol et al. (2017) finds that the urban plume of Barcelona can affect O_3 concentrations up to 60 km outside its metropolitan area, and Lemonsu and Masson (2002) show that the Paris urban plume can extend up to 50 km downwind of the Paris Metro area. Regarding NO_2, Zhu et al. (2020) estimate that moving NO_2 measuring stations just two and a half meters above ground (or six meters from the street) can decrease measurements by 16% (or 13%). This lack of spatial representativeness occurs because NO_2 concentrations change rapidly with time and have very high spatio-temporal variability (see NOAA, 2022).

Another relevant point is exploring the absence of spillover effects for NO_2 and PM_{10} in the presence of statistically significant CO coefficients. If LEZs increase CO because of their effect on traffic, why do we not find the same increase for the other two traffic pollutants? These heterogeneous effects are potentially due to the differences in the sample of monitoring stations across pollutants, i.e., CO, NO_2, and PM_{10} are measured by different stations. Finding evidence of spillovers for the sub-sample of stations measuring CO does not imply that we will find similar results for the other traffic-related pollutants. Figure B.5 shows a Venn diagram of the composition of measuring stations for traffic pollutants in our preferred doughnut specification. It illustrates that 43% of the stations measure all pollutants, 44% measure CO and NO_2, and 43% measure CO and PM_{10}. With this in mind, we run an additional exercise (Table B.2), where we restrict the sample to the

[26] Even though we cannot test for the specific dispersion properties of both particles in our sample, low and high dispersion patterns are well-known characteristics of NO_2 and O_3.

Table 6
Spillovers across different control samples.

(a) Carbon monoxide (CO)

	Raw	Buffer	Doughnut	CAAP
	0.032*	0.036*	0.028*	0.045**
	(0.015)	(0.015)	(0.013)	(0.016)
N.Obs	1252	1075	878	874
N.Stations	185	149	122	119
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(b) Nitrogen dioxide (NO_2)

	Raw	Buffer	Doughnut	CAAP
	−0.068	0.048	−0.035	0.916
	(0.641)	(0.664)	(0.726)	(0.894)
N.Obs	4423	3622	2873	2507
N.Stations	418	342	266	237
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(c) Ozone (O_3)

	Raw	Buffer	Doughnut	CAAP
	1.485*	1.494*	1.561*	1.411+
	(0.640)	(0.663)	(0.704)	(0.817)
N.Obs	3379	2718	2148	1523
N.Stations	313	254	199	148
N.Groups	8	8	8	8
N.Periods	14	14	14	14

(d) Coarse particulate matter (PM_{10})

	Raw	Buffer	Doughnut	CAAP
	−0.338	−0.246	−0.053	0.044
	(0.366)	(0.399)	(0.426)	(0.524)
N.Obs	3897	3212	2612	2211
N.Stations	387	317	253	224
N.Groups	8	8	8	8
N.Periods	14	14	14	14

Notes: CS-DD estimates for the impact of LEZs on annual air pollution concentrations for stations between 0 and 25 km from the zones' borders. We provide estimates of four different specifications of the control group. The raw control sample contains all stations further away than 25 km. The buffer excludes all stations within a 25 km buffer zone. The doughnut further excludes from the buffer sample all stations further away than 100 km from LEZs. And the CAAP only considers stations in CAAP cities. Standard errors clustered at the municipality level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{+}p < 0.10$. Ozone (O_3), nitrogen dioxide (NO_2), and coarse particulate matter (PM_{10}) reported in micrograms per cubic meter (μg/m^3) and carbon monoxide (CO) in milligrams per cubic meter (mg/m^3).

subset of stations measuring CO (*CO stations*) and stations measuring CO, NO_2, and PM_{10} (*Balanced sample*). In the *CO stations* and *Balanced sample* specifications, we find significant positive effects on PM_{10} and positive but less precisely estimated coefficients on NO_2.[27]

As with the effects inside the LEZs, Fig. 11 depicts the results of estimating seasonal spillover effects. Point estimates reveal that O_3 spillovers concentrate in the summer months with increases as high as 2.5 μg/m^3. Moreover, likely due to the higher pollution intensity of internal combustion engines at cold temperatures, we uncover significant spillovers for CO during winter.

6. Effects on overall air quality

Although numerous studies find a direct effect of air pollution on mortality and morbidity, air pollutants interact with each other and have heterogeneous health effects (Dimitriou et al., 2013). As some of these particles are negatively correlated, a simple one-pollutant estimate may be misleading if we want to assess the policy's overall health or well-being consequences. Environmental authorities try to solve this problem by using air quality indexes (AQIs). The main purpose of these indexes is not policy evaluation but to communicate a summary measure of local air quality conditions to the population. They are often defined as indicator variables, e.g., unhealthy vs. healthy or moderate vs. hazardous. Throughout the last decades, environmental authorities, like the U.S. EPA, the European Commission, and the German Environment Agency, have introduced different AQIs based on local conditions and estimated dose–response functions.

Although the specific weights for each pollutant are usually determined based on epidemiological estimates of dose–response functions, these weights are ultimately arbitrary due to the inherent problem of separating the effects of correlated pollutants in observational studies. Yet, given that AQIs are, to the best of our knowledge, the most well-known metric to assess air quality instead of individual pollutant concentrations, we estimate the effect of LEZs on the average value of the U.S. EPA AQI. Table 7 shows the ATTs of LEZs on the annual averages of the AQI for pollution monitors inside and nearby LEZs.

Table 7
Effectiveness and spillovers of LEZ introduction on the air quality index (AQI).

	Inside LEZ	Outside LEZ			
		Raw	Buffer	Doughnut	CAAP
	−5.438***	−0.635	−0.677	−0.475	−0.584
	(1.349)	(0.941)	(1.042)	(1.108)	(1.256)
N.Obs	2968	4814	3970	3118	2679
N.Stations	276	447	366	283	249
N.Groups	8	8	8	8	8
N.Periods	14	14	14	14	14

Notes: CS-DD estimates for the impact of LEZs on annual average AQI values for stations inside and outside LEZs. For inside stations, treated units are inside the zone and controls between 25 and 75 km from the zones' borders. For outside stations, treated units are between 0 and 25 km from the zones' borders and we provide results for four different specifications of the control group. The raw sample contains all stations further away than 25 km, the buffer excludes all stations within a 25 km buffer zone, the doughnut further excludes from the buffer sample all stations further away than 100 km from LEZs, and the CAAP only considers stations in CAAP cities. Standard errors clustered at the municipality level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{+}p < 0.10$.

The results show that LEZs improve air quality inside their borders. After implementation, the value of the AQI decreases by 5.4 units (or 11.6% of average pre-treatment values).[28] Our results with respect to air quality demonstrate that in our Central European setting the effect of LEZs on ozone does not negate the beneficial effects of LEZs on overall air quality. Concerning air quality effects outside the zones' borders, results are overall statistically insignificant, indicating that the O_3 and CO increases do not translate to overall air quality reductions as captured by the EPA-AQI.

We also estimate the effect of LEZs on the European and German air quality indexes (henceforth EU-AQI and DE-AQI). Unlike the EPA-AQI, its European and German counterparts only provide categorical indicators of air quality conditions. The EU-AQI threshold goes from 1 (good) to 6 (hazardous). The German AQI goes from 1 (very good) to 5

[27] The larger confidence intervals for the NO_2 results may occur because of its high reactivity, i.e., its ability to combine with other elements shortly after emission (see NOAA, 2022).

[28] Table B.3 shows the AQI results for all four different control groups. Point estimates are stable in terms of magnitude and significance across specifications.

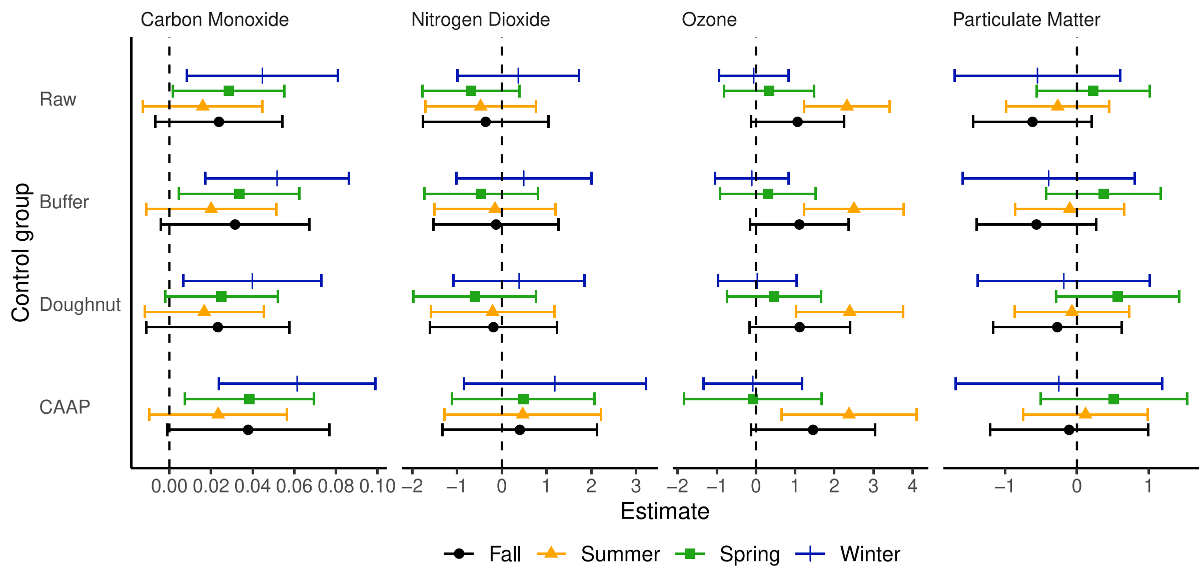


Fig. 11. Seasonal spillovers (CS-DD).

Notes: CS-DD estimates of the impact of LEZs on seasonal air pollution concentrations for stations between 0 and 25 km from the zones' borders. We provide estimates of four different specifications of the control group. The raw sample contains all stations further away than 25 km, the buffer excludes all stations within a 25 km buffer zone, the doughnut further excludes from the buffer sample all stations further away than 100 km from LEZs, and the CAAP only considers stations between 0 and 25 km from CAAP cities. Standard errors clustered at the municipality level; 95% confidence intervals depicted. Ozone (O_3), nitrogen dioxide (NO_2), and coarse particulate matter (PM_{10}) reported in micrograms per cubic meter (μg/m^3) and carbon monoxide (CO) in milligrams per cubic meter (mg/m^3).

(very poor).[29] Figure B.7 shows the effect of LEZs on the yearly average value of these two indexes. Results are consistent with the estimates of the EPA AQI, i.e., we find reductions inside the zone borders and insignificant effects at outer stations.

Figure B.8 depicts the seasonal effects of LEZs on the EPA-AQI for pollution monitors inside and outside LEZs. Regardless of the season, LEZs consistently reduce the value of the AQI inside their borders, suggesting that they are indeed effective at improving air quality despite unintended increases in O_3 concentrations. Concerning season-specific spillovers, we do not find any statistically significant LEZ effects on air quality in neighboring areas. These results suggest that persons living outside LEZs bear the costs of the driving restriction without experiencing the benefits of improved air quality.

7. Well-being and health effects

7.1. Life satisfaction effects

To analyze LEZs' impact on individual subjective well-being and health outcomes, we estimate ATTs for three different control samples. First, we include all SOEP individuals who do not move and are not always treated, as described in Section 3. Second, we exclude control persons residing within 25 km from an LEZ to account for potential spatial spillovers. Third, we further restrict the control group to individuals living in CAAP cities to increase the similarity between treatment and control units. As the sample with the 25 km buffer accounts for spillovers, is moderately sized, and exhibits parallel pre-trends, we focus on this sample for the remainder of the study.[30] Table 8 shows the estimated effects of LEZs on life satisfaction for all three samples.

Table 8
Effect of LEZ introduction on life satisfaction.

	(1)	(2)	(3)
ATT	-0.182^{***}	-0.203^{***}	-0.132^{*}
	(0.054)	(0.061)	(0.066)
N.Obs	144678	88229	23851
N.Individuals	19440	11530	2910
N.Groups	9	9	9
N.Periods	14	14	14

Notes: CS-DD estimates of the impact of LEZs on the life satisfaction of individuals living inside a LEZ. The first column lists results obtained on the full sample, the second restricts the control group to individuals living further away than 25 km from LEZs, and the third further restricts the control group to persons living in cities with a CAAP but no LEZ. Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

All point estimates are negative and statistically significant, indicating that LEZs decrease life satisfaction. This negative effect is remarkable as it occurs despite the policy's overall effectiveness at improving air quality. In the preferred buffer specification, LEZs decrease life satisfaction by 0.2 points, or 2.8% of average pre-treatment values. The magnitude of the effect is quite substantial. For example, also using SOEP data, Kassenboehmer and Haisken-DeNew (2009) find that becoming unemployed decreases life satisfaction by up to 0.9 points, suggesting that the impact of LEZs amounts to about 20% of the unemployment effect. Further restricting the control group to persons living in cities with CAAP reduces the sample to a quarter of its original size, attenuates point estimates, and increases standard errors. Nevertheless, we still observe a statistically significant 0.13-point decrease in life satisfaction.

The negative life satisfaction effects contrast with findings from the revealed preference literature, which show that people value improvements in air quality. Studies using hedonic valuation techniques based on real estate markets and studies using the life satisfaction approach generally find a positive willingness to pay for air quality improvements. We use the empirical evidence to construct a minimum

[29] We refer the reader to the website of the European Environmental Agency (EEA, 2022) and the German Environmental Agency (UBA, 2022) for further information on the European and German indexes.

[30] Appendix C.1 presents the results for the most restrictive CAAP control group. They largely align with the results obtained for the buffer sample but are often less precisely estimated due to the smaller sample size.

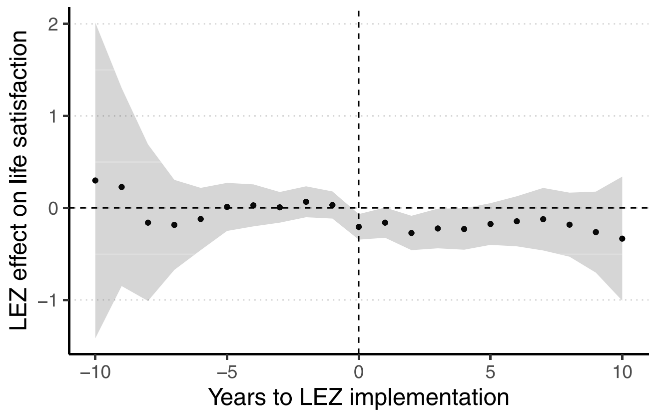


Fig. 12. Dynamic effects on life satisfaction.
Notes: Dynamic CS-DD point estimates and 95% confidence bands on the impact of LEZs on the life satisfaction of individuals living inside a LEZ. The sample of control individuals is restricted to residences further than 25 km away from the nearest LEZ. Standard errors clustered at the household level.

cost valuation for the net effect of LEZs on life satisfaction to be negative. Based on eight studies listed in Table C.1, we calculate the average marginal willingness to pay (MWTP) for a one-unit reduction in PM_{10}, one of the most frequently studied and harmful air pollutants. Average estimates from the hedonic literature indicate an MWTP of €176 per $\mu g/m^3$; average estimates based on the life satisfaction approach amount to an MWTP of €526 per $\mu g/m^3$.[31] Since the life satisfaction approach values air quality effects that are not capitalized in the housing market, we sum over both averages to approximate the total MWTP (see Van Praag and Baarsma, 2005; Luechinger, 2009). Our calculations indicate that we would need to compensate LEZ residents with about €1400 to make them indifferent to the PM_{10} reductions after the implementation of LEZs (about 3% of their average annual income).

Although life satisfaction is an ordinal variable, Table 8 treats it as continuous. To alleviate concerns regarding the scale of our outcome measure and to elicit the effect of LEZs across the distribution of life satisfaction, we estimate the impact on different threshold values on the scale by transforming our dependent variable into a binary indicator equal to one if life satisfaction is larger or equal to two, four, six, and eight. Table C.2 lists the results of this exercise, indicating that people who score higher on the perceived life satisfaction scale experience larger decreases in subjective well-being following LEZ adoption, whereas people at the lower end of the life satisfaction distribution are less affected.

Fig. 12 plots event-time ATTs for the preferred specification and Figure C.1 for the raw and CAAP samples. Results show a significant decrease in life satisfaction in the first year after the LEZ is activated. The negative impact on life satisfaction is slightly weaker in the second year, but it persists in subsequent years before reverting to insignificance some five years after the treatment.[32]

Several mechanisms may drive these treatment effect dynamics. The life satisfaction of treated persons may also increase after purchasing cleaner vehicles that remove the mobility restriction. Individuals may also become accustomed to LEZs after some time so that this issue no longer weighs on their perceived well-being. Another explanation is that individuals learn about the policy's effectiveness over time from their own experience, government agencies, or civil society discourse. Unfortunately, our data do not allow us to pinpoint the precise mechanism. However, given that the dynamics of the receding negative effect on subjective well-being closely track the policy's increasing effectiveness with respect to pollution, we believe that learning about benefits plays an important role.

Note that although all pre-treatment point estimates are insignificant, Fig. 12 reveals a drop in subjective well-being in the seventh period before LEZ implementation. To probe the validity of our empirical strategy, we conduct a falsification test by shifting LEZs' implementation dates by seven years before the actual implementation. Table C.4 and Figure C.3 show the results of this exercise. Point estimates remain insignificant across all samples and event-time periods, decreasing concerns regarding the shift in subjective well-being seven periods before treatment. Additionally, we present further falsification tests based on randomizing implementation dates across time and space in Table C.5 and Figure C.4. Point estimates are statistically insignificant across all falsification exercises.

Next, we analyze heterogeneous effects on LEZ residents to probe the robustness of our main result and to highlight potential mechanisms through which LEZs could decrease life satisfaction. First, we focus on increases in travel costs as a potential channel by exploring whether the life satisfaction impacts depend on individuals' likelihood of being restricted by the policy, their mobility needs, or access to public transport infrastructure. If LEZs impose a binding restriction on drivers' mobility, they would result in pecuniary costs associated with retrofitting or purchasing new vehicles, leading to a decrease in drivers' disposable household income. Similarly, switching to other transportation modes potentially increases travel times, thereby reducing the time available for leisure activities. Both alternatives imply decreases in subjective well-being, either through lower disposable income or less leisure time.

Table 9 investigates whether the life satisfaction impacts depend on the likelihood of being restricted by the policy. In panel (a) we split the sample based on motor vehicle ownership and estimate separate ATTs for LEZ residents with and without cars. Note that the SOEP only surveyed mobility-related information in 2015. Consequently, we base the sample split on cross-sectional differences. As expected, motor vehicle owners' life satisfaction declines significantly due to LEZs. The point estimate for individuals not owning a motor vehicle is around the same magnitude as in the whole sample but imprecisely estimated. Additionally, panel (b) splits the subsample of motor vehicle owners into households with and without diesel cars. In both samples, LEZ residents' life satisfaction decreases, but the effect on diesel car owners is about two-thirds larger than in the other group. This result is in line with LEZs' stricter standards for diesel than for gasoline engines (see Table 2).

Table 10 lists estimated life satisfaction impacts based on mobility needs. Panel (a) lists results across age subgroups, focusing on a cutoff of 65 years. People older than 65 are retirees or near retirement – they are less likely to own a car and, on average, own fewer cars than working-age individuals (Table A.4). In contrast, people younger than 65 have very different mobility requirements, e.g., due to the need to commute to work or children in the household. These differences appear in the point estimates, with strong effects on life satisfaction for individuals younger than 65 and no significant impact for people aged 65 or older. Panel (b) presents results for employed and unemployed persons. Results show that while employed persons experience significant decreases in their life satisfaction after LEZ implementation, the effect on the unemployed is not significantly different from zero. However, note that the coefficient for unemployed individuals is estimated imprecisely and is not significantly different from the coefficient for employed people. Nonetheless, the significant effect on the employed is intuitive given that these individuals are more likely to own a motor vehicle and have greater mobility needs than those who do not need to

[31] MWTPs are expressed in 2010 € to ensure comparability across studies.
[32] Results also hold when controlling for compositional changes of the treatment group at different event times. Figure C.2 shows the dynamic effects of LEZs on life satisfaction for individuals treated for at least two, four, six, and eight years, i.e., $e \in (2, 4, 6, 8)$. Subjective well-being decreases significantly with policy adoption in all samples.

Table 9
Heterogeneous effects on life satisfaction by motor vehicle ownership.

(a) By motor vehicle (MV) ownership

	With MV	Without MV
ATT	-0.214^{***}	-0.215
	(0.059)	(0.169)
N.Obs	53969	6746
N.Individuals	4959	625
N.Groups	9	9
N.Periods	14	14

(b) By diesel car ownership

	Diesel	Other fuels
ATT	-0.299^{**}	-0.183^{**}
	(0.099)	(0.069)
N.Obs	17303	36509
N.Individuals	1652	3293
N.Groups	9	9
N.Periods	14	14

Notes: CS-DD estimates of the impact of LEZs on the life satisfaction of individuals living inside a LEZ. The control group consists of individuals living further away than 25 km from the nearest LEZ. Subsamples are split based on motor and diesel vehicle ownership. Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

Table 10
Heterogeneous LEZ effects on life satisfaction by age and employment status.

(a) By age groups

	$\geq$ 65 years old	< 65 years old
ATT	-0.089	-0.279^{***}
	(0.107)	(0.070)
N.Obs	28707	59522
N.Individuals	4140	8908
N.Groups	9	9
N.Periods	14	14

(b) By employment status

	Employed	Unemployed
ATT	-0.331^{***}	-0.248
	(0.070)	(0.166)
N.Obs	46702	14534
N.Individuals	7468	4336
N.Groups	9	9
N.Periods	14	14

Notes: CS-DD estimates of the impact of LEZs on the life satisfaction of individuals living inside a LEZ. The control group consists of individuals living further away than 25 km from the nearest LEZ. Subsamples are split based on age groups and employment status. Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

Table 11
Heterogeneous LEZ effects on life satisfaction by income.

	Q1	Q2	Q3	Q4
ATT	-0.202	-0.321^{**}	-0.248^{+}	-0.198^{**}
	(0.144)	(0.119)	(0.143)	(0.072)
N.Obs	20775	21170	22649	23623
N.Individuals	2881	2882	2881	2880
N.Groups	9	9	9	9
N.Periods	14	14	14	14

Notes: CS-DD estimates of the impact of LEZs on the life satisfaction of individuals living inside a LEZ. The control group consists of individuals living further away than 25 km from the nearest LEZ. Subsamples are split based on income quartiles. Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

travel to work. These heterogeneous patterns in subjective well-being effects across age and employment groups are detectable even after conditioning on motor vehicle and diesel car ownership (Tables C.6 and C.7).

Heterogeneous life satisfaction effects could also arise from differences in households' ability to cope with increased travel costs. Table 11 shows that the negative effect on life satisfaction persists at similar magnitudes across all income quartiles.[33] Still, the impact is lower and statistically insignificant in the first income quartile, which is plausible given that more than a third of these households do not own a motor vehicle; this sample consists of low-paid workers, retirees, and welfare recipients who typically cannot afford a car. The largest impact occurs in the second quartile, where the share of vehicle owners is much higher at over 90%. Effects on the third and fourth quartiles are lower than in the second, potentially because these individuals can more easily adapt to the driving restrictions. Nevertheless, LEZs still have a statistically significant negative effect on their life satisfaction.

We further explore whether substitution possibilities between private and public transport mediate the adverse well-being effects. For this purpose, we collect public transport data to construct quality and accessibility indicators for each county.[34] We measure public transport quality and accessibility by calculating the average number of departures per stop on a workday and the average driving time to the nearest train station. We merge these indicators into our sample of SOEP respondents via their residence. Table C.8 lists descriptives for LEZ residents and our sample of control individuals. To exploit variation in public transport quality and accessibility across LEZs, we split the treated and control samples based on the median values of public transport indicators among LEZ residents. We base these sample splits on cross-sectional differences since the public transport data are only available for 2018.[35]

Panel (a) in Table 12 shows that the negative life satisfaction impacts are driven by LEZ residents with low-quality public transport options. Although individuals living in LEZs where the number of departures per stop is lower than the median experience larger coefficients than the below-median departures sample, we cannot claim that these differences are statistically different from each other at conventional levels. Similarly, the results in panel (b) suggest that the life satisfaction effects are more pronounced among residents with low public transport accessibility, as indicated by the significantly negative estimate in the sample with above-median driving time to the nearest

[33] Quartiles are based on annual net household income (after taxes) averaged over the whole sample period; the cutoffs are [0; 28,000], (28,000; 34,000], (34,000; 48,400] and > 48,400 Euros.

[34] Data are provided by the Federal Institute for Research on Building, Urban Affairs and Spatial Development (BBSR) and retrieved from https://www.inkar.de/ and contains information on the number of stops and departures of trains, subways, trams, and busses. Figure A.7 illustrates the public transport indicators for all counties in Germany. Data at a higher spatial granularity, albeit desirable, are not available to our knowledge.

[35] Using public transportation data from the post-treatment period should not significantly affect our conclusions. The expansion of public transportation infrastructure is a protracted process with long planning horizons. For example, the mileage of public transport vehicles in Berlin remained very stable between 2005 and 2018, with slight decreases of 0.7% during our sample period (Table 46181-0012, https://www-genesis.destatis.de).

Table 12
Heterogeneous LEZ effects on life satisfaction by public transport quality and accessibility.

(a) Number of departures per stop

	Low	High
ATT	−0.205**	−0.012
	(0.080)	(0.102)
N.Obs	41441	5246
N.Individuals	3654	418
N.Groups	9	7
N.Periods	14	14

(b) Driving time to nearest rail station

	Low	High
ATT	−0.044	−0.242**
	(0.097)	(0.081)
N.Obs	7252	39435
N.Individuals	607	3465
N.Groups	8	9
N.Periods	14	14

CS-DD estimates of the impact of LEZs on the life satisfaction of individuals living inside a LEZ. The control group consists of individuals living further away than 25 km from the nearest LEZ. Subsamples are split based on median values of public transport indicators in the treatment group: the "Low" column lists results for the below-median sample, "High" lists results for the above-median sample. Public transport quality is measured by the number of departures per stop on an average weekday; public transport accessibility is proxied by the average driving time to the nearest train station (in minutes). Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

rail station. These results are consistent with the idea that individuals with access to high-quality public transport incur lower time costs from travel mode switching, indicating that improving the quality and accessibility of public transport could mitigate the negative well-being effects of LEZs.[36]

Another potential channel underlying the negative life satisfaction effects is rooted in people's socializing behavior. If LEZs hinder or diminish the possibilities for social interactions through restricted mobility, this could lead to negative life satisfaction impacts. The SOEP irregularly surveyed the frequency of different leisure time activities as categorical items. Based on individuals' pre-treatment responses to these items in 2003, we analyze whether there are heterogeneous effects for subgroups with higher and lower socializing frequencies via sample splits.[37] We find significant decreases in the life satisfaction of individuals who go out to have dinner or drinks on a weekly basis in Table 13. This potentially suggests that socializing at restaurants and bars might be hindered by LEZ implementation, presumably due to limited accessibility of these venues, consistent with the idea that restricted social interactions are a channel for the negative life satisfaction effects of LEZs. Nevertheless, the imprecise estimates do not allow us to draw conclusions about the relevance of this channel.[38]

Although we provide evidence that travel costs and socializing behavior are potential channels for LEZs' negative impacts on life satisfaction, we cannot rule out alternative explanations. For example, individuals may incur psychological costs due to a loss of autonomy in their mobility decisions,[39] or they may have biased beliefs about the policy's effectiveness. These mechanisms may also play a role in explaining the negative effects on subjective well-being, in particular, following claims of stakeholders in the German motor industry about LEZs having little effects on pollution levels while restricting individuals' freedom of choice (Möller, 2007). Negative life satisfaction effects resulting from biased beliefs may be mitigated by increasing the salience of LEZs' environmental and health benefits through effective communication campaigns.

Table 13
Heterogeneous LEZ effects on life satisfaction by frequency of going out for dinner and drinks.

	Daily	Weekly	Monthly	Rarely
ATT	−0.179	−0.260*	−0.112	−0.024
	(0.569)	(0.123)	(0.106)	(0.130)
N.Obs	864	11844	17596	25405
N.Individuals	96	1305	1888	2794
N.Groups	5	9	9	8
N.Periods	14	14	14	14

Notes: CS-DD estimates of the impact of LEZs on the life satisfaction of individuals living inside a LEZ. The control group consists of individuals living further away than 25 km from the nearest LEZ. Samples are split based on individuals' responses to leisure time questions in 2003. The SOEP survey question was "Please indicate how often you take part in each activity: daily, at least once per week, at least once per month, seldom or never? Going out for dinner or drinks (café, pub, restaurant)." Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$; $^{+}p < 0.1$.

Lastly, we explore real estate market effects as potential confounding factors of the life satisfaction effects. Changes in house prices and rents could drive the estimated subjective well-being effect if the price changes happen because of LEZ adoption. Recent evidence based on offering prices from real-estate advertisements suggests that apartment rents and property offering prices inside LEZs increased after policy implementation by up to two and one percent, respectively (Gruhl et al., 2022).[40] While increasing rents could lower the life satisfaction of renters by decreasing their available income, increasing purchasing prices could benefit homeowners' subjective well-being by increasing the value of their assets. We investigate this hypothesis by splitting our sample into homeowners and renters in Table 14. The life satisfaction of individuals who own their dwelling decreases significantly, which cannot be reconciled with increasing real-estate prices. The effect on

[36] Table C.9 further shows significant life satisfaction decreases among residents of LEZs with above-median car density, while we cannot discern any statistically significant effects for the below-median group. This complements our findings on public transport quality and accessibility since car density is typically negatively correlated with these indicators.

[37] Table C.10 lists results for different frequencies of visiting friends and family, showing insignificant point estimates.

[38] Additionally, we investigated whether LEZ implementation affects the frequency of participating in different socializing activities by using dummy variables as outcome variables. The leisure questions were surveyed in 2003, 2008, and 2013. We found no statistically significant effects on socializing frequency, presumably due to a lack of statistical power. Results are available on request.

[39] Anecdotal evidence shows that people have taken legal action against LEZs despite having many alternative mobility options. For example, a doctor challenged Berlin's LEZ in court because his old GDR car with a two-stroke petrol engine was not allowed to enter the zone, even though he had three other passenger cars that were not restricted, including a Porsche (OVG Berlin-Brandenburg, 20.10.2011 - OVG 1 B 4.10).

[40] We also test whether LEZs affect real-estate market outcomes by estimating the impact of LEZ adoption on self-reported rents and house values in our sample of SOEP respondents. In doing so, we compare renters (owners) residing inside LEZs to renters (owners) from the different control groups. We find positive but insignificant coefficients for self-reported rents (Table C.11 panel a), potentially due to rent control measures for existing tenancies. The results for house values (Table C.11 panel b) are insignificant with point estimates switching sign across specifications, likely because self-assessed house values are inaccurate and our sample lacks statistical power with only two survey years available.

Table 14
Heterogeneous LEZ effects on life satisfaction of renters and owners.

	Owner	Renter
ATT	−0.187*	−0.341***
	(0.080)	(0.087)
N.Obs	59319	28838
N.Individuals	7707	4650
N.Groups	9	9
N.Periods	14	14

Notes: CS-DD estimates of the impact of LEZs on the life satisfaction of individuals living inside a LEZ. The control group consists of individuals living further away than 25 km from the nearest LEZ. Subsamples are split based on home ownership status. Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

Table 15
LEZ effect on health care utilization and health outcomes.

	LS	Doctor visits	Hypertension	Cancer
ATT	−0.191*	−1.425	−0.045*	0.018
	(0.090)	(0.983)	(0.023)	(0.014)
N.Obs	25884	25799	25884	25884
N.Individuals	8491	8482	8491	8491
N.Groups	4	4	4	4
N.Periods	5	5	5	5

Notes: CS-DD estimates of the impact of LEZs on life satisfaction and objective health outcomes of individuals living inside a LEZ. The control group consists of individuals living further away than 25 km from the nearest LEZ. Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

renters is more pronounced but not statistically different from the impact on owners.

The negative life satisfaction effects of LEZs contrast with the positive effects of improved air quality on real-estate values from the hedonic literature (e.g., Chay and Greenstone, 2005). This apparent contradiction can be resolved by recognizing that life satisfaction decreases arise from non-sorting residents who bear the costs of the mobility restriction. These non-movers account for the largest share of SOEP respondents and are most likely to be affected by the policy because they do not move out of the restricted area. Nevertheless, the positive hedonic estimates could result from individuals with stronger preferences for cleaner air sorting into LEZs. Our robustness checks fail to uncover evidence suggesting that LEZs have any systematic effect on moving behavior.[41] Yet, studying the sorting consequences of LEZs is clearly an important topic for further research. For example, estimating an equilibrium sorting model that combines individual- and household-level data with information on housing prices, socio-demographic neighborhood characteristics, and local amenities, will enable a comprehensive analysis of the welfare implications of the policy (Kuminoff et al., 2013).

Note that the negative subjective well-being effect in combination with the positive effect on rents complicates the interpretation of our results in a Tiebout (1956) context since individuals unhappy with LEZs would, ceteris paribus, sort out of communities with LEZs, decreasing demand for housing and thus putting downward pressure on rents. Assuming a Tiebout-type context applies in our setting, our results align with communities introducing LEZs while offering other amenities as part of their basket of goods and services, e.g., cultural amenities, shopping opportunities, work, and social networks. Some amenities may be introduced at the same time as an LEZ and may even be a direct result of LEZs, such as cleaner air and less congestion. It is conceivable that individuals value such amenities enough to remain in the area that added an LEZ but not enough to prevent them from losses in well-being due to the restrictions imposed by LEZs. Disentangling such countervailing effects of LEZs, or other amenities introduced at the same time as LEZs, remains a challenge in our setting and is an interesting topic for future research.[42]

7.2. Health effects

We complement our results by analyzing the impact of LEZs on objective health measures and utilization of the health care system as proxied by the number of doctor visits.[43] Note that the health effects estimated here represent a lower bound, as the health benefits of cleaner air may not necessarily manifest themselves immediately but become noticeable only sometime after policy implementation. Since 2009, the SOEP bi-annually surveys several illness categories and the number of doctor visits within the last twelve months before the interview. We restrict our sample to individuals and time periods where information on specific illnesses is available. Because health outcomes are only available from 2009 onwards, we drop individuals initially treated in 2008 and 2009 because we do not observe their pre-treatment outcomes. Moreover, since the recent empirical literature shows that LEZs decrease cardiovascular diseases (Margaryan, 2021; Pestel and Wozny, 2021), we focus on hypertension as a risk factor for cardiovascular conditions. As a falsification test, we also analyze the effect of LEZs on cancer. Because cancer often develops over long periods, it is unlikely that the impact of LEZs on pollution would trigger changes in the number of cases during our sample period.

Table 15 reports point estimates of the effect of LEZs on life satisfaction, doctor visits, hypertension, and cancer. The negative effect on life satisfaction persists in this smaller sample with a magnitude comparable to our baseline results. Moreover, there is suggestive evidence that LEZs decrease the number of doctor visits, although we cannot establish statistical significance at conventional levels. Concerning objective health outcomes, we estimate a significant decrease in the likelihood of hypertension. The probability of developing hypertension drops by 4.5 percent after implementation. The effect is immediate, manifesting itself in the first year after LEZ implementation, and point estimates are rather stable in later years (Figure C.5). Using the pre-treatment hypertension rate of 34 percentage points among LEZ individuals (Table 3) to calculate the potential reduction in hypertension cases yields an estimated 1.5 percentage points. Given that more than 6.6 million people lived inside a LEZ in 2018, a back-of-the-envelope calculation suggests that these driving restrictions avoided at least 100,000 hypertension cases in Germany. The point estimate of the LEZ effect on the probability of developing cancer is, as expected, statistically insignificant.

Lastly, we validate our results by analyzing heterogeneous health effects for different age groups (Table 16). The decrease in doctor visits is especially pronounced for middle-aged and older individuals, with almost two avoided doctor visits per year for individuals in these age groups. Next, the decrease in the probability of developing hypertension is visible across all age groups, with the effect becoming stronger for older individuals. People aged 60 to 80 benefit the most from LEZs as their probability of hypertension decreases by 8%. This result is in line with previous empirical literature showing that the health

[41] We neither find significant impacts of LEZs on the probability of moving among SOEP respondents nor on migration patterns based on administrative county-level data. Results are available on request.

[42] We thank an anonymous reviewer for this insightful point.

[43] We also considered subjective health satisfaction that is measured using the same ordinal scale as for subjective life satisfaction. However, results are largely insignificant, presumably because the objective health benefits are, on average, too subtle to manifest in overall improvements in subjective health.

Table 16
LEZ effect on health outcomes by age groups.

	Number of doctor visits			Hypertension			Cancer		
	(20–40]	(40–60]	(60–80]	(20–40]	(40–60]	(60–80]	(20–40]	(40–60]	(60–80]
ATT	−1.079	−1.469^{+}	−1.837	−0.009	−0.021	−0.080*	−0.002	0.017	0.025
	(1.160)	(0.891)	(1.994)	(0.039)	(0.033)	(0.040)	(0.003)	(0.018)	(0.030)
N.Obs	2754	10653	10338	2764	10690	10367	2764	10690	10367
N.Individuals	1485	4097	3585	1489	4102	3585	1489	4102	3585
N.Groups	4	4	4	4	4	4	4	4	4
N.Periods	5	5	5	5	5	5	5	5	5

Notes: CS-DD estimates of the impact of LEZs on objective health outcomes across different age groups. The control group consists of individuals living further away than 25 km from the nearest LEZ. Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

Table 17
LEZ spillovers in well-being and health.

	LS	Doctor visits	Hypertension	Cancer
ATT	−0.196***	0.493	0.003	−0.002
	(0.044)	(0.475)	(0.010)	(0.005)
N.Obs	32782	32671	32782	32782
N.Individuals	10518	10508	10518	10518
N.Groups	4	4	4	4
N.Periods	5	5	5	5

Notes: CS-DD estimates of the impact of LEZs on the life satisfaction and health outcomes of individuals living between 0 and 25 km from LEZs. The control group consists of individuals living further away than 25 km away from the nearest LEZ. Standard errors clustered at the household level. Significance levels denoted by $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

benefits of LEZs accrue mostly to the older population (Margaryan, 2021). Finally, the probability of developing cancer is not significantly affected by LEZ implementation in any age group.

Overall, these diverging results on the impacts of LEZs on life satisfaction and health, together with our findings of a beneficial effect on overall air quality in Section 6, reveal a disconnect between the objective benefits of LEZs and its negative impact on subjective well-being. Results are consistent with people being unaware of the reduction in air pollution and the health improvements caused by LEZs, while the restrictions they impose on mobility choices are salient.

7.3. Spillovers in life satisfaction and health outcomes

The mobility restrictions of LEZs extend to people residing outside the zones' borders if they drive a non-compliant car into the neighboring zone, e.g., to commute to work or to engage in leisure activities. Moreover, our results in Section 5 show that LEZs potentially generate air pollution spillovers in adjacent areas. This implies that individuals living in the neighboring areas not only have to bear restricted mobility but also potential costs arising from increased air pollution.

To investigate whether individuals living near LEZs experience life satisfaction and health effects, we estimate LEZs' ATTs for individuals residing within 25 km of a zone.[44] Table 17 lists the results for the same outcomes as Table 15.[45] The negative life satisfaction effect is also present outside the zones' limits, with decreases comparable to those of LEZ residents. This reduction in subjective well-being for outside dwellers is not surprising, given that people living within 25 km of a LEZ will likely travel into the corresponding city center at some point. In addition, these individuals are potentially affected by harmful pollution spillovers.

In contrast to our findings for residents of LEZs, the health outcomes of neighboring individuals are not impacted by LEZ implementation, implying that the adverse pollution spillovers are insufficient to trigger negative health effects, as measured by healthcare utilization, hypertension, and cancer rates. Our results, therefore, suggest that people residing in the vicinity of LEZs bear the costs of restricted mobility without the health benefits of improved air quality.

The similar-sized decrease in life satisfaction among people living near LEZs and LEZ residents does not imply that residents value the air quality and associated health benefits at zero. Instead, LEZ neighbors may be less restricted in their mobility because they are not forced to enter the zone and can circumvent the restricted area more easily. Thus, while LEZs residents enjoy higher air quality and health benefits, they also experience greater policy stringency. The effects of these two opposing mechanisms are likely to cancel each other out, which leads to similar ATTs for people living inside and around the restriction area.

Overall, our results suggest that LEZs decrease the subjective well-being of individuals living inside their borders. This negative life satisfaction effect is immediate but transitory, disappearing several years after policy implementation. We show that the negative life satisfaction effects are heterogeneous and depend on the likelihood of being restricted by the policy, mobility needs, and access to public transport infrastructure. In addition, we provide evidence that LEZs decrease the likelihood of developing hypertension, mostly in the older population. However, these health benefits do not seem sufficient to counteract the drop in life satisfaction due to LEZ implementation. Concerning people living nearby LEZs, we find comparable decreases in life satisfaction without any significant impacts on health outcomes.

8. Conclusion

This paper contributes to the growing literature on the effects of low emission zones by providing a comprehensive analysis of their effectiveness concerning overall air quality and subjective well-being. Moreover, we advance our understanding of LEZs by paying particular attention to spatial spillovers and seasonal heterogeneity. Looking at other determinants of air quality in addition to traffic-related contaminants allows us to broaden the scope of our analysis. Notably, we also analyze the impact of the zones on self-reported life satisfaction and objective health outcomes of individuals in Germany. To the best of our knowledge, this is the first study looking at the life satisfaction effects of driving restriction policies. To identify the causal impact of the zones, we use recent advances in difference-in-differences designs that are robust to the potential bias of staggered implementation and time-varying treatment effects.

Results show that LEZs improve overall air quality, despite causing an unintended increase in ground-level ozone. Confirming the previous literature, the air quality improvement is driven by a decrease in traffic-related pollutants. The increase in ozone does not negate the beneficial effects of LEZs on overall air quality in our setting. Our analysis of

[44] Since our data lacks information on individuals' workplaces and destinations of leisure trips, we resort to this proximity-based approach. Using a smaller distance for the treated group, e.g., individuals within 1 km, 5 km, 10 km, etc., yields point estimates that are very similar in magnitude to the 25 km specification

[45] Table 17 uses the bi-annual SOEP sample starting in 2009. Table C.12 lists results for the full SOEP sample (2005–2018) and suggests a life satisfaction effect of similar magnitude as in the reduced sample.

seasonal heterogeneity shows that LEZs are especially effective at reducing carbon monoxide during the winter when the policy's marginal effects are more pronounced due to the higher pollution intensity of internal combustion engines at low temperatures. In contrast, the increase in ozone levels during the summer aligns with greater solar radiation enhancing ozone formation. We also find that LEZs cause spatial spillovers, increasing ozone and carbon monoxide outside the zones' limits. Again, some of these effects are more potent during the summer and winter.

We also confirm that LEZs become more effective in reducing pollution over time, consistent with previous evidence that restricted vehicles are phased out in favor of newer and cleaner vehicles. However, we cannot disentangle the extent to which LEZs reduce pollution by causing changes in fleet composition or by affecting the number of miles driven within LEZs. This is an interesting area of future research that would require traffic and vehicle data at the micro level.

Our analysis of life satisfaction outcomes shows that LEZs cause a significant decrease in self-reported well-being for individuals residing inside the zones. We show that LEZs introduce an added constraint in the face of externalities, which can reduce self-reported well-being. Effects are more pronounced for owners of diesel cars, which are more restricted than gasoline vehicles, for working-age individuals below the age of 65, and for individuals residing in LEZs with lower quality and accessibility of public transport. We further confirm previous studies looking at the health effects of the zones by showing that they improve the self-reported health outcomes of individuals in our sample. These results suggest that the average well-being costs of restricting mobility outweigh the benefits of improved health outcomes in our setting. Concerning policy spillovers to outside areas, we find similar-sized reductions in life satisfaction for individuals dwelling outside but near an LEZ. In contrast, the health outcomes of persons living outside the restriction area are not affected by LEZs adoption. These results imply that people in the vicinity of LEZs bear the costs of restricted mobility without benefiting from better air quality.

Our findings suggest that the health and environmental benefits of LEZs do not translate to improvements in subjective well-being in the short run. This discrepancy could arise if citizens are more aware of the costs of the policy in the form of restricted mobility than of its benefits arising from better air quality and improved health outcomes. If individuals do not fully internalize the policy's benefits, analyzing perceived well-being will provide an incomplete picture concerning overall welfare effects.

Notably, the life satisfaction effect reverts back to insignificance after several years. This pattern of treatment effect dynamics aligns with an increase in life satisfaction after removing the mobility restriction by purchasing cleaner vehicles. Learning about the policy's benefits may also cause a decrease in bias against the policy. While our data do not allow us to pin down the exact mechanism, the alignment of the dynamics of the receding negative effect on subjective well-being with the policy's increasing effectiveness concerning pollution leads us to believe that learning about the policy's benefits is an important mechanism. In our view, further work on this topic is a fruitful avenue for future research.

The empirical results suggest that policymakers should consider refining the design of LEZs to minimize the impact of harmful spatial spillovers and unintended effects on secondary air pollutants. Possible strategies could be to increase LEZs' coverage area or focus on restricting traffic mainly during the winter months when traffic-related pollution is more elevated and the marginal effectiveness of LEZs on air pollution is highest. This may be combined with stronger restrictions for older vehicle vintages to accelerate turnover towards a cleaner fleet. Implementing additional policies targeting the VOC to NO_x emission ratio of the vehicle fleet could also attenuate the ozone effect. Policymakers should also view the drop in subjective well-being as a policy cost and consider ways to mitigate the burden of the constraint on individual behavior introduced by LEZs. One option would be to improve the quality and accessibility of public transport alternatives. Increasing the salience of the policy's benefits could also play a role in mitigating the negative impact of LEZs on subjective well-being. This could be achieved by more effectively communicating the air quality and health benefits of LEZs.

An important avenue for future research concerns the mechanisms behind the reduction in self-reported life satisfaction. For instance, this decrease could be driven by financial burdens arising from car replacement or the adverse psychological effects of restricting mobility stemming from various factors like attitudes towards environmental policies or general trust in the government. Case studies at the city level that include rich data on mobility behavior and expenses, vehicle ownership, and political attitudes could be one way to deepen our understanding of the negative life satisfaction effect. Another area for future research could be to explore the policy's implication for residential sorting. Evidence on LEZ-induced changes in sorting patterns could contribute to reconciling the negative effects on the life satisfaction of non-movers with the positive hedonic valuations found in the literature.

Declaration of competing interest

The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

Data availability

The research data is available under doi: 10.17632/zgf9xz756j.1.

Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jpubeco.2023.105014.

References

Ai, Z., Mak, C.M., Lee, H., 2016. Roadside air quality and implications for control measures: A case study of Hong Kong. Atmos. Environ. 137, 6–16.
Allcott, H., Braghieri, L., Eichmeyer, S., Gentzkow, M., 2020. The welfare effects of social media. Amer. Econ. Rev. 110 (3), 629–676.
American Lung Association, 2022. Nitrogen dioxide. https://www.lung.org/clean-air/outdoors/what-makes-air-unhealthy/nitrogen-dioxide. (Online; accessed 11 November 2022).
Baghestani, A., Tayarani, M., Allahviranloo, M., Gao, H.O., 2020. Evaluating the traffic and emissions impacts of congestion pricing in New York city. Sustainability 12 (9), 3655.
Barahona, N., Gallego, F.A., Montero, J.-P., 2020. Vintage-specific driving restrictions. Rev. Econom. Stud. 87 (4), 1646–1682.
Bon, D., Ulbrich, I., De Gouw, J., Warneke, C., Kuster, W., Alexander, M., Baker, A., Beyersdorf, A., Blake, D., Fall, R., et al., 2011. Measurements of volatile organic compounds at a suburban ground site (T1) in Mexico city during the MILAGRO 2006 campaign: measurement comparison, emission ratios, and source attribution. Atmos. Chem. Phys. 11 (6), 2399–2421.
Brancher, M., 2021. Increased ozone pollution alongside reduced nitrogen dioxide concentrations during Vienna's first COVID-19 lockdown: Significance for air quality management. Environ. Pollut. 284, 117153.
Callaway, B., Sant'Anna, P.H., 2021. Difference-in-differences with multiple time periods. J. Econometrics 225 (2), 200–230.
Chay, K.Y., Greenstone, M., 2005. Does air quality matter? Evidence from the housing market. J. Polit. Econ. 113 (2), 376–424.
Chen, Y., Jin, G.Z., Kumar, N., Shi, G., 2013. The promise of Beijing: Evaluating the impact of the 2008 Olympic games on air quality. J. Environ. Econ. Manage. 66 (3), 424–443.
Davis, L.W., 2008. The effect of driving restrictions on air quality in Mexico city. J. Polit. Econ. 116 (1), 38–81.
Davis, L.W., 2017. Saturday driving restrictions fail to improve air quality in Mexico city. Sci. Rep. 7, 41652.
De Chaisemartin, C., d'Haultfoeuille, X., 2020. Two-way fixed effects estimators with heterogeneous treatment effects. Amer. Econ. Rev. 110 (9), 2964–2996.
de Foy, B., Brune, W.H., Schauer, J.J., 2020. Changes in ozone photochemical regime in Fresno, California from 1994 to 2018 deduced from changes in the weekend effect. Environ. Pollut. 263, 114380.

DeLang, M.N., Becker, J.S., Chang, K.-L., Serre, M.L., Cooper, O.R., Schultz, M.G., Schröder, S., Lu, X., Zhang, L., Deushi, M., et al., 2021. Mapping yearly fine resolution global surface ozone through the Bayesian maximum entropy data fusion of observations and model output for 1990–2017. Environ. Sci. Technol. 55 (8), 4389–4398.

Dimitriou, K., Paschalidou, A., Kassomenos, P., 2013. Assessing air quality with regards to its effect on human health in the European union through air quality indices. Ecol. Indic. 27, 108–115.

Douenne, T., Fabre, A., 2022. Yellow vests, pessimistic beliefs, and carbon tax aversion. Am. Econ. J. Econ. Policy 14 (1), 81–110.

EEA, 2022. European air quality index. https://airindex.eea.europa.eu/Map/AQI/Viewer/. (Online; accessed 11 November 2022).

Ellison, R.B., Greaves, S.P., Hensher, D.A., 2013. Five years of London's low emission zone: Effects on vehicle fleet composition and air quality. Transp. Res. D Transp. Environ. 23, 25–33.

EPA, 2018. Technical Assistance Document for the Reporting of Daily Air Quality – the Air Quality Index (AQI). Tech Rep. EPA 454/B-18-007, Air Quality Assessment Division; Office of Air Quality Planning and Standards.

EPA, 2022. What are volatile organic compounds (VOCs)?. https://www.epa.gov/indoor-air-quality-iaq/what-are-volatile-organic-compounds-vocs. (Online; accessed 11 November 2022).

EU, 1999. Directive 1999/30/EC of 22 april 1999 relating to limit values for sulphur dioxide, nitrogen dioxide and oxides of nitrogen, particulate matter and lead in ambient air. Off. J. Eur. Communities L163, 41–60.

EU, 2000. Directive 2000/69/EC of the European parliament and of the council of 16 november 2000 relating to limit values for benzene and carbon monoxide in ambient air. Off. J. Eur. Communities L313, 12–21.

EU, 2002. Directive 2002/3/EC of the European parliament and of the council of 12 february 2002 relating to ozone in ambient air. Off. J. Eur. Communities L67, 14–30.

EU, 2008. Directive 2008/50/EC of the European parliament and of the council of 21 may 2008 on ambient air quality and cleaner air for Europe. Off. J. Eur. Union L152, 1–44.

Ewald, J., Sterner, T., Sterner, E., 2022. Understanding the resistance to carbon taxes: Drivers and barriers among the general public and fuel-tax protesters. Resour. Energy Econ. 70, 101331.

Gallego, F., Montero, J.-P., Salas, C., 2013. The effect of transport policies on car use: Evidence from Latin American cities. J. Public Econ. 107, 47–62.

Gehrsitz, M., 2017. The effect of low emission zones on air pollution and infant health. J. Environ. Econ. Manage. 83, 121–144.

Goodman-Bacon, A., 2021. Difference-in-differences with variation in treatment timing. J. Econometrics 225 (2), 254–277.

Graff Zivin, J., Neidell, M., 2013. Environment, health, and human capital. J. Econ. Lit. 51 (3), 689–730.

Grange, S.K., Lee, J.D., Drysdale, W.S., Lewis, A.C., Hueglin, C., Emmenegger, L., Carslaw, D.C., 2021. COVID-19 lockdowns highlight a risk of increasing ozone pollution in European urban areas. Atmos. Chem. Phys. 21 (5), 4169–4185.

Gruhl, H., Volkhausen, N., Pestel, N., aus dem Moore, N., 2022. Air pollution and the housing market: Evidence from Germany's low emission zones. Ruhr Econ. Pap. (977).

Holman, C., Harrison, R., Querol, X., 2015. Review of the efficacy of low emission zones to improve urban air quality in European cities. Atmos. Environ. 111, 161–169.

Hov, Ö., Hesstvedt, E., Isaksen, I.S., 1978. Long-range transport of tropospheric ozone. Nature 273 (5661), 341–344.

Kassenboehmer, S.C., Haisken-DeNew, J.P., 2009. You're fired! The causal negative effect of entry unemployment on life satisfaction. Econ. J. 119 (536), 448–462.

Kroll, J.H., Heald, C.L., Cappa, C.D., Farmer, D.K., Fry, J.L., Murphy, J.G., Steiner, A.L., 2020. The complex chemical effects of COVID-19 shutdowns on air quality. Nature Chem. 12 (9), 777–779.

Kuminoff, N.V., Smith, V.K., Timmins, C., 2013. The new economics of equilibrium sorting and policy evaluation using housing markets. J. Econ. Lit. 51 (4), 1007–1062.

Lemonsu, A., Masson, V., 2002. Simulation of a summer urban breeze over Paris. Bound.-Lay. Meteorol. 104 (3), 463–490.

Levinson, A., 2012. Valuing public goods using happiness data: The case of air quality. J. Public Econ. 96 (9–10), 869–880.

Luechinger, S., 2009. Valuing air quality using the life satisfaction approach. Econ. J. 119 (536), 482–515.

Malina, C., Scheffler, F., 2015. The impact of low emission zones on particulate matter concentration and public health. Transp. Res. A Policy Pract. 77, 372–385.

Margaryan, S., 2021. Low emission zones and population health. J. Health Econ. 76, 102402.

McGuire, J., Kaiser, C., Bach-Mortensen, A.M., 2022. A systematic review and meta-analysis of the impact of cash transfers on subjective well-being and mental health in low-and middle-income countries. Nat. Hum. Behav. 6 (3), 359–370.

Melkonyan, A., Kuttler, W., 2012. Long-term analysis of NO, NO_2 and O_3 concentrations in north Rhine-Westphalia, Germany. Atmos. Environ. 60, 316–326.

Möller, D., 2007. Sachverständigengutachten zum möglichen Einfluss der in Berlin vorgesehenen Umweltzone auf die Luftqualität bezüglich PM_{10} und NO_x – Gutachten von Prof. Dr. Detlev Möller, Ordentlicher Professor an der Brandenburgischen Technischen Universität Cottbus und Leiter des Lehrstuhls Luftchemie und Luftreinhaltung im Auftrag des Allgemeiner Deutscher Automobil-Club e. V. (ADAC). Allgemeiner Deutscher Automobil-Club e. V. (ADAC).

Monks, P.S., Archibald, A., Colette, A., Cooper, O., Coyle, M., Derwent, R., Fowler, D., Granier, C., Law, K.S., Mills, G., et al., 2015. Tropospheric ozone and its precursors from the urban to the global scale from air quality to short-lived climate forcer. Atmos. Chem. Phys. 15 (15), 8889–8973.

Murphy, J.G., Day, D.A., Cleary, P.A., Wooldridge, P.J., Millet, D.B., Goldstein, A.H., Cohen, R.C., 2007. The weekend effect within and downwind of sacramento–Part 1: Observations of ozone, nitrogen oxides, and VOC reactivity. Atmos. Chem. Phys. 7 (20), 5327–5339.

NOAA, 2022. Nitrogen dioxide. https://sos.noaa.gov/catalog/datasets/nitrogen-dioxide/. (Online; accessed 11 November 2022).

Odermatt, R., Stutzer, A., 2015. Smoking bans, cigarette prices and life satisfaction. J. Health Econ. 44, 176–194.

Parrish, D., Ryerson, T., Holloway, J., Trainer, M., Fehsenfeld, F., 1999. New directions. Does pollution increase or decrease tropospheric ozone in winter-spring? Atmos. Environ. 33 (30), 5147–5149.

Pestel, N., Wozny, F., 2021. Health effects of low emission zones: Evidence from German hospitals. J. Environ. Econ. Manage. 109, 102512.

Querol, X., Gangoiti, G., Mantilla, E., Alastuey, A., Minguillón, M.C., Amato, F., Reche, C., Viana, M., Moreno, T., Karanasiou, A., et al., 2017. Phenomenology of high-ozone episodes in NE Spain. Atmos. Chem. Phys. 17 (4), 2817–2838.

Rivera, N., 2021. Air quality warnings and temporary driving bans: Evidence from air pollution, car trips, and mass-transit ridership in Santiago. J. Environ. Econ. Manage. 108, 102454.

Robbins, L.R.B., 1932. The Nature and Significance of Economic Science, Vol. 2. Macmillan London.

Rohlf, A., Holub, F., Koch, N., Ritter, N., 2020. The effect of clean air on pharmaceutical expenditures. Econom. Lett. 192, 109221.

Sarmiento, L., 2022. Air pollution and the productivity of high-skill labor: Evidence from court hearings. Scand. J. Econ. 124 (1), 301–332.

Sarmiento, L., Wägner, N., Zaklan, A., 2021. The air quality and well-being effects of low emission zones. In: DIW Discussion Paper 1947.

Simeonova, E., Currie, J., Nilsson, P., Walker, R., 2019. Congestion pricing, air pollution, and children's health. J. Hum. Resour. 0218–9363R2.

Suarez-Bertoa, R., Astorga, C., 2018. Impact of cold temperature on Euro 6 passenger car emissions. Environ. Pollut. 234, 318–329.

Tiebout, C.M., 1956. A pure theory of local expenditures. J. Polit. Econ. 64 (5), 416–424.

Tokio Metropolitan Government, 2022. Measures against diesel emissions. https://www.kankyo.metro.tokyo.lg.jp/en/automobile/diesel.html. (Online; accessed 07 May 2023).

UBA, 2022. Calculation base air quality index. https://www.umweltbundesamt.de/en/calculation-base-air-quality-index. (Online; accessed 11 November 2022).

Van Praag, B.M., Baarsma, B.E., 2005. Using happiness surveys to value intangibles: The case of airport noise. Econ. J. 115 (500), 224–246.

Viard, V.B., Fu, S., 2015. The effect of Beijing's driving restrictions on pollution and economic activity. J. Public Econ. 125, 98–115.

Wang, H., Huang, C., Tao, W., Gao, Y., Wang, S., Jing, S., Wang, W., Yan, R., Wang, Q., An, J., et al., 2022. Seasonality and reduced nitric oxide titration dominated ozone increase during COVID-19 lockdown in eastern China. Npj Clim. Atmos. Sci. 5 (1), 24.

Wang, L., Xu, J., Qin, P., 2014. Will a driving restriction policy reduce car trips?—The case study of Beijing, China. Transp. Res. A Policy Pract. 67, 279–290.

Wilson, R., Fleming, Z.L., Monks, P., Clain, G., Henne, S., Konovalov, I., Szopa, S., Menut, L., 2012. Have primary emission reduction measures reduced ozone across Europe? An analysis of European rural background ozone trends 1996–2005. Atmos. Chem. Phys. 12 (1), 437–454.

Wolff, H., 2014. Keep your clunker in the suburb: Low-emission zones and adoption of green vehicles. Econ. J. 124 (578), F481–F512.

Yarwood, G., Stoeckenius, T.E., Heiken, J.G., Dunker, A.M., 2003. Modeling weekday/weekend ozone differences in the Los Angeles region for 1997. J. Air & Waste Manage. Assoc. 53 (7), 864–875.

Zhai, M., Wolff, H., 2021. Air pollution and urban road transport: evidence from the world's largest low-emission zone in London. Environ. Econ. Policy Stud. 23, 721–748.

Zhang, Y., Andre, M., Liu, Y., Wu, L., Jing, B., Mao, H., 2018. Evaluation of low emission zone policy on vehicle emission reduction in Beijing, China. In: IOP Conference Series: Earth and Environmental Science, Vol. 121, No. 5. IOP Publishing, 052070.

Zhang, Z., Jiang, J., Lu, B., Meng, X., Herrmann, H., Chen, J., Li, X., 2022. Attributing increases in ozone to accelerated oxidation of volatile organic compounds at reduced nitrogen oxides concentrations. PNAS nexus 1 (5), pgac266.

Zhang, X., Zhang, X., Chen, X., 2017. Happiness in the air: How does a dirty sky affect mental health and subjective well-being? J. Environ. Econ. Manage. 85, 81–94.

Zhong, N., Cao, J., Wang, Y., 2017. Traffic congestion, ambient air pollution, and health: Evidence from driving restrictions in Beijing. J. Assoc. Environ. Resour. Econ. 4 (3), 821–856.

Zhu, Y., Chen, J., Bi, X., Kuhlmann, G., Chan, K.L., Dietrich, F., Brunner, D., Ye, S., Wenig, M., 2020. Spatial and temporal representativeness of point measurements for nitrogen dioxide pollution levels in cities. Atmos. Chem. Phys. 20 (21), 13241–13251.