Air Pollution and the Housing Market: Evidence from Germany's Low Emission Zones*

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Abstract – This paper studies whether people's perception of improvements in local air quality are reflected in the housing market based on comprehensive data on real estate prices from Germany. Using a quasi-experimental research design, we exploit the staggered introduction of Low Emission Zones (LEZs) across German cities, lowering urban air pollution by limiting the access of high-emitting vehicles. We find that residents value the presence of LEZs, reflected by roughly 2% higher apartment rents. Estimates are similar, albeit smaller in magnitude, for properties for purchase. The results are driven by earlier LEZ implementations and LEZs in areas with relatively higher pre-intervention pollution levels.

Keywords – low emission zone, policy evaluation, house prices, externalities

JEL Classification – I18, R21, Q51

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1 Introduction

Urban air pollution has detrimental effects on societies' health and productivity (e.g., Currie and Neidell, 2005; Currie and Walker, 2011; Graff Zivin and Neidell, 2012; Chang *et al.*, 2019; Aguilar-Gomez *et al.*, 2022). Already in the 1950s, policymakers began to counteract rising industrial air pollution levels by passing national legislation aimed at curbing emissions. Yet, cities today still face high levels of urban air pollution, mostly caused by intensifying road traffic (Harrison *et al.*, 2021). Although policymakers avail themselves of various policy measures to curb the negative externalities of road traffic, the introduction of Low Emission Zones (LEZs), a geographic area that is restricted to certain vehicles based on their emission intensity, has become one of the more popular policy tools, especially across Europe (Ku *et al.*, 2020).

Researchers have examined LEZs from various angles, such as their positive efficacy in decreasing air pollution (Wolff, 2014; Gehrsitz, 2017; Morfeld *et al.*, 2014; Malina and Scheffler, 2015; Jiang *et al.*, 2017; Zhai and Wolff, 2021), their beneficial effects on health outcomes (Rohlf *et al.*, 2020; Pestel and Wozny, 2021; Klauber *et al.*, 2021; Margaryan, 2021), their positive impact on demand for public transportation (Aydin and Kürschner Rauck, 2022), but also their adverse effects on well-being due to resulting mobility restrictions (Sarmiento *et al.*, 2021). Importantly, studies analyzing this restriction mechanism found no effect on traffic volumes but merely on the overall vehicle stock composition. This suggests that the exogenous shock arising from LEZs are limited to air pollution levels instead of other metrics such as congestion or noise (Pestel and Wozny, 2021; Wolff, 2014).[1] Yet, it is unclear whether people value these health benefits (positive externalities) from pollution reductions even at relatively low pollution levels such as in Germany. To scrutinize a possible valuation of these positive externalities, it is common practice to examine whether lower air pollution levels are reflected in housing prices (e.g., Nourse, 1967; Chay and Greenstone, 2005; Sullivan, 2016; Liu *et al.*, 2018). In Europe, this relationship has only received very limited coverage in the academic literature. To our knowledge, no study to date has found evidence for air quality improvements' reflection in the pricing of the housing market in Europe.[2]

[1] Pestel and Wozny (2021) analyze data from German traffic monitors and find no effect of LEZs on traffic volumes. Also, Wolff (2014) finds no evidence that banned vehicles from LEZs divert into adjacent areas.

[2] For example, Le Boennec and Salladarre (2017) look at Nantes in France and find no direct evidence of air quality capitalization in house prices.

In this paper, we study how positive externalities arising from improvements in local air quality are reflected on the property market. Specifically, we test whether the introduction of LEZs in Germany, starting from relatively low pollution levels in global comparison, are valued by higher real estate prices. To examine the impact of LEZs on the housing market, we apply a quasi-experimental research design by comparing Germany's real estate prices in areas with LEZs versus comparable areas without LEZs before and after the implementation of the intervention. The staggered implementation across space and time of today's 58 active LEZs in Germany motivates for the application of a stacked difference-in-differences design (DiD). In contrast to the commonly used two-way fixed effects estimator, the stacked estimator accounts for potential biases arising from heterogeneous treatment effects across the staggered implementation waves of LEZs in Germany (Goodman-Bacon, 2021). We exploit the most comprehensive real estate data set on Germany, made available by the research data center (FDZ) Ruhr at the RWI Essen in Germany. This unique data is obtained from Germany's leading online real estate portal, which contains rich information on the geolocation of properties as well as property-related characteristics and the offering price stated by the advertisers.

Our main finding suggests that the introduction of an LEZ led to an average increase of about 2 percent in the net rents of apartments within zones, indicating residents' positive value of cleaner air. We perform several robustness checks, demonstrating that the effect is neither influenced by pre-existing trends nor by spatial spillovers. The rich nature of the data set allows us to investigate across other segments of the housing market as well. We find that the effects for the apartment and house purchasing markets are similar, albeit smaller in magnitude. Focusing on the apartment rental market, we delve into the mechanisms of this effect. We find that the results are mainly driven by pioneering areas that adapted LEZs earlier than others. We also observe that the effect is stronger in areas with higher pollution levels to begin with but find no heterogeneity for properties in closer proximity to main roads.

This paper is not the first empirical study to analyze the effects of air pollution on the housing market. One of the earliest attempts to quantify the effects of pollution on the property market is by Nourse (1967). The author employs a hedonic model and finds that the impact of air pollution on property values in the St. Louis Metropolitan Area is translated into a \$245 decrease for every 0.5 milligrams of sulfur trioxide. A perhaps more rigorous analysis is performed by

Chay and Greenstone (2005). The authors look at the case of the U.S. Clean Air Act, which can arguably be seen as an exogenous event. They compare changes in pollution levels and housing prices in U.S. counties that have been forced to reduce pollutants to meet the federal clean air requirements to the changes that occurred in counties that already met the federal standards. The authors find that in counties that were forced to meet the new environmental standards, home prices increased more than in counties that were already in compliance prior. In a more recent analysis, Sullivan (2016) argues that previous studies underestimate the effects of air pollution by failing to account for the direction of the wind. Wind is a crucial factor since it dramatically changes the effect of nearby pollution. By using an atmospheric dispersion model to account for meteorological changes, the author estimates the effect of air pollution on house prices by exploiting the exogenous variation in emissions caused by the California Electricity Crisis of 2000. Sullivan (2016) estimates a roughly 15 times larger effect than previously quoted in the literature.[3] The topic is less explored in a non-U.S. setting. In another paper, Liu *et al.* (2018) examine the relationship between haze and housing prices in Chengdu, China. Using a spatial error and lag model, they find that haze has a significant negative impact, in the magnitude of 4 percent on both the selling and rental prices of houses, albeit the effect is stronger for rentals. In the only comparable European study, Le Boennec and Salladarre (2017) analyze how air pollution and noise impact the real estate market in Nantes, France. Using a hedonic approach, they do not find any statistical significant effect between air pollution and housing but demonstrate that individuals' prior residential location may affect their current housing choice, related to air pollution and noise.

The contribution of this research is novel in three ways: Firstly, we provide evidence of how air pollution reductions induced by LEZs affect the housing market. To the best of our knowledge, no other paper has analyzed this relationship yet. Secondly, most papers that studied the link between air pollution and property markets solely focus on properties for purchase. In contrast, our paper expands the scope, by examining air quality improvements induced by a policy on both the rental and property purchase market. This extension is particularly relevant for countries with low share of owner-occupied housing such as Germany. We find different effects for rents than for purchasing prices. Thirdly, this paper is the first to provide causal

[3]In another quasi-experimental study, Currie *et al.* (2015) find similar results for specifically toxic air pollution in the U.S. They find that openings of toxic plants decrease housing prices in their near proximity.

evidence outside the U.S. on people's valuation of air pollution reductions, even at relatively low pollution levels.

The paper proceeds as follows. In Section 2 we discuss the research context. Section 3 introduces the used data and lists the main descriptive statistics. In Section 4 we elaborate on the identification strategy, followed by the main results and discussion in Section 5. Section 6 concludes the paper.

2 Institutional Background

Already in the mid-1990s the European Union (EU) established a binding legal framework for improving air quality in all of its member states. The directives 1999/30/EC and 2008/50/EC define measurement mechanisms and set alert thresholds for various air pollutants. Violations of air quality standards require member states to adopt action plans to reduce air pollution. In case of non-compliance, the EU may initiate an infringement procedure. Despite this obligation, air pollution remains a major concern, as more than 130 cities across Europe persistently exceeded permissible air pollutant levels (European Commission, 2017). In Germany, the 16 federal states are responsible for compliance of the EU air quality standards. In case of violations of EU standards, each respective federal state government is required to develop a city-specific Clean Air Plan. While also other stakeholder such as city administrations, local businesses, or environmental organizations are involved in the discussion, the final measure is ultimately enacted by the federal state. Strictly speaking, a city-specific Clean Air Plan is *exogenously* imposed either by the federal state governments or court rulings (Pestel and Wozny, 2021).

Among the various tools to curtail traffic emissions in urban areas, implementing a Low Emission Zone (LEZ) is presumably one of the most concrete policy measures. An LEZ is a signposted area where access by certain high-emitting vehicle types is prohibited. Access to the LEZ is regulated based on the EU's vehicle emission standards. Vehicles' emission intensities are categorized by color-coded windshield stickers: no stickers for the highest emission level Euro 1, while red, yellow, and green stickers are for the 'cleaner' vehicles with emission levels for Euro 2, 3, and 4, respectively (see Figure A1). The introduction of an LEZ usually takes place in phases, initially only banning Euro 1 vehicles, followed by a ban of Euro 2 and 3 cars, and finally only allowing Euro 4 vehicles exclusively. The first LEZs in Germany were introduced in 2008, initially only banning Euro 1 vehicles. Over the next years, LEZ gradually increased and intensified across the country, mostly banning Euro 1 and 2 vehicles and prohibiting all vehicles below Euro 4 (green sticker) from 2013 onwards. Figure 1 depicts this development. Since 2018, there are 58 LEZ introductions in Germany, mostly in urban areas of western and southwestern Germany, all except for one active LEZ allowing access only to Euro 4 vehicles with a green sticker (see Table A1). In the same period, the share of older active Euro 1 to Euro 3 vehicles declined in Germany from more than 60% in 2008 to around 10% in 2021, implying a lower stringency of LEZs in the later periods (Kraftfahr-Bundesamt, 2022).

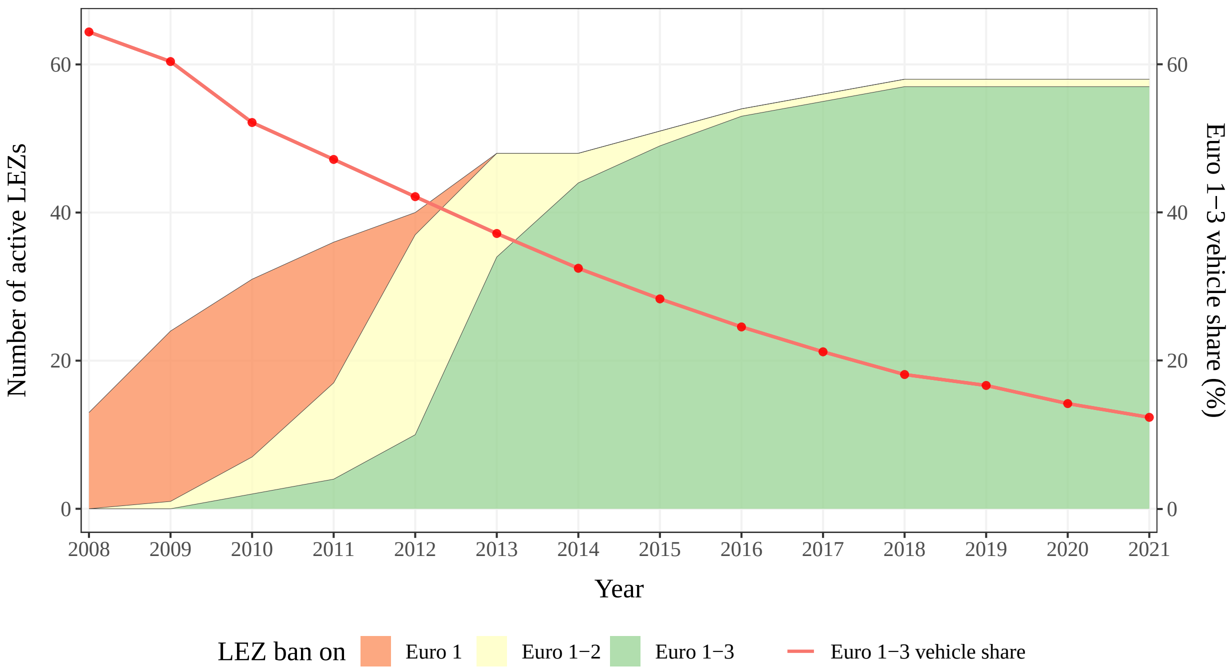


Figure 1: LEZs by emission standard in Germany over time

3 Data

Our data set consists of two main pillars: First, we use the *RWI-GEO-RED* real estate data set, made available by the research data center (FDZ) Ruhr at the RWI (Schaffner, 2020). The unique data set on German real estate prices is obtained from Germany's leading online real estate portal *ImmobilienScout24*. For a fee, users can advertise their properties by filling out a detailed questionnaire on property-related characteristics. The advertised price on the platform needs to be interpreted as a non-binding *offering price*. Yet, the price information is available for almost all advertisements in the data set. Advertisers usually also include many other property-specific characteristics to increase their chances of selling such as the property type, size, location, and building characteristics. The data is available on a monthly basis. Our version ranges from January 2007 until June 2021. Overall, the *RWI-GEO-RED* data set consists of four separate sub-data sets: houses for sale, houses for rent, apartments for sale, and apartments for rent. The main focus of this paper relies on the 'apartments for rent' sub-data set only since, with almost 50%, Germany has the highest rental share in the EU (Charlton, 2021). However, we also examine our main results using the other *RWI-GEO-RED* sub-data sets. For the second pillar of the working data set, we used extensive geospatial vector data on LEZs in Germany, which we obtained from the publicly available geographic database *OpenStreetMap*. The data set contains the exact location and boundary of each LEZ in Germany. We further enriched the data with the exact introduction dates of each LEZ to obtain a longitudinal data set from their first introduction until the end of 2021. To combine the data, we merged the two data sets based on the geolocation of the properties and the LEZs. We then determined whether a property is located outside or inside an active LEZ and computed the distance of each property to the nearest LEZ border. The main descriptive statistics of the final data set are reported in Table 1.[4,5] Overall, the main variable of interest, the net rent of an apartment, is on average higher for areas within an LEZ region. This is perhaps not too surprising since LEZs are predominantly located within urban areas, which are per se more attractive to renters. Otherwise, apartment characteristics for properties outside and inside an LEZ are relatively similar and differences in variable means could also represent the same fact that LEZ areas tend to be urbanized. Apartments inside an area that introduced an LEZ have on average a smaller living space and number of rooms and are less likely to have a balcony. At the same time, they are more likely to have an elevator and located on a higher floor of an older building.

[4] The specific selection of the control and treatment group is defined in Section 4.

[5] We disregard extreme outlier observations and units with missing information on rental prices and repeated entries. More specifically, we exclude the top and bottom 1 percent on the net rent and living space distribution in each year and then the top and bottom 1 percent of the price per square meter distribution.

Table 1: Descriptive Statistics

	Properties outside LEZs			Properties inside LEZs		
	mean (sd)	min max	n	mean (sd)	min max	n
log of rent (in €)	6.13 (0.46)	4.95 7.75	4,940,916	6.18 (0.50)	4.95 7.75	4,891,190
Distance to LEZ border (in m)	39,379 (47,095)	0.48 201,489	4,940,916	2,620 (3,448)	0.01 3,273	4,891,190
log of living space (in m^2)	4.17 (0.35)	2.85 5.19	4,940,916	4.17 (0.36)	2.84 5.19	4,891,190
Number of rooms	2.49 (0.90)	1 10	4,940,255	2.42 (0.89)	1 10	4,890,353
Elevator present	0.19 (0.40)	0 1	4,940,916	0.21 (0.41)	0 1	4,891,190
Floor of object	1.75 (1.88)	0 14	4,940,916	1.84 (1.68)	0 14	4,891,190
Balcony present	0.60 (0.49)	0 1	4,940,916	0.53 (0.50)	0 1	4,891,190
Construction year	1966 (34.64)	1500 2020	3,149,946	1956 (38.91)	1500 2020	2,814,797

4 Identification Strategy

The primary goal of this paper is to estimate the impact of LEZs on property values. To do so, we propose two separate identification frameworks: Firstly, we introduce a *difference-in-differences* (DiD) estimation strategy to estimate the average treatment effect (ATE) of LEZs on property rental prices. For this, we specify a standard DiD model with the objective to select comparable treatment and control groups which would have developed on a parallel trend in absence of treatment. Secondly, we adapt this DiD model in a stacked regression format to address the potential bias emerging from a staggered policy introduction with heterogenous treatment effects in treatment times.

4.1 Standard Difference-in-Differences Design

The introduction of LEZs across certain cities in Germany at different times justifies the use of a DiD design with staggered treatment adoption. Let $p_{i,t,g}$ represent the net rental price excluding utilities and other bills, on a logarithmic scale of property i at time t in grid[6] g.

[6]The grid is of 1-square-kilometer raster cells covering all of Germany, created by the RWI's FDZ. Grid cells are each matched to municipalities and districts as of the end of 2015 (Schaffner, 2020).

$LEZ_{i,g,t}$ is a binary variable for properties i at a time t in grid g, which equals one if a property is located in a grid lying at least partly within the boundaries of an active LEZ and zero otherwise. In our contextual setting, we follow Pestel and Wozny (2021) and restrict the comparison group to cities with a population of at least 100,000 inhabitants.[7] As rents evolve differently between rural and urban areas (Glaeser *et al.*, 2001) and LEZs have been mainly introduced in larger urban areas (see Table A1 in the Appendix), rural areas and smaller cities would not constitute a plausible comparison group. Furthermore, the control group includes nevertreated properties in municipalities which introduced an LEZ. Figure A2 in the Appendix displays the regions falling under this criterion, the 58 active LEZ areas, and all other regions. Overall, 111 municipalities are included in the comparison group. Therefore, we compare all apartments located in an active LEZ area with apartments in adequately sized comparable cities that never introduced an LEZ and cities which did not yet introduce a Low Emission Zone.

The vector $\mathbf{C}_{it}$ controls for various property-related attributes. These are the living space in square meters, the number of rooms, the year of construction, the floor of the apartment, and whether the apartment has an elevator and balcony. The classical DiD model can hence be formulated as:

$$log\, p_{i,t,g} = \alpha\, LEZ_{i,g,t} + \beta\, \mathbf{C}_{i,t} + \lambda_g + \phi_{c,t} + \varepsilon_{i,t,g}, \tag{1}$$

where the coefficient of interest α represents the estimation of the *average treatment effect* (ATE), λ_g controls for any time-invariant local characteristics by using grid fixed effects, $\phi_{c,t}$ are county by time fixed effects accounting for different trends across different German regions, and $\varepsilon_{i,t,g}$ is an identically and independently distributed error term, clustered at the county level.

4.2 Stacked Difference-in-Differences Design

The implementation of LEZs leads to variation across regional units and time, which is why it was common practice to apply a two-way fixed effects DiD (TWFE-DiD) estimation, to control for units and time. However, numerous scholars have recently shown that the coefficient arising from such TWFE-DiD estimation is in fact a weighted average of multiple different treatment

[7] We identify the relevant cities with population data on the municipality ("Gemeinde") level from the Federal Statistical Office (Destatis).

effects if the implementation is staggered. In particular, Goodman-Bacon (2021) demonstrates that the TWFE-DiD estimator is a combination of early, late, and never treated units, in which units treated in the middle of the study period have larger weights than the ones at the beginning or end. Especially problematic are comparisons of later treated with earlier treated units which can bias the coefficients if effects are heterogeneous across treatment times. In our research context, early implemented and more restrictive LEZs could have had a stronger effect on the housing market than later implemented ones since the amount of older, high-emitting vehicles declined over time. Hence, using TWFE-DiD estimator may be problematic since it can assign negative weights, results in biased coefficients, and may even reverse the sign of the overall effect (Borusyak *et al.*, 2022; de Chaisemartin and D'Haultfœille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Imai and Kim, 2021; Sun and Abraham, 2021).

To address these concerns, we implement equation (1) in a stacked regression design, which aligns every treatment event by occurrence instead of calendar date. Essentially, all treatment events are stacked together to occur at the same time instead of in a shifted format, preventing an uneven weighting of the events due to their innate timing (Cengiz *et al.*, 2019; Deshpande and Li, 2019; Klauber *et al.*, 2021). Following Klauber *et al.* (2021), we create distinct data sets for each LEZ implementation wave in which at least one LEZ was implemented. In our study period between 2007 and 2021, there are 27 separate LEZ implementation waves j. Grids which introduced an LEZ as part of an implementation wave are considered as the treatment group while others that did not (yet) introduce an LEZ qualify for the comparison group. By using a stacked DiD model, we are able to refine the selection of comparison property units per wave. Specifically, we only include property advertisements in grids that are within a time window between 12 months before and 24 months after the implementation wave. Besides properties in 'nevertreated' grids, we only include properties in treated grids in the control group, which are not treated within the post-treatment period of the respective treatment wave. Thereby, we ensure a clean comparison group in each implementation wave and avoid that the comparison group is on a diverging trend from the treatment group.

The standard DiD model is now specified in its stacked form as

$$log\, p_{i,t,g,j} = \alpha\,(LEZ_{i,g,j} \times Post_{t,j}) + \beta\,\mathbf{C}_{i,t} + \zeta_{t,j} + \eta_{g,j} + \lambda_g + \phi_{c,t} + \varepsilon_{i,t,g,j}, \qquad (2)$$

where the dependent variable p is now specified for properties i in grid g and year t per treatment wave j. The binary variable $LEZ_{i,g,j}$ now equals one if grid g is covered by an LEZ in implementation wave j, and zero otherwise. The binary variable $Post_{t,j}$ is equal to one if year t is after the treatment implementation of wave j. The indicator $\zeta_{t,j}$ identifies fixed effects for each combination of event time (months) and implementation wave j. Every wave j also has its own indicator $\eta_{g,j}$ equalling 1 if grid g is covered by an LEZ in that particular implementation wave. With the stacked DiD design we are able to eliminate unobservables that may affect both treatment selection and outcome. First, $\zeta_{t,j}$ controls for unobserved treatment wave specific trends that develop in the years prior to the implementation of LEZs such as underlying socioeconomic traits affecting the attractiveness of a property and implementation decision of an LEZ. Simple calendar time effects would not effectively capture such pre-trends. Second, by introducing $\eta_{g,j}$, we capture time-invariant differences between treatment and comparison groups for each and between different LEZ implementation waves j. Essentially, we control for unobservables that affect outcomes and selection into LEZ adoption as well as early or late adopters. Similar to equation 1, $\mathbf{C}_{i,t}$ controls for observed property characteristics, λ_g represents the employed grid level fixed effects and $\phi_{c,t}$ captures county by time fixed effects.

5 Results

5.1 LEZs and the apartment rental market

First, we focus on the main results of this paper, that is the impact of LEZs on the apartment rental market in Germany. Table 2 compares the standard and stacked DiD model specification, estimated with the regression design of equation (1) and equation (2). Due to the staggered nature of LEZ implementations, the stacked DiD design is preferable since it addresses the valid concerns of negative weighting and potentially biased coefficients. Yet, it is helpful to compare these estimates against the classical standard DiD model. All regression specifications include 'grid' as well as 'county by time' level fixed effects. Standard errors clustered at the county level are reported in parentheses. Across both specifications, we find a positive effect of the presence of LEZs on apartment rents. In other words, the pollution reduction from LEZs translates into higher offering prices of apartments. In our most stringent specification, column (8) of Table 2, where we control for the most extensive list of property characteristics, we find that, on average, the presence of a LEZ yields a 2.1 percent higher apartment rent than in areas without LEZs.

This estimate is statistically significant at the 1 percent level. While the estimates of the other specifications are qualitatively in line with this estimate, the results of *Panel A* are far more imprecise.[8] Appendix A.4 shows that these effects differ according to the size of apartments, finding that the effect increases in magnitude for larger apartments. The effects range from 1.17 percent for the first quartile of apartment size to 2.77 percent for the fourth quartile. The effect for the first quartile of apartment size is only statistically significant at the 10 percent level and statistically significantly different from the estimate for the fourth quartile.

Table 2: Estimation Results: Apartments for rent

	Dependent variable: log (rent)			
	Panel A: Standard DiD			
	(1)	(2)	(3)	(4)
LEZ	0.0391*	0.0378*	0.0372*	0.0461**
	(1.82)	(1.73)	(1.73)	(2.50)
log(size)	no	yes	yes	yes
Balcony, floor, rooms, elevator	no	no	yes	yes
Year of construction	no	no	no	yes
Grid FE	yes	yes	yes	yes
County×time FE	yes	yes	yes	yes
Number of observations	9,831,351	9,831,351	9,829,853	5,963,279
	Panel B: Stacked DiD			
	(5)	(6)	(7)	(8)
LEZ	0.0196***	0.0201***	0.0198***	0.0210***
	(3.77)	(3.04)	(3.02)	(3.48)
log(size)	no	yes	yes	yes
Balcony, floor, rooms, elevator	no	no	yes	yes
Year of construction	no	no	no	yes
Grid FE	yes	yes	yes	yes
County×time FE	yes	yes	yes	yes
Event time×treatment wave FE	yes	yes	yes	yes
Treated unit×treatment wave FE	yes	yes	yes	yes
Number of observations	33,327,160	33,327,160	33,322,114	19,539,258

Standard errors clustered at county level. t-statistics in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

[8] In Appendix A.2, we additionally investigate whether the introduction of LEZs leads to responses in the time that apartments advertisements are online which might reflect other changes in the market, e.g. by changes in the supply of apartments. We do not find an effect on the timing of the publication of an advertisement, which supports the hypothesis that the result is driven by the change in its amenity value.

Parallel trends assumption

The key assumption needed to estimate the true effect of a the policy with a DiD approach is the common trend assumption. That is, in absence of treatment, the outcome would have followed the same trend in the comparison group and the treatment group. The assumption cannot be conclusively tested, but to provide some evidence that the assumption is more likely to hold it is common practice to perform an *event study*. This approach tests effects of the treatment in period t in each period s. Importantly for our case, it allows us to see whether we detect statistically significant effects on property prices in the treatment group compared to the comparison group before an LEZ became effective. An absence of these effects provides some evidence that the trends of treatment and control group are not diverging statistically dependent on the future treatment in one of them. This implies that the common trends assumption is more likely to hold. Methodologically, the concept can be expressed in its stacked form by

$$log\, p_{i,t,g,j} = \sum_{s=\underline{t}}^{t-1} \alpha_s (LEZ_{i,g,j} \times Post_{s,j}) + \sum_{s=t}^{\bar{t}} \alpha_s (LEZ_{i,g,j} \times Post_{s,j}) \\ + \beta\, \mathbf{C}_{i,t} + \zeta_{t,j} + \eta_{g,j} + \lambda_g + \phi_{c,t} + \varepsilon_{i,t,g,j}, \quad (3)$$

where $\underline{t}$ is the first event period when to expect anticipation effects, while $\bar{t}$ represents the last period for which to expect adjustment effects. The first sums term represents the anticipatory effects of an LEZ introduction while the second sums term captures the reactive effects after an LEZ became effective.[9] For the periods in the event studies, we group the event times at the year quarter level to reduce the influence of noise compared to using the month level (see Klauber *et al.* (2021)).

Figure 2 plots an event study of the stacked DiD specification per equation (3) of column (4) of Table 2. The post-treatment patterns suggest that LEZs induced an enduring effect on apartment rents that does not decline in two years after the treatment. Furthermore, coefficients prior to treatment are statistically insignificant at conventional levels, which is in line with the common trends assumption of LEZ and non-LEZ regional units.

[9] We set α_{-1} equal to zero, so that the year quarter before the treatment introduction is the reference period.

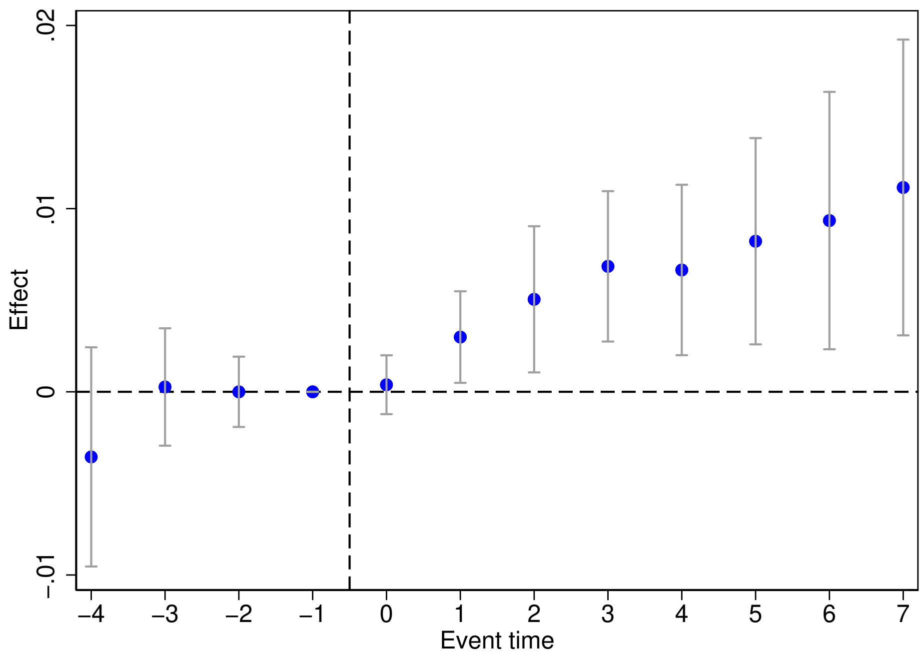


Figure 2: Event Study Results: Stacked DiD

Stable Unit Treatment Value Assumption (SUTVA)

In our setting, it is plausible that the effect of the policy spills over into the proximity of the treated area. Air quality improvements likely spread across space. Driving restrictions can deter people from owning or driving a restricted car close to a LEZ if the zone deters them from entering the more central part of a city. If the treatment effect indeed spills over into the control group, the SUTVA assumption is violated and our estimates will be downward biased. We investigate to what extent the measured treatment effect of equation (2) is influenced by possible spatial spillovers. To do so, first we employ a spatial regression discontinuity design (RDD), allowing us to estimate the local average treatment effect (LATE), by exploiting a possible sharp discontinuity that may arise from the introduction of the LEZs at their borders. By employing a spatial RDD, we can determine the LEZ's impact on properties close to a zone's border. If spillovers play a substantial role, we will identify a smaller or no significant effect comparing properties located close around LEZ's borders.

Following the methodological approach by Koster *et al.* (2021), we use a spatial RDD, where the running variable is the distance to the nearest border of an existing or future LEZ. The effect of the LEZ on properties is captured by a discrete jump in price values after its introduction.

Let $p_{i,l,g,z,t}$ be the rent of property i near a border of an LEZ area l in month t and $LEZ_{i,t}$ be a dummy indicating whether an LEZ has been implemented that covers the property. The variable $d_{i,l}$ denotes the distance to the border, where $d_{i,l} > 0$. The vector $C_{i,t}$ controls again for observed property characteristics. However, it may be problematic if differences in unobservables of properties between LEZ areas and neighboring areas are correlated with the implementation of an LEZ. For instance, differences in the attractiveness of certain locations may be present, which are correlated to $LEZ_{i,t}$ and influence $p_{i,l,g,z,t}$ simultaneously. Thus, we employ grid fixed effects λ_g, which control for time-invariant differences between locations, $\xi_{z,t}$, which capture zip code by time fixed effects capturing trends in unobservables which are different at different border areas (e.g. provision of public goods) and LEZ area by time fixed effects $\nu_{l,t}$ controlling for different trends across LEZs. We estimate:

$$\begin{aligned} log\, p_{i,l,g,z,t} = \alpha\, LEZ_{i,t} + (\psi_1 + \psi_2\, t) LEZ_{i,t} d_{i,l} + (\psi_3 + \psi_4 t)(1 - LEZ_{i,t}) d_{i,l} + \beta\, C_{i,t} \\ + \lambda_g + \nu_{l,t} + \xi_{z,t} + \varepsilon_{i,l,g,z,t}\ , \text{ if } d_{i,l} < b \quad (4) \end{aligned}$$

where α is the parameter of interest. ψ_1 and ψ_3 capture the possibility that distance trends in listings may be different on both sides of the border before and after the treatment. ψ_2 and ψ_4 aim to capture differences in those trends over time by including a linear interaction with time. Parameter b denotes the distance band, i.e. the cutoff point until which we include observations in the analysis. In other words, this equation expresses a comparison in price changes along the borders of LEZ areas to see whether changes over time in prices have changed in the treated areas because of the presence of an LEZ.

We estimate the effect formally according to equation (4) and present for different bandwidths ranging from 0.5 to 3 kilometers in Table 3. Across specifications we find no effects that are statistically significantly different from zero, suggesting that properties just outside an LEZ are affected by treatment similar to properties just inside the zone, e.g. by benefits from air quality improvements. This finding is in line with Sarmiento *et al.* (2021) who find significant air quality spillovers in close proximity to LEZs. The coefficients tend towards more precise null estimates, when we add precision by extending the sample size using larger bandwidths.

Table 3: Estimation Results: Spatial RDD

	Dependent variable: log (rent)					
	0.5 km	1.0 km	1.5 km	2.0 km	2.5 km	3.0 km
	(1)	(2)	(3)	(4)	(5)	(6)
LEZ implemented	0.0060	-0.0024	-0.0007	-0.0015	0.0005	0.0001
	(0.52)	(-0.26)	(-0.08)	(-0.17)	(0.06)	(0.01)
Property controls	yes	yes	yes	yes	yes	yes
Spatio-temporal trend variables	yes	yes	yes	yes	yes	yes
Grid FE	yes	yes	yes	yes	yes	yes
LEZ-area×month FE	yes	yes	yes	yes	yes	yes
Zip code×month FE	yes	yes	yes	yes	yes	yes
Number of observations	649,091	1,383,114	1,925,037	2,387,490	2,778,454	3,115,901

Standard errors clustered at county level. t-statistics in parentheses. Property controls include the living space in square meters, the number of rooms, the year of construction, the floor of the apartment, and whether the apartment has an elevator and balcony. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Therefore, we find evidence for positive spatial spillovers of the policy into areas in close proximity of the treated area. To investigate how much spillovers affect our estimates and to what extent we might underestimate the true treatment effect, we drop apartments from the control group that are located in a certain proximity of a LEZ. Figure 3 illustrates the procedure. We drop a buffer around each LEZ to reduce the influence of observations that are affected by possible spillovers and estimate our main specification on the new sample.

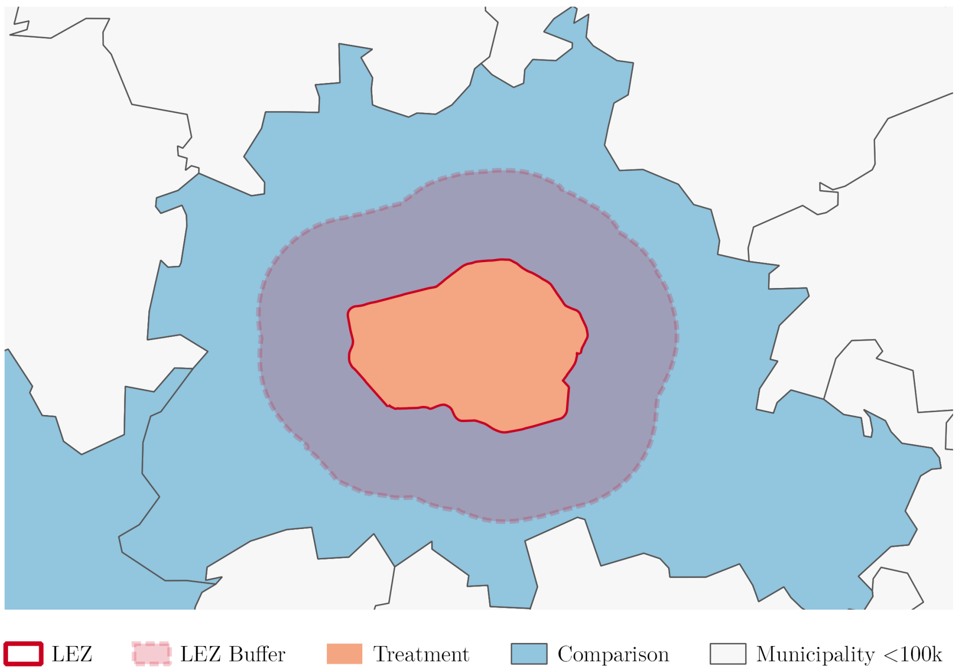


Figure 3: Treatment and comparison group without close control areas

Table 4 presents the results when dropping observations in a 5km, 10km and 15km buffer. Our results stay qualitatively the same. The coefficients from the specifications which reduce the influence of spillovers tend to be stable. The point estimates with a 5km and 10 km buffer are larger than the main estimate which is expected if the influence of positive spillovers into the control group is reduced. However, with increasing buffer size, the results become less precise since the number of observations decreases.

Table 4: Estimation Results: Without close control areas

	Dependent variable: log (rent)			
	Main results	5km buffer	10km buffer	15km buffer
	(1)	(2)	(3)	(4)
LEZ	0.0198***	0.0264***	0.0238**	0.0182*
	(3.02)	(2.81)	(2.00)	(1.78)
Property controls	yes	yes	yes	yes
Event time×treatment wave FE	yes	yes	yes	yes
Treated unit×treatment wave FE	yes	yes	yes	yes
Grid FE	yes	yes	yes	yes
County×time FE	yes	yes	yes	yes
Number of observations	33,322,114	21,542,713	17,385,334	16,361,561

Standard errors clustered at county level. t-statistics in parentheses. Property controls include the living space in square meters, the number of rooms, the year of construction, the floor of the apartment, and whether the apartment has an elevator and balcony. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

LEZs and other property markets

Although Germany's property market is dominated by rentals, most prominently rental apartments, we also explore whether we find an effect of Low Emission Zones for house rental and the purchasing (apartments and houses) market. This allows us to test whether the amenity channel holds for different samples. Furthermore, we can investigate whether estimates differ across the different parts of the housing market. First, we examine how our apartment rental estimates compare to apartment purchasing estimates. Using the stacked DiD design as our preferred model specification, column 1 and 2 of Table 5, present the effect of the LEZ introduction on apartment prices. Under the most restrictive specification, controlling for apartment size and other property characteristics, as well as employing various fixed effects, we find that, on average, the presence of LEZs yield a 1.2 percent higher apartment value than areas without LEZs. Broadly, these estimates are by about 1 percentage point lower than the apartment rental market, depending on the exact specification. A similar picture can be observed in the house purchasing market. Columns 3 and 4 of Table 5 display the LEZ introduction effect on house purchasing prices. For sake of completeness, we also examine the house rental market. Columns 5 and 6 of Table 5 below present the effect of LEZ introduction on house rents. Here the estimates become statistically not distinguishable from zero, with even smaller point estimates. We might not be able to detect an effect since this sample is the smallest of the four sub-markets with an especially small share of properties that lie inside LEZ areas. In the sample – before changing the data into the stacked data set – only 21.31 percent of houses (45,543 properties) are located inside LEZ areas compared to 49.75 percent (4,891,190 properties) of rental apartments in our main analysis.

Table 5: Estimation Results: Alternative samples

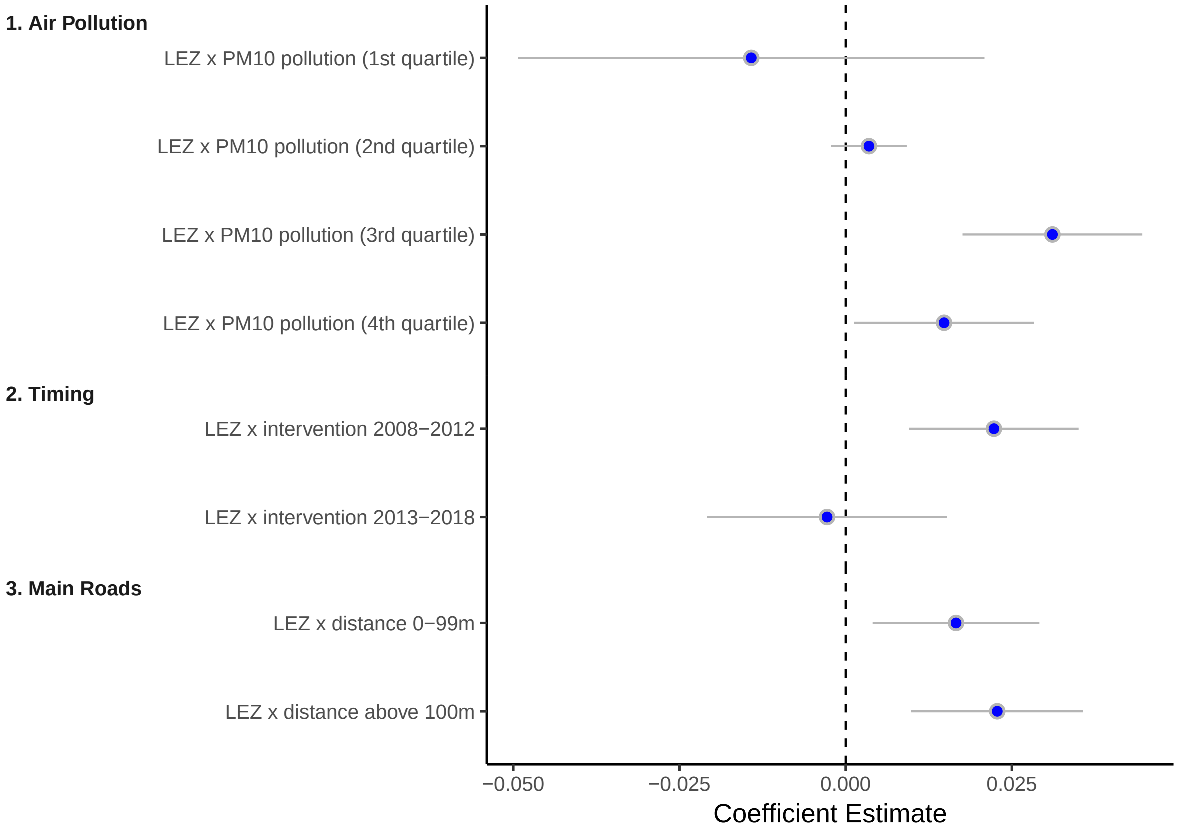
	Dependent variable: log (price) / log (rent)					
	Panel A: Apartment prices		*Panel B:* House prices		*Panel C:* House rents	
	(1)	(2)	(3)	(4)	(5)	(6)
LEZ	0.0116**	0.0120***	0.0115***	0.0082***	0.0048	0.0033
	(1.97)	(2.81)	(5.48)	(4.96)	(1.38)	(1.59)
Property controls	no	yes	no	yes	no	yes
Grid FE	yes	yes	yes	yes	yes	yes
County×time FE	yes	yes	yes	yes	yes	yes
Stacked FEs	yes	yes	yes	yes	yes	yes
Number of observations	3,681,437	2,953,746	19,838,644	14,198,927	3,176,751	2,296,125

Standard errors clustered at county level. t-statistics in parentheses. Stacked fixed effects include a fully set of treatment time by treatment wave fixed effects and a set of treatment group times treatment wave fixed effects. Property controls include the natural logarithm of living space, the number of rooms and the construction year for houses. For apartment prices we additionally control for the presence of a balcony and elevator and the floor of the apartment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

5.2 Potential Mechanisms

Air Pollution

We find that the introduction of LEZs has a positive impact on prices in the housing market. However, we cannot infer whether this is driven by the amount of actual pollution reductions or by the mere announcement of an LEZ introduction (expectation effect). To better understand which of these mechanisms drives the bottom line result, we exploit publicly available air pollution data from the German Federal Environment Agency. The data set contains, among others, annualized measurements of particulate matter (PM10) levels at 375 measuring stations across Germany (Umweltbundesamt, 2022). We investigate whether the treatment effects differ across the average pollution level of LEZ areas before the treatment took place. Hence, we compute the average PM10 levels in the treated area of each treatment wave in the last untreated calendar year. We then analyze whether the effects of the LEZ-introduction differ across the distribution across treatment waves of these wave-specific pollution levels.[10] Figure 4 presents the results of this analysis (see Table A5 for details and various model specifications). For the first and second quartile of pre-intervention PM10 pollution levels, we find no statistically significant treatment effects of LEZs on apartments' rental prices. However, for the third and fourth quartile, we find a positive average effect of 3.11 and 1.48 percent, statistically significant at the 1 and 5 percent level respectively. These results indicate that prices change more when pollution levels were relatively higher before the market intervention. This suggests that not only the announcement of an LEZ, but also the relationship of the policy with air pollution itself plays a critical role.



Notes: The dots depict the point estimates with the respective 95 percent confidence intervals around them. Specifications use grid fixed effects, county times month-year fixed effects, treated unit times treatment wave fixed effects, event time times treatment wave fixed effects and control for living space in square meters, the number of rooms, the floor of an apartment, presence of an elevator, and the presence of a balcony. Standard errors are clustered at the county level.

Figure 4: Overview of Results: Mechanism analysis

[10] Not for all treatment waves a pollution monitor was inside the treated grids in the year prior to treatment. Therefore, we only have the average wave-specific pollution level for 21 out of the 27 treatment waves. Only these waves will be analysed in this analysis.

Timing

In a similar spirit, we investigate whether the effects are driven by a temporal component, that is whether there is a difference in the effect between earlier and later LEZ introductions. Treatment time is positively correlated with pre-treatment pollution levels, but likely also with a higher stringency of LEZs since earlier introductions also correlate with a higher share of cars that are not allowed to drive in the most restrictive LEZs (see Figure 1).[11] Therefore, earlier LEZ introductions might have also led to larger pollution reductions. We split the treatment waves into earlier and later introductions. We chose January 2013 as the cutoff since it lies in the middle between the earliest introduction in January 2008 and the last in January 2018. Figure 4 presents the results (see Table A6 for details and various model specifications). We find that the earlier LEZ introductions had on average a positive effect of 2.23 percent (statistically significant at the 1 percent level). Yet, we find no statistically significant effect of later LEZ introductions. Both, the heterogeneity in pre-treatment pollution levels and treatment timing, suggests that the presence of treatment effects is related to actual air pollution levels. However, our data does not allow us to conclusively distinguish between three possible explanations which might also be interrelated. First, people are more aware of air pollution at higher levels and therefore value air pollution reductions more at these critical levels. Second, the awareness of early introduced LEZs was higher since a higher share of people were restricted from entering the zones with their cars. Third, LEZs which were introduced in areas that had relatively high pollution levels also had larger pollution reductions in absolute terms. Ideally, we would have the exact pollution reduction for each treatment wave to investigate further whether the third reason predominates. Future work using this information could answer this question more comprehensively.

Proximity to main roads

Lastly, we also check whether apartment rents located at main roads are stronger affected by the potential air quality improvements resulting from the introduction of LEZs as opposed to apartments located further away. We want to investigate whether there might also be within city and treatment wave differences in the effect sizes potentially due to variation in pollution exposure within areas that is proxied by main roads. To do so, we rely on a publicly available

[11] Note that the first LEZs did not have the highest level of stringency yet. Yet, they were still banning the most polluting and especially widespread diesel cars from entering the zones (Klauber *et al.*, 2021).

landscape model that contains topographical objects, including every main road within Germany (Bundesamt für Kartographie und Geodäsie, 2021). We then compute the distance of each property to the closest main road and split the sample according to properties close or far away from a main road. We choose a cutoff at 100m since it is close to the median distance (52.08 percent of apartments have a smaller distance than 100m distance and 47.92 percent a larger distance). Figure 4 presents the results of this analysis (see Table A7 for details and various model specifications). The results are not statistically significantly different from one another. We find an effect of 1.66 percent for apartments relatively closer to main roads that is statistically significant at the 5 percent level and an effect of 2.28 percent for apartments further away from main streets that is statistically significant at the 1 percent level.

6 Conclusion

Low Emissions Zones have become a prevailing policy measure to combat rising levels of urban air pollution in Europe, specifically in Germany. This paper studies the effect of LEZs on the housing market by providing evidence that urban air pollution reduction policies translate into higher offering prices for rental apartments in the German context.

We exploit Germany's most comprehensive housing market data set spatially matched with its active LEZs. Besides a classical difference in differences method, we employ a stacked difference in differences design to account for bias arising from heterogeneous effects across LEZ implementation times. We find positive average effects of LEZs on apartment rents. The effect is also found for other parts of the housing market (house and apartment purchases) but is smaller in magnitude. In conclusion, we find evidence that people value LEZs and the associated reductions in urban air pollution levels. However, we also argue that these average effects are primarily driven by LEZ introductions at relatively earlier times with many high emitting vehicles present and in areas with relatively higher pre-intervention pollution levels.

The analysis presented in this paper has two main limitations which should be addressed by future research. First, while we are able to access very rich geo-referenced data on offering prices for properties for rent or sale, we are not able to observe the actually realized prices in equilibrium. Second, while we provide evidence that our findings are mainly driven by earlier implementation waves characterized by higher pre-treatment pollution levels, the exact mechanisms still remain unclear. In particular, we cannot distinguish to what extent higher real estate prices materialize either because people actually recognize local air quality improvements or the LEZ implementations themselves are perceived as a more aggregate signal of better air quality.

The results of this study may be informative for ongoing policy debates about further and more stringent driving restrictions aiming for additional improvements in local air quality in inner-city areas. Many cities around the globe will be banning internal combustion engines entirely over the next years, which will substantially reduce emission of air pollutants from traffic to zero, if fully implemented. Whether our findings of a positive impact on housing prices extend to these settings remains unclear since substantial air quality improvements would also be accompanied by severe driving restrictions for today's still predominant fossil-fueled vehicle population.

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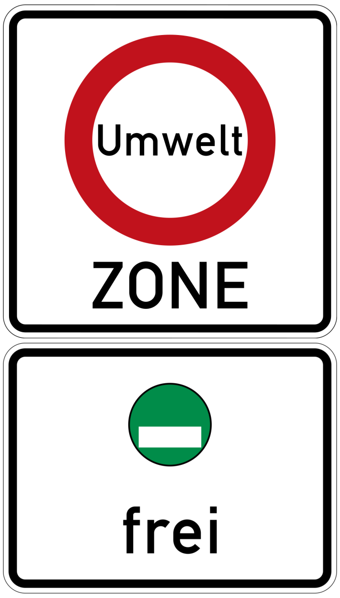
Appendix

A.1 LEZs in Germany

(a) Euro 2-4 windshield emission stickers



(b) LEZ signpost Euro 4 vehicles only

Figure A1: LEZ vehicle stickers and signpost example

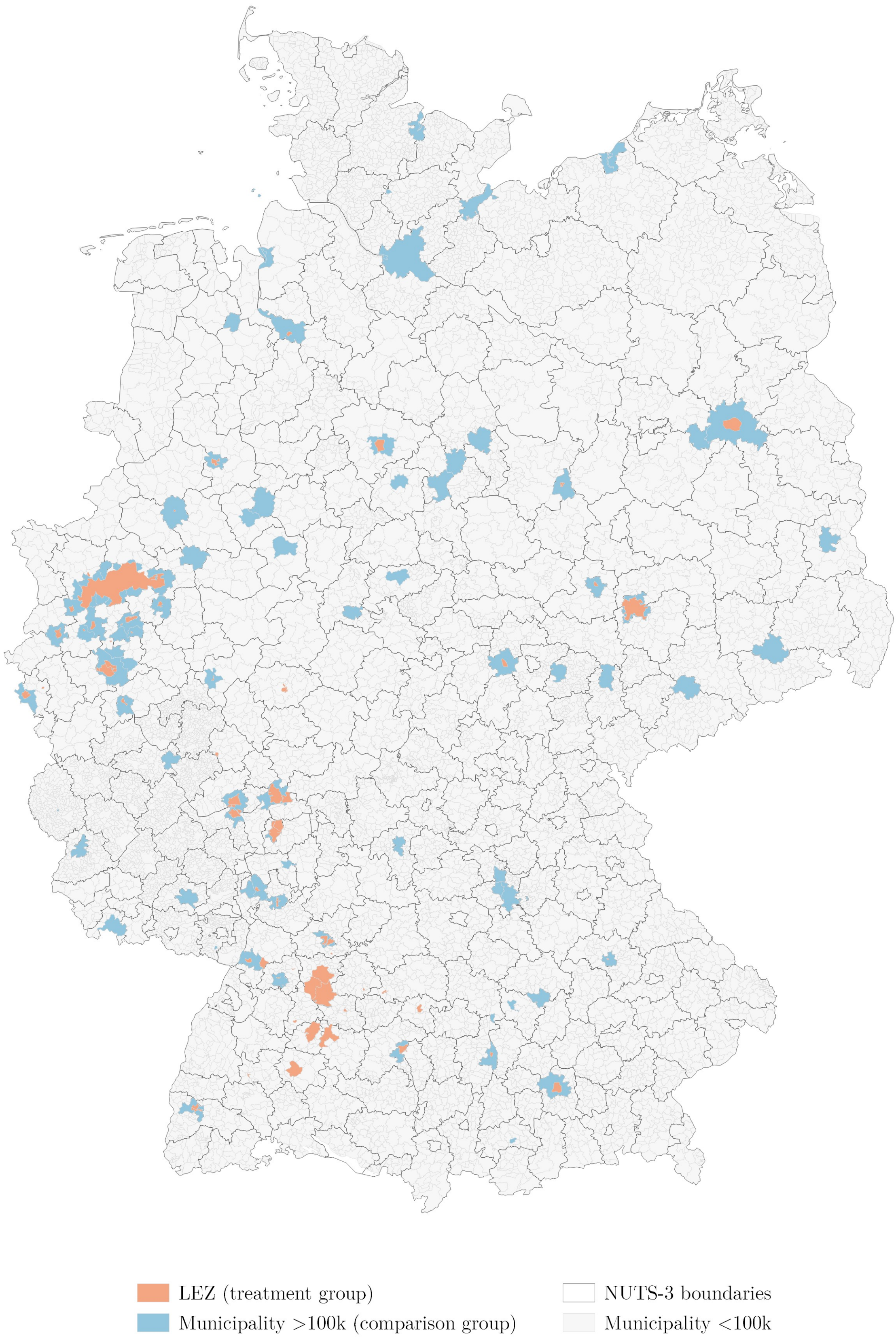


Figure A2: Treated cities and cities in control group

Table A1: LEZs in Germany

LEZ	Federal State	LEZ type	LEZ type active since	Area in km2	Circumference in km
Balingen	BW	Green	01.04.2017	90	50
Freiburg	BW	Green	01.01.2010	25	58
Heidelberg	BW	Green	01.01.2010	10	33
Heidenheim	BW	Green	01.01.2012	17	28
Heilbronn	BW	Green	01.01.2009	38	55
Herrenberg	BW	Green	01.01.2009	4	9
Ilsfeld	BW	Green	01.03.2008	2	5
Karlsruhe	BW	Green	01.01.2009	11	16
Leonberg / Hemmingen	BW	Green	02.12.2013	131	60
Ludwigsburg	BW	Green	01.01.2013	139	58
Mühlacker	BW	Green	01.01.2009	1	7
Mannheim	BW	Green	01.03.2008	7	16
Pfinztal	BW	Green	01.01.2010	31	30
Pforzheim	BW	Green	01.01.2009	2	9
Reutlingen	BW	Green	01.01.2009	109	91
Schramberg	BW	Green	01.07.2013	4	16
Schwäbisch Gmünd	BW	Green	01.03.2008	6	17
Stuttgart	BW	Green	01.03.2008	204	108
Tübingen	BW	Green	01.03.2008	108	73
Ulm	BW	Green	01.01.2009	28	26
Urbach	BW	Green	01.01.2012	2	8
Wendlingen	BW	Green	02.04.2013	4	9
Augsburg	BY	Green	01.07.2009	6	12
Müchen	BY	Green	01.10.2008	43	28
Neu-Ulm	BY	Yellow	01.11.2009	2	21
Regensburg	BY	Green	15.01.2018	1	7
Berlin	B	Green	01.01.2008	87	37
Bremen	HB	Green	01.01.2009	7	13
Darmstradt	HE	Green	01.11.2015	106	90
Frankfurt a.M.	HE	Green	01.10.2008	98	60
Limburg an der Lahn	HE	Green	31.01.2018	6	15
Marburg	HE	Green	01.04.2016	15	34
Offenbach	HE	Green	01.01.2015	39	35
Wiesbaden	HE	Green	01.02.2013	63	77
Hannover	NI	Green	01.01.2008	43	30
Osnabrück	NI	Green	04.01.2010	17	33
Aachen	NW	Green	01.02.2016	24	28
Bonn	NW	Green	01.01.2010	9	18
Düsseldorf	NW	Green	15.02.2009	14	16
Dinslaken	NW	Green	01.07.2011	4	9
Eschweiler	NW	Green	01.06.2016	2	7
Hagen	NW	Green	01.01.2012	9	19
Köln	NW	Green	01.01.2008	95	88
Krefeld	NW	Green	01.01.2011	10	16
Langenfeld	NW	Green	01.01.2013	1	6
Mönchengladbach	NW	Green	01.01.2013	21	26
Münster	NW	Green	01.01.2010	1	6
Neuss	NW	Green	15.02.2010	2	6
Overath	NW	Green	01.10.2017	0	3
Remscheid	NW	Green	01.01.2013	1	6
Ruhrgebiet	NW	Green	01.01.2012	870	276
Siegen	NW	Green	01.01.2015	3	11
Wuppertal	NW	Green	15.02.2009	25	48
Mainz	RP	Green	01.02.2013	34	35
Leipzig	SN	Green	01.03.2011	182	111
Halle (Saale)	SA	Green	01.09.2011	7	12
Magdeburg	SA	Green	01.09.2011	7	21
Erfurt	TH	Green	01.10.2012	16	19
Mean				49.02	35.53
Median				12.50	21.00
SD				119.63	42.13

Notes: Table based on Pestel and Wozny (2021) - Area, circumference, and summary statistics authors' own calculations.

A.2 Alternative outcome

We want to check whether our effect could be driven by changes housing market equilibrium. If less apartments are on the market after LEZ introduction or landlords set higher than optimal prices, this might also explain the increase in prices. Since these effects might show up in the duration that an advertisement is online until a tenant is found, we use this duration as an alternative outcome variable. From our data set, we know how many calendar months an advertisement spell is online. Since this variable is right-skewed (many advertisement spells are taken offline in the same month they were posted online), we create a binary variable equaling one if the apartment advertisement is still online in the following month after it was posted and zero otherwise. Table A2 presents the results. We find that the effect is not statistically significantly different from zero. This provides some evidence that the treatment did not lead to other change in the housing market, which would explain part of our main results. Therefore, our main positive treatment effect is more likely to be caused by the amenity of improved urban air pollution levels.

Table A2: Estimation Results: Alternative Outcome

Dependent variable: Ad active in same month

	(1)
LEZ	-0.0116
	(-1.29)
Property controls	yes
Event time×treatment wave FE	yes
Treated unit×treatment wave FE	yes
Grid FE	yes
County×time FE	yes
Number of observations	19,539,258

Standard errors clustered at county level. t-statistics in parentheses. Property controls include the living space in square meters, the number of rooms, the year of construction, the floor of the apartment, and whether the apartment has an elevator and balcony. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.3 Negative externality

There is a second potential mechanisms in place when restricting the access of high emission vehicles to certain urban areas. On the one hand, tenants and property owners within an LEZ may benefit from *positive externalities* (cleaner air, less noise pollution etc.) as opposed to residents outside the LEZs. Such externality may be reflected in higher rents and property values. On the other hand, tenants and property owners may be penalized due to a restricted access to their domiciles and more broadly a potential loss of the ability to use their car in the area. Such *negative externality* may result in lower rents and property values. Sarmiento *et al.* (2021) find that LEZs had a negative effect on peoples' well-being. The effect is more pronounced for groups that own a car, particularly a Diesel vehicle. Therefore, we try to comprehend if the second mechanism also is reflected in housing prices and potentially makes us underestimate the positive effect. To do so, we proxy 'car dependence' by exploiting the available information of a parking spot availability in the apartment advertisement. We divide the sample into apartments with an advertised parking spot versus apartments without an available parking spot. Table A3 reports the results. We do not find a statistically significant difference in the treatment effect between the two groups. However, the number of observations of apartments that have a parking spot advertised is relatively low, which makes an exact estimation difficult.

Table A3: Estimation Results: Parking spots

	Dependent variable: log(rent)		
	Parking space	No parking space	Interaction
	(1)	(2)	(3)
LEZ	0.0093	0.0196***	0.0191***
	(1.07)	(2.63)	(3.06)
LEZ×parking			-0.0119
			(-1.64)
Parking			0.0442***
			(6.86)
Property controls	yes	yes	yes
Municipality controls	yes	yes	yes
Event time×treatment wave FE	yes	yes	yes
Treated unit×treatment wave FE	yes	yes	yes
Grid FE	yes	yes	yes
County×period FE	yes	yes	yes
Number of observations	1,703,238	17,835,427	19,539,258

Standard errors clustered at county level. t-statistics in parentheses. Property controls include the living space in square meters, the number of rooms, the year of construction, the floor of the apartment, and whether the apartment has an elevator and balcony. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.4 Heterogeneity: Apartment size

Table A4: Estimation Results: Apartment square meters

	Dependent variable: log (rent)					
	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Interaction	Interaction
	(1)	(2)	(3)	(4)	(5)	(6)
LEZ	0.0167**	0.0081	0.0220***	0.0262***	0.0277***	
	(0.0068)	(0.0055)	(0.0075)	(0.0072)	(0.0094)	
LEZ×first quartile					-0.0160**	0.0117*
					(0.0075)	(0.0064)
LEZ×second quartile					-0.0118*	0.0159***
					(0.0068)	(0.0051)
LEZ×third quartile					-0.0053	0.0223***
					(0.0052)	(0.0069)
LEZ×fourth quartile						0.0277***
						(0.0094)
Quartile dummies	no	no	no	no	yes	yes
Property controls	yes	yes	yes	yes	yes	yes
Event time×treatment wave FE	yes	yes	yes	yes	yes	yes
Treated unit×treatment wave FE	yes	yes	yes	yes	yes	yes
Grid FE	yes	yes	yes	yes	yes	yes
County×period FE	yes	yes	yes	yes	yes	yes
Number of observations	8,330,930	8,331,373	8,292,496	8,366,682	33,322,114	33,322,114

Standard errors clustered at county level. Standard errors in parentheses. Property controls include the living space in square meters, the number of rooms, the floor of the apartment, and whether the apartment has an elevator and balcony. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.5 Mechanisms: detailed results

Table A5: Estimation Results: Air Pollution

	Dependent variable: log (rent)								
	Median Split			Quartile Split					
	Below	Above	Interaction	Quartile 1	Quartile 2	Quartile 3	Quartile 4	Interaction	Interaction
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
LEZ	0.0002	0.0223***	-0.0015	-0.0029	0.0031	0.0326***	0.0100**	0.0148**	
	(0.0054)	(0.0071)	(0.0060)	(0.0201)	(0.0031)	(0.0065)	(0.0044)	(0.0069)	
LEZ×AP median split			0.0247***						
			(0.0090)						
LEZ×AP quartile 1								-0.0290	-0.0142
								(0.0192)	(0.0179)
LEZ×AP quartile 2								-0.0113	0.0035
								(0.0074)	(0.0029)
LEZ×AP quartile 3								0.0163*	0.0311***
								(0.0097)	(0.0069)
LEZ×AP quartile 4									0.0148**
									(0.0069)
Property controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Grid FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
County×time FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Event time×treatment wave FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Treated unit×treatment wave FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of observations	11,909,754	15,122,089	27,031,930	3,893,059	8,016,324.	7,252,721	7,869,141	27,031,930	27,031,930

Standard errors clustered at county level. Standard errors in parentheses. Property controls include the living space in square meters, the number of rooms, the floor of the apartment, and whether the apartment has an elevator and balcony. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Estimation Results: Treatment Time

	Dependent variable: log (rent)		
	< 2013	≥ 2013	Interaction
	(1)	(2)	(3)
LEZ	0.0217***	-0.0034	0.0223***
	(0.0066)	(0.0068)	(0.0065)
LEZ×Late			-0.0252**
			(0.0112)
Property controls	yes	yes	yes
Grid FE	yes	yes	yes
County×time FE	yes	yes	yes
Event time×treatment wave FE	yes	yes	yes
Treated unit×treatment wave FE	yes	yes	yes
Number of observations	21,384,493	11,937,523	33,322,114

Standard errors clustered at county level. Standard errors in parentheses. Property controls include the living space in square meters, the number of rooms, the floor of the apartment, and whether the apartment has an elevator and balcony. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Estimation Results: Main streets

	Dependent variable: log (rent)		
	$\leq 100m$	$\geq 100m$	interaction
	(1)	(2)	(3)
LEZ	0.0144**	0.0251***	0.0166**
	(0.0056)	(0.0075)	(0.0064)
LEZ×distance dummy			0.0062***
			(0.0023)
Distance dummy			0.0114***
			(0.0022)
Property controls	yes	yes	yes
Event time×treatment wave FE	yes	yes	yes
Treated unit×treatment wave FE	yes	yes	yes
Grid FE	yes	yes	yes
County×time FE	yes	yes	yes
Number of observations	17,353,074	15,968,899	33,322,114

Standard errors clustered at county level. Standard errors in parentheses. Property controls include the living space in square meters, the number of rooms, the floor of the apartment, and whether the apartment has an elevator and balcony. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$