Three Essays on the Economic Effects of Travel and Transportation on Urban and Regional Economies

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DISSERTATION

THREE ESSAYS ON THE ECONOMIC EFFECTS OF TRAVEL AND TRANSPORTATION ON URBAN AND REGIONAL ECONOMIES

Nicolas Volkhausen

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To my wife *Lina* — my love and life's partner.

Thank you for your support and always believing in me.

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Abstract

In this thesis, I study the economic effects of travel and transportation on urban and regional economies. Using three distinct quasi-natural experiments, I analyze (i) on a *city level* the effect of short-term rentals on the housing market in Los Angeles, (ii) on a *country level* the ramifications of low emission zones on the housing market in Germany, and (iii) on a *regional level* the consequences of an aviation market reform on economic development in the European Union.

In the first essay, I study the regulation effects of the online short-term rental platform Airbnb on the housing market. In Los Angeles County, 18 out of 88 cities have severely restricted short-term rentals by adopting Home Sharing Ordinances. I apply a panel regression-discontinuity design around the cities' borders. Ordinances reduced listings by 50 percent and housing prices by 2 percent, on average. Additional difference-in-differences estimates show that ordinances reduced rents also by 2 percent. These estimates strongly differ according to geography and are particularly pronounced in touristic areas.

In the second essay, I analyze whether people's perceptions 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, I exploit the staggered introduction of Low Emission Zones (LEZs) across German cities, lowering urban air pollution by limiting the access of high-emitting vehicles. I find that residents value the presence of LEZs, reflected by roughly 2 percent 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.

In the final essay, I exploit market changes induced by the Single European Aviation market liberalization initiative to bring new evidence on the link between regional airports and economic development. Using administrative level data for the EU-15, I apply a difference-in-differences research design to identify the causal effects and spillovers of regional airports on local economic activity, population, and employment. The results suggest that the effect of the presence of regional airports on economic activity in the EU is positive, ranging between 2 and 6 percent. An additional instrumental variable estimation points towards a similar outcome, with each additional 1 million passengers yielding a positive effect on GDP of between 2 and 3 percent.

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1
Introduction

In the field of empirical economics, we strive to answer interesting economic questions by providing credible quantitative answers. Many of these questions that we seek to answer are about *causal effects* — how does an outcome variable behave if we change another variable ('what if' questions). When it comes to estimating such effects, experimental data is considered to be the 'gold standard' since it gives researchers "the ability to let questions determine the data to be obtained, instead of the data determining the questions that can be asked" (Duflo, 2005). However, most of the time we are bound to non-experimental data to establish anything about causal relationships. Clearly, the quality of these estimates is highly dependent on how convincingly we deal with problems of omitted variables bias, reverse causality, measurement error, et cetera. While some caution is justifiable, many advances in empirical economics (and other fields) have come from the use of such non-experimental studies (Litschig, 2015).

In the 19^{th} century, London suffered from a severe outbreak of cholera, which killed more than 600 people. Although most scientists at the time thought that the disease was caused by particles in the air, the English physician John Snow believed that contaminated water was the real culprit behind the epidemic. Due to ethical reasons, Dr. Snow could not simply test his hypothesis in an experimental setting, where some people chosen at random would receive fresh water while others received the potentially contaminated water. Snow observed that people living in close proximity to a particular water pump near Broad Street in the Soho district were more likely to become sick than others. Yet all of them were breathing the same air, which did not help to rebut the commonly accepted hypothesis of an airborne disease. Snow meticulously studied the rising cases of cholera and noticed that in both a brewery on Broad Street itself and a nearby workshop no one became sick — both had their own well. An additional piece of evidence came in the form of a few isolated cholera cases in districts further away, where residents had consumed water from the particular well on Broad Street.

John Snow recognized that the quality of drinking water was "as good as randomly assigned" since residents in the same area, and sometimes even in the same building, may have received their drinking water from different sources. This revelation made him realize that the outbreak was a large scale *quasi-natural experiment*.

> "No fewer than three hundred thousand people [...] were divided into two groups without their choice, and, in most cases, without their knowledge; one group being supplied with water containing the sewage of London, and, amongst it, whatever might have come from the cholera patients, the other group having water quite free from such impurity" (Snow, 1849).

This 'naturally' occurring selection into treatment and comparison groups allowed Snow to prove that the contaminated water was indeed the source of the 1854 Broad Street cholera outbreak. This discovery not only changed the field of epidemiology but is also seen as a general advancement in identifying causal effects from non-experimental data (Coleman, 2019).

This thesis is built on the notion of *quasi-natural experiments*, whereby large groups are randomly assigned into treatment and comparison groups. Chapters 2 to 4 are based on variation due to the introduction of a specific policy, which *spatially* allocates units into either group.

Chapter 2 studies the regulation effects of the online short-term rental platform Airbnb on the housing market, using a quasi-experimental research design. In Los Angeles County, 18 out of 88 cities have severely restricted short-term rentals by adopting Home Sharing Ordinances. I apply a panel regression-discontinuity design around the cities' borders and find that home sharing ordinances reduced listings by 50 percent and housing prices by 2 percent, on average. Additional difference-in-differences estimates show that ordinances also reduced rents by 2 percent. These estimates imply large effects of Airbnb on property values in areas attractive to tourists (e.g., an increase of 15 percent in house prices within 2.5km of Hollywood's Walk of Fame).

Chapter 3 analyzes whether people's perceptions of improvements in local air quality are reflected in the housing market. Using comprehensive data on real estate prices from Germany, I apply a quasi-experimental research design by exploiting the staggered introduction of Low Emission Zones (LEZs) across German cities, which lowered urban air pollution by limiting the access of high-emitting vehicles. I find that residents value the presence of LEZs, reflected by roughly 2 percent 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.

Chapter 4 exploits market changes induced by the Single European Aviation market liberalization initiative to bring new evidence on the link between regional airports and economic development. Using administrative level data for the EU-15, I apply a difference-in-differences research design to identify the causal effects and spillovers of regional airports on local economic activity, population, and employment. The results suggest that the effect of the presence of regional airports on economic activity in the EU is positive, ranging between 2 and 6 percent. An additional instrumental variable estimation points towards a similar outcome, with each additional 1 million passengers yielding a positive effect on local GDP of between 2 and 3 percent.

Chapter 5 concludes the thesis with a summary of the main findings and an outlook for possible future research.

2

Short-term Rentals and the Housing Market: Quasi-Experimental Evidence from Airbnb in Los Angeles

2.1 Introduction

Short-term housing rentals (STRs) have become very important due to the rise of online STR-platforms, such as Airbnb, which provide opportunities for households to informally offer accommodation to visitors. The surge in popularity of STR-platforms has led to substantial opposition because of a decrease in housing affordability (Samaan, 2015; Sheppard and Udell, 2016), unfair competition, and illegal hotelization (CBRE, 2017). Negative externalities (*e.g.* noise, reduction in perceived safety) due to the presence of tourists in residential buildings are also frequently mentioned (see *e.g.* Lieber, 2015; Williams, 2016; Filippas and Horton, 2018).

Local governments around the globe have responded quite differently towards regulating STRs. Most cities have not significantly regulated these platforms, but a limited number of cities have recently put severe restrictions in place. Berlin, for instance, requires STR-hosts to occupy the property for at least 50% of the time (O'Sullivan, 2016).

This chapter is based on Koster et al. (2021). We thank Jan Brueckner, Guillaume Chapelle, David Gomtsyan, Eric Koomen, Robert Elliott, Stephen Sheppard, Mariona Ségu, as well as the seminar audiences at the Higher School of Economics (St. Petersburg), the Southwestern University of Finance and Economics (Chengdu), the 13th Meeting of the Urban Economic Association (New York), University of Birmingham, Paris School of Economics, and Zhejiang University (Hangzhou) for useful comments. Dr. Koster acknowledges the support of the HSE University Basic Research Program.

San Francisco imposes a 14% hotel tax (*i.e.* a *Transient Occupancy Tax*) and a cap of maximum 90 rental days per year (Fishman, 2015). Amsterdam even imposes a maximum cap of 30 rental days per year as of 2019.

In this paper, we aim to measure the impact of Airbnb, by far the largest STR-platform, on housing markets. We focus on the effects of policies that restrict the market for STRs. There are arguably three main mechanisms of how regulation of short-term renting impacts property markets:

1. *Efficient use effect*. Short-term rentals generate income from idle space, increasing value due to additional income opportunities. Moreover, residential properties can now be used by their most profitable use (*i.e.* by short-term renters). This should be an efficiency gain that spurs housing demand, which increases house prices (see *e.g.* Turner et al., 2014).

2. *Rental housing supply effect*. Short-term rentals may in turn lead to a reallocation of existing housing stock away from the long-term rental market towards privately-owned housing, which increases rents (see *e.g.* Quigley and Raphael, 2005).

3. *Externality effect*. Short-term rentals may create negative nuisance externalities, lowering nearby property values. If neighbors fear turnover or unfamiliar people in their neighborhood, this may reduce demand for housing (see *e.g.* Filippas and Horton, 2018).

To identify the effects of short-term housing rentals regulation on the housing market, we exploit exogenous variation provided by the implementation of so-called Home-Sharing Ordinances (HSOs) in Los Angeles County. 18 out of 88 cities implement regulations that essentially ban *informal* vacation rentals; hosts renting out entire properties are now subject to the same formal regulations as regular hotels and bed and breakfasts. Short-term home-sharing is not always prohibited, albeit restricted in those cities.

There are several reasons why we focus on Los Angeles County. First, it is an area that is attractive to tourists and has thousands of listings on Airbnb. It is in the global top 10 of the cities with the most Airbnb listings and is the second most popular Airbnb city in the U.S. after New York. Second, there is substantial spatio-temporal variation in the implementation of HSOs within this county. For example, HSOs have been implemented in cities that receive many tourists (*e.g.* Santa Monica), as well as in cities that are more at the edge of the Los Angeles Conurbation (*e.g.* Pasadena). We think this might add to the external validity of the results shown in the paper. Third, by focusing on 18 cities, rather than on the introduction of an HSO in one single city, we substantially reduce the likelihood that our results are contaminated by an unobserved event (*e.g.*, a change in a city-specific policy) that occurs around the same time as the introduction of the HSO. Fourth, in Los Angeles County, in contrast to for example New

York, renters are (usually) not allowed to list a property on Airbnb (Lipton, 2014).[1] This facilitates the interpretation of the distributional consequences of our results: renters generally lose from Airbnb-induced higher rents (and hardly benefit from the opportunity of subletting to short-term renters).

The variation in restrictions between cities enables us to use a spatial regression discontinuity design (RDD), which we combine with a difference-in-differences (DiD) set-up: we essentially focus on *changes* in the number of Airbnb listings, as well as in house prices, close to the borders of cities that have implemented HSOs. More specifically, we use micro-data on Airbnb listings and house prices between 2014 and 2018. Our main results are then based on observations within approximately 2 km of borders of HSO areas.

By applying the Panel RDD we identify the first effect – the efficient use effect – which is arguably the key mechanism to explain the effects on house prices. Conditional on local area fixed effects, properties close to the border of an area where an HSO is implemented are otherwise identical, except that in one area short-term renting is restricted. Theory then indicates that there is a discrete decrease in house prices at HSO borders because houses within a treated area offer less value to homeowners.

One potential issue with the Panel RDD approach is that by comparing house prices (as well as listings) in two neighboring cities – one which implemented an HSO and the other which did not implement any HSO – substitutability between houses on the two sides of the city border may inflate the effect of the HSO implementation. We provide a range of statistical tests which all show that this 'manipulation' is non-existent. The economic intuition for the absence of manipulation is that tourist demand tends not to be very local (*e.g.*, tourists are indifferent between locations which are a couple of minutes drive from each other), so tourist accommodations compete with each other over long distances. Hence, given an elastic demand function for tourist accommodation, there is no incentive to move listings just across the border.[2]

Short-term rental platforms also reduce housing supply available for local (long-term) rental markets, which increases rents (Hilber and Vermeulen, 2016) – the rental housing supply effect. When the expected economic returns on rental and privately owned properties are the same, then the housing supply effect estimated in the rental market should be the same as the efficient use effect (estimated using house prices).[3] We

[1] The extent of illegal subletting is unknown, but note that the host is always responsible for breaking the law, rather than Airbnb (Petterson, 2018). This strongly reduces the benefits of illegal subletting because of hefty fines and potential lawsuits.

[2] In line with this line of reasoning, we will show that prices per night for Airbnb accommodations are not affected by HSOs. The latter suggests that the market for short-term rentals is highly competitive and that tourist demand for local accommodation is highly elastic.

[3] However, note that the effects of short-term rentals on house prices may be different from those on rents in the short run, because house prices may include anticipation effects towards future changes in policies. However, we do not find evidence for this.

cannot measure the rental housing supply effect by applying a Panel RDD for rents, because properties that are next to each other, but on different sides of the HSO border experience identical changes in housing supply and offer the same value to renters (see Glaeser and Ward, 2009). This implies that there should be no discrete jump at HSO borders for rents.[4] To capture the rental housing supply effect we employ an alternative strategy: we use ancillary data on aggregate rents for zip codes and a DiD estimation strategy, while we focus on properties further away from the HSO borders. The DiD approach relies on more restrictive identifying assumptions than the Panel RDD approach. We assess the validity of the DiD approach by applying the same approach to house prices, finding very similar effects as with the Panel RDD approach.

We also test for the third effect – the externality effect – by investigating the price change of properties outside HSOs but close to areas where HSOs have been implemented. Many papers find that housing market spillovers are very local (see *e.g.* Linden and Rockoff, 2008; Autor et al., 2014; Fisher et al., 2015; Pope and Pope, 2015; Ahlfeldt and Holman, 2018; Diamond and McQuade, 2019; Koster and Van Ommeren, 2019). We therefore also test for differences between the effects of Airbnb on prices, while distinguishing between condominiums and single-family homes. One expects that local externalities are particularly important for condominiums, so if the effect of Airbnb on condominium prices would be lower this means that an external effect could be present. We do not find evidence that the externality effect is important for LA County.

We have two main results. Our first result is that HSOs are very effective in reducing Airbnb listings. The ordinances *strongly* reduced the number of Airbnb listings of entire properties and rooms by about 50% in the long run. We further show that room listings have not been reduced when offering rooms is still allowed, which is the case in 4 out of the 18 cities with HSOs. Our second result is that the HSO reduced house prices and rents by about 2% on average. This effect is robust to a wide range of placebo-tests and specification choices. Hence, the decision to implement an HSO is a political one, with a clear group of winners and losers, and strong distributional effects: owners lose from HSO-induced house price reductions, whereas (long-term) renters benefit from lower rents.

Our setup allows us also to estimate the effect of Airbnb demand on the housing market. Causal inference of this effect is not straightforward, as Airbnb listings are concentrated in central areas that are also attractive to residents. Hence, one is predisposed to find a strong positive correlation between Airbnb listings and house prices or rents. We estimate the effect of Airbnb demand on housing prices using an IV approach. We measure demand using the Airbnb listings rate – the share of listings to the number of housing units. Using HSOs as supply-shifting instruments for the listings rate around the border, we show that short-term rental demand for accommodation increases prices of residential properties – a standard deviation increase in the Airbnb listings rate increases prices by 5.5%. Using the DiD estimation strategy, we further show that rents

[4] A Panel RDD analysis of rents confirms the absence of a discontinuity in rents.

decrease by about the same amount as house prices, likely because of the reduced supply of rental housing.

We then show that Airbnb implies modest property value increases for LA County as a whole: the total average property value increase due to Airbnb since 2008 is 3.6%. However, this masks the fact that a large part of LA County is not very urbanized and does not attract tourists. By contrast, the effects of Airbnb on the housing market can be large in central urban areas – within 2.5km of Hollywood's Walk of Frame, property values have increased by almost 15% due to Airbnb. Within 2.5km of beaches, prices have increased by 5.8%.

Related literature. In recent years, the *sharing economy* has received increasing attention. Economists have examined home sharing from various angles such as racial discrimination in the online marketplace (Edelman et al., 2017; Kakar et al., 2016), negative externalities of tourism (Van der Borg et al., 2017; Gutiérrez et al., 2017) and its effects on the hotel industry (Zervas et al., 2017). This is not the first empirical study on the effect of short-term rentals on the housing market. Sheppard and Udell (2016) conclude that housing values increased by about 31% due to Airbnb. Horn and Merante (2017) show that a high Airbnb density increases asking rents by 1.3-3.1%. Barron et al. (2021) show that Airbnb listings increase house prices and rents in U.S. cities.[5] Garcia-López et al. (2020) also report a positive effect on rents in Barcelona. Almagro and Domínguez-Iino (2021) develop a structural model for Amsterdam in which Airbnb is used as a shock in consumption amenities. They find that a 10% increase in listings increases rents by 0.5%. Identification of the model parameters relies on a particular structure of the unobserved component (*i.e.* an ARMA structure). A few reports (NYCC, 2015; Samaan, 2015; Lee, 2016; Wachsmuth and Weisler, 2018) – which essentially rely on correlations – have studied the impact of Airbnb as well. In contrast to these studies, we study the effect of regulation of Airbnb itself, which is of key policy interest. In addition, we exploit quasi-experimental variation provided by changes in regulation to estimate the effect of Airbnb on the housing market.

Our paper also relates to a literature studying the effects of tourism and amenities on housing markets. Carlino and Saiz (2008), for example, show that the number of tourists visiting a city is a good predictor of the growth of U.S. metropolitan areas in the 1990s. Ahlfeldt et al. (2017) and Gaigné et al. (2022) find that the density of pictures taken by tourists and residents increases the land value and attracts the wealthy. Moreover, a large number of papers show that high amenity locations have higher housing values (see *e.g.* Van Duijn and Rouwendal, 2013; Ahlfeldt and Kavetsos, 2014; Koster and Rouwendal, 2017). In these studies, it is impossible to disentangle the ef-

[5] Barron et al. (2021) focuses on US cities and applies a difference-in-differences strategy using an instrument based on both the popularity of Airbnb and how touristy an area is to address endogeneity issues. One criticism of this approach is that high-amenity areas, in particular US inner cities, have both attracted tourists and residents in recent decades (see Couture and Handbury, 2019). Reassuringly, our estimates are of a similar order of magnitude as Barron et al. (2021), despite the differences in identification strategy and focus.

fects of tourism and amenities. An exception is a recent paper by Faber and Gaubert (2019), which shows that tourism generates substantial local and national economic gains driven by spillovers on manufacturing and national integration respectively. Our paper, therefore, contributes to this literature by using a quasi-experimental research set-up, enabling us to isolate the effects of tourism demand, proxied by Airbnb listings.

Conceptually, our paper is close to a literature measuring the effect of land use regulation and zoning, as the HSO can be seen as an example of a zoning regulation. Most studies in this field show that housing supply constraints are associated with increasing housing costs, a strong reduction in new construction, and rapid house price growth (Glaeser et al., 2005; Green et al., 2005; Ihlanfeldt, 2007; Hilber and Vermeulen, 2016). However, they do not identify the underlying mechanisms that lead to price increases. Glaeser and Ward (2009) find that local constraints do not increase the price between localities, because areas that are geographically close are reasonably close substitutes. Using a spatial regression discontinuity design, Koster et al. (2012), Turner et al. (2014) and Severen and Plantinga (2018) also study the local effects of regulation and find that the effects of regulation for homeowners may be up to 10% of the housing value. One major difference with these studies (except for Severen and Plantinga, 2018) is that our research design does not rely on cross-sectional variation in land use regulation, but rather identifies the effect based on changes in regulation over time.

Finally, our paper is related to a large literature on housing regulation, including rent-controlled or public housing (Olsen and Barton, 1983; Fallis and Smith, 1984; Moon and Stotsky, 1993; Glaeser and Luttmer, 2003; Anderson and Svensson, 2014; Autor et al., 2014), and affordable housing (Quigley and Raphael, 2004; Diamond and McQuade, 2019; Koster and Van Ommeren, 2019). In this literature, it is common to study a policy where a fixed share of houses is regulated to help poor households. Regulation creates then an efficiency effect as well as a housing supply effect. Studies typically focus either on the efficiency effect (see Glaeser and Luttmer, 2003; Anderson and Svensson, 2014) or the housing supply effect (see Fallis and Smith, 1984). In contrast to the existing literature, we study a regulation type that induces efficiency and housing supply effects for the full housing market, rather than a sub-segment of the market. Recent studies also explicitly take into account spillovers of providing subsidized housing and find that these spillovers are very local (see Autor et al., 2014; Diamond and McQuade, 2019).

This paper proceeds as follows. In Section 2.2 we discuss the research context. Section 2.3 introduces the data and provides descriptives. In Section 2.4 we elaborate on the identification strategy, followed by graphical evidence in Section 2.5. We report and discuss the main results in Section 2.6, which is followed by Section 2.7 studying the overall price effects. Section 2.8 concludes.

2.2 Context

2.2.1 Airbnb in Los Angeles County

In 2007, Brian Chesky and Joe Gebbia came up with the idea of putting an air mattress in their living room and turning it into a bed and breakfast, marketed through an online platform (Lagorio-Chafkin, 2010). The website – later called Airbnb and officially launched in 2008 – is a platform that connects hosts that own accommodation (rooms, apartments, houses) with guests seeking temporal accommodation. Prospective hosts list their spare rooms or entire apartments for a self-established price and offer the lodging to potential guests.[6] Airbnb charges a fee to both the host and guest.

Airbnb has grown rapidly since its launch in Los Angeles County (as in other major cities across the globe), with now more than 40 thousand listings. 60% of those listings are entire properties (Inside Airbnb, 2017).[7] Figure 2.1 clearly shows that Airbnb listings are heavily concentrated in popular areas like Venice, Santa Monica, Hollywood, and Downtown LA. Nevertheless, we also record many listings in areas that are further away from the center (*e.g.* Pasadena, Hermosa Beach).

Many cities around the world have imposed some form of regulation, *e.g.* by requiring hosts to register their STR activities with the local authorities. However, an increasing number of cities also explicitly restrict short-term housing rentals, which are defined as lettings of up to 30 consecutive days. Cities that impose so-called Home-Sharing Ordinances (HSOs) typically distinguish between two types of *informal* STRs: 'rooms', whereby at least one of primary residents lives on-site throughout the visitor's stay, and 'entire properties', which are for the exclusive use of the visitor.

In Figure 2.1 we show the names of 18 cities that have imposed HSOs during our study period 2014-2018. The other 60 cities – including the largest one, the City of Los Angeles – did not impose regulations in this period.[8] These 18 cities, which contain close to 5 percent of the whole housing stock of this County, essentially ban informal vacation rentals by requiring hosts to have a business license and comply with health and safety laws, as well as levying a Transient Occupancy Tax on the listing price (up to 15%). Most cities completely ban short-term letting of both rooms and entire properties. 4

[6] With more than 4 million listings – more properties than the top 3 hotel brands, Marriott, Hilton, and IHG, combined (Airbnb, 2017) – Airbnb emerged as one of the main figureheads of the sharing economy, in which technology companies disrupt well-established business models by facilitating direct, peer-to-peer exchanges of goods and services (Lee, 2016).

[7] According to Airbnb, it generated $1.1 billion in economic activity in the City of Los Angeles. Its typical host earned $7,200 per year from hosting and it helped 13% of its hosts to save their home from foreclosure and another 10% from losing their home to eviction (Airbnb, 2016; Inside Airbnb, 2017).

[8] In 45 cities, short-term renting is technically illegal, because it is not mentioned in the residential housing code. However, in phone interviews undertaken by the authors, local officials state that nothing is done to enforce the residential housing code and to prevent homeowners to list their properties on Airbnb. This appears to be common knowledge. We make sure that listings in those 45 cities are not lower compared to other places (see Section 2.6.3).

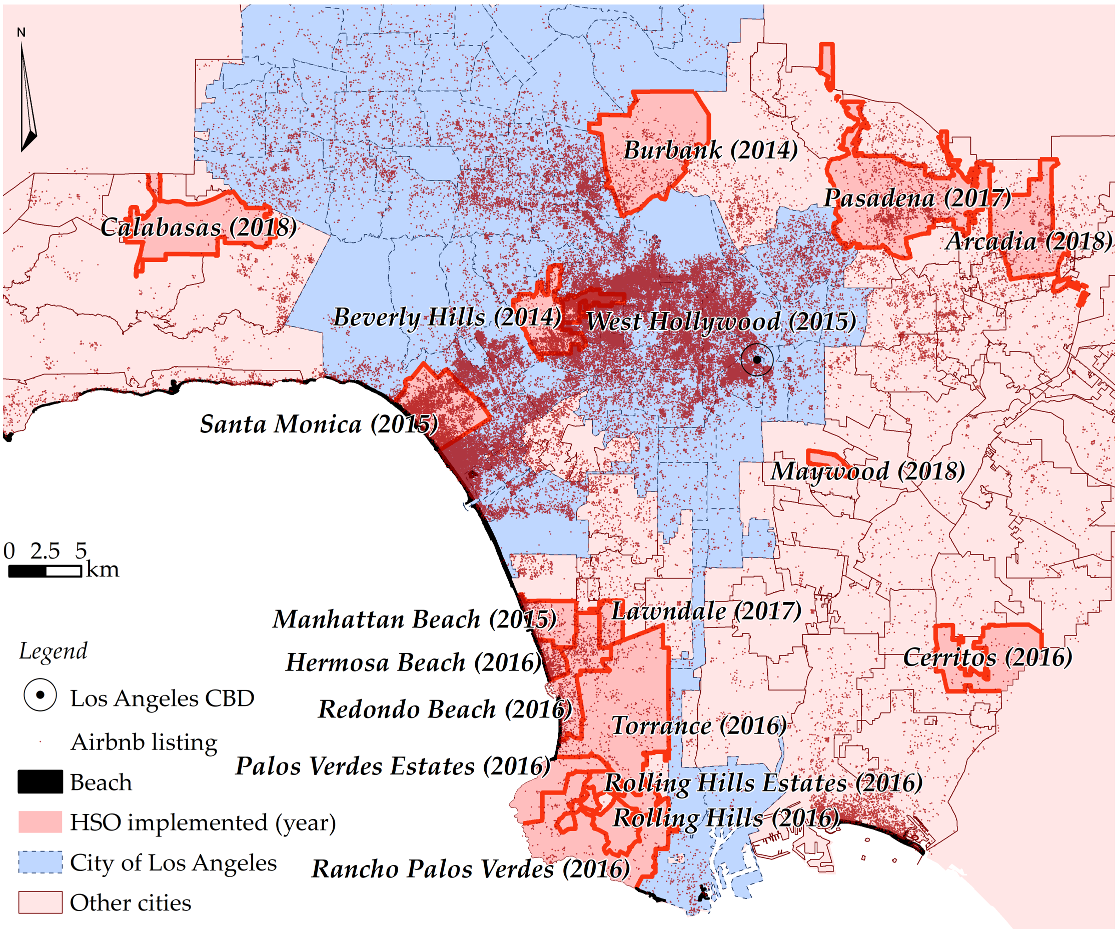


Figure 2.1: Airbnb in Los Angeles County

out of 18 cities (Calabasas, Pasadena, Santa Monica, and Torrance) still allow for room rentals, although restrictions apply. In Santa Monica, for example, the HSO allows for room rentals up to 30 days per year but prohibits hosts to operate more than one room listing at the same time. The HSOs in LA County are usually enforced. For example, the City of Santa Monica has collected more than \$4.5 million in taxes from Airbnb and other short-term home rental businesses and has fined hosts violating the law for \$80,000. Listings that are operating illegally may be issued fines of up to \$500 per day and face criminal prosecution if they do not cease operations (City of Santa Monica, 2017).[9] In Appendix 2.9 we report for each city in LA County more details regarding STR regulation.

[9]Note that our estimates of the HSOs reflect the actual levels of enforcement of the cities investigated in Los Angeles County. For example, it is plausible that the effects on the number of listings as well as property prices are higher in cities where enforcement is more strict.

Our estimated effect of HSOs on house prices, but not on rents, may potentially depend on future regulation changes. It seems plausible that some economic actors anticipate the introduction of future HSOs in cities that currently have none, which may affect house prices. This raises the question of whether our study captures the permanent effect of HSOs. Because we do not find evidence for anticipation effects in Section 2.6.2, it is plausible that the estimated effects can be interpreted as coming from permanent changes. Furthermore, if anticipation effects are present, then we would find an underestimate of the effect of the HSO on house prices. Note that we are aware of only one fundamental future change in regulation after the period analyzed by us, which is for the City of Los Angeles. This city announced in December 2018, so approximately half a year after the period for which we observe house prices, that it will introduce an HSO in July 2019 (so about 18 months after the period for which we observe house prices). It is very unlikely that this future HSO has affected house prices, also because this HSO is less restrictive than the HSOs introduced in the 18 cities analyzed by us (*i.e.* it restricts the maximum number of yearly rental days to 120, which is hardly restrictive).

Our empirical approach relies on the fundamental assumption that around the implementation of the HSOs other policies did not change in the 18 cities compared to their immediate surroundings. We are not aware of such policy changes (but have actively searched for this) and offer statistical support for this assumption. One may argue that also weaker conditions may violate the main identifying assumption. For example, there may be differences in unobserved factors that might affect house price *growth*.[10] As these unobserved factors should be discrete at the spatial border, and because we focus on a relatively short study period, these factors are unlikely to play a major role. This is particularly so because we do not observe pre-trends in house prices or listings, respectively, once focusing on areas close to the borders of cities. In Section 2.6.3 we further perform a range of placebo tests using the information on price changes around the borders of other sets of cities and using the same borders but in other periods. All these tests indicate that there are no changes in listings and prices at the placebo borders investigated. This makes it implausible that other policies or other unobserved factors (or *e.g.* differences in school quality) changed exactly around this period.[11]

2.3 Data and descriptives

2.3.1 Data

We employ Airbnb listings data obtained from web scrapes for 15 different months from the websites `www.insideairbnb.com` between October 2014 and September 2018 for Los Angeles County. We double-check these data with data on listings from

[10]City borders may sometimes intersect with natural features (*e.g.* canyons or rivers). These natural features are unlikely to cause changes in price growth because preferences usually do not change within a short time frame.

[11]This conclusion is supported by the absence of differences in (changes in) public good provisions between cities that are known to affect house prices. See for evidence on school quality Appendix 2.9.

www.tomslee.net.[12] LA County is the most populous county in the United States (more than 10 million inhabitants as of 2018). We know the location (up to 200m) and whether a property is listed in one of the 15 months of observation.[13] For the analysis where we analyze the effects of HSOs on listings, we construct a panel dataset of all accommodations that have been listed at least once between 2014 and 2018. We create a variable that equals one when the accommodation is listed in a certain month. We refer to Appendix 2.9 for more details.

We also use micro-data on housing transactions, obtained from the Los Angeles County Assessor's Office. The data provides information on sales prices and a range of property characteristics (*e.g.*, condominium, single-family home, construction year) for all transacted residential properties. We focus on transactions from January 2014 until early 2018, as these match closely to the period our Airbnb data refers to. Ancillary data on properties' locations, exact building locations, and neighborhood characteristics are obtained from Los Angeles County's GIS Data Portal. We disregard extreme outlier observations and transactions with missing information on either prices or property size or type (condominium or single-family home), as well as transactions referring to multiple parcels or units.[14]

For the analysis of the effect of Airbnb demand on house prices, there are two technical issues when matching listings data to house prices. First, the data on listings are based on 15 snapshots during our study period. Second, we do not have information on listings from January to October 2014. We deal with both issues by constructing an *imputed* measure which imputes the listing probability based on the nearest two dates for which we have information.[15]

To capture Airbnb demand, we use the Airbnb listings rate – defined by the number of

[12]Airbnb is not the only STR-platform available to prospective hosts. This is unlikely problematic because hosts who consider using other platforms are likely also to use Airbnb, which is the dominant platform, as the cost of advertising on Airbnb is negligible. According to www.beyondpricing.com, *HomeAway* – Airbnb's most important competitor – had 3,578 listings in Los Angeles in 2016, while Airbnb had 8,367 listings (which is less than observed in our data). Data on individual *HomeAway* listings is not available to us.

[13]Note that a listing always refers to the same property but properties may sometimes change listings because owners of properties have the option to remove their listing and start a new one. This has no consequence for the consistency of our estimates. Through *Inside Airbnb*, we also have information for a subset of listings on the number of reviews, which we will show for descriptive purposes.

[14]More specifically, we remove transactions referring to properties cheaper than \$50000 or more expensive than \$5 million. We also omit transactions with a m^2 price that is below \$200 or above \$20000. We further disregard repeat sales with yearly price differences larger than 50%. Additionally, we exclude properties smaller than $50m^2$ or larger than $1000m^2$ and parcels smaller than $50m^2$ or larger than 10ha.

[15]For example, when we observe that a property is listed in March, but not in May, the imputed listing probability is 0.5 in April 2015. Before October 2014 we use data on listings from October 2014.

listings divided by the number of housing units – within 200m of each property.[16]

We further gather monthly data on listed median rents and house prices *at the zip code level* from *Zillow*, which is a large real estate database company.[17] *Zillow* has micro-data on over 110 million homes across the United States; not just those homes currently for sale but also for rent. For each zip code in each month, *Zillow* posts the median listed rent and median listed sales price. For LA County, we have information on 114 (out of 311) zip codes.

In the econometric analysis, we will also distinguish between geographical areas within the County of Los Angeles. An area is defined by us as a City; or a neighborhood within the City of Los Angeles (which is by far the largest city); or a so-called 'unincorporated' area. In total, we have 252 areas.

2.3.2 Descriptives

Table 2.1 reports the main descriptive statistics for the Airbnb listings. We observe that, on average, rental prices per night in areas where HSOs are implemented are somewhat higher than in other areas. Hence, the HSOs are predominantly implemented in areas where there is more demand for tourist accommodation. In other observable characteristics, such as accommodation size, the number of reviews, and the share of entire properties, listings in HSO areas seem to be similar to listings in other areas. The most notable difference is that the distance to the beach is lower in areas where HSOs are implemented, as several beach towns, such as Santa Monica, Manhattan Beach, and Redondo Beach, have implemented HSOs.[18]

Figure 2.2 provides information about changes in the number of listings over time (for the exact number of listings per wave, we refer to the Appendix 2.9). We observe that there is a strong positive trend in the number of listings in LA County. In September 2018 the number of listings was almost 4 times higher than in October 2014. However, the growth in listings has been much lower in areas where HSOs were implemented during our study period. The trend in listings particularly diverges in 2017 once more

[16] Information on the location of housing units is obtained from the *American Community Survey*, which provides information at the census block group (of, on average, 540 housing units). We draw circles around each property and calculate the area-weighted number of housing units within 200m. To avoid outliers for a low number of housing units, we replace the lowest 2.5% of the number of housing units by the value of the 2.5$^{\text{th}}$ percentile. In Appendix 2.9 we show that our results are rather insensitive to outliers. As an alternative to the listings rate, we have also used the density of listings (within 200m) to calculate Airbnb demand, which provides largely similar results.

[17] The most detailed data publicly available is at the so-called *Zillow*-neighborhood. Because these data are only available for a few neighborhoods in LA County, we use the more aggregated zip code level.

[18] The condominium share of Airbnb listings exceeds the condominium share of housing transactions (see Table 2.2). Hence, the forbidding of Airbnb in condominium buildings in March 2015 by Owners Associations (*e.g.* to reduce within-building externalities) is unlikely widespread (see Watts v. Oak Shores Community Association, 2015).

cities implemented HSOs.

Table 2.1: Descriptive statistics for Airbnb data

Panel A: Inside HSO areas	mean	sd	min	max
Price per night *(in \$)*	172.1	140.0	25	999
HSO implemented	0.769	0.421	0	1
Property type – apartment	0.515	0.500	0	1
Property type – single-family home	0.408	0.491	0	1
Property type – unknown	0.0769	0.266	0	1
Rental type – entire home/apartment	0.617	0.486	0	1
Rental type – home sharing	0.383	0.486	0	1
Accommodation size *(in number of persons)*	3.421	2.346	1	16
Number of reviews	19.27	37.62	1	602
Distance to border of HSO area *(in km)*	0.712	0.643	0.0000622	3.140
Distance to the beach *(in km)*	12.19	12.56	0	44.78
Panel B: Outside HSO areas	mean	sd	min	max
Price per night *(in \$)*	147.2	132.7	25	999
HSO implemented	0	0	0	0
Property type – apartment	0.476	0.499	0	1
Property type – single-family home	0.435	0.496	0	1
Property type – unknown	0.0886	0.284	0	1
Rental type – entire home/apartment	0.597	0.491	0	1
Rental type – home sharing	0.403	0.491	0	1
Accommodation size *(in number of persons)*	3.477	2.505	1	20
Number of reviews	21.62	40.45	1	700
Distance to border of HSO area *(in km)*	4.616	4.947	0.000143	64.83
Distance to the beach *(in km)*	15.31	10.68	0	96.40

Notes: Prices are missing, unrealistically low (<\$25) or high (>\$1000) in 1% of the cases. The number of listings for HSO areas is $53,980$. Outside HSO areas it is $344,813$.

We report descriptive statistics for the housing transactions data in Table 2.2. The house price and the price per m^2 are substantially higher in HSO areas, respectively 52% and 68%. The listings rate is about 0.7% in HSO areas and 0.5% outside HSO areas. The spatial (see Figure 2.1) and temporal (see Figure 2.2) variation in the listings rate is large: for the majority of houses (65%), there are no listings within 200m.

Properties in HSO areas are about 10% larger, but at the same time, the share of condominiums is about twice as high in these areas. This may reflect that HSOs tend to be implemented in upscale areas where prices are higher and which are inhabited by rich households that have high demands for space. These figures emphasize the need to focus on observations that are close to HSO borders to have a comparable control group. HSO areas tend to be more touristy: the distance to the beach is on average about half in these areas, whereas the density of tourist pictures is about twice as high, compared to non-HSO areas.

The table also indicates that the share of housing transactions in HSO area is about 10%

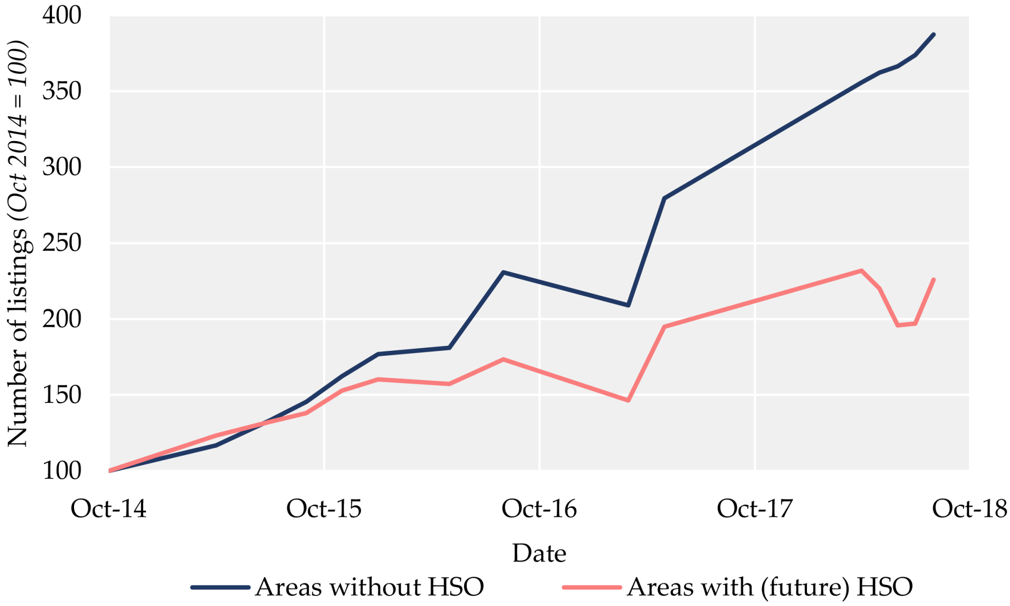


Figure 2.2: Airbnb in LA County

of total transactions. This observation is useful because it shows that the effect of HSO policies on listings in the non-HSO areas is likely small. The reason is that spillovers, which likely exist because short-term renters have an incentive to increase their demand in non-HSO areas, are expected to be of secondary importance. We come back to this later by providing empirical evidence that cross-border spillovers are non-existent, see Section 2.5.

Table 2.2: Descriptive statistics for housing transactions

Panel A: Inside HSO areas	mean	sd	min	max
House price *(in \$)*	1,024,013	673,898	50,000	5,000,000
House price per m^2 *(in \$)*	6,187	2,724	274.3	20,000
HSO implemented	0.391	0.488	0	1
Listings rate <200 *(in %)*	0.746	1.340	0	42.67
Property size *(in m^2)*	167.6	78.79	50	842
Parcel size *(in m^2)*	1,447	3,247	57	54,655
Apartment	0.371	0.483	0	1
Number of bedrooms	2.934	1.014	1	9
Number of bathrooms	2.447	0.968	1	5
Construction year of property	1,971	22.07	1,897	2,017
Distance to border of HSO area *(in km)*	0.718	0.619	0.000137	2.992
Distance to the beach *(in km)*	14.61	14.14	0.0140	45.50
Tourist picture density *(per ha)*	5.569	7.780	0.114	31.95
Year of observations	2,016	1.158	2,014	2,018
Panel B: Outside HSO areas	mean	sd	min	max
House price *(in \$)*	610,301	476,562	50,000	5,000,000
House price per m^2 *(in \$)*	4,064	2,189	247.5	20,000
HSO implemented	0	0	0	0
Listings rate <200 *(in %)*	0.564	1.900	0	85.64
Property size *(in m^2)*	152.6	69.39	50	921
Parcel size *(in m^2)*	2,110	6,333	50	95,285
Apartment	0.208	0.406	0	1
Number of bedrooms	2.980	0.948	1	10
Number of bathrooms	2.198	0.901	1	5
Construction year of property	1,968	23.63	1,884	2,018
Distance to border of HSO area *(in km)*	11.09	12.33	0.000952	70.67
Distance to the beach *(in km)*	27.46	19.99	0.00346	107.5
Tourist picture density *(per ha)*	2.145	6.833	0	112.9
Year of observations	2,016	1.169	2,014	2,018

Notes: The number of transactions for HSO areas is 32971. Outside HSO areas it is $250,490$.

Finally, we turn to the data on rents and house prices from *Zillow* for zip code areas. We report descriptives in Table 2.3. The average rent per m^2 is about \$26 in both areas. Although rents are very similar for both areas, we find a 17% lower average house price per m^2 outside HSO areas. The listings rate is lower in HSO areas (0.8%) than outside these areas (1.4%). Also at the zip code level, there is substantial variation in the listings rate. The zip code area with the highest rate, 14.3%, is located in Venice (City of LA), followed by a zip code in Hollywood (City of LA) with 8.9%.

A priori, it is difficult to judge the quality of the information offered by *Zillow*. Quite reassuringly, the correlation between median house prices in Zillow and median house prices using the Assessor Office's data (which we use for microanalyses) is high ($\rho = 0.941$). However, when we demean prices by zip code and month fixed effects, the correlation is only moderate ($\rho = 0.322$). This suggests that results might be dataset-specific. However, we will show that our results are not driven by the choice of the dataset.

Table 2.3: Descriptive statistics for Zillow data

Panel A: Inside HSO areas	mean	sd	min	max
Rent price per m^2 *(in \$)*	26.32	8.837	15.79	65.31
House price per m^2 *(in \$)*	6,692	2,464	4,035	17,830
HSO implemented	0.579	0.494	0	1
Listings rate	0.826	0.790	0	4.452
Distance to border of HSO area *(in km)*	1.029	0.399	0.374	2.029
Distance to the beach *(in km)*	11.50	14.53	0.580	42.82
Distance to the CBD *(in km)*	25.54	7.136	12.85	41.08
Housing units per *(in ha)*	14.31	10.44	1.239	40.98
Year of observations	2,016	1.345	2,014	2,018
Panel B: Outside HSO areas	mean	sd	min	max
Rent price per m^2 *(in \$)*	24.67	9.543	7.927	76.52
House price per m^2 *(in \$)*	5,563	2,622	1,089	15,428
HSO implemented	0	0	0	0
Listings rate	1.355	1.710	0	14.26
Distance to border of HSO area *(in km)*	10.28	13.56	0.0594	58.65
Distance to the beach *(in km)*	23.86	21.54	0.137	96.28
Distance to the CBD *(in km)*	29.41	17.17	1.420	80.59
Housing units per *(in ha)*	11.48	9.730	0.320	45.66
Year of observations	2,017	1.272	2,014	2,018

Notes: The number of observations for HSO areas is 815. Outside HSO areas it is 2676.

2.4 Econometric framework

We are interested in the effect of short-term housing-rentals regulation on the housing market. One way to estimate this effect is to compare adjacent cities that differ in the regulation of Airbnb and then use a Spatial RDD around the cities' borders. This ignores however that cities differ in other ways than in their regulation of Airbnb. We address the latter by exploiting variation *over time* in the HSO around the borders of HSO areas. The HSOs induced exogenous changes in the propensity to list a property on Airbnb, which may have resulted in changes in house prices. Consequently, as we will use panel data (for listings as well as house prices), we will employ a Spatial Panel Regression-Discontinuity Design. In this design, we will assume that cross-border spillovers are absent (*i.e.* we assume that the Stable Unit Treatment Value Assumption (SUTVA) holds). We provide ample evidence using graphical as well as econometric analyses. Alternatively, we will also estimate difference-in-differences models (for rents, but also for prices), which do not rely on this assumption.

2.4.1 HSOs and Airbnb listings

The first step is to estimate the effect of the HSO on a property's probability of being listed on Airbnb. We distinguish between the probability of being listed as an entire home and the probability of being listed as rooms. We will estimate linear probability models, where we estimate the effects of the HSO on both probabilities separately.[19] We use a Spatial RDD, where the running variable is the distance to the nearest border of an area where an HSO is implemented or will be implemented in the future. The effect of the HSO is captured by a discrete jump in the probability of being listed after its introduction.[20] Let ℓ_{ikt} be a dummy variable indicating whether a property i near a border of an HSO area k is listed in month t and h_{ikt} be a dummy indicating whether the HSO has been implemented. The variable d_{ik} denotes the distance to the border, where $d_{ik} > 0$.

One may argue that differences in unobservables of properties between HSO areas and neighboring areas may be correlated to the implementation of an HSO. For example, differences in the attractiveness of certain locations that are discrete at, or even further away from, the border (*e.g.*, school quality) may be present, which are correlated to h_{ikt} and influence ℓ_{ikt} at the same time. We, therefore, include property fixed effects λ_i, which control for difficult-to-observe but time-invariant differences between locations, and μ_{kt}, which capture HSO-border area by months fixed effects. More specifically, these are dummy variables that are equal to one on both sides of the shared border between two adjacent cities (or a neighborhood in the City of LA) in a specific month (hence, we include a fixed effect for each month/web scrape in each HSO-border area). This implies:

$$\begin{aligned}\ell_{ikt} = \alpha h_{ikt} + (\psi_1 + \psi_2 t) h_{ikt} d_{ik} + (\psi_3 + \psi_4 t)(1 - h_{ikt}) d_{ik} + \\ \lambda_i + \mu_{kt} + \xi_{ikt}, \quad \text{if} \quad d_{ik} < b,\end{aligned} \tag{2.1}$$

where α is the parameter of interest and ψ_1, ψ_2, ψ_3, ψ_4, λ_i and μ_{kt} are other parameters to be estimated. In this specification, 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 treat-

[19] Our motivation not to estimate multinomial discrete choice models, but to estimate separate models is that, by construction, listings in our data never switch between being listed as an entire home and rooms. Note that homeowners may switch listings, but this will be recorded as being a new listing. In any case, switching types will not affect the consistency of our methodology).

[20] Note that in our data we observe listings. A listing always refers to a certain property, but sometimes properties change listing so different listings may refer to the same property inducing a slight loss in efficiency of the estimates when using property fixed effects.

ment. ψ_2 and ψ_4 aim to capture differences in those trends over time by including a linear interaction with time. Note that because we include property fixed effects, λ_i, we effectively only use data on properties that have been listed at least once. It also implies that $h_{ikt}d_{ik}$ and $1-h_{ikt}d_{ik}$ are perfectly collinear with λ_i, which we address by imposing that $\psi_3 = \psi_4 = 0$. Hence, in essence, we have a regression-discontinuity design, which aims to identify a discontinuity in changes over time in listings at the border, where we allow for different distance-time trends at both sides of the border.

In this setup, we only include observations that are within a small distance b of a border of an HSO. We use a uniform kernel function with a bandwidth b, and do not include higher-order polynomials of the border trends (see Imbens and Lemieux, 2008). This approach is supported by Gelman and Imbens (2016) who show that such an approach is preferred over specifications including high-order polynomials of the running variable.

In RDDs, estimated parameters are often sensitive to the choice of the bandwidth b. We, therefore, show results for different bandwidths. Our preferred specification is based on an approach proposed by Imbens and Kalyanaraman (2012) to determine the optimal bandwidth, b^*, which is calculated conditional on control variables (property fixed effects and HSO-area$\times$month fixed effects). We discuss the procedure to determine b^* in more detail in Appendix 2.9. In our context, the optimal bandwidth is about 1.8 km, so quite small. Importantly, we show that our results are rather insensitive to the choice of bandwidth, also when choosing much smaller bandwidths.[21]

2.4.2 HSOs and house prices

We employ a similar approach to measure the effect of the HSO on house prices. The main difference is that we include census block fixed effects rather than property fixed effects, as we have fewer repeated observations. Let p_{ijt} be the house price of property i in census block j near a border of an HSO area k in month t with time-invariant housing characteristics x_{ijk}. We estimate:

$$\begin{aligned}\log p_{ijkt} = \beta h_{ijkt} + \zeta x_{ijk} + (\omega_1 + \omega_2 t) h_{ikt} d_{ik} + (\omega_3 + \omega_4 t)(1 - h_{ikt}) d_{ik} + \\ \eta_j + \theta_{kt} + \epsilon_{ijkt}, \quad \text{if} \quad d_{ik} < b,\end{aligned} \tag{2.2}$$

where β is the parameter of interest. Similar as above, ω_1, ω_2, ω_3 and ω_4 capture parameters related to the spatial trends before and after the treatment (first difference) and over

[21] When choosing very small bandwidths (<350m), the estimates become less precise. For that reason, we will also estimate (2.1) while imposing that $\psi_1 = \psi_2 = \psi_3 = \psi_4 = 0$. This is essentially a 'non-parametric' approach as discussed by Imbens and Lemieux (2008), and applied by Dube et al. (2010). Usually, the bias of this estimator is anticipated to be relatively high but is expected to be small in our context, because within a few hundred meters, it is plausible that the spatial variation in the listing rate within the areas at both sides of the border is absent.

time (second difference).[22] η_j and θ_{kt} refer to census block and HSO border×month fixed effects respectively. We calculate standard errors by clustering at census blocks. This equation implies that we compare price changes along the borders of HSO areas to see if prices have changed in the treated areas due to the HSO. Again, we will show results given different bandwidths, but our preferred specification is based on the optimal bandwidth.

The above approach ignores that there may be variation over time in the effect of HSOs. This is important, as anticipation effects of new laws may underestimate the effects of HSOs. Furthermore, we wish to take into account that house prices usually adjust gradually over time (implying that long-term effects may be stronger).[23] In the empirical analysis, we estimate specifications where we allow the HSO-effect to be time-specific, so we are also able to test for anticipation and adjustment effects of HSOs.

2.4.3 HSOs, Airbnb listings and rents

An HSO may also affect rents. A reduction in short-term rentals may lead to a reallocation of existing housing stock towards the long-term rental market away from private housing used for short-term renting, increasing the supply of available rental stock for locals, which should decrease rents.

In contrast to house prices, given the assumption of a spatial equilibrium, long-term rents should *not* be different *at* HSO borders given two assumptions: *(i)* rental properties at different sides but very close to these borders are close substitutes and offer the same value to renters; and *(ii)* renters are not allowed to list their property on Airbnb.

We will test the first assumption by estimating regressions where we only include properties close to HSO borders (*i.e.* 1km), which should lead to a statistically insignificant rent effect. The second assumption is also likely to hold, as in Los Angeles almost all rental leases include a provision explicitly forbidding to sublet the property (Lipton, 2014).

Given that theory does not suggest a discontinuity in rents at the border and that we have information on rents at the zip code level (which would make the use of a discontinuity design in any case less convincing), we pursue a standard difference-in-differences approach where we regress rents, r_{jt}, on h_{jt}, where j refers to zip codes areas. We then have:

$$\log r_{jt} = \phi h_{jt} + \eta_j + \theta_t + \epsilon_{jt}, \tag{2.3}$$

where ϕ is the parameter of interest, η_j are zip code fixed effects, and θ_t are month fixed effects. This is a standard difference-in-differences specification, with the notion that

[22] ω_3 and ω_4 are now identified because we include census block, rather than property, fixed effects.

[23] Moreover, we will see that the HSO-induced reduction in listings is limited within the first year after the introduction, making it more plausible that the price reaction will be slower.

we have multiple treatments at different times in our study period.[24]

The key assumption underlying a DiD strategy is that there is a common trend between the treatment and control group. This assumption cannot be tested, but, as is standard, we examine this concern by undertaking an event study in the empirical analysis and show that there is no statistically significant effect before the HSO was implemented, which suggests (but does not prove) that the common trend assumption holds. Importantly, this strategy is less convincing than the Panel RDD. We will demonstrate that when applying a DiD strategy to house prices, then the house price effects are comparable to the ones obtained using the more credible Panel RDD approach. The latter makes it plausible that the rent results are reliable.

2.4.4 The effects of Airbnb listings on house prices and rents

The results from equation (2.2) are informative on the *average* treatment effect of the HSO on house prices, where the average applies to estimates along the borders of HSO areas. However, it is plausible that the effect strongly varies over space depending on local tourist demand for accommodation. The latter strongly covary with the demand for Airbnb, captured by the listings rate a_{ijkt}, potentially reducing the external validity of the estimated average treatment effect. In particular, one expects that areas that are popular with tourists are more strongly affected than areas that are not. We will therefore also estimate the effect of the listings rate in the direct vicinity, a_{ijkt}, on prices using an IV approach.[25] Because a_{ijkt} is endogenous (as listings are imputed and so are measured with error, and residents and visitors have preferences for similar locations), we use arguably-exogenous variation in the listings rate caused by HSOs.

The second stage is then given by:

$$\begin{aligned}\log p_{ijkt} = \gamma \hat{a}_{ijkt} + \zeta x_{ijk} + (\omega_1 + \omega_2 t) h_{ikt} d_{ik} + (\omega_3 + \omega_4 t)(1 - h_{ikt}) d_{ik} + \\ \eta_j + \theta_{kt} + \epsilon_{ijkt}, \quad \text{if} \quad d_{ik} < b,\end{aligned} \tag{2.4}$$

where $\hat{a}_{ijkt}$ is obtained from:

$$\begin{aligned}a_{ijkt} = \tilde{\delta} h_{ijkt} + \tilde{\zeta} x_{ijk} + (\tilde{\omega_1} + \tilde{\omega_2} t) h_{ikt} d_{ik} + (\tilde{\omega_3} + \tilde{\omega_4} t)(1 - h_{ikt}) d_{ik} + \\ \tilde{\eta}_j + \tilde{\theta}_{kt} + \tilde{\epsilon}_{ijkt}, \quad \text{if} \quad d_{ik} < b,\end{aligned} \tag{2.5}$$

where the $\sim$ refer to first-stage coefficients and $\tilde{\delta}$ is the effect of the HSOs on the listings rate. We expect $\tilde{\delta}$ to be negative. We also apply an IV approach to determine the effects of the listing rate on rents, where we control for zipcode and month fixed effects as in (2.5).

[24] We make sure that using a weighted measure based on the number of housing units per area leads to similar results.

[25] We refer to section 2.3.1 for how we constructed the listings rate variable.

2.5 Graphical evidence

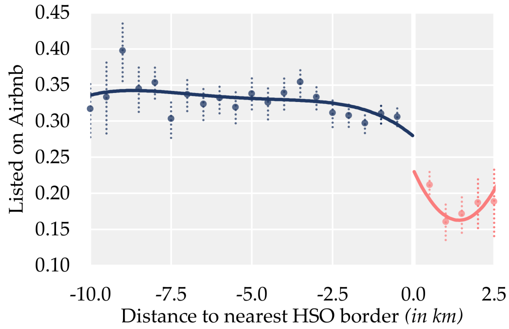
Before we turn to the regression results, we illustrate our research design graphically. In Figure 2.3a, we first focus on the impact of the HSO on Airbnb listings. We include property and border segment×month fixed effects, and include a 4th-order polynomial of distance to the border outside HSO areas and a 2nd-order polynomial of distance to the border multiplied by the treatment inside treated areas (as we have fewer data points that are closer to the border inside HSO areas).[26] The inclusion of property and border segment×month fixed effects implies that we identify the effects *over time*. In Figure 2.3a, we plot the conditional probability of listing on Airbnb. We observe a sizable drop in the type of listings in areas where HSOs have been implemented. The difference is about 8 percentage points.[27] Given a listing probability of about 0.30 (for residences that have been listed at least once), this implies a reduction in listings of 27%. Hence, in line with anecdotal evidence, this suggests that the HSO was very effective in reducing STRs.

Figure 2.3a is also important, as it provides evidence of the complete absence of cross-border crossings of listings due to the HSO. We do not find any evidence that a drop in listings at the HSO side of the border is accompanied by an increase in listings just at the other side (if anything, the figure implies the opposite). We come back to this issue in section 2.6.1.

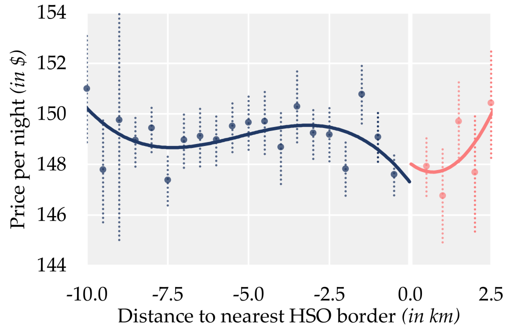
Let us now investigate whether there are differences in changes of characteristics of houses listed on Airbnb between treated areas and areas in the close vicinity. Figure 2.3b shows that there is essentially no difference in how Airbnb prices per night and availability changed over time between HSO areas and neighboring areas. Hence, it is not the case that properties just outside HSO areas become more expensive. The latter suggests that the demand for Airbnb listings is locally elastic and the market is extremely competitive, which is consonant with the absence of cross-border listing effects. Some HSOs still allow for room rentals. In Figure 2.3c we investigate if there is a decrease in the share of listings of entire homes relative to rooms. We do not find a statistically significant jump in the change in the share of entire homes at the border. In Figure 2.3d we show that the type of accommodations on offer does not seem to change

[26]The choice of the order of the polynomial does not make any difference. This indicates that displacement effects – Airbnb hosts that move their listings to a location just outside a treated area – are unlikely to be important, as displacement effects would have induced an increase in listings just outside treated areas.

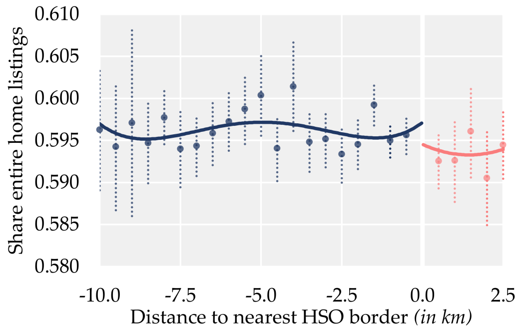
[27]The standard error becomes smaller close to the border because the estimated effect at the border does not depend on the estimated polynomial of distance, as the distance is zero at the border given the chosen specification. In Appendix 2.9 we also compare the probability of being listed *before* and *after* the HSOs were implemented on both sides of the border, without conditioning on census block group fixed effects. This analysis suggests there was essentially no difference between HSO areas and surrounding areas in terms of the number of listed entire properties before the implementation, whereas the probability is about 10-20 percentage points lower after it was implemented, in line with Figures 2.3a.



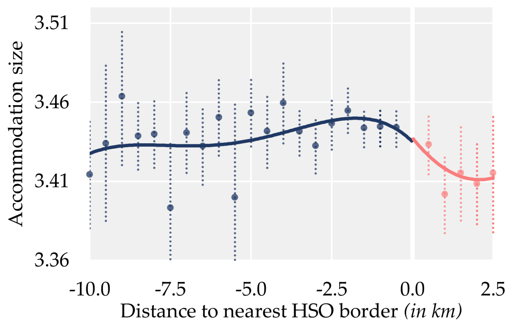
(a) Listing probability



(b) Price per night *(in $)*



(c) Accommodation type



(d) Accommodation size

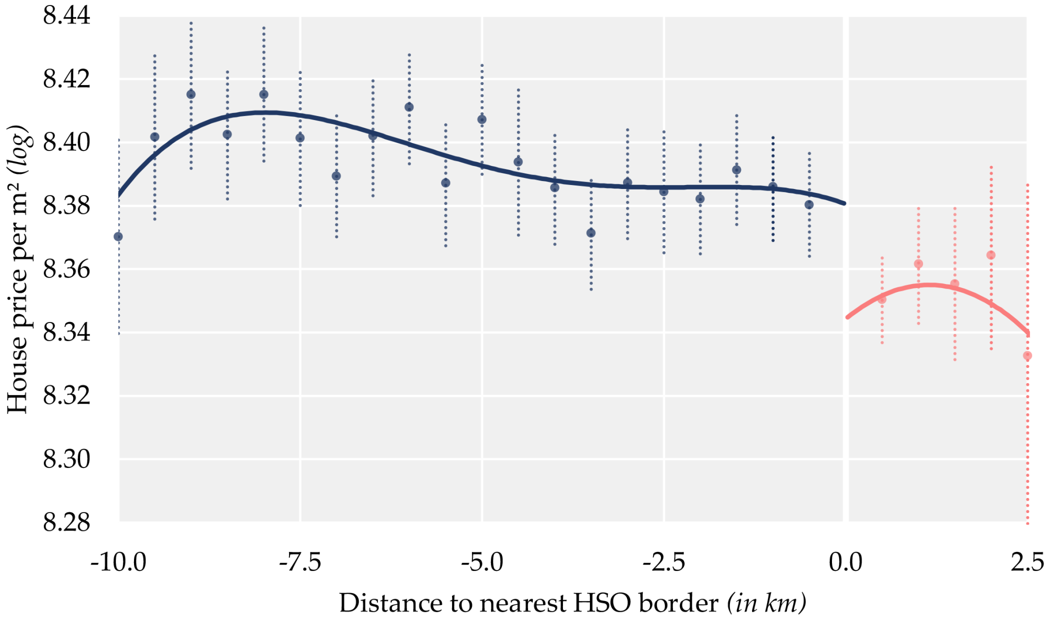
Notes: Spatial differences in variables are *conditional* on property (or listing) and border segment×month fixed effects. Hence, we identify the effects *over time*. Negative distances indicate areas outside HSO areas and areas inside HSO areas but *before* treatment. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block level. We include a 4$^{\text{rd}}$-order polynomial in untreated areas and a 2$^{\text{nd}}$-order polynomial in treated areas.

Figure 2.3: Airbnb listings: variation near the HSO borders

due to HSOs, as the change in accommodation size is not statistically significantly different at the border.

We repeat the exercise but now focus on house prices. The results are reported in Figure 2.4. Prices *decrease* by about 4% at the HSO border. It appears that this effect is highly statistically significant. One may be concerned that this result is mainly explained by the very local decrease in house prices within 500m of the border. In the next section we show that, once we include more detailed census block or property fixed effects, the estimated effect becomes more precise and is very robust to bandwidth choice. Again we do not find any evidence of cross-border effects, as house prices close to but just outside HSO areas are not higher. In Appendix 2.9, we further investigate whether discontinuities in changes in housing characteristics exist at the border. We do not find evidence for this.

In spatial RDDs, one should be concerned about sorting. It might be that a discontinuity in prices due to implementation is partly caused by a discrete change in the de-



Notes: Price differences are *conditional* on census block and border segment×month fixed effects. Hence, we identify the effects *over time*. Negative distances indicate areas outside HSO areas and areas inside HSO areas but *before* treatment. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block level. We include a 4^{rd}-order polynomial of price differences in untreated areas and a 2^{nd}-order polynomial in treated areas.

Figure 2.4: House prices: variation near the HSO borders

mographic composition of the neighborhood around the border (see Bayer et al., 2007, for cross-sectional evidence on school districts). We do not find any evidence of this in Appendix 2.9.

In a non-spatial RDD, it is common to investigate whether the density of the running variable is continuous at the threshold because a discontinuity reveals that some individuals manipulate their position around the threshold. In spatial RDDs – using data on the housing stock in built-up areas – manipulation is less of an issue because real estate hardly changes in the short term (in the absence of notable large-scale demolitions of buildings or new constructions). We investigate changes in the density of listings and transactions before and after the HSO was implemented using McCrary's (2008) methodology. In Appendix 2.9 we do not find meaningful differences in changes in densities across borders before HSOs were implemented.

2.6 Results

2.6.1 HSOs and Airbnb listings

In Table 2.4 we report the baseline results of the impact of HSOs on Airbnb listings. In Panel A, we focus on listings of entire homes or apartments. In column (1) we start with the RDD using the Imbens and Kalyanaraman-bandwidth, which includes observations up to 1.67km of the nearest HSO border. The result points towards a strong reduction in Airbnb listings of 6.1 percentage points after the implementation of the HSO. Given that the share of listings around the border was about 0.3 before implementation, this implies a decrease in listings of 20%.

In column (2) we add border segment×month fixed effects. That is, we determine for each HSO area the segment of the border that is shared with another city (or neighborhood in the City of Los Angeles). In this way, we mitigate issues related to differences in the provision of public goods. Although this implies the inclusion of 1350 instead of 270 fixed effects, this hardly impacts the results (the R^2 is not much impacted either, so arguably this is not very informative on omitted variable bias, see Oster, 2019).

Imbens and Lemieux (2008) and Lee and Lemieux (2010) stress the importance of showing the robustness of the results to the choice of bandwidth. In column (3) we, therefore, multiply the optimal bandwidth by 2 and in column (4) divide it by 2. The point estimates range between 4.4 and 8.2%. One may still be concerned that the bandwidth is on the high side. We, therefore, divide the optimal bandwidth by 5 in column (5), so that we include only observations within 334m of the borders. We find a lower, albeit somewhat less imprecise, effect of 2.9 percentage points. The somewhat lower effect is not surprising as measurement error in the location is amplified when focusing only on listings close to the border. Reducing the distance even further is not informative, as the location of listings is known up to a 200m radius.[28] The finding that the effect is similar for very small bandwidths is particularly important as this implies that listings do not move just across the border of an HSO area, which would imply that we may overestimate the effects of HSOs.

In column (6) of Panel A, we make a distinction between different types of HSOs. Recall that four cities that have implemented HSOs still allow for room rentals. As we focus here on listings of entire homes, one expects that the different types of HSOs have similar effects. We, therefore, include an interaction of the HSO with a dummy indicating whether letting of rooms is allowed. In line with expectations, we do not find that the effect on listings of entire properties – which are always restricted – is different between the two types of HSOs.

[28] When we apply the 'non-parametric' approach, implying that $\psi_1 = \psi_2 = \psi_3 = \psi_4 = 0$, on observations within 334m of the borders, we find a 7.1 percentage point effect, precisely estimated with a standard error of only 1.4.

The imprecise reporting of the location by Airbnb (*i.e.* the location of listings is accurate up to a distance of 200m) may affect our estimates, as it implies a misclassification error in the treatment variable if Airbnb misreports the city of each listing. This should lead to underestimates as the treatment variable is observed with measurement error.[29] To examine this, we have estimated models where we exclude observations within 200m of the border, see column (7) in Table 2.4. We indeed find a slightly stronger effect.[30]

In Panel B of Table 2.4 we analyze the effects of HSOs on listings of rooms. We repeat the same set of specifications as in Panel A. The effect is about 50% smaller than for entire homes/apartments. More specifically, the coefficient in column (1) implies that the probability to list a room has decreased by 2.5 percentage points. This effect is somewhat stronger (-3.6 percentage points) once we include border segment$\times$month fixed effects. Given an average probability to be listed of 0.28, this implies a decrease of 13%. The finding that the percent effect on room rentals is smaller makes sense as some cities do not completely forbid room rentals (*e.g.* Santa Monica). If we include border segment$\times$month fixed effects (column (2)) or change the bandwidth (columns (3) to (5)), this leaves the results essentially unaffected.[31]

In column (6) we again include an interaction of the HSO with a dummy indicating whether room rentals are allowed. As one expects, we do not find that rooms listings have been reduced in areas where room rentals are still allowed, whereas rooms listings have been substantially reduced in areas where short-term renting is completely banned, with a percentage point reduction that is about the same as for entire homes/apartments. We think this provides strong evidence that the changes in the listing probabilities are related to the implementation of HSOs. Column (7) highlights that measurement error is not really an issue, as it leads to almost the same estimate as the baseline estimate.

[29] One may argue that there may also be measurement error in *the running variables*, *i.e.* the distances to the border. Measurement error usually induces bias in the estimates, potentially even more so within a regression discontinuity framework (Davezies and Le Barbanchon, 2017). Arguably, the bias in our HSO estimates will be small, because the extent of the measurement error is small. For example, given the plausible assumption that measurement error is uniformly distributed between 200m, the measurement error variance appears only 5-10% of the distance variances (on both sides of the border), indicating that the attenuation bias in the effect of the running variables should be an order of magnitude smaller than the estimated effect size of the running variables. As this argument may not be entirely convincing, we have examined the importance of measurement error by estimating models, while excluding the distance to the border variables. We find then almost the same HSO effects, indicating that measurement error is unlikely to affect our estimates.

[30] We have also estimated models using an RDD, where the probability of treatment is assumed to be a function of the distance to the border, given the assumption that measurement error in the distance is uniformly distributed within 200m, which is inspired by Hullegie and Klein (2010). Again we find similar estimates.

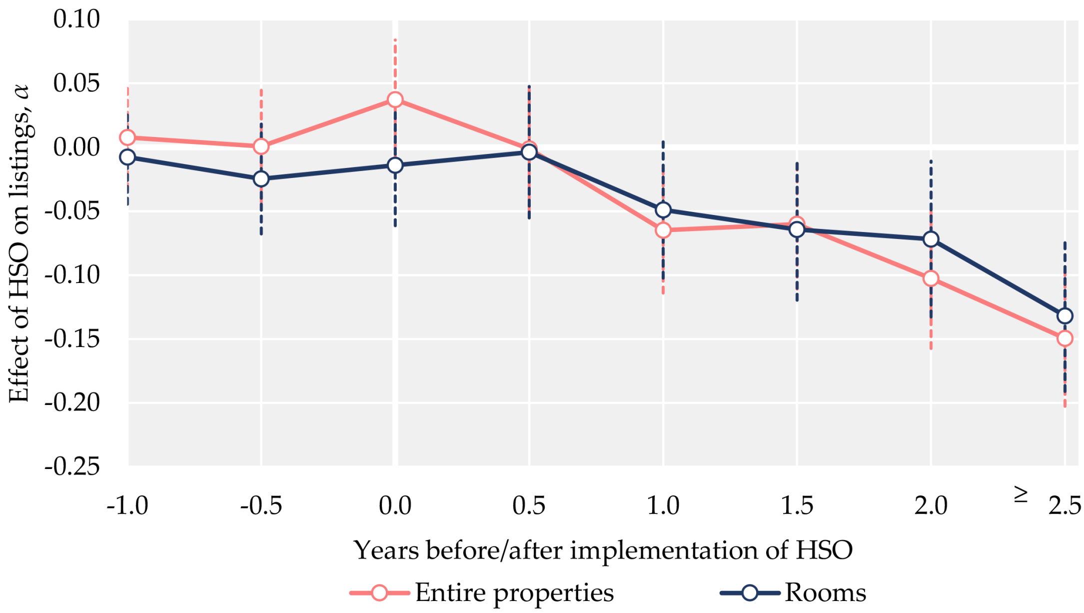
[31] When we exclude the spatio-temporal trend variables on the observations as in (5), we find a coefficient of 0.036 percentage point effect, with a standard error of 0.02.

Table 2.4: Baseline results for Airbnb listings

(Dependent variable: Airbnb property is listed)

	Panel RDD	*+ Border segment f.e.*	*Bandwidth: $h^* \times 2$*	*Bandwidth: $h^*/2$*	*Bandwidth: $h^*/5$*	*Rooms not allowed*	*Measurement error*
Panel A: Entire homes/apartments	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HSO implemented	-0.0614***	-0.0696***	-0.0815***	-0.0444***	-0.0290		-0.1058***
	(0.0122)	(0.0123)	(0.0110)	(0.0154)	(0.0226)		(0.0168)
HSO implemented× rooms allowed						-0.0682***	
						(0.0205)	
HSO implemented× rooms not allowed						-0.0701***	
						(0.0127)	
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	270,906	270,621	425,117	154,015	80,896	270,741	253,448
Bandwidth, b *(in km)*	1.6716	1.6708	3.3416	.8354	.3342	1.6712	1.9639
R^2	0.3481	0.3515	0.3550	0.3481	0.3439	0.3514	0.3546
Panel B: Rooms	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HSO implemented	-0.0253	-0.0363**	-0.0436***	-0.0296	-0.0082		-0.0551**
	(0.0170)	(0.0172)	(0.0156)	(0.0226)	(0.0315)		(0.0224)
HSO implemented× rooms allowed						0.0309	
						(0.0264)	
HSO implemented× rooms not allowed						-0.0595***	
						(0.0187)	
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Property fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	171,778	171,448	259,880	94,365	45,267	171,433	156,710
Bandwidth, b *(in km)*	1.815	1.812	3.3384	0.8346	0.3338	1.8117	2.0061
R^2	0.3339	0.3438	0.3424	0.3524	0.3558	0.3439	0.3482

Notes: We exclude within 200m of the borders of HSO areas in column (7). Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.



Notes: The optimal bandwidth $b^* = 1.6692$ for 'entire properties' and $h^* = 1.8120$ for 'rooms'. The dashed lines denote the 95% confidence bands.

Figure 2.5: An event study to the effect of the HSO on Airbnb listings

In Figure 2.5 we show an event study on how the effect of the HSO on Airbnb listings varies *over time* by re-estimating our preferred specification shown in column (2) of Table 2.4, while interacting the effect of the HSO with time dummies. Our reference category then is the whole period until one year before the HSO as well as areas that are never treated. Just before and at the moment of implementation, there is no effect of the HSO. Hence, there do not seem to be pre-trends in listings related to the implementation of HSOs. However, after a year, we find a (marginally) statistically significant reduction in listings of entire properties of about 6.5 percentage points. After 2.5 years, the effect has increased to 15 percentage points for entire homes, which implies a reduction in listings of almost 50%. Therefore, in the long-run, the HSO had a very strong effect on the listings of entire properties. A similar pattern emerges for room listings, where we find that the long-run decrease in listings is 13 percentage points (or 47%). Why does the effect become stronger over time? One explanation is that, in the beginning, households/investors did not yet know whether and to what extent the ordinance would be enforced. After a while, it became clear that it was being enforced, implying potentially hefty fines.[32]

We also investigate the effects of the HSO on Airbnb rental prices of properties in Ap-

[32]In Appendix 2.9 we investigate the effects of the HSOs on the listing probability as well as prices for each city separately. We show that the coefficients are generally negative, or when positive, statistically insignificant. However, standard errors are often somewhat large, so we cannot make precise statements for individual cities.

pendix 2.9. We do not expect that *at the border* rental prices do change, because tourists are unlikely to differentiate between accommodation in a treated area and neighboring areas. We indeed find that rental prices of Airbnb properties are not significantly different at the border when applying a panel regression discontinuity design. However, one may expect differences further away from the border if tourists have a strong preference of (not) staying in a certain area. We therefore also estimate DiD models where we exclude properties close to HSO borders (<1km). Still, we do not find any effect of HSOs on Airbnb rental prices. These results are in line with the belief that the market for short-term rentals is highly competitive: restrictions on short-term rental supply by HSOs (as well as additional Transient Occupancy taxes) do not impact the spatial equilibrium of rental Airbnb prices.[33]

2.6.2 HSOs and house prices

We have seen that the HSO strongly reduces the probability of using a property for short-term renting. We expect that this will have a negative effect on house prices. In Table 2.5 we report the results. We start with a Panel RDD, including census block and HSO area×month fixed effects, as outlined above. The results in column (1) indicate a negative effect of the policy of $\exp(-0.0178) - 1 = 1.8\%$.[34]

In column (2) we add border segment×month fixed effects leading to essentially the same result. The results do not materially change when we choose other bandwidths in columns (3) and (4). However, it becomes too imprecise to be statistically significant at conventional levels in column (4). We even further reduce the bandwidth to only 362m in column (5). Now, the point estimate is very close to the baseline estimate in column (2), albeit imprecise. Clearly, columns (4) and (5) indicate that cross-border effects are absent (as otherwise, the point estimates should have increased for shorter distances).[35] Column (6) tests whether HSOs that allow for room rentals have weaker price effects. This appears not to be the case: the price effect in areas that allow for room sharing is not statistically significantly different from the effect of HSOs in areas that do not allow for this. An interpretation is that most of the price effect is caused by investors buying

[33]We also investigate the effects of HSOs on the number of formally registered traveler accommodations in Appendix 2.9, using data from the *County Business Patterns*. Because we have data on only a few years and the data is only available at the zip code level, the results are imprecise. However, the point estimates seem to point towards a sizable 5% increase in the number of formal traveler accommodations after implementation of an HSO. Hence, we interpret this as suggestive evidence that HSOs have led to an increase in formal accommodation.

[34]The housing control variables either have plausible signs and magnitudes or are statistically insignificant. A 1% increase in house size leads to a price increase of 0.5%. We further find that condominiums are approximately 25-30% less expensive than single-family homes. The results are robust to the exclusion of housing controls.

[35]Reassuringly, when we apply the 'non-parametric' approach, implying that $\omega_1 = \omega_2 = \omega_3 = \omega_4 = 0$, to the observations in column (5), we find again a similar effect of -2.2%, which is statistically significant at the 5 percent level, despite the strong reductions in the number of observations.

homes and using them for short-term renting.

Back-of-the-envelope calculations indicate that these results are within a plausible range. For example, using the average list price per night and the average house price, combined with a mortgage interest rate of 3.3% and maintenance costs of 3%, implies that typical hosts who rent out their property on Airbnb for 10 nights per year earn revenue from short-term renting equivalent to 2.5% of their housing expenditure, suggesting that house prices would increase by that amount (in the absence of variable costs, such as cleaning, changing sheets). This calculation ignores the effect of professional investors, who typically outbid households, suggesting that much higher price effects are plausible if the listings rate of Airbnb properties is substantial.[36]

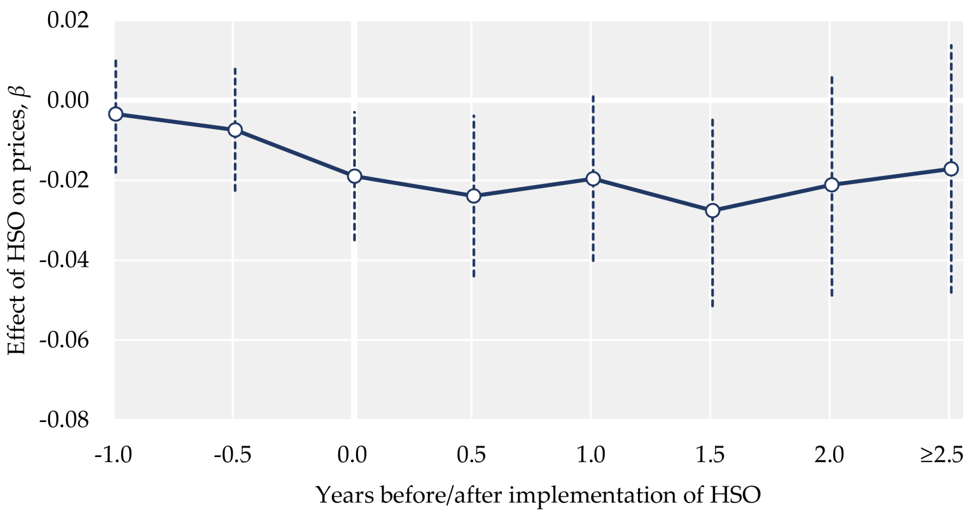
[36]Professional investors' daily revenue from renting out short-term is about twice the daily revenue from renting out long-term. Given that the renting costs (excluding the capital costs of acquiring the property) are about 20% of the revenue (here we use information from agencies that manage short-term renting for households), then the willingness to pay by professional investors exceeds those of the current marginal house owners by about 60%.

Table 2.5: Baseline results for house prices

(Dependent variable: log of house price)

	Panel RDD	*+ Segment month f.e.*	*Bandwidth: $h^* \times 2$*	*Bandwidth: $h^*/2$*	*Bandwidth: $h^*/5$*	*Rooms not allowed*
	(1)	(2)	(3)	(4)	(5)	(6)
HSO implemented	-0.0178**	-0.0177**	-0.0209***	-0.0133	-0.0195	
	(0.0071)	(0.0078)	(0.0069)	(0.0098)	(0.0150)	
HSO implemented× rooms allowed						-0.0190*
						(0.0109)
HSO implemented× rooms not allowed						-0.0173**
						(0.0082)
Property size *(log)*	0.4988***	0.4952***	0.4876***	0.5002***	0.5373***	0.4952***
	(0.0100)	(0.0090)	(0.0077)	(0.0116)	(0.0174)	(0.0090)
Parcel size *(log)*	0.0425***	0.0436***	0.0383***	0.0415***	0.0282***	0.0436***
	(0.0045)	(0.0040)	(0.0039)	(0.0053)	(0.0064)	(0.0040)
Bedrooms	0.0047**	0.0051**	0.0019	0.0083***	0.0043	0.0051**
	(0.0023)	(0.0022)	(0.0018)	(0.0029)	(0.0041)	(0.0022)
Bathrooms	0.0184***	0.0184***	0.0219***	0.0131***	0.0161***	0.0184***
	(0.0026)	(0.0026)	(0.0022)	(0.0034)	(0.0051)	(0.0026)
Apartment	-0.3170***	-0.3200***	-0.3226***	-0.3135***	-0.3377***	-0.3200***
	(0.0109)	(0.0107)	(0.0103)	(0.0129)	(0.0189)	(0.0107)
Construction year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes
Number of observations	63,487	63,275	98,594	39,192	19,110	63,276
Bandwidth, b *(in km)*	1.8029	1.8087	3.6174	0.9044	0.3617	1.8088
R^2	0.9024	0.9090	0.9052	0.9113	0.9218	0.9090

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.



Notes: The optimal bandwidth $b^* = 1.8089$. The dotted lines denote the 95% confidence bands.

Figure 2.6: An event study to the effect of the HSO on prices

A well-known issue with exploiting changes in house prices over time is that one has to take anticipation effects into account. Anticipation effects may have been important as discussions on the HSO predate implementation. On the other hand, it might have taken some time before the HSO capitalized into house prices. We have tested this, with results shown in Figure 2.6. We find that before implementation of the HSO there is no statistically significant price decrease (compared to the period until one year before the HSO), suggesting the absence of anticipation effects. At the moment of implementation, we find that prices are about 2% lower. The price effect is stable over time. The absence of sizable anticipation effects also implies that there is no strong evidence for pre-trends, which would potentially invalidate our research design. We test more extensively for pre-trends in Appendix 2.9 where we include data from earlier years.

In Appendix 2.9, we investigate to what extent negative external effects related to tourism play a role. Recall that the estimates discussed above are the *net* effects of 2 opposing mechanisms: the first is that the HSO reduces demand for housing, which decreases house prices. The second mechanism is that it reduces negative tourist externalities, which in turn increases house prices. Alternatively, because we find that the net effect of the HSO is negative, the estimates may be interpreted as underestimates of the efficient use effect, where the size of the underestimate depends on the size of the externality effect. We do not find strong evidence for the presence of a local external effect, implying that the estimated effect of the HSO almost exclusively reflects an efficient use effect. We also make sure that the results hold for median prices in the Zillow data: we show that the house price effects using the DiD estimation strategy deliver similar results as the ones reported in Table 2.5. This suggests that the DiD strategy which we will apply to rents is a plausible alternative estimation strategy. Moreover, these ancillary regressions highlight that the average treatment effect identified through a Panel RDD is about equal to the average treatment effect identified through a DiD strategy.

2.6.3 Placebo checks and sensitivity

It is important to show the robustness of our results. In this subsection, we will show some 'placebo'-estimates and summarize the most important robustness checks. In Table 2.6 we estimate regressions where we consider placebo HSOs for other areas. Panel A reports the results for the effects on listings, while Panel B investigates the effects on house prices.[37]

One obvious candidate for a placebo-test is to shift the borders of HSO areas 1km outwards to make sure that we do not capture some spatial trend that is correlated to the treatment variables. It seems that this is not an issue, as the effects of the placebo-HSOs on listings and house prices are statistically indistinguishable from zero.

In the second placebo test, we investigate the issue that in some cities Airbnb is officially not allowed because the zoning code does not allow for short-term renting, but as discussed in Section 2.2, these zoning codes are not enforced. We treat those cities (listed in Appendix 2.9) as if an HSO would have been implemented. To determine the timing of the placebo HSOs for each of those cities, we take the timing of the nearest city that has implemented an HSO. The results in column (2) confirm that those cities do not see a decrease in listings or house prices.

[37] In Panel A, we exclude transactions within 200m of HSO areas because the location of listings is known up to 200m.

Table 2.6: Placebo estimates

Panel A: (Dep.var.: Airbnb property is listed)	*Shift border 1km outwards* (1)	*Areas with zoning code* (2)	*City of LA* (3)	*Unincorporated areas* (4)	*5 years earlier* (5)	*10 years earlier* (6)
Placebo-HSO implemented	0.0076	-0.0164	-0.0081	0.0007		
	(0.0105)	(0.0202)	(0.0076)	(0.0158)		
Spatio-temporal trend variables	Yes	Yes	Yes	Yes		
Property fixed effects	Yes	Yes	Yes	Yes		
Border segment×month fixed effects	Yes	Yes	Yes	Yes		
Number of observations	371,565	452,385	717,315	271,215		
Bandwidth, *b (in km)*	1.5145	1.3981	1.0593	1.5953		
R^2	0.3550	0.3713	0.3615	0.3786		
Panel B: (Dep.var.: log of house price in \$)	(1)	(2)	(3)	(4)	(5)	(6)
Placebo-HSO implemented	-0.0058	0.0100	-0.0088	0.0164*	-0.0120	-0.0108
	(0.0186)	(0.0068)	(0.0092)	(0.0092)	(0.0104)	(0.0088)
Property characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	53,248	123,250	102,249	94,331	60,323	68,266
Bandwidth, *b (in km)*	1.8705	1.3014	1.4922	1.345	1.2936	1.9172
R^2	0.9068	0.9076	0.9109	0.9087	0.9029	0.8654

Notes: In Panel A, we exclude listings within 200 meters of HSO areas (because the location is known up to 200m). In Panel B we exclude transactions in HSO areas. Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

As a third placebo check, we treat each neighborhood in the City of Los Angeles with a placebo HSO. This is relevant as the City of LA had plans to restrict Airbnb. At the time of writing, Airbnb is still allowed, but hosts may only operate one short-term rental at a time and will only be able to rent out their properties for 120 days a year. Again, to determine the timing, for each neighborhood in LA, we take the nearest city that has implemented an HSO. Column (3) in Table 2.6 shows that there is no effect of this placebo HSO on listings or prices.

Column (4) continues by checking whether 'unincorporated' areas, which have identical regulations concerning public goods and STRs, have seen changes in listings and prices. To determine the timing of the placebo HSOs we again use the date of implementation of the nearest HSO area. The coefficients clearly indicate that there is no effect of the placebo HSO. We find a slight *positive* effect on prices, but we think this is merely a Type II error, given the absence of an effect on listings.

In the final placebo checks, we investigate whether we can detect any effect on housing prices using data from exactly 5 and 10 years earlier (from 2009 until 2013 and from 2004 until 2008) and assume that the HSO would have been implemented exactly 5 or 10 years earlier. Because Airbnb data is not available from before 2014, we cannot estimate this placebo test for listings. For house prices, we again find that estimates are economically small and statistically indistinguishable from zero. This is important because this suggests that there are not statistically significant pre-trends in prices that may explain the price effect we find in our analysis.

Therefore, the placebo-estimates reported in Table 2.6 confirm that the finding of a reduction in listings and house prices due to implementation of the HSO is not a statistical artifact and unlikely the result of a differential provision in the change of public goods or other regulation

We subject this conclusion to a wide range of other sensitivity checks in Appendix 2.9. More specifically, in Appendix 2.9 we report results where we estimate city-specific effects for the effects of HSOs on listings and house prices, as discussed earlier. Appendix 2.9 investigates whether the HSO impacted rental prices of Airbnb. As mentioned earlier, we do not find that this is the case. On the other hand, we find suggestive evidence that the number of formal accommodations has increased in HSO areas (see Appendix 2.9). Appendix 2.9 further investigates the possibility of pre-trends in prices. In Appendix 2.9 we investigate whether standard errors change when taking into account cross-sectional dependence. We show that standard errors are even somewhat smaller, although very comparable to the baseline estimates where we cluster at the census block level.

Appendix 2.9 reports first-stage results of the impact of the HSO on the listings rate. In Appendix 2.9 we examine robustness of our results if we *(i)* include property rather than census block fixed effects, *(ii)* use flexible distance to the border$\times$year trends instead of choosing a bandwidth, *(iii)* include picture density$\times$year trends to control for changes in attractiveness of touristy areas, *(iv)* control for changes in demographic variables, *(v)* include straight border×year fixed effects to further address any omitted variable bias, the idea being that straight border segments are likely uncorrelated to geographical features of location, as argued by Turner et al. (2014), *(vi)* exclude outliers in the listings rate. The results are generally robust.

2.6.4 HSOs and rents

So far, we focused on the effects of HSOs on house prices. One may wonder whether the results also hold if we extend the analysis to rents. We reiterate here that differences in rents should capture the housing supply effect – short-term rentals may lead to a reallocation of existing housing stock away from the long-term rental market towards privately-owned housing. However, there is a more fundamental difference. In contrast to owners, renters are thought to be indifferent to (otherwise identical) properties that are close to HSO borders, so the use of a Panel RDD is not the appropriate strategy to identify the housing supply effect (as it should show a zero effect of HSOs on rent). We, therefore, use a more standard difference-in-differences strategy and include observations further away from the border. Table 2.7 reports the results. In Panel A we test for the effect of HSOs on rents.

In column (1), Panel A, we show that due to HSOs, rents have decreased by 2.3%. Column (2) shows that the effect is similar when we exclude properties that are further away than 25km from any HSO area, which ensures that we exclude the low-density outskirts of LA County where rent trends may be very different. In column (3), we also drop observations close to (1km) but outside HSO areas. The results indicate an effect that is only slightly stronger (2.2%).

This estimate is very close to the preferred estimate for prices, reported in column (2), Table 2.5. Column (4) explicitly tests whether rents are continuous at the borders of HSO (within 1km of both sides). We indeed find no statistically significant difference between HSO areas and areas outside HSOs. Moreover, the point estimate is very close to zero. This suggests that properties that are close to the HSO border are indeed close substitutes, although we cannot rule out the possibility that this result is driven by a small sample effect. In column (5), Panel A, we control for second-order polynomial distance to the CBD×year and distance to the beach×year trends, leading to slightly lower effects. Finally, we only keep observations in column (6) that are inside HSO areas and further away than 2.5km from any HSO border. We find that rents then decrease by 2% when an HSO is implemented.

Table 2.7: DiD results for rents

(Dependent variable: log of median rent per m²)

	All obs.	*Outside HSO, <25km*	*Outside HSO, >1km, <25km*	*Outside HSO, <1km*	*Outside HSO, >1km, <25km*	*Outside HSO, >2.5km, <25km*
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
HSO implemented	-0.0230***	-0.0201**	-0.0223**	-0.0074	-0.0187**	-0.0202**
	(0.0087)	(0.0088)	(0.0092)	(0.0102)	(0.0082)	(0.0092)
Dist. to CBD×year	No	No	No	No	Yes	Yes
Dist. to beach×year	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,491	3,231	2,951	722	2,951	2,472
R^2	0.9888	0.9838	0.9829	0.9850	0.9841	0.9848

Notes: In all specifications we include observations *inside* HSO areas.
Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We also test whether pre-trends and/or anticipation effects are an issue. Figure 2.7 replicates the specification with distance to CBD and beach trends, zipcode and month fixed effects. This specification only includes zip codes that are further than 1 and less than 25km from a (future) treated area. We find no effect before implementation: the effect is small and statistically insignificant. After half a year, the effect of HSOs becomes statistically significant at the 5% level. The long-run effect after 2 years is about 5%, albeit somewhat imprecise.

2.6.5 Airbnb listings and house prices and rents

One could argue that the average treatment effect estimated around the border of HSO areas does not say much about the effect of Airbnb on house prices, because neighborhoods with a higher tourist accommodation demand are more strongly affected by the ordinances (as a relative decline in the listings probability implies a stronger absolute decrease in the listings rate in areas with a higher initial listings rate). We, therefore, estimate the direct impact of the listings rate on house prices using an IV approach. To deal with endogeneity issues – omitted variable bias and potential measurement error in the listings rate – we employ an instrumental variable approach using the HSOs in the different cities. Because we have seen that listings only gradually reduce after the introduction of the HSO, we exclude observations in the 6 months immediately after the introduction.

Notes: We include observations *inside* HSO areas and observations between 1 and 25km of an HSO area. The dotted lines denote the 95% confidence bands.

Figure 2.7: An event study to the effect of the HSO on rents

Table 2.8 reports the regression results for the two-stage Panel RDD.[38] We observe in Table 2.8 that the instrument is strong in all specifications as the first-stage F-statistic is above the rule-of-thumb value of 10 in all specifications. The first-stage estimates are reported in Appendix 2.9. They indicate that the listings rates have decreased by about 0.4-0.6 percentage points, which is about 50-70% of the mean. In other words, the first-stage results are comparable to what we already established in the previous subsection: the HSO has strongly reduced the number of Airbnb listings.

In column (1), Panel A, we find that a 1 percentage point increase in the Airbnb listings rate increases property prices by 5.1%. In column (2) we include border segment×month fixed effects. The effect reduces to 3%. A standard deviation increase in the listings rate is associated with a $1.845 \times 0.0226 = 5.5\%$ increase in prices, so the effect of Airbnb is substantial. The elasticity of prices with respect to the average listings rate in the sample is $0.0300/0.585 = 0.0513$. When we only focus on areas where an HSO has been implemented this elasticity is very similar and equal to 0.0402.[39] Changing the bandwidth substantially does not change the results much,

[38] Table 2.8 also report the bandwidths. We obtain the bandwidth from the first stage: a regression of the listings rate on the HSO dummy.

[39] These estimates are of a similar order of magnitude as Barron et al. (2021), who use a completely different identification strategy.

although the coefficient becomes imprecise for small bandwidths.[40]

In columns (5) and (6) of Table 2.8 we make sure that the choice to determine the listings rate within 200m is not affecting our results. When we use the listings rate within 100m, the coefficient is 0.0251, which is very similar to the baseline estimate. Moreover, when using the listings rate within 500m the coefficient is slightly higher than the baseline estimate. Hence, our results are rather insensitive to the area choice.

Because we have to impute listings data for the months where we do not have Airbnb data, one may criticize the listings rate variable. To show robustness, we first use only the months for which we have actual Airbnb data. In column (7) we show that this leads to a very imprecise estimate and a rather weak first stage. In any case, note that the point estimate is higher than the baseline estimate.

[40]We also considered to further reduce the bandwidth, as in the previous tables. However, because of a weak first stage, the results are uninformative and imprecise, and available upon request. Given that both reduced-form effects of HSOs on listings and prices are statistically significant for small bandwidths, we do not consider this a major issue.

Table 2.8: Airbnb listings and house prices: 2SLS estimates

(Dependent variable: log of house price)

	Panel RDD	*+ Border segment f.e.*	*Bandwidth: $h^* \times 2$*	*Bandwidth: $h^*/2$*	*Different threshold* 100m	500m	*Selected dates*	*Approximated listings rate*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Listings rate <200m *(in %)*	0.0511**	0.0300*	0.0338**	0.0281			0.0934	0.0444*
	(0.0219)	(0.0156)	(0.0168)	(0.0218)			(0.0679)	(0.0228)
Listings rate <100m *(in %)*					0.0251*			
					(0.0130)			
Listings rate <500m *(in %)*						0.0372*		
						(0.0207)		
Property characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	83,766	83,037	134,074	52,115	91,623	70,232	15,509	82,076
Bandwidth, b *(in km)*	2.9429	2.9257	5.8513	1.4628	3.4276	2.297	3.8509	2.8797
Kleibergen-Paap F-statistic	28.45	57.57	52.25	30.13	33.24	111.6	11.73	55.36

Notes: We exclude transactions occurring within half a year after implementation of the HSO. We instrument the listings rate a dummy indicating whether an HSO has been implemented. Robust standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

To investigate further whether our proxy for Airbnb listings matter, we use a different proxy for Airbnb intensity, by approximating listings using the first and last review and assuming that the property is continuously listed in between, following Zervas et al. (2017) and Barron et al. (2021). The mean *approximated* listings rate is 0.54, which is very comparable to the mean *imputed* listings (0.59). The cross-sectional correlation between the imputed and approximated measures is quite high ($\rho = 0.812$). However, more relevant, as we exploit variation over time in this measure, is that the correlation over time between these two measures is much lower ($\rho = 0.416$). In column (8) we show that we also find a positive and marginally significant effect of this alternative measure. If anything, the impact is somewhat stronger, albeit not statistically significantly different from the baseline estimate.

Table 2.9 focuses on rents. Again we instrument for the listings rate with the HSO dummy. The results show that the instruments are sufficiently strong and in Appendix 2.9 we show that the HSO dummy also has the expected sign and is statistically significant and negative in all cases. In column (1) we find that when the listings rate increases by 1 percentage point (0.69 standard deviations), rents increase by 4.9%. The effect is slightly lower when we only include observations within 25km of an HSO border (column (2)). This effect is comparable to the results we found in Table 2.8.

The effect becomes higher when we exclude zip codes outside HSO areas that are within 1km of an HSO border (column (3)). In line with previous results, we do not find any effect of the listings rate when focusing on zip codes close to HSO borders (column (4)). If we control for distance to CBD×year and distance to the beach×year trends, the effects are comparable. The estimates in columns (5) and (6) are a bit higher than the effects on prices, but note that they are not statistically significantly different from the baseline estimate for prices.

Table 2.9: DiD results for rents

(Dependent variable: log of median rent per m^2)

	All obs.	*Outside HSO, <25km*	*Outside HSO, >1km, <25km*	*Outside HSO, <1km*	*Outside HSO, >1km, <25km*	*Outside HSO, >2.5km, <25km*
	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Listings rate	0.0491**	0.0366*	0.0497**	0.0095	0.0422**	0.0488**
	(0.0241)	(0.0185)	(0.0247)	(0.0133)	(0.0180)	(0.0216)
Dist. to CBD×year	No	No	No	No	Yes	Yes
Dist. to beach×year	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,491	3,231	2,951	722	2,951	2,472
Kleibergen-Paap *F*-statistic	16.61	22.30	15.88	15.65	15.23	10.39

Notes: In all specifications we include observations *inside* HSO areas. We instrument the listings rate with a dummy indicating whether an HSO has been implemented. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

2.7 Overall price effects

We continue to calculate the overall effects of Airbnb and HSO on the housing market using our IV estimates reported in column (2) of Table 2.8. As we use a spatial regression discontinuity approach, these effects reflect the preferences of local households who live around the borders of the boundaries. We will assume that the estimated local effects apply to the whole county. In principle, we don't know to what extent these local estimates are representative of the whole county. However, in additional analysis, where we do not control for the distance to the border, we get somewhat larger effects of listings on prices, suggesting that the estimate we use is likely conservative. Furthermore, when we apply a difference-in-difference strategy we obtain similar results.

In Table 2.10 we investigate the total effects of Airbnb and HSOs on average property prices for LA County as a whole and for specific areas, based on our estimates combined with descriptive information on house prices and the number of listings in these areas. To be more precise, we evaluate the total effect of Airbnb using the listings rate in these areas as of September 2018. We then consider two counterfactual scenarios; one where no HSOs would have been implemented and another where HSOs would apply to all cities in LA County. As the rent effect is very similar to the effects on prices (for which we provided evidence in Section 2.6.4), we just report price effects here, and use a discount rate of 3.3% (obtained from Koster and Pinchbeck, 2022).

In all scenarios, we assume that the effect of Airbnb listings is linear.[41] This assumption is likely innocuous when we focus on the price effects for LA County as a whole, and likely reasonable when we focus on areas with listings rates not too far from the average listing rate (equal to 1.21%), but for areas with very high listings rates (such as Venice), the predicted price effects should be interpreted with caution.

Our estimates imply that the gains of Airbnb for LA County as a whole are quite modest (3.6%). This result makes sense because many areas in LA counties have a low listings rate. However, there are also areas with higher listings rates. It is for example interesting to focus on areas within 5km of Hollywood's Walk of Fame, a major tourist hotspot, where the listings rate is more than four times the County's average. When we focus on these areas, the house price effect due to Airbnb is estimated to be 14.7%, which we consider being substantial. When we limit ourselves to areas within 2.5km of the Walk of Fame, we find even a more pronounced effect of 20%. One may wonder whether these effects are realistic and how they compare with nominal changes in prices during this period. It appears that nominal house prices within 5km of Hollywood have increased by more than 40% in the last 10 years, so it seems that our estimated effects are not unrealistically high, and explain about 1/4 of the nominal price increase.

We also consider the effects in beach towns. Within 2.5km of the beach, the price increase due to Airbnb is estimated to be equal to 4.8%. If we concentrate on specific cities and neighborhoods, the price effects of Airbnb vary substantially. In one of the most popular LA neighborhoods – Venice – the total price increase is more than 30%. On the other hand, in Pasadena (which is about 15km from Downtown LA), the effects of Airbnb are modest.

Let us consider the two counterfactual scenarios. First, we consider that all HSOs are abandoned. Within 2.5km of a beach, this implies that the listings rate and house prices increase respectively by about 5% and 0.3%. For Santa Monica, which is well known for its strict HSO, the listings rate would increase by 60% and the house price by almost 2.5%, which is non-negligible. For locations near Hollywood, abandoning HSOs does not imply large changes in property values, because hardly any areas within close distance of Hollywood are targeted by HSOs.

By contrast, if all cities would implement HSOs this can have large effects in areas attractive to tourists. For example, in Venice, the listings rate would drop by 30% and house price by 11.6%. Hence, HSOs are likely to have large effects in areas attractive to tourists.

Our results also imply that in neighborhoods attractive to tourists, the distributional consequences of Airbnb are grave: in popular areas, incumbent homeowners have benefited more than \$3-15 thousand per year due to Airbnb, whereas renters likely lost a

[41] Because we have only one single instrument, this assumption is essentially non-testable given our econometric approach.

similar amount, as renters are not allowed to list their property on Airbnb while paying higher rents at the same time.

As a consequence, there are clear distributional implications of HSOs. Homeowners will lose from the HSO, as the demand for housing will decrease. This effect is due to less efficient use of housing (because properties are not available for their most profitable use). However, (long-term) renters are likely to gain because more houses become available for rent so rents decrease. This offers a plausible explanation as to why cities around the world that have heavily restricted STRs typically have a high share of renters.[42]

[42]Conditional on income, there is a positive correlation between the share of renters and the introduction of the HSO within the Los Angeles area, see Appendix 2.9. We perceive this result as suggestive only, as we do not have exogenous variation in the share of renters. For 29 other U.S. cities, we also find suggestive evidence as the correlation between the maximum allowed number of rental days (an inverse measure of stringency) and the share of renters is -0.25.

Table 2.10: Overall price effects of Airbnb (in 2018)

		Baseline scenario			Counterfactual scenario 1: no HSOs			Counterfactual scenario 2: only HSOs		
	Average house price (in 1000 $)	*Listings rate (in %)*	*in % of the house price*	*Yearly effect (in $)*	*Listings rate (in %)*	*in % of the house price*	*Yearly effect (in $)*	*Listings rate (in %)*	*in % of the house price*	*Yearly effect (in $)*
Total predicted price effects of Airbnb listings:										
LA county	1,053	1.21	3.62	1258	1.26	3.78	1313	0.91	2.73	949
Total predicted price effects near Hollywood:										
Hollywood <10km	1,688	3.07	9.22	5,136	3.10	9.29	5,174	2.86	8.59	4,786
Hollywood <5km	1,960	4.89	14.66	9,483	4.92	14.77	9,549	4.54	13.63	8,814
Hollywood <2.5km	2,446	6.68	20.05	16,182	6.70	20.10	16,225	6.17	18.53	14,955
Total predicted price effects near the beach:										
Beach <10km	1,099	1.58	4.75	1,723	1.64	4.93	1,788	1.38	4.14	1502
Beach <5km	1,128	1.93	5.79	2,154	2.03	6.09	2,266	1.69	5.06	1,884
Beach <2.5km	1,113	2.44	7.32	2,691	2.57	7.73	2,839	2.13	6.38	2,344
Total predicted price effects for specific neighborhoods:										
Venice	1,212	12.77	38.33	15,327	12.77	38.33	15,327	8.92	26.78	10,709
West Hollywood	1,593	3.55	10.65	5,597	5.10	15.29	8,038	3.55	10.65	5,597
Malibu	2,193	5.89	17.67	12,791	5.89	17.67	12,791	4.15	12.45	9,009
Santa Monica	1,645	1.76	5.29	2,870	2.80	8.40	4,564	1.76	5.29	2,870
Redondo Beach	888	1.17	3.51	1029	1.49	4.46	1,308	1.17	3.51	1029
Pasadena	928	0.96	2.88	882	1.29	3.87	1184	0.96	2.88	882

Notes: Information is for September 2018. To estimate the yearly effects, we assume a discount rate of 3.3% (obtained from Koster and Pinchbeck, 2022). We further assume that rents are equal to discounted house prices.

2.8 Conclusion

We have seen a spectacular growth of online short-term housing rental platforms in recent years. So, what is the effect of regulation of short-term housing-rentals on the housing market? We focus on Los Angeles County, where 18 cities have implemented Home Sharing Ordinances that restrict short-term rentals between 2014 and 2018. Using microdata for house prices, and listings, we apply a Spatial Panel Regression-Discontinuity Design around the borders of those areas and exploit the differences in the timing of the HSOs. Home Sharing Ordinances reduce Airbnb listings by about 50% and reduce house prices by 2% on average, which captures the fact that houses cannot be used for their most profitable use anymore. Using aggregate data and a difference-in-differences estimation strategy we find essentially the same effects for rents. Forbidding short-term rentals may lead to a reallocation away from privately-owned housing towards the long-term rental market – a housing supply effect.

Our estimates imply that Airbnb regulation has stark distributional implications because it induces losses for homeowners that are very substantial in areas that are popular for tourists. The opposite holds for households who typically rent and who can only gain from regulation as it increases rental housing supply and therefore reduces rents.

Our estimates imply that the total effect of Airbnb on property values in LA County is modest (3.6%). This makes sense because in large parts of this County, Airbnb is not so popular. However, in areas attractive to tourists, where the Airbnb listings rates are quite high, the effects of Airbnb are substantial. Within 2.5km of Hollywood, for example, the increase in property values is almost 15%.

2.9 Appendix

Data appendix

Below, in Table 2.11, we report the results of our data gathering endeavors. Ready-to-use data on Home Sharing Ordinances is not available, so we have browsed the Internet and phoned local officials to know whether the city has implemented an HSO sometime during our study period. For each city, we report whether it has implemented an HSO, whether the listing of rooms is permitted, whether an STR needs to register at the municipality and whether officially STRs are not allowed according to the residential zoning code. Furthermore, we list the sources from which we get the information.

In Figure 2.8, we provide the number of active listings per wave. The Figure also indicates that we have approximately 90 thousand unique listings.

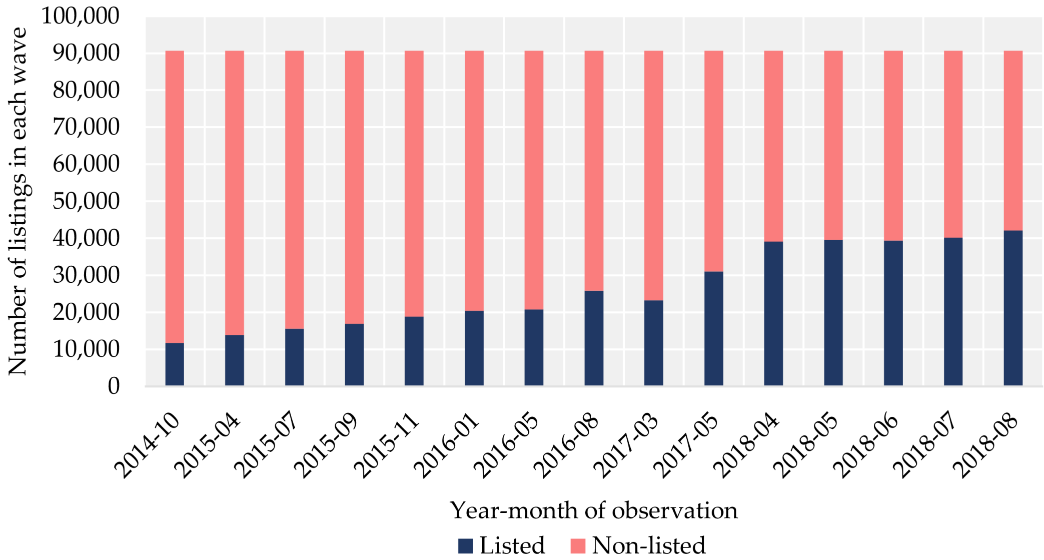


Figure 2.8: Evolution of number of listings per wave

Table 2.11: HSO and STR regulations in LA County

Name of city	Year & month of implementation		HSO	Home sharing not allowed	Register STR	STR not in zoning code	Source
Agoura Hills			0	0	0	0	phone
Alhambra			0	0	0	0	phone
Arcadia	2017	7	1	1	1	0	phone
Artesia			0	0	0	0	web
Azusa			0	0	0	1	web
Baldwin Park			0	0	0	1	phone
Bell			0	0	0	1	web
Bell Gardens			0	0	0	1	web
Bellflower			0	0	1	0	phone
Beverly Hills	2014	9	1	1	1	0	web
Bradbury			0	0	0	1	web
Burbank	2014	6	1	1	1	0	web
Calabasas	2018	1	1	0	1	0	web
Carson			0	0	0	1	phone
Cerritos	2016	8	1	1	1	0	web
Claremont			0	0	0	1	phone
Commerce			0	0	0	1	web
Compton			0	0	1	0	web
Covina			0	0	0	1	phone
Cudahy			0	0	0	1	web
Culver City			0	0	0	1	phone
Diamond Bar			0	0	0	1	web
Downey			0	0	0	1	phone
Duarte			0	0	0	1	web
El Monte			0	0	0	1	phone
El Segundo			0	0	0	0	web
Gardena			0	0	0	1	web
Glendale			0	0	0	0	phone
Glendora			0	0	0	1	web
Hawaiian Gardens			0	0	0	0	web
Hawthorne			0	0	0	1	web
Hermosa Beach	2016	6	1	1	1	0	web
Hidden Hills			0	0	0	1	web
Huntington Park			0	0	0	1	web
Industry			0	0	0	0	web
Inglewood			0	0	0	1	web
Irwindale			0	0	0	1	web
La Canada Flintridge			0	0	0	1	web
La Habra Heights			0	0	0	1	web
La Mirada			0	0	0	0	web
La Puente			0	0	0	1	web
La Verne			0	0	0	1	web
Lakewood			0	0	0	0	web
Lancaster			0	0	0	1	web
Lawndale	2017	7	1	1	0	0	web
Lomita			0	0	0	0	web
Long Beach			0	0	0	0	web
Los Angeles			0	0	0	0	web

Table 2.12: HSO and STR regulations in LA County (*continued*)

Name of city	Year & month of implementation		HSO	Home sharing not allowed	Register STR	STR not in zoning code	Source
Lynwood			0	0	0	1	web
Malibu	2016	10	0	0	1	0	web
Manhattan Beach	2015	6	1	1	1	0	web
Maywood	2018	4	1	1	0	0	web
Monrovia			0	0	0	0	web
Montebello			0	0	0	1	web
Monterey Park			0	0	0	1	web
Norwalk			0	0	0	1	web
Palmdale			0	0	0	1	web
Palos Verdes Estates	2016	9	1	1	1	0	web
Paramount			0	0	0	1	web
Pasadena	2017	10	1	0	1	0	web
Pico Rivera			0	0	0	1	web
Pomona			0	0	0	1	web
Rancho Palos Verdes	2016	7	1	1	1	0	web
Redondo Beach	2016	6	1	1	1	0	web
Rolling Hills	2016	12	1	1	1	0	web
Rolling Hills Estates	2016	12	1	1	1	0	web
Rosemead			0	0	0	1	web
San Dimas			0	0	0	1	phone
San Fernando			0	0	0	0	phone
San Gabriel			0	0	0	1	phone
San Marino			0	0	0	1	web
Santa Clarita			0	0	0	0	phone
Santa Fe Springs			0	0	0	0	web
Santa Monica	2015	6	1	0	1	0	web
Sierra Madre			0	0	0	0	web
Signal Hill			0	0	0	0	web
South El Monte			0	0	0	1	web
South Gate			0	0	0	0	web
South Pasadena			0	0	0	1	phone
Temple City			0	0	0	1	phone
Torrance	2016	4	1	0	1	0	web
Vernon			0	0	0	0	web
Walnut			0	0	0	1	web
West Covina			0	0	0	0	web
West Hollywood	2015	9	1	1	1	0	web
Westlake Village			0	0	0	0	phone
Whittier			0	0	0	1	phone
Unincorporated			0	0	1	0	web

Bandwidth selection

We use the approach proposed by Imbens and Kalyanaraman (2012), who show that the optimal bandwidth can be estimated as:

$$b^* = C_K \cdot \left(\frac{\hat{\sigma}^2_-(c) + \hat{\sigma}^2_+(c)}{\hat{f}(c) \times \left((\hat{m}^{(2)}_+ - \hat{m}^{(2)}_-)^2 + (\hat{r}_+ + \hat{r}_-) \right)} \right)^{\frac{1}{5}} \times N^{-\frac{1}{5}}, \tag{2.6}$$

where the constant $C_K = 3.4375$ and N is the number of observations. $\hat{\sigma}^2_-$ and $\hat{\sigma}^2_+$ are the conditional variances of respectively ℓ_{ikt} or $\log p_{ijt}$ given $d_i = c$ on both sides of the threshold (indicated with '$-$' and '$+$'). $\hat{f}(c)$ denotes the estimated density of d_i at c. $\hat{m}^{(2)}_-$ and $\hat{m}^{(2)}$ are estimates of the second derivatives of a function of the dependent variable on the distance to the boundary d_i. $\hat{r}_+$ and $\hat{r}_-$ are estimated regularization terms that correct for potential error in the estimation of the curvature of $m(d)$ on both sides of the threshold.

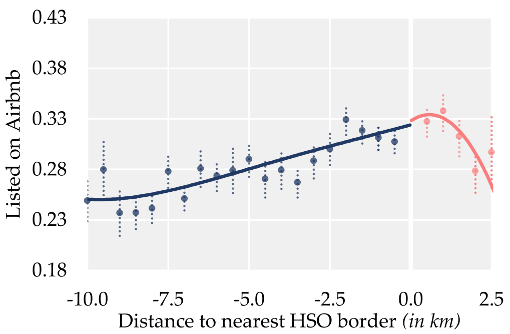
Because we exploit variation in prices and the HSO over time to determine the bandwidths, we first demean the variables by month and property or census block fixed effects. In many specifications we add additional covariates (*e.g.* housing characteristics). We then determine the conditional variance of the dependent variable given all covariates and fixed effects at the threshold, so $\hat{\sigma}^2_-(c \mid x_{ikt}, \lambda_i, \theta_k t)$ and $\hat{\sigma}^2_+(c \mid x_{ikt}, \lambda_i, \theta_k t)$. Usually, adding covariates does not affect the optimal bandwidth much (Imbens and Kalyanaraman, 2012). Indeed, adding a wide array of controls barely influences the optimal bandwidth in our specifications.

Other graphical evidence

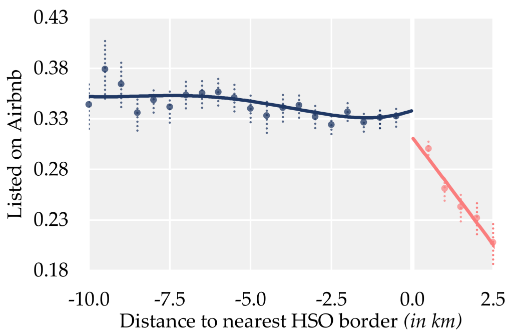
In this Appendix, we review ancillary graphical evidence that supports the identifying assumptions we make in our research design. In Appendix 2.9 we first consider cross-sectional variation in the listing probability and house prices around the borders of HSO areas. In Appendix 2.9 we investigate sorting and the provision of public goods. Appendix 2.9 considers discontinuities in housing characteristics and Appendix 2.9 investigates jumps in densities of key variables after the HSO has been implemented.

Cross-sectional differences in listings

In Figure 2.9 we illustrate cross-sectional differences in listings *before* and *after* the HSOs were implemented. In Figure 2.9a, we compare the probability of being listed *before* an HSO was implemented on both sides of the border. It is clear that there was essentially no difference between HSO areas and surrounding areas. However, after the HSO was implemented, the probability of being listed is approximately 4 percentage points lower (see Figure 2.9b).



(a) Airbnb listings, before HSO



(b) Airbnb listings, after HSO

Notes: Spatial differences in variables are *conditional* on month fixed effects. Hence, we identify the effects *over space*. Negative distances indicate areas outside HSO areas and areas inside HSO areas. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block level. We include a 4$^{\text{rd}}$-order polynomial in untreated areas and a 2$^{\text{nd}}$-order polynomial in treated areas.

Figure 2.9: Variation near HSO borders before and after the HSO

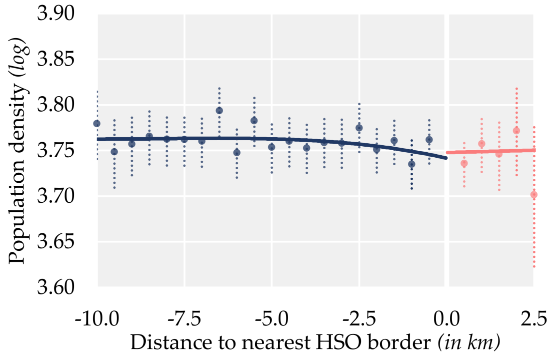
Sorting and public goods

In spatial RDDs, one should be concerned about sorting. It might be that a discontinuity in prices due to implementation is partly caused by a change in the demographic composition of the neighborhood (see Bayer et al., 2007, for cross-sectional evidence on school districts). Using Census Block Group level data from the *American Community Survey* (ACS) 2014-2016, Figure 2.10 shows that all household characteristics are continuous at the border. Importantly, changes in population density and the share of owner-occupied housing are the same on both sides of the border (Figure 2.10a). The latter is noticeable as one might expect to see a relative increase in home-ownership (as to be able to rent out to tourists) in the areas where Airbnb is still allowed if rents do not change. The reason may be that in the short run it may be hard to evict long-term renters. Hence, HSOs did not seem to have led to a fundamental change in housing tenure. We also do not detect changes in the household composition, measured by income, the share of blacks, single households, or median age. Nevertheless, in sensitivity analyses (see Appendix 2.9) we will control for changes in the housing stock and demographic characteristics and show that this does not affect the results.

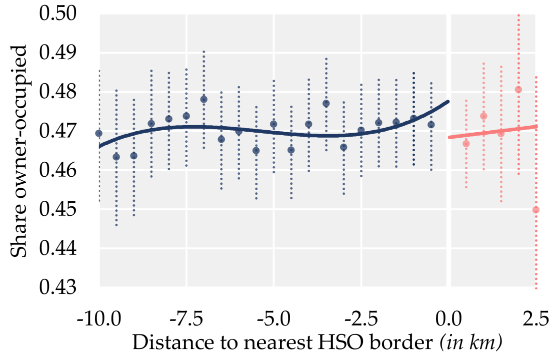
One could also be concerned that a discontinuity in prices arises because of a differential provision of public goods. While temporal changes in the quality of public goods are usually not abrupt, large cross-sectional differences in public good quality may provoke sorting. An important public good is school quality (see Black, 1999; Bayer et al., 2007).[43] Using 2017 test score data of students (observed at the individual school level) between the 3rd and 11th grade on English and Mathematics from the California Assessment of Student Performance and Progress (CAASPP), we checked for possible discontinuities in changes of student performance around the HSO borders. Figures 2.10g and 2.10h show that no such discontinuity exists, indicating that the HSO is unlikely to be correlated to school quality.[44]

[43]We also checked for other spatial differences in *e.g.* property taxes, but we did not find any meaningful difference.

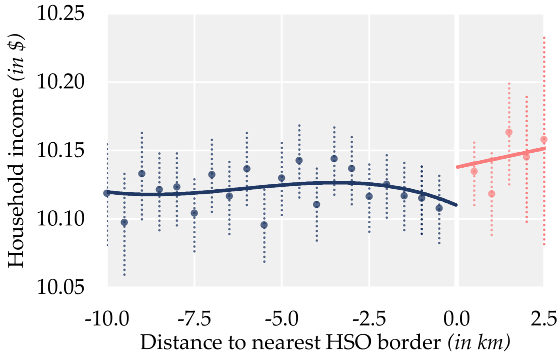
[44]Note that not all school districts are pertaining to one city. For example, the City of Carson is served by the Los Angeles and Compton school districts. West-Hollywood is also part of the LA school district.



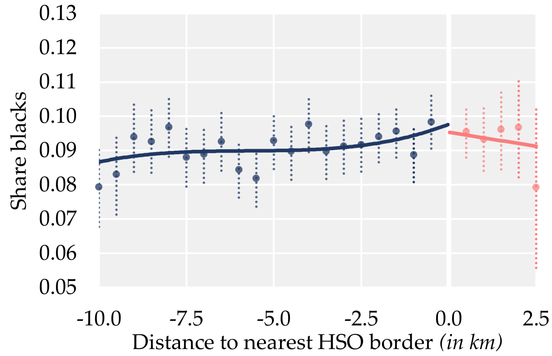
(a) Population density *(log)*



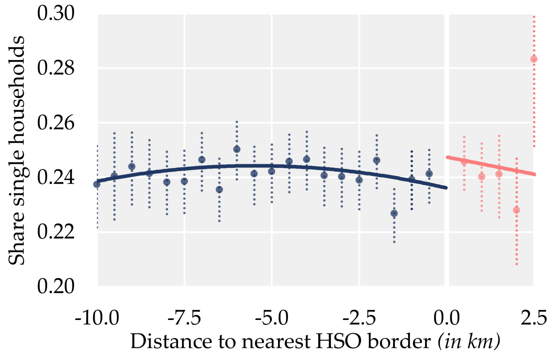
(b) Share owner-occupied housing



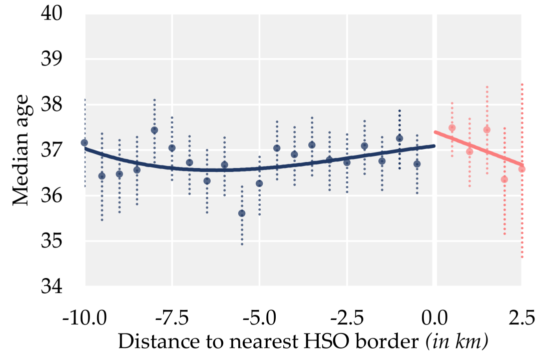
(c) Income per capita *(log)*



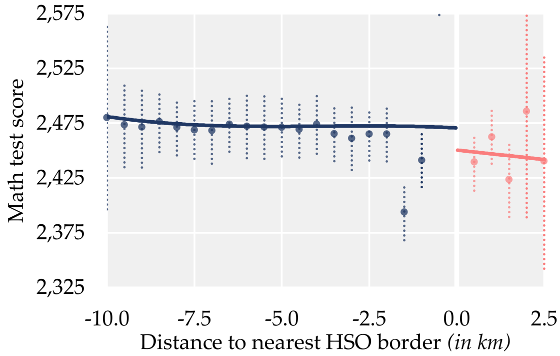
(d) Share black



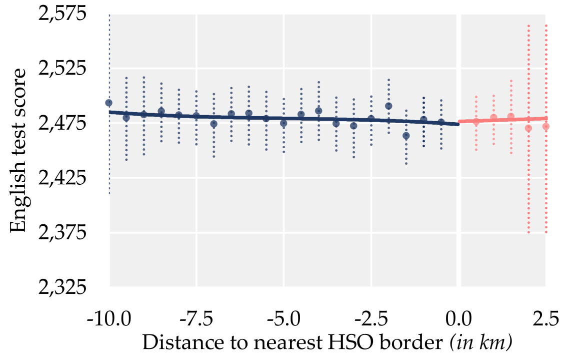
(e) Share single households



(f) Median age



(g) Math test scores



(h) English test scores

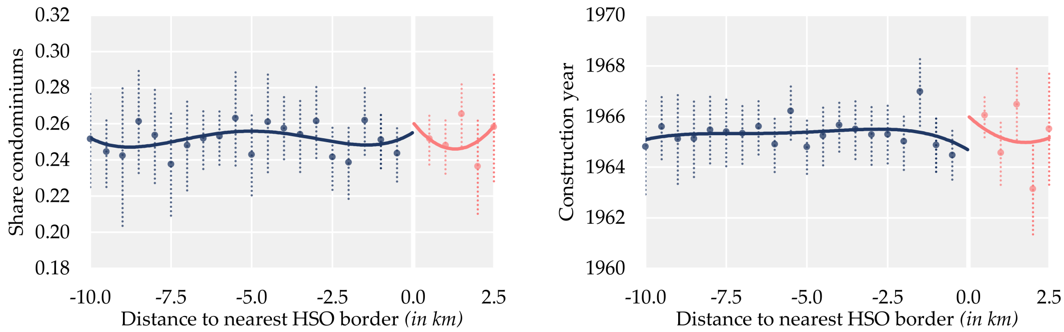
Notes: Spatial differences in variables are *conditional* on census block group and border segment×month fixed effects. Hence, we identify the effects *over time*. Negative distances indicate areas outside HSO areas and areas inside HSO areas but *before* treatment. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block group level. We include 3$^{\text{rd}}$-order polynomials in untreated areas and a linear function in treated areas.

Figure 2.10: Sorting along the border

Discontinuities in housing characteristics

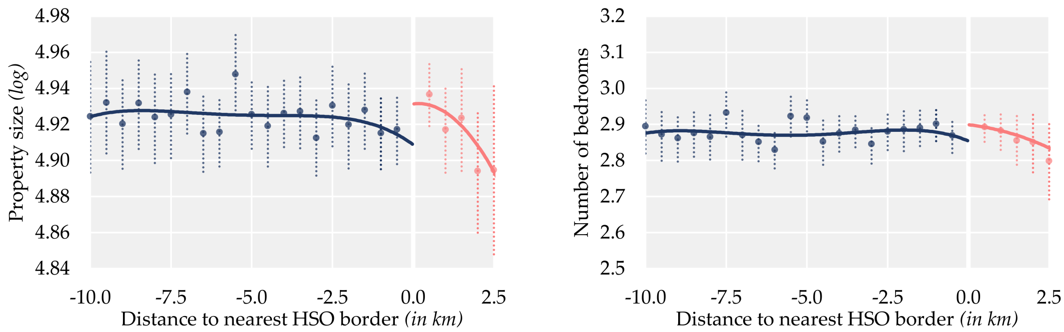
An important assumption in the Panel Regression Discontinuity Design is that changes in covariates, except for the treatment variable, are continuous at the border. We, therefore, investigate in Figure 2.11 whether changes in housing characteristics over time do not show discontinuities.

Figure 2.11a highlights that the change in the share of condominiums is not statistically significantly different at the border of HSO areas. Figure 2.11b further shows that there is small discrete jump in the change in construction year at the border, but this jump is only statistically significant at the 10% level. This would imply that after an HSO has been implemented, slightly newer properties are traded in HSO areas. To control for this effect we include construction decade dummies in the regressions.



(a) Share condominiums

(b) Construction year



(c) Property size *(log)*

(d) Bedrooms, *demeaned*

Notes: Spatial differences in variables are *conditional* on census block and border segment×month fixed effects. Hence, we identify the effects *over time*. Negative distances indicate areas outside HSO areas and areas inside HSO areas but *before* treatment. The dots are conditional averages at every 500m interval. The dotted lines denote 95% confidence intervals based on standard errors clustered at the census block level. We include a 4rd-order polynomial in untreated areas and a 2nd-order polynomial in treated areas.

Figure 2.11: Housing transactions: variation near HSO borders

For property size, we also find a small jump, but the difference is again only marginally statistically significant. To the extent the price effect partly captures changes in the houses on offer in HSO areas, we control for the property size in the house price regressions. Finally, we do not find a statistically significantly different jump in the number of bedrooms (Figure 2.11d).

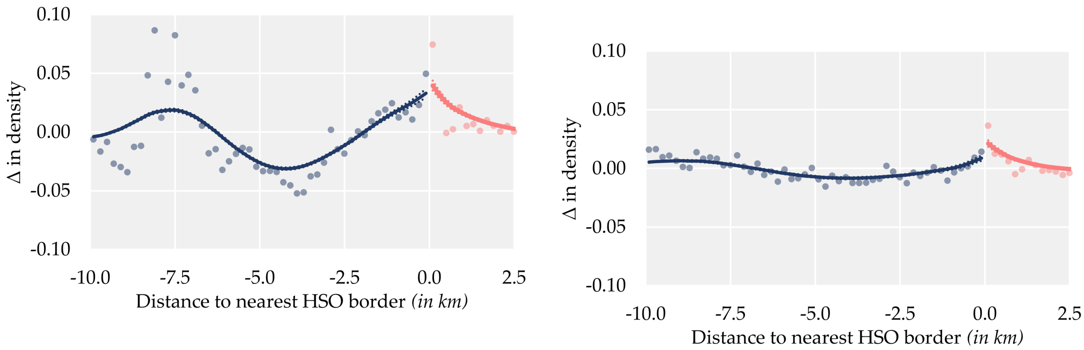
Conditional McCrary tests

A test for discontinuities in densities of the running variable *before the introduction of the HSO* might be informative, as a discontinuity might be indicative of unobserved housing or household traits (*e.g.* different types of households sorting themselves into treated areas) that are potentially correlated with the treatment. However, this test should take into account the geography of the area and borders of the areas, as discontinuities in listings or housing transactions may also indicate that some areas border mountainous areas, parks or the sea.

We, therefore, estimate a two-step density test in the spirit of McCrary (2008). In the first step, we estimate the spatial distribution for buildings employing McCrary's methodology. In the second step, we estimate this distribution for listings and housing transactions respectively. Our test is then the difference in the estimated densities between the second and first step. Hence, a negative (or positive) density differential would indicate that there are fewer (or more) listings/transactions than expected given the spatial distribution of buildings.

The results are reported in Figure 2.12. Figure 2.12a tests for the continuity of the density differential of listings *before* an HSO was implemented. We find that there is no difference in the density for listings at the HSO border. We repeat the same exercise, but now for housing transactions in Figure 2.12b. This test indicates a discontinuity due to a higher density of housing transactions just across the border in HSO areas. Note however that the discontinuity is economically very small, so we do not consider this as a problem.

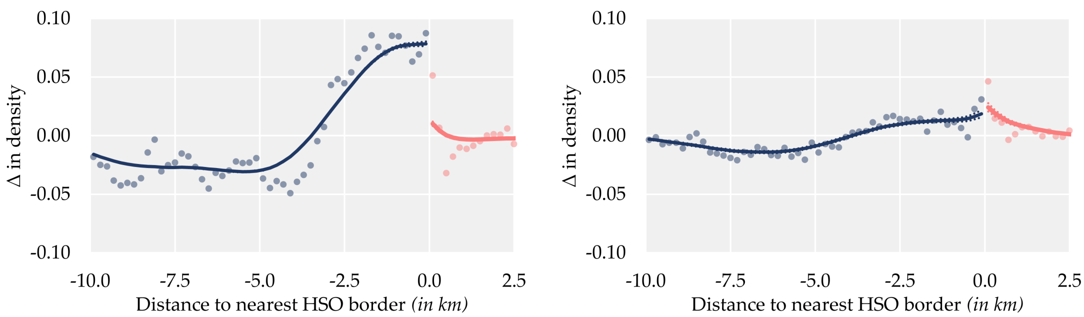
We repeat this exercise by estimating the adapted McCrary's density test *after* an HSO was implemented, but given the spatial distribution of buildings in 2014. In Figure 2.13a we show that Airbnb listings are now discontinuous after the HSO. The density is much lower in treated areas, which is in line with the finding that listings have been reduced due to the implementation of HSOs. For house prices (Figure 2.13b) we find essentially the same difference in density of transactions as in 2.12b, which we think is reassuring: the HSO did not lead to a different market turnover on both sides of the HSO borders.



(a) Density differential of listings (b) Cond. density differential of transactions

Notes: We focus on observations *before* implementation of the HSO. Negative distances therefore indicate areas outside HSO areas. The dots are conditional densities at every 200m interval. The dotted lines denote 95% confidence intervals. On the y-axis we ploy the difference in densities of McCrary's density test between respectively listings and housing transactions and the density of buildings.

Figure 2.12: Conditional McCrary density tests before HSOs



(a) Density differential of listings (b) Density differential of transactions

Notes: We focus on observations *before* implementation of the HSO. Negative distances therefore indicate areas outside HSO areas. The dots are conditional densities at every 200m interval. The dotted lines denote 95% confidence intervals. On the y-axis we ploy the difference in densities of McCrary's density test between respectively listings and housing transactions and the density of buildings.

Figure 2.13: Conditional McCrary density tests after HSOs

Other regression results and robustness

In this part of the Appendix, we will subject our results to a wide range of robustness checks and report some additional results. Appendix 2.9 first investigates the effects of HSOs in different cities on the listing probability and prices. In Appendix 2.9 we investigate whether the HSO influenced Airbnb rental prices, close to and further away from the border of HSO areas. Appendix 2.9 further investigates whether the supply of hotels has changed due to HSOs. Appendix 2.9 further investigates the possibility of pre-trends in listings, prices, and rents. In Appendix 2.9 we examine whether the standard errors change when accounting for cross-sectional and temporal dependence. We then proceed by reporting the first-stage results in Appendix 2.9. We subject our results to a wide array of additional robustness checks in Appendix 2.9. In Appendix 2.9 we check for sensitivity of the results using the *Zillow* data, so the results using a DiD estimation strategy.

City-specific effects

Here we analyze city-specific effects. We re-estimate the preferred specification where we include border segment$\times$month fixed effects. Given that the number of observations for many cities is limited, one expects that only for a handful of cities the coefficient is statistically significant. On the other hand, if there is a substantial number of coefficients with the wrong sign, and these coefficients are statistically significant, then our identification strategy would be less convincing. We first show for each HSO city the number of treated and untreated observations in Table 2.13. It is shown that some cities, such as Cerritos, Maywood, Rolling Hills, and Rolling Hills Estates have very few observations. Hence, we should be careful not to take these results too seriously. We report the results in Table 2.14.

In columns (1) and (2) we report the results for listings of entire properties and rooms, respectively. Column (1) shows that the point estimates related to HSOs are almost always negative and in three cases highly statistically significant. Column (2) also shows that most coefficients are statistically significant and negative and comparable to the results in column (1). For Maywood and Rolling Hills, we find statistically significant effects with an incorrect sign, but this is due to very low number of observations (see Table 2.13).

In column (3) we focus on house prices. The results show that the effect is in most cases negative, although often imprecise. We find statistically significant negative effects, whereas the positive effects are far from being statistically significant.

Table 2.13: City-specific number of observations

	Airbnb – homes		*Airbnb – rooms*		*Housing transactions*	
	Before HSO	*After HSO*	*Before HSO*	*After HSO*	*Before HSO*	*After HSO*
	(1)	(2)	(3)	(4)	(5)	(6)
Arcadia	1930	967	3590	1793	1819	262
Beverly Hills	0	13644	0	4821	174	670
Burbank	0	4212	0	4863	373	2848
Calabasa	570	288	720	357	1118	11
Cerritos	112	98	320	280	974	502
Hermosa Beach	2315	2666	996	1118	528	294
Lawndale	450	227	590	293	480	105
Manhattan Beach	696	4542	242	1555	553	882
Maywood	33	12	0	0	146	0
Palos Verdes	178	163	142	117	508	256
Pasadena	7463	3733	5327	2662	5328	297
Rancho Palos Verdes	485	553	509	583	1277	774
Redondo Beach	2376	2733	2293	2603	2183	1320
Rolling Hills	24	21	0	0	59	17
Rolling Hills Estates	80	70	72	63	361	126
Santa Monica	4170	27041	2178	14221	928	1457
Torrance	799	1209	1469	2193	2567	2108
West Hollywood	6709	18522	2803	7636	719	949

Table 2.14: City-specific effects for listings and prices, all observations

	(Dep.var.: entire property is listed)	(Dep.var.: home sharing is listed)	(Dep.var.: log of house price in $)
	(1)	(2)	(3)
HSO implemented×Arcadia	-0.0471	-0.0346	-0.0683**
	(0.0644)	(0.0487)	(0.0278)
HSO implemented×Beverly Hills			0.0195
			(0.0266)
HSO implemented×Burbank			-0.0600**
			(0.0272)
HSO implemented×Calabasas	0.0529	0.1003	-0.1876***
	(0.0932)	(0.0825)	(0.0417)
HSO implemented×Cerritos	-0.1834	-0.1716	-0.0496**
	(0.2053)	(0.1160)	(0.0224)
HSO implemented×Hermosa Beach	-0.2177***	-0.3009***	-0.0542
	(0.0551)	(0.0752)	(0.0398)
HSO implemented×Lawndale	0.0907	0.1008	0.0094
	(0.1022)	(0.1101)	(0.0511)
HSO implemented×Manhattan Beach	-0.0642*	-0.0557	-0.0632**
	(0.0339)	(0.0437)	(0.0257)
HSO implemented×Maywood	0.6983***		
	(0.0268)		
HSO implemented×Palos Verdes Estates	-0.0090	-0.0504	-0.0520
	(0.1320)	(0.1307)	(0.0326)
HSO implemented×Pasadena	0.0508	0.0630	-0.0141
	(0.0429)	(0.0496)	(0.0298)
HSO implemented×Rancho Palos Verdes	-0.1633	0.0371	-0.0124
	(0.1151)	(0.0878)	(0.0174)
HSO implemented×Redondo Beach	-0.0272	-0.0418	-0.0192
	(0.0479)	(0.0549)	(0.0195)
HSO implemented×Rolling Hills	0.5544**		-0.0819
	(0.2218)		(0.0559)
HSO implemented×Rolling Hills Estates	-0.1772	-0.1196	-0.0551**
	(0.1872)	(0.0953)	(0.0263)
HSO implemented×Santa Monica	-0.1712***	-0.0224	0.0022
	(0.0220)	(0.0259)	(0.0227)
HSO implemented×Torrance	0.0783*	0.0915*	-0.0379***
	(0.0462)	(0.0513)	(0.0147)
HSO implemented×West-Hollywood	-0.0702***	-0.0622***	0.0263
	(0.0132)	(0.0211)	(0.0246)
Property characteristics	No	No	Yes
Spatio-temporal trend variables included	Yes	Yes	Yes
Listing fixed effects	Yes	Yes	No
Census block fixed effects	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes
Number of observations	270,336	171,448	63,277
Bandwidth, b *(in km)*	1.6674	1.8255	1.8089
R^2	0.3523	0.3446	0.8447

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

HSOs and Airbnb short-term rental prices

Did HSOs have an impact on short-term rental prices of Airbnb properties? We explore this in Table 2.15. These are hedonic price analyses using observations of properties that are listed (in our dataset). We emphasize that spatial equilibrium theory indicates that *at the border* short-term rental prices would not change, because tourists are unlikely to differentiate between accommodation in HSO areas and immediately adjacent areas and are therefore unlikely to be willing to pay higher prices in areas that have implemented HSOs.

In column (1) we estimate the Panel RDD and do not find a statistically significant effect of an HSO on Airbnb rental prices. This also holds if we include border segment$\times$month fixed effects in column (2) and change the optimal bandwidth in columns (3), (4), and (5). In column (6) we include property fixed effects. In all cases the effect of an HSO on prices is economically negligible and statistically insignificant, confirming spatial equilibrium theory.

Table 2.15: HSOs and Airbnb prices

(Dependent variable: log of price per night)

	Panel RDD	*+ Border segment f.e.*	*Bandwidth: $h^* \times 2$*	*Bandwidth: $h^*/2$*	*Bandwidth: $h^*/5$*	*Property f.e.*
	(1)	(2)	(3)	(4)	(5)	(6)
HSO implemented	0.0051	0.0061	0.0052	0.0054	0.0081	0.0040
	(0.0090)	(0.0093)	(0.0089)	(0.0115)	(0.0093)	(0.0066)
Private room	-0.3534***	-0.3527***	-0.3569***	-0.3489***	-0.2140***	-0.2242***
	(0.0086)	(0.0086)	(0.0069)	(0.0119)	(0.0327)	(0.0193)
Shared room	-0.7756***	-0.7770***	-0.7658***	-0.8469***	-0.3083***	-0.3310***
	(0.0305)	(0.0306)	(0.0215)	(0.0447)	(0.0842)	(0.0430)
Accommodation size *(log)*	0.5197***	0.5164***	0.5055***	0.5081***	0.0992***	0.1114***
	(0.0092)	(0.0088)	(0.0073)	(0.0116)	(0.0194)	(0.0112)
availability	0.1921***	0.1934***	0.1920***	0.1885***	0.0242***	0.0263***
	(0.0065)	(0.0065)	(0.0054)	(0.0086)	(0.0038)	(0.0024)
Minimum of required nights *(log)*	0.0229***	0.0227***	0.0260***	0.0209***	-0.0155***	-0.0132***
	(0.0034)	(0.0034)	(0.0029)	(0.0044)	(0.0031)	(0.0024)
Maximum of required nights *(log)*	0.0057***	0.0057***	0.0050***	0.0060***	-0.0005	-0.0004
	(0.0016)	(0.0016)	(0.0013)	(0.0022)	(0.0024)	(0.0016)
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Listing fixed effects	No	No	No	No	No	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes
Number of observations	137,487	137,351	211,883	79,503	38,415	104,833
Bandwidth, b	1.7842	1.7826	3.5652	0.8913	0.3565	1.3353
R^2	0.7745	0.7772	0.7769	0.7843	0.9771	0.9768

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We extend these results by using the same difference-in-differences approach as in Section 2.6.4. In Table 2.16 we report the results. In column (1) we include all observations in LA County. The effect of HSOs is small and statistically insignificant. This also holds if we only include observations within 25km of any HSO border. In column (3), we exclude observations that are close to (<1km) a border. Column (4) further controls for distance to CBD and distance to the beach trends. In the final column we exclude observations within 5km of an HSO border. All results are economically small and far from being statistically significant.

All in all, we do not find any evidence that Airbnb rental prices are affected by HSOs, which is in keeping with the notion that the market for Airbnb properties is competitive and tourist demand for local accommodation is elastic.

Table 2.16: HSOs and Airbnb prices, DiD results

(Dependent variable: log of price per night)

	All obs.	*Outside HSO, <25km*	*Outside HSO, >1km, <25km*	*Outside HSO, >1km, <25km*	*Outside HSO, >2.5km, <25km*
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS
HSO implemented	-0.0008	-0.0007	-0.0010	0.0011	-0.0000
	(0.0045)	(0.0045)	(0.0046)	(0.0046)	(0.0051)
Airbnb property characteristics	Yes	Yes	Yes	Yes	Yes
Distance to CBD×year trends	No	No	No	Yes	Yes
Distance to beach×year trends	No	No	No	Yes	Yes
Listing fixed effects	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes
Number of observations	339,322	336,468	286,193	286,193	214,113
R^2	0.9782	0.9778	0.9779	0.9779	0.9782

Notes: In all specifications we include observations *inside* HSO areas. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

HSOs and formal accommodation

We investigate here how the formal hotel industry benefited from the implementation of HSOs. Again, at the border, we expect few effects. However, when comparing HSO areas with areas further away from the border, we might expect to see an increase in the number of officially registered traveler accommodations, which we investigate here (we do not have information on hotel rates).

We obtain yearly data from the *County Business Patterns* at the zip code level and keep

NAICS-sector 72111, which are traveler accommodations, including hotels, casino hotels, and other traveler accommodations. Because the latest County Business Pattern data is from 2016, we also include 2012 and 2013, so that we have data for 5 years. We take the same approach as in Section 2.6.4, where we use a DiD design. Table 2.17 reports the results of several Poisson regressions.

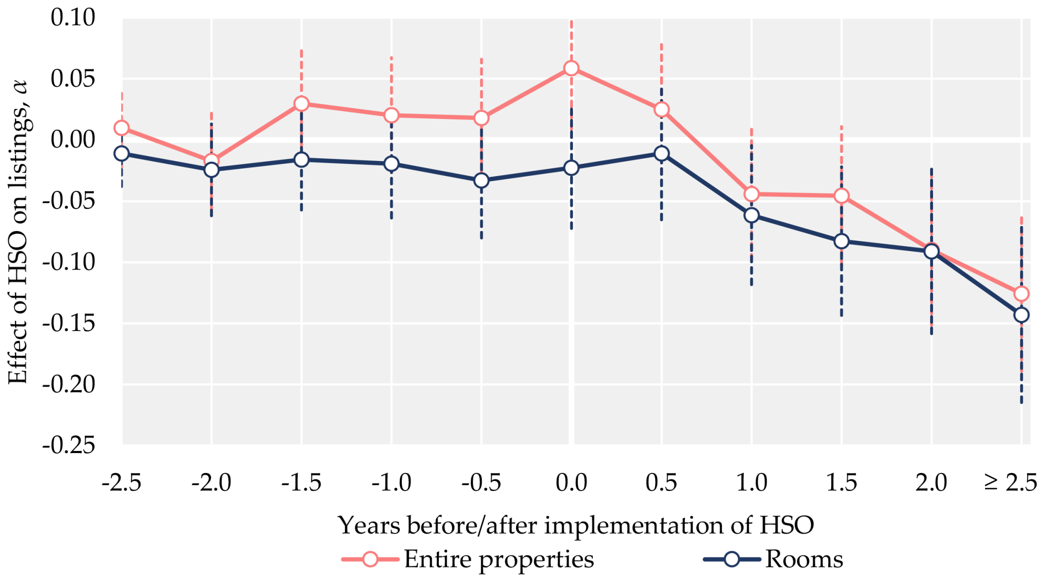
In column (1) we include all zipcodes in LA County. The point estimate suggests that the number of traveler accommodations increases due to HSOs by $\exp(0.0557) - 1 = 5.7\%$, which is sizable. However, the coefficient is quite imprecisely estimated. This also holds for the other specifications, where we include zip codes that are further away from HSO borders (>1km or >2.5km). Hence, we think Table 2.17 provides suggestive evidence that the number of travelers accommodations has increased due to the HSO.

Table 2.17: HSOs and traveler accommodations

(Dependent variable: number of accommodations)

	All obs.	*Outside HSO, <25km*	*Outside HSO, >1km, <25km*	*Outside HSO, <1km*	*Outside HSO, >1km, <25km*	*Outside HSO, >2.5km, <25km*
	(1) Poisson	(2) Poisson	(3) Poisson	(4) Poisson	(5) Poisson	(6) Poisson
HSO implemented	0.0557	0.0511	0.0550	0.0709	0.0508	0.0566
	(0.0500)	(0.0502)	(0.0505)	(0.0534)	(0.0541)	(0.0560)
Dist. to CBD×year	No	No	No	No	Yes	Yes
Dist. to beach×year	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1,183	1,121	1,051	149	1,051	872
Log-likelihood	-1,952	-1,851	-1,733	-256.8	-1,731	-1,426

Notes: In all specifications we include observations *inside* HSO areas. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.



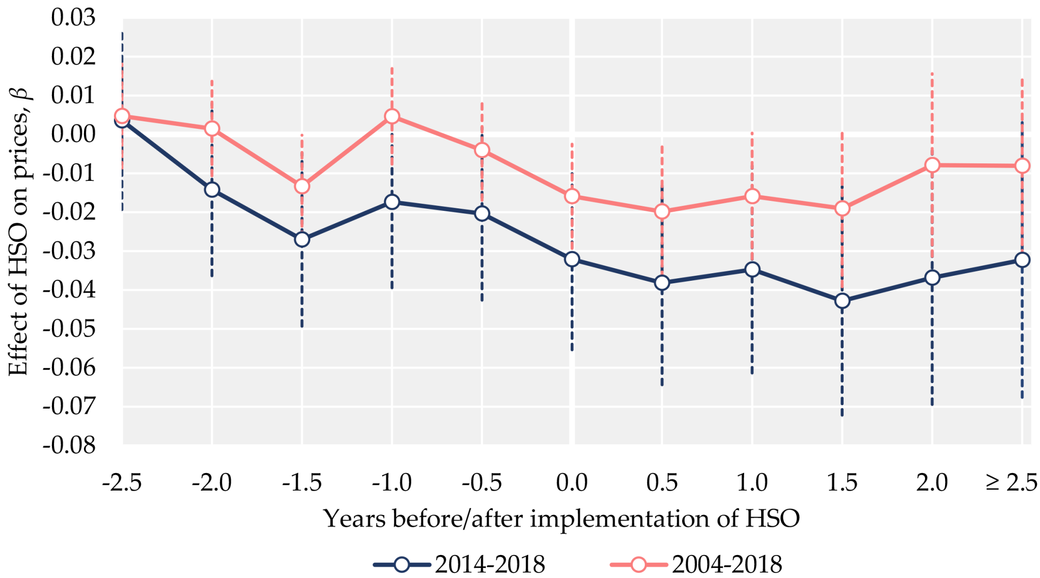
Notes: The optimal bandwidth $b^* = 1.6692$. The dotted lines denote the 95% confidence bands.

Figure 2.14: An event study to the effect of the HSO on listings

Anticipation effects and pre-trends

Here we investigate pre-trends of listings, house prices, and rents in more detail. In Figure 2.14 we focus on listings. Clearly, we do not observe a pre-trend even if we extend the period of observation to two and a half years before the implementation of the HSO. The effects turn negative after about a year, although these are only statistically significant at the 5% level two years after the treatment.

Let us now turn to the effect of HSOs on prices. Note that the Panel RDD would imply that price trends in HSO areas and neighboring areas are the same before the HSO. However, if anticipation effects are important because HSOs are announced or anticipated, prices may already adjust before the actual treatment. We did not find evidence for this in Figure 2.6 in Section 2.6.2, but we investigate this further in Figure 2.15 by allowing for price changes 2.5 years before treatment. This implies that the reference category is composed of transactions in areas that will be treated in 2.75 years or later; and of transactions in areas close to treated areas.



Notes: The optimal bandwidth $b^* = 1.8089$. The dotted lines denote the 95% confidence bands.

Figure 2.15: An event study to the effect of the HSO on prices

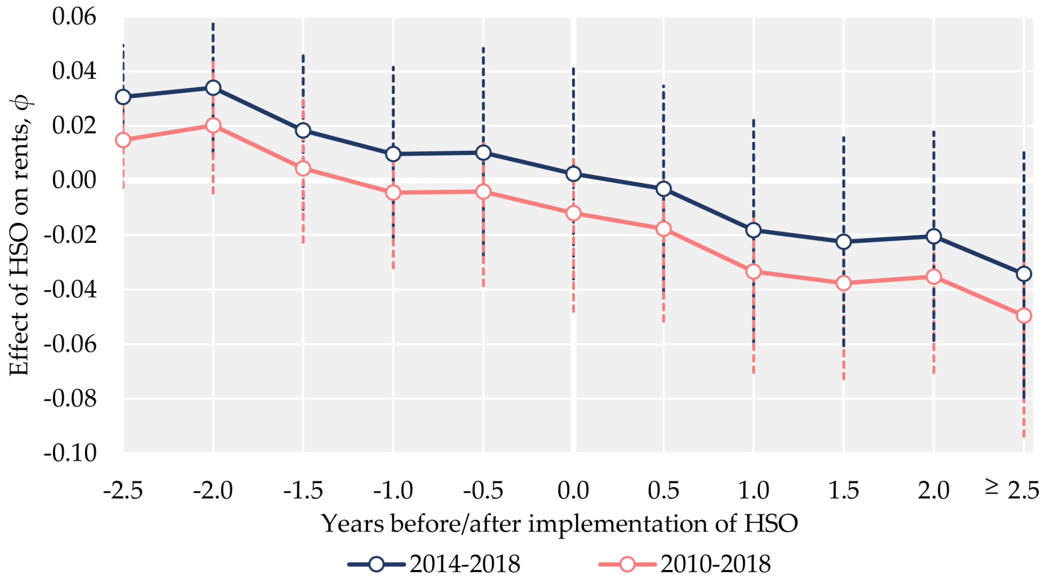
We do not find evidence for pre-trends. That is before the treatment year there are generally no statistically significantly lower prices in treated areas if we focus on our baseline sample that includes housing transactions between 2014-2018. One may be concerned that this is an issue of precision, as the point estimate is still negative and around 2% the year before treatment. We emphasize that this may be due to the announcement of HSOs before actual implementation.[45] To the extent anticipation effects are important we are inclined to find an underestimate. Still, the long-run effect after 3 years is still about 1-2%, albeit imprecise because of few observations. However, if we focus on a longer sample based on data since 2004, we find no evidence whatsoever for pre-trends, as the effect is not statistically significant and close to zero before treatment.

We test for even longer pre-trends (respectively 5 and 10 years before the treatment) in Section 2.6.3 by taking samples of house prices preceding the current sample. By running placebo regressions we show that there is no evidence that prices were already decreasing in areas where HSOs are to be implemented.

The robustness of pre-trends for the dataset on rents is investigated in Figure 2.16. Here we use either the baseline data or extend the data to 2010. We do not find strong evidence for pre-trends, confirming earlier evidence, particularly when we use data since

[45]For example, in the City of Los Angeles regulation was announced about half a year before it was implemented in July 2019. However, discussions about what type of regulation should be implemented in the City of LA have taken even longer.

2010 (*i.e.* the first year for which the *Zillow* data are available).



Notes: In all specifications we include observations *inside* HSO areas and observations between 1 and 25km of an HSO area. The dotted lines denote the 95% confidence bands.

Figure 2.16: An event study to the effect of the HSO on rents

Spatial HAC standard errors

Spatial data is usually not interdependent. More specifically, unobserved characteristics of a property (*e.g.* crime, maintenance quality) are likely correlated over space and time. Although these variables are unlikely to be correlated with the HSO and therefore do not affect the consistency of the estimated coefficients, spatial dependence may imply that the estimated standard errors are biased.

In this paper we cluster at the census block level to partly address this issue (see Moulton, 1990), but clustering implies strong parametric assumptions as to how observations relate to other observations. We aim to allow for more general forms of dependence. We therefore use Conley's (1999) procedure to allow for spatial dependence. We use a linear Bartlett kernel to determine kernel weights, indicating how one observation relates to the other. We use an initial spatial window, denoted by sw, equal to the bandwidth used in the RDD.

In column (1) of Table 2.18 we report the baseline specification with standard errors clustered at the census block level. If we then allow for cross-sectional dependence within 1.86km in column (2), we find very similar, and even slightly smaller, standard

Table 2.18: Spatial HAC standard errors

(Dependent variable: log of house price in $)					
	Baseline	$sw = 1 \times b^{*} km$	$sw = 2 \times b^{*} km$	$sw = 5 \times b^{*} km$	$sw = 10 \times b^{*} km$
	(1)	(2)	(3)	(4)	(5)
HSO implemented	-0.0177***	-0.0177***	-0.0177***	-0.0177***	-0.0177***
	(0.0078)	(0.0067)	(0.0068)	(0.0069)	(0.0070)
Spatio-temporal trend variables	Yes	Yes	Yes	Yes	Yes
Property characteristics	Yes	Yes	Yes	Yes	Yes
Census block FE	Yes	Yes	Yes	Yes	Yes
Border segment×month FE	Yes	Yes	Yes	Yes	Yes
Number of observations	63,275	63,275	63,275	63,275	63,275
R^2	0.9090	0.9090	0.9090	0.9090	0.9090
Bandwidth, b *(in km)*	1.8087	1.8087	1.8087	1.8087	1.8087
Spatial cut-off *(in km)*	—	1.8087	3.6174	9.0435	18.0872

Notes: We estimate standard errors corrected for cross-sectional dependence using a Bartlett kernel and given the indicated spatial cut-offs. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

errors. In the following specifications, we increase the spatial window to up to 10 times the optimal bandwidth (almost 20km) in column (5). If anything, the standard errors become slightly smaller but are very comparable to the results clustered at the census block level. Hence, we conclude that spatial dependence is not an issue of major concern.

Negative external effects related to tourism

In Table 2.19, we investigate to what extent negative external effects related to tourism play a role. This is important because the estimates discussed above are the *net* effects of 2 opposing mechanisms. One mechanism is that the HSO reduces demand for housing, which decreases house prices, whereas the other mechanism is that it reduces negative tourist externalities, which increases house prices. Alternatively, because we find that the net effect of the HSO is negative, the estimates may be interpreted as underestimates of the efficient use effect, where the size of the underestimate depends on the size of the externality effect.

To investigate the importance of the externality effect, we consider two approaches. The first approach is based on the idea that if an HSO reduces negative effects of tourism locally (*e.g.*, up to 500m of a property which is not allowed to use short-term letting), then this has two consequences: *(i)* an HSO reduces tourism externalities for properties just outside treated areas; *(ii)* the reduction in negative externalities due to the HSO is less for properties just inside HSO borders compared to properties that are fully sur-

Table 2.19: HSOs and house prices: external effect

(Dependent variable: log of house price)

	Share HSO 0-500m	*Share HSO 0-100m*	*House type*	
	(1)	(2)	(3)	(4)
HSO implemented	-0.0243	-0.0424		
	(0.0152)	(0.0427)		
Share of land in HSO 0-500m	0.0095			
	(0.0183)			
Share of land in HSO 0-100m		0.0264		
		(0.0444)		
HSO implemented×single-family			-0.0155*	-0.0153*
			(0.0084)	(0.0084)
HSO implemented×condominium			-0.0209**	-0.0205**
			(0.0091)	(0.0091)
HSO implemented×condominium× before Watts v. Oak Shores				-0.0197
				(0.0169)
Property characteristics	Yes	Yes	Yes	Yes
Spatio-temporal trend variables	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes	Yes
Number of observations	63,261	63,340	63,297	63,297
Bandwidth, *b (in km)*	1.8081	1.8103	1.8103	1.8103
R^2	0.9090	0.9090	0.9090	0.9090

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

rounded by treated locations. Consequently, when the externality effect is substantial, the price effect may be different close to HSO borders.

To investigate this, we calculate the share of land within a 500m ring of a treated area. Hence, for houses further away than 500m from the border, the share is either zero (when located outside an HSO area or inside an HSO area but before treatment) or one (when located in a treated area), whereas for houses within 500m, the share is between zero and one. If there are substantial negative external effects of Airbnb, *conditional on the treatment dummy*, one expects to see price increases when the share of land in HSOs is higher (see for a similar approach in the context of measuring the external effects of land use regulation, Turner et al., 2014). As the added measure is quite collinear with the HSO measure, we do not find statistically significant effects for the HSO and share of HSO land within 500m. However, the point estimates have the expected signs: the treatment effect is now -2.4%, so slightly more negative, in line with the idea that we now only capture the effect on demand, while the effect of the share of land in treated areas is positive, 0.9%, in the direction suggested by theory. In addition, to examine whether the effect is more local (because the externalities may not spread out over 500m), we have as an alternative included the share of land in treated areas within 100m in column (2). This blows up standard errors even more because of severe collinearity, so not much can be learned from the latter exercise.[46]

It is imaginable that the externality effect of Airbnb is even more local, such that it only shows up within buildings. To investigate this, in column (3) we include an interaction term with housing type. If local negative externalities of Airbnb listings (*e.g.*, noise) *within buildings* are important, one expects that prices of condominiums have decreased less due to the HSO. This is not what we find (the difference in the effects for condominiums and single-family homes is not statistically significant). If anything, the effect of the HSO is slightly more pronounced for condominiums.

It may be the case that the latter estimates are affected by a court decision in March 2015 (see Watts v. Oak Shores Community Association, 2015). This decision empowered homeowner associations to impose limits and fees on short-term rentals and therefore affected condominiums, but not any other form of housing. As a substantial share of housing is subject to homeowner associations nowadays (Clarke and Freedman, 2019), and this decision may have affected our HSO estimate for condominiums, we have added an interaction term of the HSO for condominiums with a dummy indicating whether the transaction took place before the court decision in March 2015. It appears that this additional control variable does not change our results; if anything, the coefficient has the opposite sign.

All in all, we do not find strong evidence for the presence of a local external effect, implying that the estimated effect of the HSO only reflects an efficient use effect.

Airbnb listings and house prices: first-stage results

In this part of the Appendix, we consider the first-stage results. The second-stage results are reported in Table 2.8. The dependent variable is the Airbnb listings rate within 200m of the property in Table 2.20.

In column (1), Table 2.8, the coefficients imply that the HSO has reduced listings on average by about 0.4 percentage points, which is 67% of the mean listings rate. However, there is substantial heterogeneity, as expected. The effect of HSOs on the listings rate tends to become somewhat stronger once we include HSO border segment×month fixed effects. Columns (3) and (4) in Table 2.8 show similar effects once we respectively increase or decrease the bandwidth. Columns (5) and (6) show that the first-stage coefficient becomes somewhat stronger if we calculate the listings rate within 100m, while

[46]We have played around with different thresholds, but the conclusion that the external effect of Airbnb is too imprecise to pin down still holds.

it is somewhat lower if we take the listings rate within 500m.

Column (7) only considers the months for which we have listings data. This leads to a very similar first stage. Also if we consider an alternative approximated measure for listings in column (8), the first stage is very similar.

Table 2.20: Listings and house prices: First-stage results

(Dependent variable: listings rate in %)

	Panel RDD	*+ Border segment f.e.*	*Bandwidth: $h^* \times 2$*	*Bandwidth: $h^*/2$*	*Different thresholds*		*Selected dates*	*Approximated listings rate*
					100m	*500m*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
HSO implemented	-0.3919***	-0.5882***	-0.5229***	-0.4875***	-0.7245***	-0.4519***	-0.5577***	-0.4148***
	(0.0735)	(0.0775)	(0.0723)	(0.0888)	(0.1257)	(0.0428)	(0.1628)	(0.0528)
Property characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend variables included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HSO area×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	83,766	83,037	134,074	52,115	91,623	70,232	15,509	99,284
Bandwidth, b *(in km)*	2.9429	2.9257	5.8513	1.4628	3.4276	2.297	3.8509	2.8797
R^2	0.6955	0.7248	0.6954	0.7432	0.5504	0.8886	0.7082	0.7087

Notes: We exclude transactions occurring within half a year after implementation of the HSO. We instrument the listings rate a dummy indicating whether an HSO has been implemented. Robust standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Additional sensitivity analyses

Here, we subject our results to an additional range of robustness checks. We report the reduced-form results for prices in Table 2.21.

The first column improves on identification by including property fixed effects rather than census block fixed effects. Because we look at a relatively short period, this greatly reduces the number of degrees of freedom because most properties are sold only once between 2014 and 2018. Still, we find a negative and statistically significant effect of HSOs that is even somewhat higher: an HSO seems to be associated with a price decrease of 4.9%.[47] However, using a Hausman *T*-test, it appears that this coefficient is not statistically significantly higher than the baseline estimate where we include census block fixed effects.

In this paper, we use a Panel RDD to identify the house price effects based on an optimal bandwidth. As a sensitivity check instead of choosing a bandwidth, we include a second-order polynomial of distance to the nearest HSO border interacted with the treatment variable and time, as well as a 4^{th}-order polynomial of distance for the non-treated observations interacted with the treatment variable and time while including all observations. In column (2) we see that this has limited repercussions for the results. If anything, the effects of HSOs are slightly stronger.

One may still be worried that the effects of Airbnb are partly determined by locational attractiveness. Column (3) aims to further alleviate these concerns by including flexible second-order trends of pictures and year. The results are hardly affected.

In column (4) we match the transactions data to neighborhood characteristics (at the census block group level). That is, we match each transaction to the log of population density, the share of blacks, Hispanics, and Asians, household compositions, the share of renters, and the median age, in the previous year. Given that the effects are then very similar, this suggests that the effect of the HSOs (Airbnb) is predominantly due to a reduction (increase) in demand, rather than due to changes in the neighborhood composition.

Column (5), Table 2.21, further improves on identification by including straight border segment$\times$year fixed effects in spirit of Turner et al. (2014). The idea is that straight border segments are likely uncorrelated to geographical features of a location, which may impact price trends (*e.g.* through the propensity to build on the land). Because the average length of a straight border segment is just below 50m, we cannot include border segment$\times$month fixed effects, as this will lead to a too low number of degrees of freedom. We do not find that the price effects of the HSO are very different. The estimate

[47]Note that using property fixed effects implies that identification mainly occurs based on transactions sold both in 2014 and 2018, because properties are usually not transacted in subsequent years. This implies that we identify here a long-run effect of HSOs.

is similar but imprecisely estimated because of the high number of fixed effects.[48]

Table 2.21: Sensitivity analysis for reduced-form effects

(Dependent variable: log of house price)

	Property fixed effects	*Distance to border trends*	*Picture trends*	*Neighborhood characteristics*	*Straight segment trends*
	(1)	(2)	(3)	(4)	(5)
HSO implemented	-0.0486***	-0.0257***	-0.0192**	-0.0149*	-0.0155
	(0.0155)	(0.0081)	(0.0080)	(0.0079)	(0.0105)
Property characteristics	Yes	Yes	Yes	Yes	Yes
Spatio-temporal trend vars	Yes	Yes	Yes	Yes	Yes
Flexible spatio-temporal trend vars	No	Yes	No	No	No
Pictures×year trends	No	No	Yes	No	No
Neighborhood characteristics	No	No	No	Yes	No
Straight border segment×year FE	No	No	No	No	Yes
Property FE	Yes	No	No	No	No
Census block FE	Yes	Yes	Yes	Yes	Yes
Border segment×month FE	Yes	Yes	Yes	Yes	Yes
Number of observations	10,120	272,485	63,297	61,719	58,453
Bandwidth, b *(in km)*	2.1616	—	1.8101	1.8218	1.8148
R^2	0.9730	0.9132	0.9090	0.9095	0.9240

Notes: Standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

We repeat a similar set of specifications when estimating the impact of Airbnb listings on house prices. In all specifications, we instrument the listings rate with the HSO dummy. The results are reported in Table 2.22. Column (1) uses property fixed effects. The estimated effect is similar but very imprecise.

In column (2) we find a considerably stronger effect of the listing rate when we include trends instead of selecting a particular bandwidth: a 1 percentage point increase in the listings rate is associated with a price increase of 8.7%, which seems to be unrealistically strong. This suggests that using a local linear approach is preferred over including all observations (see Gelman and Imbens, 2016). When we control flexibly for differential price trends between more and less touristy areas in column (3), the coefficient of listings rate is somewhat higher than in the preferred specification. The same holds in

[48]Note that it is not entirely obvious how these freeways and mountains could invalidate our research design. First, amenity differences or preferences for these amenity differences need to change over time because we include census block fixed effects. We have a short time window, so this does not seem to be a major issue. Second, and more unlikely, these amenity differences over time and space should be correlated to the treatment variable. This implies that for HSO, there should be an improvement in amenities throughout, but only in HSOs, not on the other side of the border. Hence, we do not see how freeways or mountain ranges could invalidate our research design because they do not generate an amenity change, let alone an amenity change always at one side of the border.

column (4) when we control for changes in neighborhood characteristics, and column (5) where we use straight border-segment$\times$year fixed effects.

Column (6) that the results where we test the impact of the listings rate on prices are not driven by a few, potentially unrealistic, outliers. Indeed, when we exclude observations with a rate above 15%, the results are, if anything, stronger.

Sensitivity analyses for difference-in-differences estimation strategy

In this Appendix section we check for sensitivity of the results using the *Zillow* data, so the results using a DiD estimation strategy. We first report first-stage regression results in Table 2.23, corresponding to the second-stage results reported in Panel B of Table 2.7. It can easily be seen that HSOs reduce the listings rate by about 0.45-0.55 percentage points, which is comparable in magnitude as reported in Table 2.20.

Table 2.22: Sensitivity analysis: the impact of listings on house prices

(Dependent variable: log of house price)

	Property fixed effects	*Distance to border trends*	*Picture trends*	*Neighborhood characteristics*	*Straight segment trends*	*Listings rate <15%*
	(1)	(2)	(3)	(4)	(5)	(6)
Listings rate <200m *(imputed)*	0.0225	0.0839***	0.0528***	0.0450**	0.0638*	0.0641**
	(0.0605)	(0.0299)	(0.0204)	(0.0215)	(0.0367)	(0.0267)
Property characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Distance to border×year trends	No	Yes	No	No	No	No
Pictures×year trends	No	Yes	No	No	No	No
Neighborhood characteristics	No	No	Yes	Yes	No	No
Straight border segment×year fixed effects	No	No	No	Yes	No	Yes
Census block fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Border segment×month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	13,672	272,465	86,858	81,166	79,900	82,191
Bandwidth, b *(in km)*	3.0898	—	2.9319	2.7671	2.8364	2.7134
Kleibergen-Paap F-statistic	29.12	31.31	42.53	43.01	25.28	34.62

Notes: The listings rate is instrumented witha dummy variable indicating whether an HSO has been implemented. Robust standard errors are clustered at the census block level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In Table 2.24 we repeat the DiD analysis, but now we take the median list price in the *Zillow* data as the dependent variable. We find negative effects of the HSO in all specifications, with magnitudes that are very comparable as previously reported. Note that if we only include observations within 1km in column (4) we find a strong negative impact of HSOs, although the coefficient is somewhat imprecise. This is in contrast to the absence of any effect of HSOs on rents within 1km, and in line with the idea that long-term rents will not be discontinuous at the HSO border, while prices are. The reason is that two rental properties will be close substitutes and people are unlikely to be willing to pay more for a property that is just inside an HSO area.

In Panel B we report the results when instrumenting the listings rate with the city-specific HSO dummies. We find stronger effects than the baseline, but the coefficients are quite imprecise and usually only marginally statistically significant. This particularly holds for columns (3) and (4). Nevertheless, the point estimates are similar to the baseline results reported in Table 2.8.

Table 2.23: DiD results for rents, first-stage results

(Dependent variable: listings rate)

	All obs.	*Outside HSO, <25km*	*Outside HSO, >1km, <25km*	*Outside HSO, <1km*	*Outside HSO, >1km, <25km*	*Outside HSO, >2.5km, <25km*
	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
HSO implemented	-0.4690***	-0.5494***	-0.4485***	-0.7722***	-0.4437***	-0.4141***
	(0.1151)	(0.1163)	(0.1125)	(0.1952)	(0.1136)	(0.1283)
Dist. to CBD×year	No	No	No	No	Yes	Yes
Dist. to beach×year	No	No	No	No	Yes	Yes
Zipcode fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,491	3,231	2,951	722	2,951	2,472
R^2	0.9594	0.9619	0.9645	0.9554	0.9772	0.9589

Notes: In all specifications we include observations *inside* HSO areas. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 2.24: DiD results for prices, Zillow data

(Dependent variable: log median list price)

	All obs.	*Outside HSO, <25km*	*Outside HSO, >1km, <25km*	*Outside HSO, <1km*	*Outside HSO, >1km, <25km*	*Outside HSO, >2.5km, <25km*
Panel A: Effects of HSOs	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS
HSO implemented	-0.0315**	-0.0246*	-0.0172	-0.0312	-0.0285**	-0.0266**
	(0.0136)	(0.0134)	(0.0132)	(0.0228)	(0.0115)	(0.0114)
Dist. to CBD×year	No	No	No	No	Yes	Yes
Dist. to beach×year	No	No	No	No	Yes	Yes
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,429	3,169	2,889	660	2,889	2,410
R^2	0.9935	0.9895	0.9894	0.9869	0.9915	0.9919
Panel B: Effects of listings	(1) 2SLS	(2) 2SLS	(3) 2SLS	(4) 2SLS	(5) 2SLS	(6) 2SLS
Listings rate *(in %)*	0.0676**	0.0450*	0.0385	0.0411	0.0645**	0.0653**
	(0.0319)	(0.0238)	(0.0294)	(0.0263)	(0.0325)	(0.0324)
Dist. to CBD×year	No	No	No	No	Yes	Yes
Dist. to beach×year	No	No	No	No	Yes	Yes
Zipcode FE	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,429	3,169	2,889	660	2,889	2,410
Kleibergen-Paap *F*-statistic	16.30	21.91	15.51	14.08	14.97	9.860

Notes: In all specifications we include observations *inside* HSO areas. We exclude observations occurring within one year after implementation of the HSO. In Panel B we instrument the listings rate with a dummy indicating whether an HSO has been implemented. Standard errors are clustered at the zipcode level and in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Renters, income, and HSOs

Using data from the Community Survey on demographics in 2013, we regress a dummy indicating whether a city will implement an HSO on the share of renters. Table 2.25 reports the results.

Table 2.25: Renters and HSOs

(Dependent variable: HSO will be implemented)

	(1) Probit	(2) Probit	(3) Probit	(4) Probit
Share of renters	0.0007	0.6158***	0.9598**	1.2009**
	(0.2138)	(0.1936)	(0.3839)	(0.6089)
Average income per capita *(log)*		0.3606***	0.2793*	0.2686*
		(0.0546)	(0.1445)	(0.1473)
Share of blacks			0.2161	0.4103
			(0.6995)	(0.7339)
Share of Asians			0.1405	0.0953
			(0.2122)	(0.2001)
Share of other ethnicity			-1.2536	-1.5453
			(0.8508)	(0.9831)
Share of families			1.7343*	0.9631
			(0.9781)	(1.0725)
Share of couples			5.0080	3.8216
			(3.1254)	(3.1757)
Median age			0.0060	-0.0021
			(0.0124)	(0.0126)
Share single-family homes				0.6254
				(0.5887)
Share other homes				-2.3543
				(2.1849)
Observations	90	90	90	90
Pseudo-R^2	0.0000	0.3011	0.3703	0.4014

Notes: We report average marginal effects. Standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

When only including the share of renters, there is no effect. However, the share of renters is strongly negatively correlated to (log) neighborhood income ($\rho = 0.551$). If we control for log income, we find a strong positive association between the share of renters and the probability to have an HSO implemented. Also, income is positively correlated to this probability, likely because rich people do not care so much about the potential revenues from Airbnb, while poorer households could use the money. This is confirmed in column (3) where we further include a set of other demographic controls. There seems to be a proportional increase in the share of renters with respect to the probability to receive an HSO. In column (4) where we control for house type, the coefficient becomes even somewhat stronger. Although we refrain from giving a causal interpretation to these regressions, we think the correlations are in line with the idea that renters have more incentives to vote for the implementation of an HSO.

3

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

3.1 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;

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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).[49] 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.[50]

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

[49] 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.

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

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.[51] 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.

[51] 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.

The contribution of this research is twofold: Firstly, we provide causal evidence of how LEZ-induced air pollution reductions affect the housing market. Although numerous existing papers have analyzed the general effect of (air) pollution on the housing market, our research focuses on the policy effect of LEZs in particular as well as the less well-studied European context. Secondly, most papers that studied the link between air pollution and property markets solely focus on properties for purchase. 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.

The paper proceeds as follows. In Section 3.2 we discuss the research context. Section 4.3.1 introduces the used data and lists the main descriptive statistics. In Section 4.3.2 we elaborate on the identification strategy, followed by the main results and discussion in Section 3.5. Section 3.6 concludes the paper.

3.2 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 3.5). 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 3.1 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 3.6). 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).

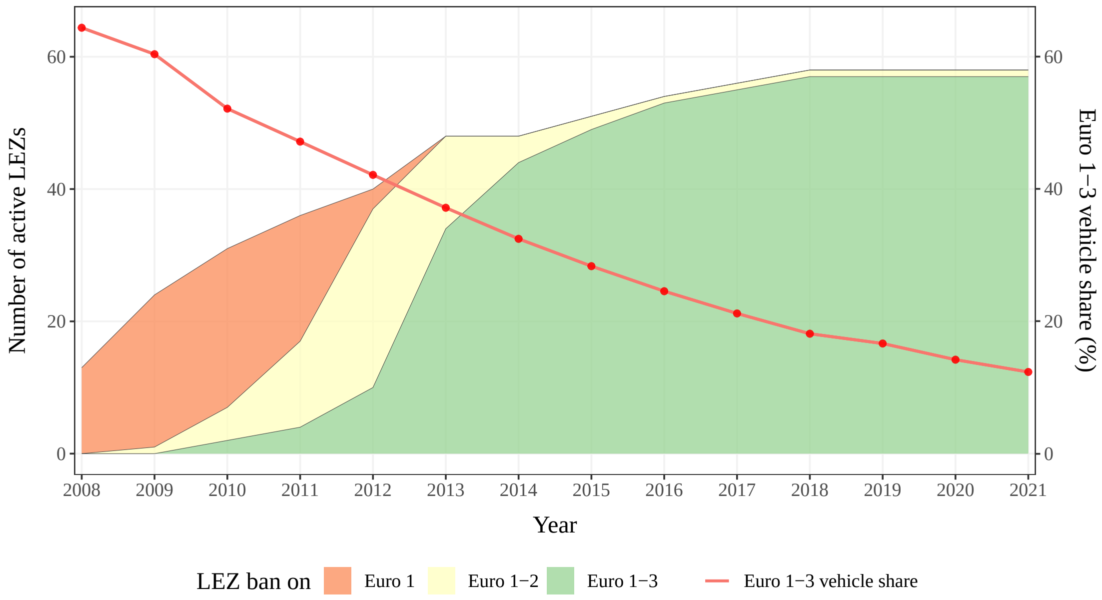


Figure 3.1: LEZs by emission standard in Germany over time

3.3 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 4.1.[52,53] 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.

[52]The specific selection of the control and treatment group is defined in Section 4.3.2.

[53]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 3.1: Descriptive statistics

Panel A: Outside LEZ areas	mean	sd	min	max	n
Log of rent (in €)	6.13	0.46	4.95	7.75	4,940,916
Distance to LEZ border (in m)	39,379	47,095	0.48	201,489	4,940,916
Log of living space (in m^2)	4.17	0.35	2.85	5.19	4,940,916
Number of rooms	2.49	0.90	1	10	4,940,255
Elevator present	0.19	0.40	0	1	4,940,916
Floor of object	1.75	1.88	0	14	4,940,916
Balcony present	0.60	0.49	0	1	4,940,916
Construction year	1966	34.64	1500	2020	3,149,946
Panel B: Inside LEZ areas	mean	sd	min	max	n
Log of rent (in €)	6.18	0.50	4.95	7.75	4,891,190
Distance to LEZ border (in m)	2,620	3,448	0.01	3,273	4,891,190
Log of living space (in m^2)	4.17	0.36	2.84	15.19	4,891,190
Number of rooms	2.42	0.89	1	10	4,890,353
Elevator present	0.21	0.41	0	1	4,891,190
Floor of object	1.84	1.68	0	14	4,891,190
Balcony present	0.53	0.50	0	1	4,891,190
Construction year	1956	38.91	1500	2020	2,814,797

3.4 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.

3.4.1 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[54] g. $LEZ_{i,g,t}$ 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.[55] 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 3.6 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 3.6 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:

$$log\, p_{i,t,g} = \alpha\, LEZ_{i,g,t} + \beta\, \mathbf{C}_{i,t} + \lambda_g + \phi_{c,t} + \varepsilon_{i,t,g}, \tag{3.1}$$

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.

3.4.2 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 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

[54]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).

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

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 (3.1) 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

$$log\, p_{i,t,g,j} = \alpha\,(LEZ_{i,g,j} \times Post_{t,j}) + \beta\,\mathbf{C}_{i,t} + \zeta_{t,j} + \eta_{g,j} + \lambda_g + \phi_{c,t} + \varepsilon_{i,t,g,j}, \qquad (3.2)$$

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 3.1, $\mathbf{C}_{i,t}$ controls for observed property characteristics, λ_g represents the employed grid level fixed effects and $\phi_{c,t}$ captures county by time fixed effects.

3.5 Results

3.5.1 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 3.2 compares the standard and stacked DiD model specification, estimated with the regression design of equation (3.1) and equation (3.2). 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 3.2, 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.[56] Appendix 3.7.4 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.

[56] In Appendix 3.7.2, 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.

Table 3.2: Estimation Results: Apartments for rent

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

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

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

$$\begin{aligned} log\, p_{i,t,g,j} = \sum_{s=\underline{t}}^{t-1} \alpha_s(LEZ_{i,g,j} \times Post_{s,j}) + \sum_{s=t}^{\bar{t}} \alpha_s(LEZ_{i,g,j} \times Post_{s,j}) \\ + \beta\, \mathbf{C}_{i,t} + \zeta_{t,j} + \eta_{g,j} + \lambda_g + \phi_{c,t} + \varepsilon_{i,t,g,j}, \quad (3.3) \end{aligned}$$

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.[57] 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 3.2 plots an event study of the stacked DiD specification per equation (3.3) of column (4) of Table 3.2. 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.

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

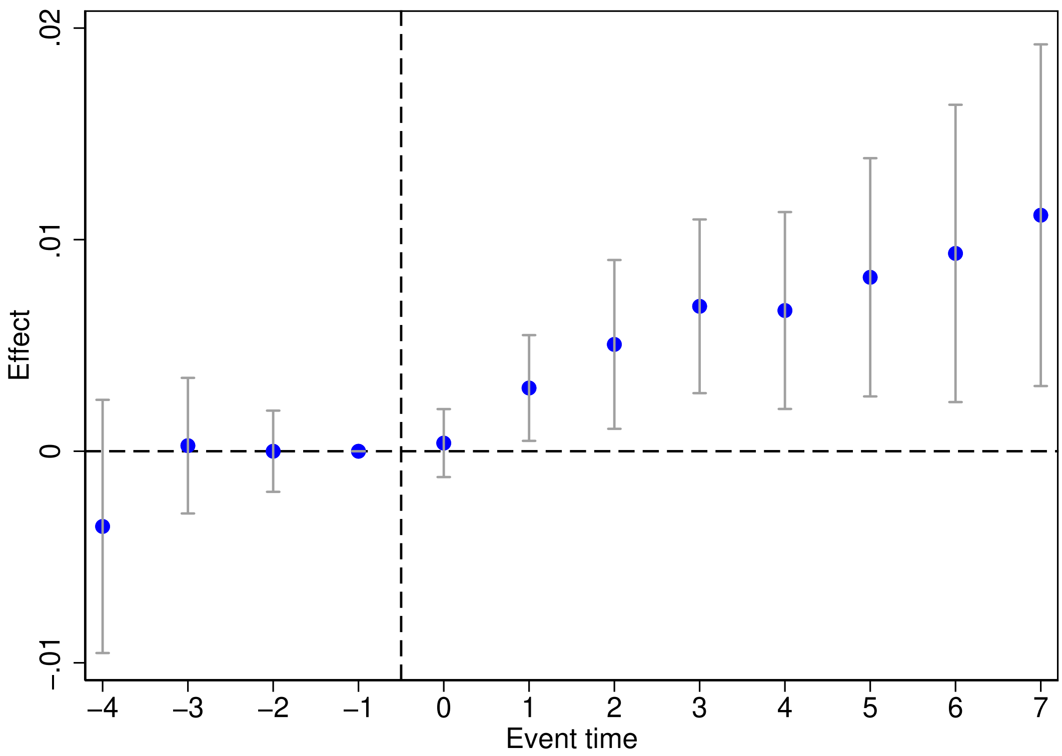


Figure 3.2: Event Study Results: Stacked DiD

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 (3.2) 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:

$$
\begin{aligned}
log\, p_{i,l,g,z,t} = \alpha\, LEZ_{i,t} + (\psi_1 + \psi_2\, t) LEZ_{i,t} d_{i,l} + (\psi_3 + \psi_4 t)(1 - LEZ_{i,t}) d_{i,l} + \beta\, C_{i,t} \\
+ \lambda_g + \nu_{l,t} + \xi_{z,t} + \varepsilon_{i,l,g,z,t}\text{ , if } d_{i,l} < b \quad (3.4)
\end{aligned}
$$

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 (3.4) and present for different bandwidths ranging from 0.5 to 3 kilometers in Table 3.3. 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 3.3: 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 3.3 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.

Table 3.4 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.

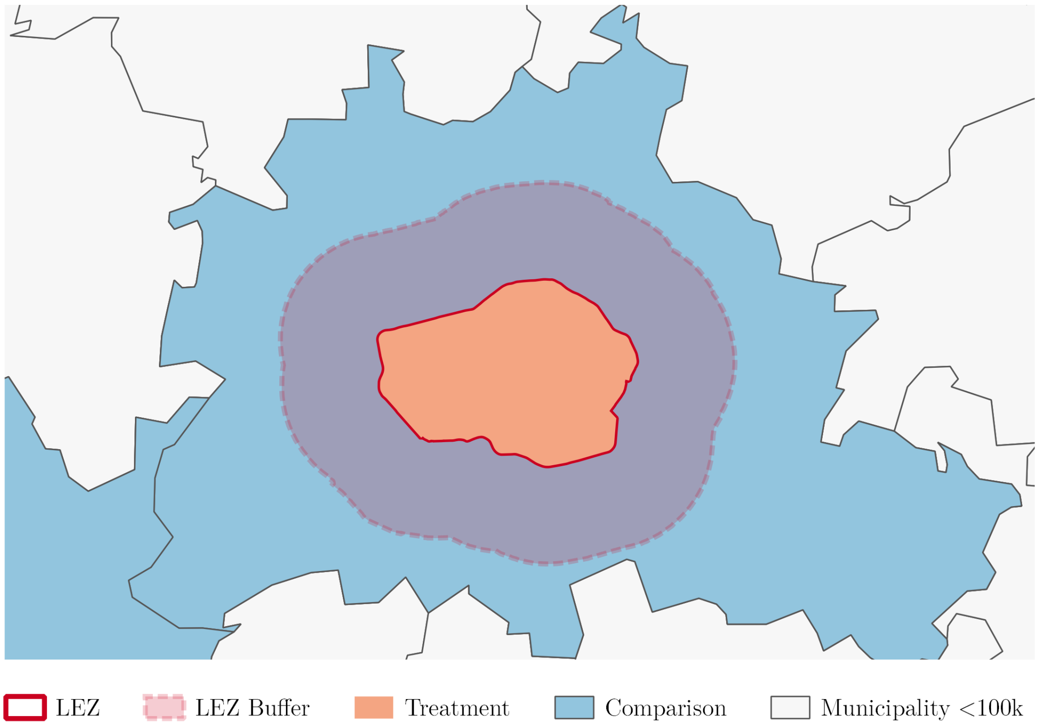


Figure 3.3: Treatment and comparison group without close control areas

Table 3.4: 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 3.5, 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 3.5 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 3.5 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 3.5: 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
Event time×treatment wave FE	yes	yes	yes	yes	yes	yes
Treated unit×treatment wave FE	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. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

3.5.2 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.[58] Figure 3.4 presents the results of this analysis (see Table 3.10 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.

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 3.1).[59] 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 3.4 presents the results (see Table 3.11 for details and various model specifications). We find that the earlier LEZ introductions had on average a positive effect of 2.23 percent (statisti-

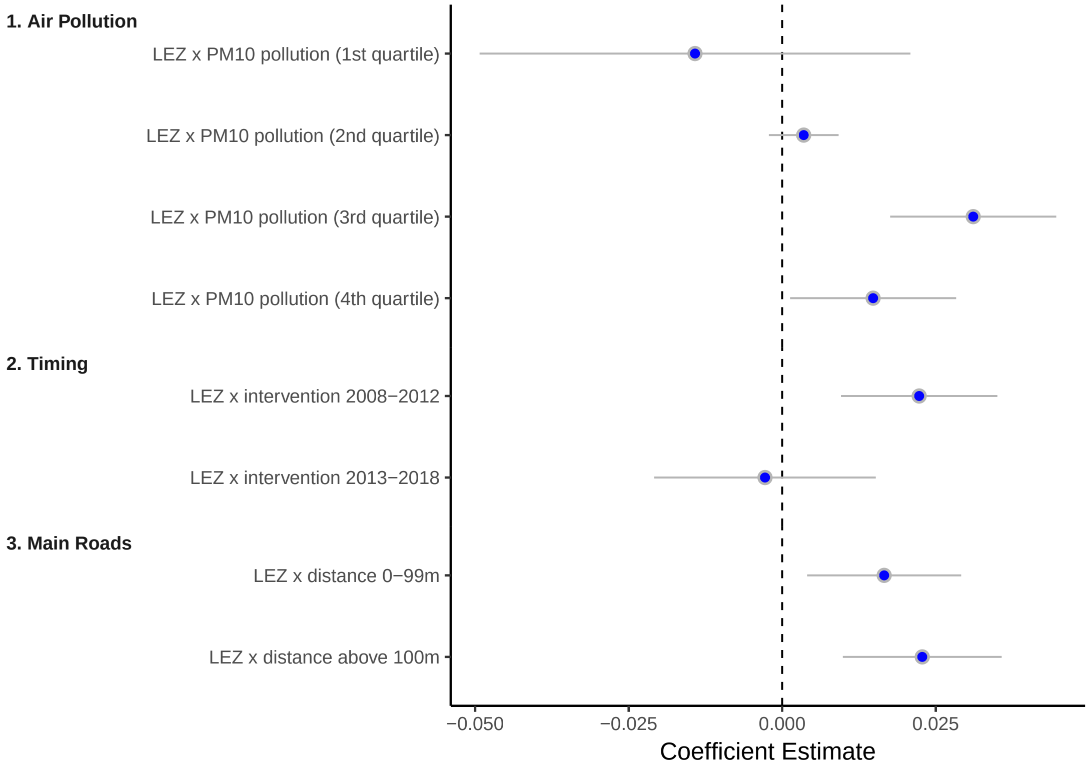
[58] Not for all treatment waves a pollution monitor was inside the treated grids in the year prior to treatment. Therefore, we only have the average wave-specific pollution level for 21 out of the 27 treatment waves. Only these waves will be analysed in this analysis.

[59] 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).

cally 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 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 3.4 presents the results of this analysis (see Table 3.12 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.



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 3.4: Overview of Results: Mechanism analysis

3.6 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.

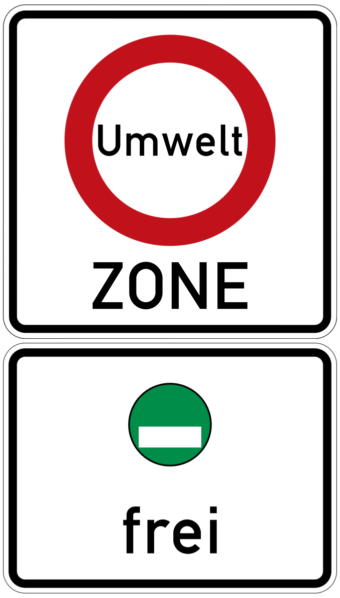
The analysis presented in this paper has two main limitations which should be addressed by future research. First, while we are able to access very rich geo-referenced data on offering prices for properties for rent or sale, we are not able to observe the actually realized prices in equilibrium. Second, 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.

3.7 Appendix

3.7.1 LEZs in Germany

(a) Euro 2-4 windshield emission stickers



(b) LEZ signpost Euro 4 vehicles only

Figure 3.5: LEZ vehicle stickers and signpost example

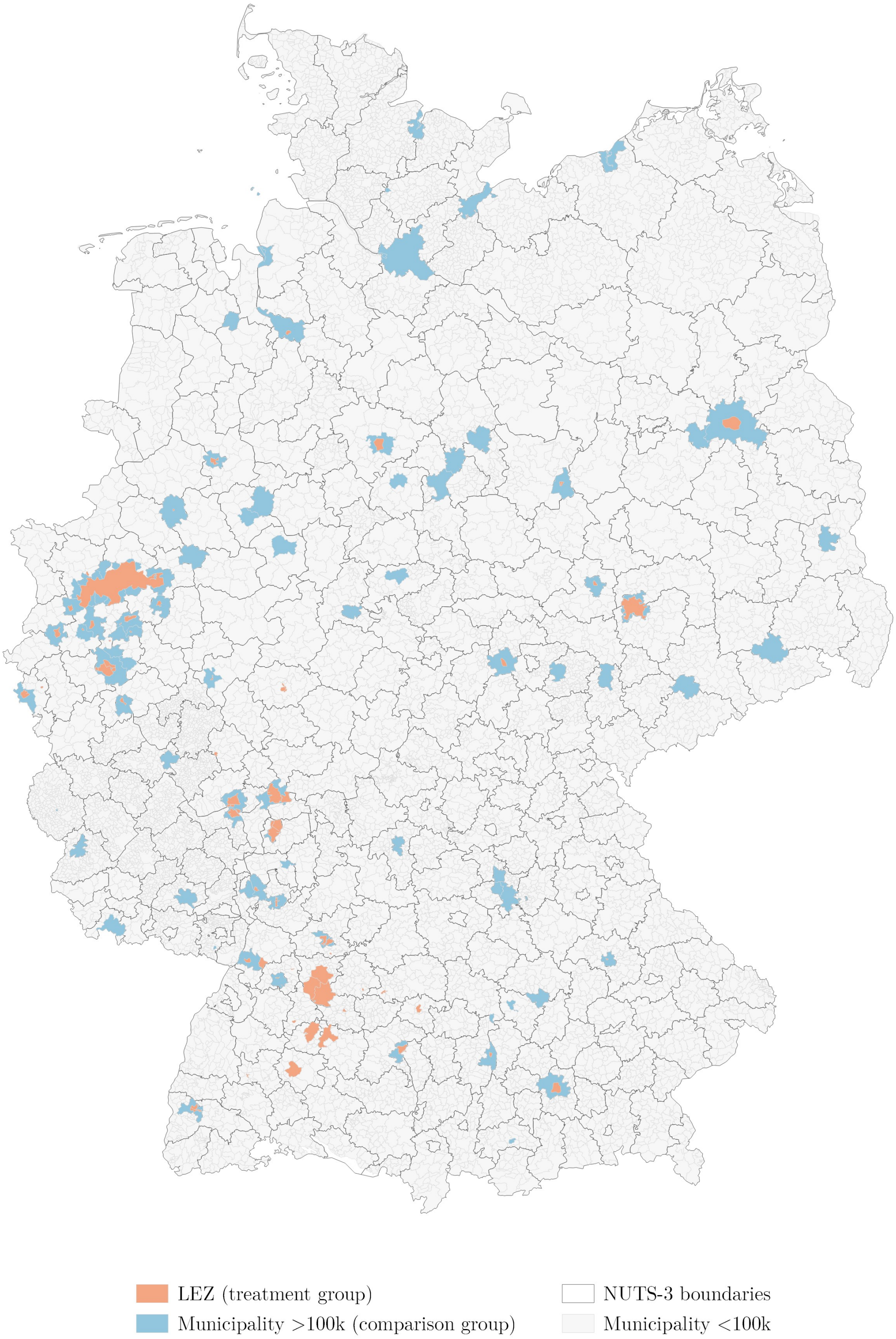


Figure 3.6: Treated cities and cities in control group

Table 3.6: 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.

3.7.2 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 3.7 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 3.7: Estimation Results: Alternative Outcome

	Dependent variable: Ad active in same month
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

3.7.3 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 3.8 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 3.8: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

3.7.4 Heterogeneity and Mechanisms

The following section contains detailed results on possible heterogeneous effects of our estimates as well as a compilation of the detailed results of the mechanisms, as previously discussed in Section 3.5.2.

Table 3.9: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.10: 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 3.11: 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 3.12: 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4

Regional Airports and Economic Growth: Evidence from the Single European Aviation Market

4.1 Introduction

Inspired by the initial success of the 1978 U.S. Airline Deregulation Act, which transformed its aviation industry into a single domestic aviation market, the European Union sought to embark on a similar, yet perhaps more challenging journey. The fragmented and primarily international nature of the EU aviation industry required a more cautious approach to reform its air transport landscape. At the time, the European aviation market was governed by numerous bilateral agreements between member states who rigorously controlled the supply of air capacity. Air fares were largely set by fixed agreements between the carriers, which acted under the patronage of the International Air Transport Association (IATA) (Butcher, 2010). In 1983, the European Commission started its deregulation initiative, the *Single European Aviation (SEA)* market, a policy of gradual liberalization to reshape Member State's transport networks into a competitive market environment (Graham, 1997). On a broader scale, the reform was part of the *Single European Act*, the EU's ambitious effort of establishing a single internal market.

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The aviation deregulation was split into three separate air transport liberalization packages. The first and the second package were implemented in 1988 and 1990, entailing, among others, the allowance of multiple designations, fifth-freedom rights, and three-bounded fare zones (Breidenbach, 2020)[60] Although the first two packages introduced important elements for the EU's air transport liberalization, it was undeniably the third package that produced significant adjustments to the aviation market environment. Its implementation stretched over a four-year period between January 1993 and April 1997. Major changes included the harmonization of airline licensing processes, entire liberalization of ticket fares, and the abolition of capacity regulations between member states (Graham, 1997). In sum, under the new environment all EU carriers gained open access to virtually every route in the European aviation market.

Following the completion of the marker deregulation, the aviation industry underwent an unprecedented expansion: the emergence of low-cost carriers (LCCs) such as Ryanair or easyJet, fueled an increase in air travel demand, resulting in major capital investments at airports across the EU. Not only major hubs but also smaller airports were able to benefit from this development. Due to increasing congestion at larger airports and LCCs focusing on point-to-point operations instead of a traditional hub-and-spoke system, smaller regional airports experienced a remarkable growth, with traffic increasing by more than 173% between 1993 and 2015 (ACI, 2017). During this period, policymakers successfully lobbied for extensive capital investments into their regional airport facilities, despite limited air traffic demand or proximity to larger, already well-established, airports.

Typically, an airport's profitability depends on its annual passenger throughput. For example, airports with fewer than 1 million passengers per year typically struggle to cover operating costs and often only continue to exist because of government subsidies (European Commission, 2014). In 2014, the European Commission outlined in its *"Guidelines on State aid to airports and airlines"* that government subsidies to airports violate European competition law. Consequently, subsidies for (regional) airports were prohibited. While larger airports had to operate self-sufficiently at once, smaller airports, with less than 3 million passengers per year, were granted a ten-year grace period until 2024 to adapt to the changes. Although very small airports, with passenger volumes below 200,000 per year, are exempt from this directive, the new guidelines are expected to have far-reaching implications. It is estimated that more than 50% of all airports within the EU operate on state aid (Grimme et al., 2018). Additionally, the current ramifications of the global COVID-19 pandemic further accelerate the situation

[60] *Multiple designations* refers to the concept of one country designating more than one of its airlines to fly a particular international route. For example, nowadays multiple U.S. airlines operate flights on the same routes across the Atlantic.
Fifth-freedom rights is the legal privilege of an airline from country A to fly between country B and C. For example, Air New Zealand, operates fifth-freedom flights between Los Angeles and London.
Three-bounded fare zones enable airlines to supply tickets below a standard minimum fare. This concept is of particular importance for low-cost airlines, which mostly generate revenues from ancillary services instead of the classic base fare.

such that many regional airport operators are set to face existential problems in the near future. Against this backdrop, the main research objective is twofold: First and foremost, it is imperative to understand how and to what extent regional airports affect their adjacent local economy. In other words, what is the link between the existence of a regional airport and its local economic development? Secondly, these findings need to be put into context to make a judgment on the hitherto existing government subsidies. What is the economic impact of a regional airport and to what extent may these subsidies be justifiable?

Estimating the economic effects of air transport infrastructure is challenging because of a strong interdependence between infrastructure investments and regional development. Economically thriving regions usually tend to invest more in infrastructure to further stimulate regional economic development (Breidenbach, 2020). Thus, the main challenge is to isolate the *causal effect* of transportation infrastructure on economic growth. To overcome these challenges in identifying the relationship between air transport infrastructure and regional economic growth, a quasi-natural research design is employed by relying on the exogenous nature of the *Single European Aviation market reform* of 1997. Using a difference-in-differences (DiD) design[61], areas with regional airports versus areas without regional airports are compared, both before and after they qualified as regional airport areas (based on their passenger numbers), to estimate the average treatment effect of regional airports on the local economy.

This paper proceeds as follows. Section 4.2 discusses the related literature on infrastructure, airports, and economic growth. Section 4.3 introduces the data and the applied methodology. Section 4.4 reports and discusses the main results of the analysis, including estimation results, various sample specifications, and sensitivity analyses. Section 4.5 concludes the paper.

4.2 Related literature

This research contributes to three main branches of the literature. First, it relates to the overall literature of linking infrastructure investments and economic growth. Infrastructure investments are widely acknowledged to play a vital role in the transportation sector and economic growth as a whole (Magazzino and Maltese, 2021). Particularly transport infrastructure improves connectivity between firms, workers, and customers (Li et al., 2020). Advanced infrastructures enable goods and services to move freely from one area to another and are a distinct condition for a functioning transport system. While congestion slows down productions and overall economic growth, mobility is the foundation of resilient nations (Rietveld, 1989). Unsurprisingly, expenditure on

[61]The DiD approach is a commonly used statistical method to establish causal inference when randomization is not possible. The main idea of the DiD identification relies on mimicking an experimental setting by finding equivalents of 'treatment' and 'control group'. Its key assumption relies on the notion that observed differences between treatment and control groups are time-invariant (World Bank, 2022).

transport infrastructure very often amounts to a significant share of public expenditures (Farhadi, 2015). This paper's main finding supports the view that infrastructure investments have positive effects on local GDP, employment, and population growth.

Second, this research contributes to the airports-economic growth nexus literature. Predominately, the relationship between aviation and economic development is seen positively, however, the extent and underlying driving forces are by no means clear. Particularly regional pre-conditions are likely correlated with the existence of an airport, resulting in biased estimates. In other words, since the choice of an airport construction is not random, it can be expected that the location of an airport is more likely to be in regions with overall greater (expected) economic prosperity. There are several non-academic studies that examine the linkage between airports and economic growth.[62]. The notion of spillover effects from aviation to the local economy has also received significant attention in the academic literature, although most of these analyses focuses on large metropolitan areas. For example, Brueckner (2003) estimates the effect of air travel on production in U.S. metropolitan areas. By using the distance from the population center and the airport's hub status for a major commercial airline as an instrumental variable, he shows that air traffic growth has a positive impact on employment in service-related industries but no significant effect on manufacturing employment.[63] In a later analysis, Green (2007) tests whether the activity at a metropolitan area's airport predicts population and employment growth. Using an airport's physical size as well as industry level employment figures as instruments, he finds a positive relationship. Blonigen and Cristea (2012) exploit the variation of long-run growth rates around the 1978 U.S. Airline Deregulation Act as a source of variation in air traffic levels. They find a positive effect of airports on growth, with the magnitude of the effects differing by metropolitan size and industrial specialization. Sheard (2014) instruments for the distribution of airports, using a legislation that enabled federal funding for the construction and improvement of airports. He finds improved air service to have a positive effect on the size of the local service sector, but no effect on non-tradable services. Campante and Yanagizawa-Drott (2018) study the impact of international long-distance flights on the spatial allocation of economic activity. Using satellite-measured night lights, they show that airport expansions have a positive effect on local economic activity.

Third, this paper contributes to the underdeveloped literature on the effects of *regional airports* on local economic development. Debbage (1999) examined the linkage between airport operation and the structural composition of the regional economy in North and

[62]Aviation advocates such as IATA or the Air Transport Action Group (ATAG), tend to emphasize the role of air traffic growth as an economic catalyst for a region, by means of increasing connectivity and therefore commerce and employment (see e.g. IATA (2019) or Air Transport Action Group (2018)). However, it quickly becomes apparent that these estimates suffer from omitted variable bias due to a lack of a convincing identification strategy (see e.g. InterVISTAS (2015) or SEO Amsterdam Economics (2015))

[63]It is questionable whether the instrument is truly valid since the location of an airport hub may not necessarily be exogenous but instead the result of a deliberate choice by an airline to be located in a geographically advantageous location.

South Carolina. Although he finds that areas with significant increase in air passenger volume achieved employment gains, he does not address the issue of causality in his analysis. Yao and Yang (2008) show by means of an extensive panel data analysis that regional airport development in China is positively related with regional economic growth. A recent paper by Breidenbach (2020) offers a promising approach to measure the economic effects of regional airports in Germany. The author's identification strategy relies on exploiting the SEA market liberalization in the EU as a quasi-natural experiment, similarly to Blonigen and Cristea (2012) in the United States. He employs a DiD estimation by comparing regions with and without regional airports before and after the market reform was in place. Likely due to a lack of statistical power, the author finds no evidence that the expansion of regional airports in Germany generated regional growth. It remains unclear why the author limited the analysis to merely one country despite that the market reform affected the European Union as a whole. This research builds on and extends the methodology of Breidenbach (2020) and significantly expands the geographic scope of the analysis. To the best knowledge, this paper is the first attempt to investigate the relationship between regional airports and local economic development on a Pan-European level. Other papers have used similar regional administrative datasets, for example Percoco (2010) who studies airport activity and local development in the Italian context, but the focus remained on a country level. Finally, this paper also complements previous research that investigated the EU market liberalization from a pure transportation perspective, such as, Laurino and Beria (2014) who investigate at the relationship between LCCs and secondary airports in Italy in the context of the EU market deregulation.

4.3 Data and methodology

4.3.1 Data

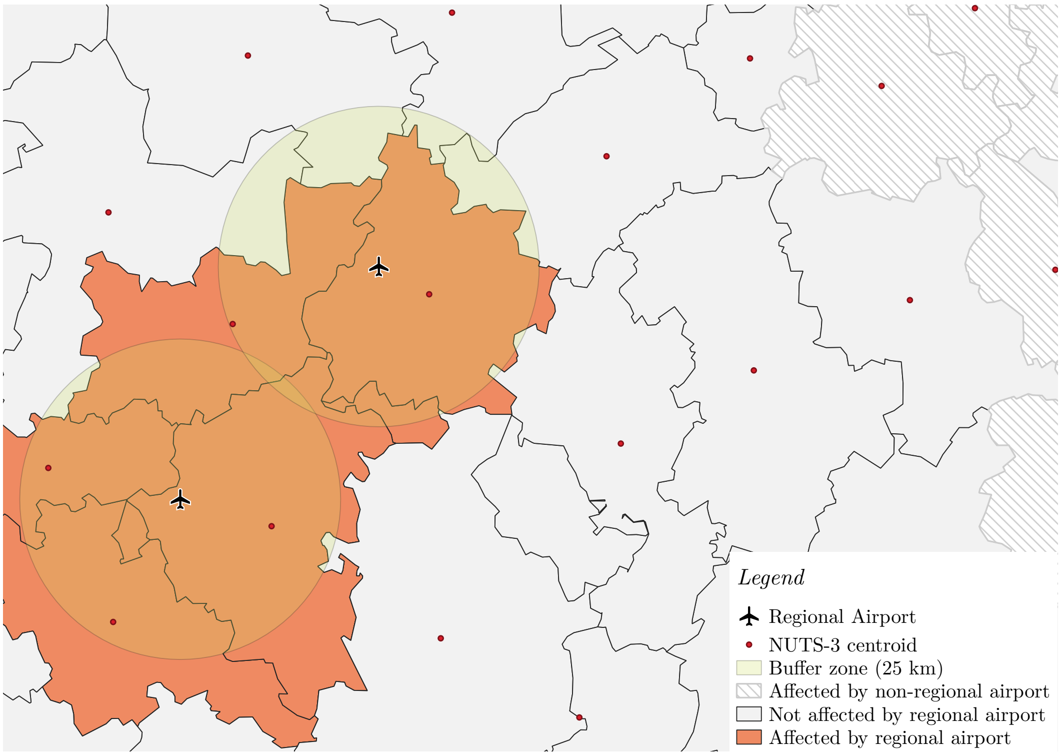
The panel data set consists of three pillars: Firstly, current and historical data on all commercial airports in the European Union were collected, containing information on location, size, as well as construction year and purpose. The airport data set is a combination and manual cross-check from local airport statistics as well as the publicly available data set from the website `www.ourairports.com`. Defining which of these airports constitutes a *regional* airport, is by no means clear. While some studies link the notion of a regional airport to the fact that the catchment area of an airport is located outside a major city, there is no common definition at an EU level to date. Given the context of expiring EU state aid for airports, the European Commission's (EC) subsidy ceiling of 3 million passengers per year is used as an upper boundary. Airports below the 3 million passenger threshold are defined as *regional* airports, while airports above this threshold are considered as *non-regional* airports. Hence, in the panel data an airport can be first defined as *regional* and at a later stage as *non-regional*, if it surpasses the 3 million passenger threshold.[64] As robustness checks, passenger thresholds of 2 and

[64]The share of commercially active regional airports declined from over 90% in 1980 to about 75% in 2016.

1 million passengers per annum are also used. Secondly, Eurostat administrative level data on regional socio-economic activity is compiled, containing NUTS-3 level data on local GDP, population, and employment, ranging from 1980 until 2016 (Eurostat, 2018).[65] Thirdly, satellite-generated data on terrain elevation is processed to estimate the extent of aviation-related usable land in the EU-15. Terrain ruggedness is related as a constraining factor to the development of (regional) airports. To do so, the standard deviation of elevation is computed to control for ruggedness, as well as the average height of a particular area, both measured at a 2.5 km radius around the centroid of each NUTS-3 polygon. The satellite images are obtained from the EU Digital Elevation Model (EU-DEM), provided by Copernicus Land Monitoring Service (Copernicus Land Monitoring Service, 2020).

To combine the data sets, the airports' location is matched with the respective NUTS-3 region. For the analysis, NUTS-3 regions containing an airport are considered to be *affected*. However, likely the presence of an airport also affects variables of interest across space, meaning that regions that do not inhibit an airport but are near one, are still affected by its activity. To account for such spillover effects, the distance from the centroid of each NUTS-3 region to the nearest regional and non-regional airport is computed. If the centroid of a particular NUTS-3 region is located within a certain proximity to a (regional) airport, the regional unit was considered as *affected* by the airport, while the remaining regions are considered to by *unaffected*. When a particular NUTS-3 region is affected by both a regional and a non-regional airport, the more conservative measure was chosen, that is the region was classified as a non-regional airport region. The exact distance threshold that classifies NUTS-3 regions into *affected* and *unaffected* areas is discussed in detail in section 4.4.3. Figure 4.1 illustrates this concept graphically using a 25 km buffer zone. Each NUTS-3 centroid that falls within the radius of a regional airport, is considered as 'affected' by the regional airport. In this exemplary excerpt, five NUTS-3 fall under this category (highlighted in orange color). The second category of regions are NUTS-3, which centroid is not encompassed by the buffer of a regional airport (highlighted in gray color). Finally, some NUTS-3 regions are excluded from the specification if their NUTS-3 centroid already falls within the buffer of a non-regional, i.e., international airport (highlighted in hatched gray).

[65]NUTS-3 (Nomenclature of Territorial Units for Statistics) corresponds to the most granular spatial unit, primarily used for socio-economic analyses of small regions.



Notes: Exemplary regional airport definition based on European Commission's threshold.

Figure 4.1: NUTS-3 regions and airport buffer zones

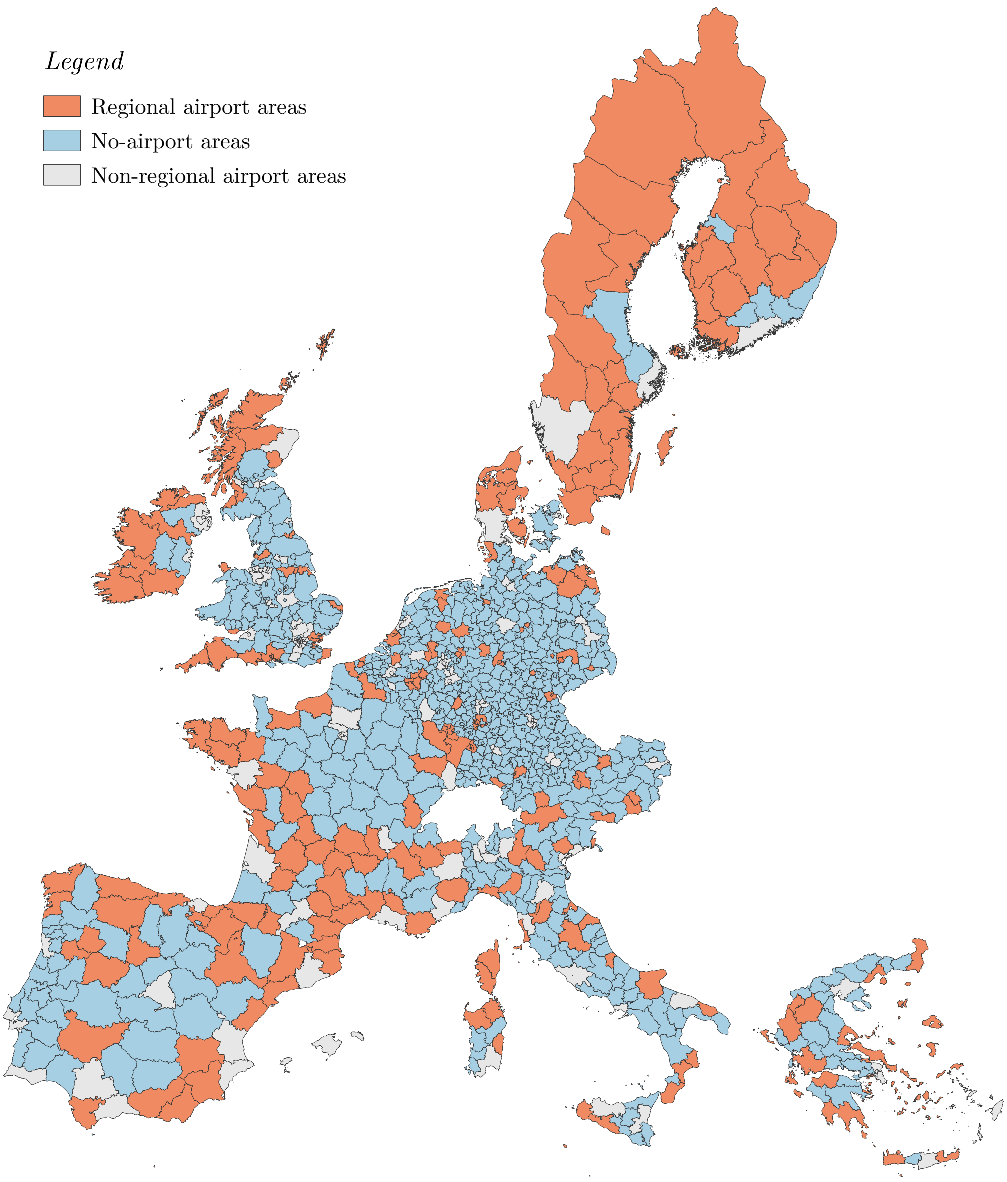
Table 4.1 summarizes the key descriptive statistics across the EU-15 for regions that are either affected or unaffected by regional airports between 1980 and 2016. The dedicated buffer zone to capture spillover effects is 25 km while the threshold that classifies an airport into a regional airport is 3 million passengers per year or less, in line with the European Commission's definition. Note that NUTS-3 regions containing or being adjacent to a non-regional airport (under a 20 km buffer definition) are excluded, since the aim of the paper is to solely analyze the economic effects of *regional airports*. On average, regions that are affected by regional airports tend to have higher levels of economic activity, measured by *total GDP in Purchasing Power Standards (PPS)*. Also, *employment*, *total population*, and *population density* are on average higher in those regions. Furthermore, the average elevation in a 2.5 km radius around the centroid of each NUTS-3 region is compared to capture possible geographic constraints for construction. Regions unaffected by regional airports exhibit on average higher elevations, including a higher degree of ruggedness, expressed by a higher standard deviation, than regions that are affected by regional airports. By construction, NUTS-3 regions affected by regional airports have a smaller mean distance from their centroid to the nearest regional airport. Note that the mean and max value are larger than the defined 25 km buffer in the regions that are affected by regional airports.

This is because NUTS-3 regions containing a regional airport, but its centroid is not encapsulated in the 25 km buffer zone, are still considered as affected. This distinction is important for large rural areas such as in northern Scandinavia where regional airports are scarce but still affect the sparsely inhabited region. Lastly, affected NUTS-3 areas are on average further away from the nearest non-regional airport than unaffected areas, indicating the importance to control for this variable. Figure 4.2 summarizes this allocation between affected NUTS-3 regions and unaffected NUTS-3 regions graphically. The quantity and size of NUTS-3 regions differs significantly among countries in the EU-15. For instance, Germany contains most of the NUTS-3 regions, however, also the smallest. In contrast, for example the Scandinavian countries encompass fewer but larger NUTS-3 regions. Overall, there are more no-airport than regional airport areas in the EU-15, based on the discussed concept of Figure 4.1. These NUTS-3 areas are mostly located in the central and less populated areas (highlighted in blue color). On the other hand, regional airport areas are often located in popular tourist areas, such as the South of France or Italy, or in areas with a sizable local demand but below the defined threshold of an international airport, for instance in northern Spain or France (highlighted in orange color). Finally, note that most non-regional airport areas, that is NUTS-3 areas that inhibit an international airport, are primarily located around the capitals and the most populous areas of each member state (highlighted in gray color).

Table 4.1: Descriptive statistics

Panel A: Regional airport areas	mean	sd	min	max	n
log(GDP)	22.24	1.05	16.81	25.02	11,781
log(employment)	11.65	0.91	7.05	13.88	11,781
log(population)	12.53	0.88	8.84	14.95	11,781
log(population density.)	5.15	1.65	0.61	9.44	11,781
log(elevation)	4.83	1.32	-0.25	7.37	11,781
Distance regional airport (km)	22.30	15.69	1.87	111.94	11,781
Distance non-regional airport (km)	161.24	120.01	21.38	826.33	11,781
Panel B: No airport areas	mean	sd	min	max	n
log(GDP)	21.85	0.86	17.73	25.32	24,732
log(employment)	11.28	0.71	8.11	13.88	24,732
log(population)	12.14	0.71	9.66	14.21	24,732
log(population density)	5.09	1.18	2.18	8.93	24,732
log(elevation)	5.12	1.38	-2.89	7.59	24,732
Distance regional airport (km)	66.71	29.93	25.02	187.89	24,732
Distance non-regional airport (km)	106.92	78.15	20.13	582.83	24,732

Notes: Based on 25 km buffer zone and EC regional airport passenger definition.

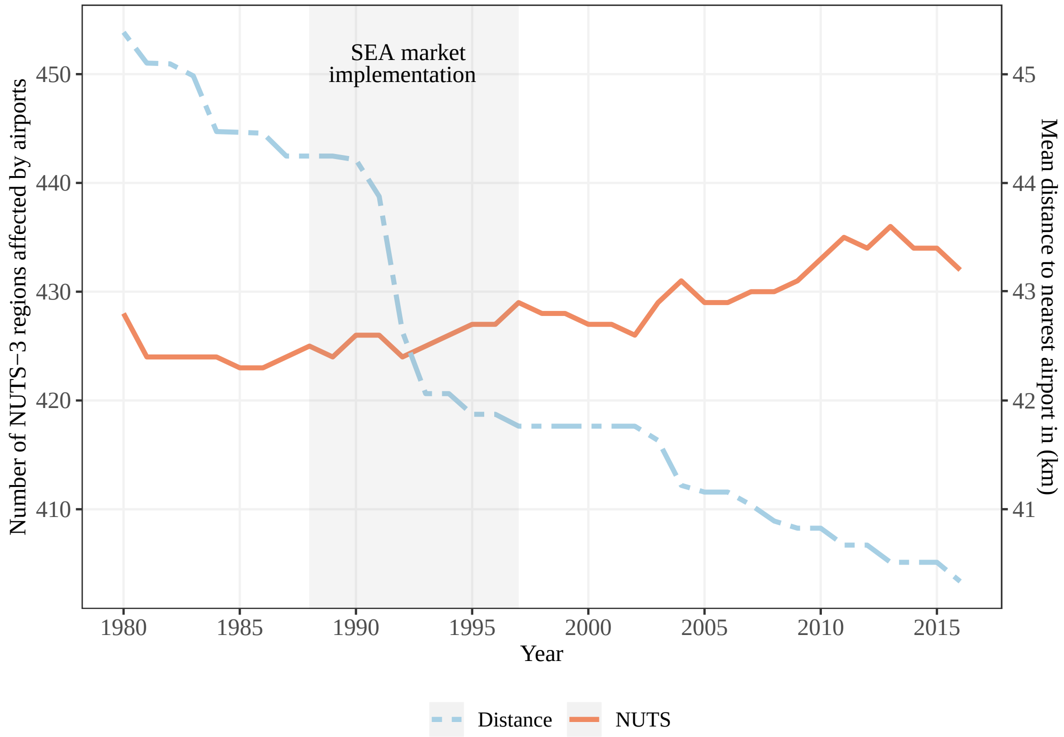


Notes: Based on 25 km buffer zone and EC regional airport passenger definition in 2016.

Figure 4.2: EU-15 NUTS-3 treatment vs. comparison group

Figure 4.3 displays the relationship between the introduction of the SEA market reform and the growth of airports in the EU-15. Most notably, the number of NUTS-3 regions affected by an airport began increasing since the SEA implementation phase in the mid-1990s (orange line). Either smaller airports were converted into airports with commercial operations or entirely new airports were constructed during this period.

Not surprisingly this surge in airports also resulted in better connectivity of the NUTS-3 regions across the EU, depicted by a decreasing average distance to the next airport (dashed blue line).



Notes: Based on 25 km buffer zone and EC regional airport passenger definition.

Figure 4.3: Number of EU-15 NUTS-3 regions affected by airports

4.3.2 Methodology

Although the nature of the panel data set permits adding time and region fixed effects to control for time-invariant characteristics, one would still neglect possible endogeneity concerns. Most prominently, major capital investments into infrastructure such as airports are usually no coincidence but instead the outcome of a deliberate economic decision to serve the existing and future economic needs of a region, resulting in a biased estimate. To overcome this, it is assumed that the unobserved differences between treatment and comparison groups, in the absence of the treatment, are the same over time. In this case, when comparing the difference of treatment and control groups *before* and *after* receiving the treatment, the *average treatment effect* is estimated.

In this context, a *DiD* methodology is suitable to estimate this effect. Following a similar methodological estimation approach by Dröes and Koster (2016), $Y_{i,t}$ represents the economic activity, measured by GDP, population, or employment levels of NUTS-3 regional unit i in year t. $A_{i,t}$ is a binary indicator variable that that equals one in years t when an airport's passenger volumes are within the European Commission's defined range of regional airports and within d kilometers of NUTS-3 regional unit i. v_i is the treatment versus control group dummy, where the treatment group are those NUTS-3 units within d kilometers of a regional airport. The basic DiD model can hence be formulated as:

$$log\, Y_{i,t} = \alpha A_{i,t} + \gamma v_i + \lambda_t + \varepsilon_{i,t}, \tag{4.1}$$

where α captures the average treatment effect, λ_t captures year fixed effects, and $\varepsilon_{i,t}$ is an identically and independently distributed error term. In the main specification, a distance of $d = 25\ km$ is assumed, which is later on pressure tested with various robustness checks (see section 4.4.3).

There are two sources of identifying variation to measure the treatment effect. Firstly, the difference in economic activity between regions *within* versus *outside* the 25 km buffer around a regional airport is compared. Secondly, because not all airports are in existence at the same time or meet the necessary passenger volumes to classify as a regional airport, the difference in economic activities is compared in areas that have a regional airport versus areas without a regional airport at that point in time but will be classified as one at a later stage.

To control for differences in the regional economic composition of the control and treatment group, relevant control variables are added to the specification:

$$log\, Y_{i,t} = \alpha A_{i,t} + \beta C_{i,t} + \gamma v_i + \lambda_t + \varepsilon_{i,t}, \tag{4.2}$$

where $C_{i,t}$ is a set of regional (economic) controls such as distance to the nearest non-regional airport, sectoral employment shares, and geographic construction constraints, measured by terrain ruggedness.

Since airports are not randomly distributed across space, for instance as they may be more likely to be constructed in areas with an existing economic need, one might be concerned with selection bias. This, however, should be captured by the constructed treatment group dummy variable v_i. Yet, there are likely also other unobserved factors (e.g., public service obligations that require the construction of an airfield in certain remote areas) that also affect regional economic activity. To a large extent, these factors are time invariant, allowing to control for them by using region-specific fixed effects:

$$log\, Y_{i,t} = \alpha A_{i,t} + \beta C_{i,t} + \gamma v_i + \mu_i + \lambda_t + \varepsilon_{i,t}, \tag{4.3}$$

where μ_i represents the fixed effect for a respective NUTS-3 regional unit. Employing regional fixed effects helps to deal with all unobserved time-invariant spatial attributes that may cause the location of a regional airport to be correlated with $\varepsilon_{i,t}$. Although the NUTS-3 classification is the most granular level for socio-economic analyses of small regions, there is a substantial difference in the size of the NUTS-3 regions per country (see Table 4.7 in the Appendix).

While it is not possible to adjust for the difference in the unit of analysis, it may make sense however to account for unobserved local trends (e.g., local strategic policies) that are correlated with the presence of a regional airport. To do so, equation (4.3) is estimated using restricted sample within a relative short distance (50 km) around each regional airport.[66] This leads to a smaller sample size, but it addresses the issue of unobserved trends. One might also expect that the economic effect of regional airports becomes less pronounced for NUTS-3 regions that are located further away from the airport. Therefore, the treatment effect is altered over different distances from the nearest regional airport:

$$log\, Y_{i,t} = \sum_z \alpha_z A_{i,t,z} + \beta C_{i,t} + \gamma v_i + \mu_i + \lambda_t + \varepsilon_{i,t}, \tag{4.4}$$

where $A_{i,t,z}$ equals one if it meets the passenger volume classification as before *and* is located within the corresponding band z. Essentially, the 50 km radius (restricted sample) is split around a regional airport into buffers (up to 15 km, up to 17.5 km, etc.), where the remainder serves as the reference group. Methodologically, it is tested at which point the cut-off value α_z yields to statistically insignificant results, relative to the reference group. Note that the main assumption is that the effect of the reference group is zero.

Finally, one would expect the effect to differ before and after the airport is defined as a *regional airport*. The construction or announced expansion of an airport is usually known some time before, meaning that regional economic activity could have already incorporated this information prior. This will imply a possible underestimation of the causal effect if anticipation effects were not considered. Since the variable $A_{i,t}$ changes at different times in different NUTS-3, in the spirit of Granger (1969), the idea is to perform a causality test where past $A_{i,t}$ predict $Y_{i,t}$, while future $A_{i,t}$ do not (Angrist and Pischke, 2008). In other words, the key assumption underlying a DiD strategy is the presence of a common trend between the treatment and control group (common trend

[66]This is also tested with even smaller values i.e. a more restricted sample. However, under these restrictions the variable *regional airport presence* becomes omitted as it is probably collinear with the fixed effects of the model.

assumption). This assumption cannot be tested, but, it is common practice to examine this concern by undertaking an *event study* to show that there is no statistically significant effect before a NUTS-3 region was affected by a regional airport, which suggests (but does not prove) that the common trend assumption holds. Methodologically, this concept can be expressed by:

$$log\, Y_{i,t} = \sum_{s=\underline{t}}^{t-1} \alpha_s A_{i,s} + \sum_{s=t}^{\bar{t}} \alpha_s A_{i,s} + \beta C_{i,t} + \gamma v_i + \mu_i + \lambda_t + \varepsilon_{i,t}, \tag{4.5}$$

where $\underline{t}$ is first year when to expect anticipation effects, while $\bar{t}$ represents the last year for which to expect adjustment effects. $A_{i,s}$ equals one when the NUTS-3 area is considered as treated in year s and zero otherwise. Therefore, the first sums term represents the anticipatory effects of an airport construction or expansion while the second sums term captures the post-treatment effects after a particular NUTS-3 area contains or is affected by a regional airport. For this analysis, a time window of ten years before and five years after a NUTS-3 region is affected by a regional airport is used, that is $\underline{t} = t-10$ and $\bar{t} = t + 5$.

4.4 Results

The results section is organized along the twofold objective of this research: Firstly, it is essential to understand how and to what extent regional airports affect their adjacent local economy. To do so, a rigorous process of various sample specification and estimation techniques is traversed to establish a convincing range of point estimates of this effect (section 4.4.1). Secondly, these findings are put into context of expiring government subsidies by evaluating the total economic impact of regional airports in the EU (4.4.2). Finally, various sensitivity checks are presented to contextualize these findings (4.4.3).

4.4.1 Economic effects of regional airports

In order to derive a credible range of point estimates for the economic effects of regional airports, two distinct sample specifications as well as an alternative identification strategy are examined.

Unrestricted sample specification

To investigate whether the level of regional GDP correlates with the existence of an airport, a simple regression model is specified where the binary variable *airport presence*, which equals one if a NUTS-3 region or its adjacent locality inhibits an airport and zero otherwise, is regressed on the dependent variable of interest *GDP*. Note that this specification is performed on the whole data set, that is *all commercially active airports* EU-15. As a single control variable, the distance to the nearest non-regional airport is added. Furthermore, year and region fixed effects are added to control for heterogeneity. In column (1) of Table 4.2, a positive and statistically significant relationship is found between the presence of an airport and its local GDP. Holding all else equal, the presence of an airport increases economic activity on average by $\exp(0.0376) - 1 \approx 3.83\%$, in comparison to no-airport areas. In column (5) this analysis is repeated, now, by substituting the dependent variable of interest with the level of population. The estimate suggests that the presence of an airport leads to on average 1.02% higher population levels. Lastly, in column (9) the effect on employment levels is tested. The estimate indicates that airports positively affected employment levels, although the effect is statistically insignificant at the 5% level.

The remaining columns of Table 4.2 focus on the main objective of this research, the effects of *regional airports*. Column (2) shows the regression estimates of equation (4.3), the standard DiD model, controlling for year and region fixed effects. Also sectoral employment shares and terrain ruggedness are added as additional control variables to account for possible regional heterogeneity. A vast body of literature has shown that urban areas with a more favorable environment tend to develop faster over time (see e.g. Saiz (2010)). In the context of airports, it is reasonable to assume that airports are constructed and expanded in areas with a more favorable geography. However, since there is no variation in elevation over time, the measure would be perfectly collinear and therefore be dropped in the estimation. Therefore, the interaction term 'Elevation x Year' is created to capture the geography's impact on regional growth. The estimate remains positive and significant at the 5% level, indicating that regional airport regions on average have approximately 2.09% higher GDP. The effect is also positive for the level of population in column (6), albeit insignificant. The results also suggest that employment in the *construction* and *industry sector* benefited from the presence of a regional airport. In contrast, the coefficients for *agriculture*, *services*, and *non-market services*[67] show a negative sign. This effect on the local economy, however, is not necessarily causal.

Yet, there might be other unobserved traits such as changes in local aviation policies, that might be correlated with the presence of a regional airport and hence affect the estimates. To account for such trends, equation (4.3) is estimated using a restricted sample: affected NUTS-3 regions are compared versus a *local* comparison group of unaffected NUTS-3 regions, up to a distance of 50 km away from the regional airport.[68] The estimate increases to a value of approximately 3.46% for the average treatment effect on GDP in column (3) and 3.73% for the effect on population in column (7), both statistically significant at the 5 and 1% level respectively.

[67]Public administration, defense, education, human health and social work activities.

[68]Section 4.4.3 compares how the estimates holds by using other distance cutoffs.

Despite a convincing DiD identification setup, one may raise concerns whether the location of an airport is truly exogenous, that is unrelated to the underlying economic conditions of a region. If this is not the case, then the estimates may still suffer from endogeneity. To overcome this concern, the airports' initial construction purpose is exploited, as already discussed by Breidenbach (2020). The sample is restricted to only include airports that were either initially constructed for military purposes (military air bases) or private aviation intent (aero clubs) and later converted into a civilian and commercial airport. This restriction lends additional credibility to the exogenous nature of the treated regional airport regions, since now airports that were merely constructed because of economic motives are excluded from the analysis. Under this specification, the coefficient in column (4) shrinks to a statistically insignificant value of approximately 0.84% for the average treatment effect on GDP. Also, for the effect on population levels in column (8), the effect is smaller but still statistically significant with a value of about 1.92%. Lastly, there is also a statistically significant value for the effect of regional airports on employment. The estimate in column (12) suggests a small but positive effect of approximately 0.22%.

While the estimates above represent the *average treatment effect*, there might be heterogeneity due to innate regional characteristics such as local policies or legislation. In Section 4.6 of the Appendix, such potential heterogeneous effect is investigated, yet it can be argued that the average treatment effect at the Pan-European level is likely the more insightful estimate in the context of evaluating the economic significance of regional airports in the EU.

Table 4.2: DiD Estimation Results

	Log(GDP)				Log(Population)				Log(Employment)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	All airports	Regional fixed effects	Restricted sample	Military & aero club	All airports	Regional fixed effects	Restricted sample	Military & aero club	All airports	Regional fixed effects	Restricted sample	Military & aero club
Reg. airpt. presence	0.0376***	0.0207**	0.0340**	0.0084	0.0101**	0.0048	0.0366***	0.0190***	0.0022	-0.0073	-0.0149	0.0204**
	(0.0089)	(0.0086)	(0.0172)	(0.0120)	(0.0039)	(0.0037)	(0.0071)	(0.0057)	(0.0065)	(0.0061)	(0.0126)	(0.0091)
log(dist. non-reg. airpt.)	-0.0105***	0.0242***	0.0117***	-0.0011	-0.0050***	0.0078***	0.0072***	-0.0000	0.0059***	0.0385***	0.0304***	0.0218***
	(0.0019)	(0.0027)	(0.0042)	(0.0037)	(0.0008)	(0.0012)	(0.0017)	(0.0018)	(0.0015)	(0.0020)	(0.0031)	(0.0029)
Employment share												
Agriculture		-0.0099	0.0563	-0.0438		-0.1265***	-0.1164***	-0.0464**				
		(0.0298)	(0.0435)	(0.0404)		(0.0129)	(0.0181)	(0.0194)				
Construction		0.4694***	0.8165***	0.2904***		0.2511***	0.2042***	0.3917***				
		(0.0415)	(0.0561)	(0.0496)		(0.0180)	(0.0234)	(0.0239)				
Services		-0.1820***	-0.1607***	-0.1052*		-0.1248***	-0.1054***	0.0775***				
		(0.0428)	(0.0542)	(0.0567)		(0.0185)	(0.0227)	(0.0271)				
Industry		0.4287***	0.4317***	0.2031***		-0.0912***	0.0466**	-0.0668***				
		(0.0323)	(0.0439)	(0.0396)		(0.0140)	(0.0183)	(0.0191)				
Non-mkt. services		-0.2105***	-0.2035***	-0.4651***		-0.1577***	-0.1239***	-0.1483***				
		(0.0311)	(0.0433)	(0.0407)		(0.0135)	(0.0181)	(0.0196)				
Elevation x Year	no	yes	yes	yes	no	yes	yes	yes	no	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	38,567	33,828	18,086	18,911	39,662	34,746	18,539	19,482	38,321	33,821	17,885	18,853
Adjusted R^2	0.9868	0.9853	0.9873	0.9881	0.9960	0.9959	0.9967	0.9962	0.9885	0.9883	0.9903	0.9895

Notes: Based on 25 km buffer zone and EC regional airport passenger definition. tandard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Military airports sample specification

While the notion of *DiD* appears to be a suitable method to estimate the *average treatment effect* of regional airports on the local economy, one may wonder whether the above selection into equivalent treatment and comparison groups is perfectly convincing. Figure 4.6 of the Appendix depicts the composition of the NUTS-3 areas into treatment and comparison groups. In the above specification, NUTS-3 areas that were affected by a regional airport are compared against NUTS-3 areas that simple did not inhibit any airport. However, the key notion of finding equivalents of *treatment* and *comparison groups* in which everything apart from the variable of interest (or other things that can be controlled for) are assumed to be the same, might be violated. Possibly there are further underlying and unobserved characteristics that determined whether a region received a (regional) airport, such as a favorable geographic location. For example, rural and remote areas are per se not as attractive to an airport developer and operator as opposed to areas with an underlying economic need for an airport such as areas in proximity to urban agglomerations. Conceivably, a more convincing classification into *treatment* and *comparison groups* may be the initial construction purpose of an airport. Henceforth, regions and their adjacent surroundings with regional airports that were initially constructed for military purposes and later converted into commercial service airports are classified as the treatment group. On the other hand, the comparison group is defined as military airports and their surroundings that remained non-commercial, that is continued to operate as a military airport. All other regions, that is regions that are affected by non-regional airports or simply do not inhibit nearby airports, are excluded from the analysis. Figure 4.7 depicts the adapted classification into treatment and comparison areas as well as excluded areas from the analysis.

Table 4.3 contains the regression estimates based on equations (4.3) and (4.4), now under the more restricted military construction purpose sample. Note that the sample size shrinks due to the novel classification of treatment and comparison group, in contrast to the results from Table 4.2. Based on the region fixed effects specification, the effect of a regional airport on GDP is about 2.28%, similar in magnitude to the estimate of Table 4.2, column (2), statistically significant at the 5% level. While the average treatment effect on GDP jumps to more than 6% for the restricted sample specification, it should be noted that the sample size has also been drastically reduced. Furthermore, a statistically significant average treatment effect of 1.11% on population levels is found under the model specification of column (3). The effect becomes however insignificant under the restricted sample specification of column (4). Finally, there is no statistical effect of regional airports on the level of employment.

Table 4.3: DiD Estimation Results: Military airports

	Log(GDP)		Log(Population)		Log(Employment)	
	(1)	(2)	(3)	(4)	(5)	(6)
	Region fixed effects	Restricted sample	Region fixed effects	Restricted sample	Region fixed effects	Restricted sample
Regional airport presence	0.0225**	0.0625**	0.0111**	0.0140	-0.0076	-0.0056
	(0.0110)	(0.0287)	(0.0047)	(0.0117)	(0.0078)	(0.0156)
log(distance non-regional airport)	0.0126***	-0.0045	0.0018	-0.0213***	0.0263***	-0.0104***
	(0.0032)	(0.0063)	(0.0014)	(0.0026)	(0.0023)	(0.0038)
Employment share						
Agriculture	-0.1452***	0.1698**	-0.1957***	-0.0983***		
	(0.0365)	(0.0664)	(0.0158)	(0.0269)		
Construction	0.3444***	0.4238***	0.2476***	0.2706***		
	(0.0463)	(0.0737)	(0.0202)	(0.0300)		
Services	-0.2119***	-0.0925	-0.1044***	0.0505		
	(0.0499)	(0.0844)	(0.0216)	(0.0342)		
Industry	0.3480***	0.1241**	-0.1523***	-0.0286		
	(0.0378)	(0.0613)	(0.0164)	(0.0249)		
Non-market services	-0.3549***	-0.3976***	-0.2327***	-0.1114***		
	(0.0360)	(0.0669)	(0.0157)	(0.0273)		
Elevation x Year	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Observations	26,300	7,833	27,015	8,045	26,212	9,860
Adjusted R^2	0.9860	0.9886	0.9960	0.9974	0.9892	0.9916

Notes: Based on 25 km buffer zone and EC regional airport passenger definition. Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

Identification revisited: Bartik IV

Until now, the effects of regional airports on the local economy is analyzed by using the binary variable 'regional airport presence'. The results from equation (4.3) are informative on the *average treatment effect* of regional airports on the local economy. Yet, additional insights might be revealed by exploiting the information of a continuous variable $P_{i,t}$, representing the actual number of passengers. However, simply using it as an independent variable is problematic since it is highly endogenous with the dependent variables 'GDP', 'Population', and 'Employment'. To address this concern, an instrumental variable (IV) approach is applied. The instrument, denoted by $Z_{i,t}$, is defined as the interaction between the base year's passenger numbers and the EU-15 level passenger growth rate. This methodology, also known as a *Bartik IV*, has been widely used across many fields in economics, regional science, political economy, and urban studies (see e.g. Bartik (1991), Blanchard et al. (1992), Goldsmith-Pinkham et al. (2020)). Therefore the effect of the number of air passengers, $P_{i,t}$, on economic activity, $Y_{i,t}$, is estimated by using a Bartik IV approach.

The second stage is then given by:

$$log\, Y_{i,t} = \delta \hat{P}_{i,t} + \beta C_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t}, \tag{4.6}$$

where $\hat{P}_{i,t}$ is obtained from:

$$P_{i,t} = \tilde{\theta} Z_{i,t} + \tilde{\beta} C_{i,t} + \tilde{\mu}_i + \tilde{\lambda}_t + \tilde{\varepsilon}_{i,t}, \tag{4.7}$$

where the $\sim$ refer to first-stage coefficients and $\tilde{\theta}$ is the effect of the Bartik IV $Z_{i,t}$ on the continuous passenger variable $P_{i,t}$.

Table 4.4 summarizes the two-stage least squares (2SLS) estimation results. For both the 'region fixed effects' and 'restricted sample' specification, the instrument is strong as the F-statistic is above the rule-of-thumb value of 10. Yet, for the 'military & aero club' specification the instrument appears to be weak, reflected by a very low F-statistic, possibly driven by a smaller sample size. For GDP, specifications (1) and (2) yield to a positive and statistically significant effect. Expressed in millions of passengers, each additional unit yields on average to a 2.0 to 2.7% increase in GDP. For the remaining specifications with population and employment levels as the response variable, the effect is neither positive nor statistically significant. The first-stage estimates are reported in Table 4.8 of the Appendix 4.6.

Table 4.4: Bartik IV 2SLS Estimation Results

	Log(GDP)			Log(Population)			Log(Employment)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Region fixed effects	Restricted sample	Military & aero club	Region fixed effects	Restricted sample	Military & aero club	Region fixed effects	Restricted sample	Military & aero club
Passengers (in millions)	0.0203***	0.0266**	-0.9370	-0.0258	-0.0305	-0.0000	-0.0200	-0.0660	-0.0003
	(0.0074)	(0.0104)	(3.9200)	(0.0195)	(0.0282)	(0.0124)	(0.0036)	(0.0052)	(0.0023)
Distance non-regional airport	yes	yes	yes	yes	yes	yes	yes	yes	yes
Employment shares	yes	yes	yes	yes	yes	yes	no	no	no
Elevation x Year	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	7,937	7,469	4,049	8,125	7,640	4,159	7,779	7,294	3,989
Cragg-Donald F-Statistic	112.217	188.097	0.058	106.234	193.641	4.739	100.694	183.666	4.166

Notes: I instrument for the number of passengers by interacting the base year's number of passengers with the European level passenger growth rate. Based on 25 km buffer zone and EC regional airport passenger definition. Standard errors are reported in parentheses.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

4.4.2 Economic impact of regional airports

The second main research objective is to calculate the total economic impact of regional airports on the economy to gain a better understanding of the quantitative implications of the empirical results. Table 4.5 reports the results of the estimation. Two types of economic impact are considered: a conservative effect of 2.1% (see column (2), Table 4.2) and a more optimistic estimate of 3.0% (see column (3), Table 4.2). In the more conservative scenario, the total economic impact of regional airports on the EU-15 economy results in approximately 41 billion Euros in 2015 when multiplying the average treatment effect of 2.1% with the share of regional airports per NUTS-3 region of 25.7% and the overall Gross Domestic Product in 2015. Under the more optimistic scenario, the total impact of regional airports amounts to approximately 60 billion Euros. On a per NUTS-3 level, this translates into an economic value generation by regional airports between 146 and 214 million Euros, depending on the economic impact scenario. Putting these figures into context, it is estimated that airlines and airports together contribute to more than 140 billion Euros to the European Union's GDP each year (European Commission, 2014). Therefore, the point estimate of regional airports' economic impact is likely expected to be closer to the conservative estimate. Note that this back-of-the-envelope calculation should be interpreted with caution. Aside from looking at the *average treatment effect* despite possible spatial heterogeneity, a main limitation is the partial equilibrium of the employed identification strategy. In the context of regional airports, it is assumed that individuals do not move or commute from a no-airport to an airport region. In fact, the net effect on the national economy could be zero. Therefore, future research would benefit from using a structural model approach with the goal of estimating a general equilibrium.

Table 4.5: Economic effect of regional airports

	Regional airports	
Assumed economic impact	2.1%	3.0%
Total gain (€, in millions)	40,981	60,185
Gain per NUTS-3 (€, in millions)	146	214

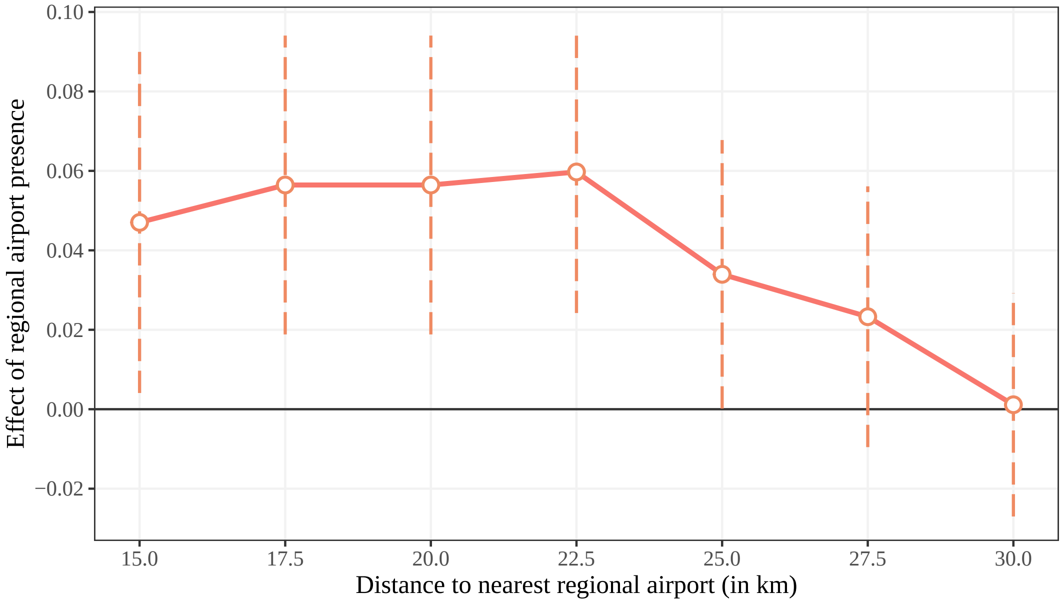
Notes: Based on 25 km buffer zone and EC regional airport definition.
All estimates are in 2015 values.

4.4.3 Sensitivity analyses

Finally, the consistency of these results is checked by discussing the used buffer zone specification, employing an event study on the possible anticipation and adjustment effects, as well as further robustness checks on the underlying regional airports definition and timing.

Buffer zone

As outlined in Figure 4.1, the chosen threshold to spatially classify NUTS-3 regions into affected versus unaffected areas depends on a 25 km buffer zone. To verify whether such 25 km buffer zone is a valid choice, the treatment effect is analyzed depending on the distance, as formalized in equation (4.4). Figure 4.4 depicts the results. As one would expect, the treatment effect diminishes as the studied buffer is increased, meaning that NUTS-3 regions further away are less affected by a regional airport than regions in closer proximity. Under the smallest and statistically significant depicted buffer of 15 km the treatment effect has a value of approximately 5.0 percent. Although the treatment effect slightly increases until a buffer specification of 22.5 km, the effect then begins to rapidly decline while the 95% confidence interval begins to narrow. Under the 25 km distance specification the treatment effect has approximately a value of 4.1 percent. The results do not necessarily imply that there is no effect of regional airports beyond the 25 km buffer, but the treatment effect appears to be statistically insignificant and declining at increasing distance to the nearest regional airport. Hence, it is plausible to use the 25 km buffer zone as the preferred buffer zone choice for this analysis since it located furthest away but still yields to statically significant values.

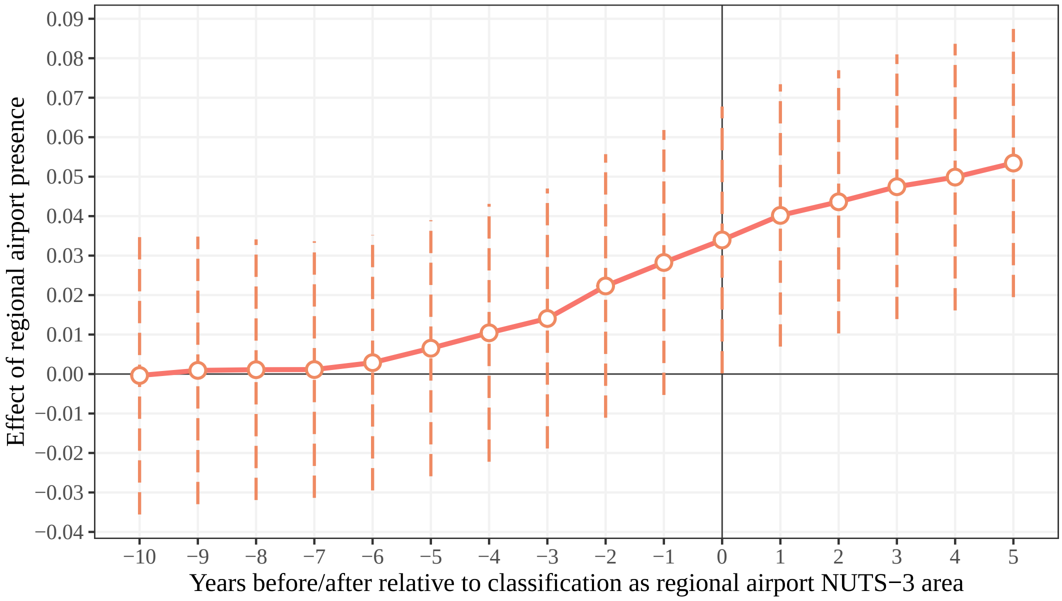


Notes: The dots depict the conditional averages per distance buffer, based on regression estimates of equation (4.4). The vertical lines represent 95 percent confidence intervals. Buffers below 15 km are not shown since the effect is small and statistically insignificant. The reference group contains observations that are further away than the buffer but still within 50 km of a regional airport. NUTS-3 regions within 20 km of a non-regional airport are excluded. The European Commission's regional airport passenger definition is used.

Figure 4.4: Treatment effect by buffer size

Event study

Economic activity may already have increased in anticipation of an airport expansion or construction. Equation (4.5), estimates a model that decomposes the treatment effect before and after a particular NUTS-3 is considered to be affected by a regional airport. Figure 4.5 reports the resulting estimated coefficients. The results suggest that anticipation effects do not play a role. For instance, one year prior, the null hypothesis that the coefficient estimates are statistically significantly different from each other cannot be rejected. In the years after, the magnitude of the coefficient grows and remains statistically significant, indicating an adjustment effect. In other words, effects on economic activity appear to be persistent and larger than the baseline estimate over time (see column (3) of Table 4.2).



Notes: The dots depict the conditional averages for a given year, based on equation (4.5), before/after a NUTS-3 region is classified as affected by a regional airport under the 25 km specification. The vertical lines represent 95 percent confidence intervals. The reference group contains observations that are further away than the buffer but still within 50 km of a regional airport. NUTS-3 regions within 20 km of a non-regional airport are excluded. The European Commission's regional airport passenger definition is used.

Figure 4.5: Anticipation and adjustment effects

Robustness checks

The following section summarizes and discusses the most important robustness checks. The results of the robustness checks on *GDP* are reported in Table 4.6, while checks on *population* and *employment* as the dependent variable are reported in section 4.6 in the Appendix. First and foremost, it is sensible to check the seemingly arbitrary European Commission's passenger threshold that classifies airports into regional versus non-regional airports. Note that up to now, the three million annual passenger traffic figure has been consistently used as a ceiling to define an airport as *regional*, or if above as *non-regional*. In column (1) of Table 4.6, this threshold is lowered to 1 million passengers per year. Now only airport areas that inhibit airports with a traffic of up to 1 million passengers annually are defined as regional airports. All other airport areas with larger airports are considered non-regional airports and are hence excluded from the analysis. Under this more restrictive regional airport definition, the coefficient of interest increases to approximately 4.55%, statistically significant at the 1% level, compared to the baseline of 2.09% of column (2) in Table 4.2. Under the 2 million regional passenger definition in column (2) of Panel A, the coefficient of interest with a value roughly 2.45% is again closer to the baseline coefficient. Panel B repeats these robustness checks, now for the restricted military airports sample of section 4.4.1. Here, the coefficients of column (1) and (2) are both lower than their baseline of Table 4.3, column (3), however, statistically insignificant. In column (3) of Table 4.6, a further robustness check is performed on the passenger threshold definition. Using again the European Commission's 3 million annual passenger ceiling, it now excludes very small airports from the analysis. Arguably, many small airports have a very different operating model than larger regional airports. For instance, some smaller airports only exist due to public service obligations (PSO), that is serving remote parts of the European Union by legislation instead of economic rationale. To adjust the sample for such airports as well as other smaller operating airport models, airports with an annual traffic below 200,000 passengers are excluded. Note that also the European Commission exempts airports with an annual traffic below 200,000 passengers from the proposed financing restrictions of its revised "Guidelines on State aid to airports and airlines". While the coefficient is statistically insignificant in Panel A, under the military airports' specification in Panel B, the coefficient becomes significant with a value of approximately 3.52%.

As a second main type of robustness check, the sample is adjusted based on the proximity to a regional airport. To recall, in equation (4.3) affected NUTS-3 regions are compared versus a *local* comparison group of unaffected NUTS-3 regions, up to a distance of 50 km away from the airport. Now, in column (4) and (5) of Table 4.6, that value is increased in 25 km increments. In Panel A, the value decreased for both restriction values to approximately 2.51 and 2.68% compared to the baseline coefficient of 3.46% in Table 4.2, column (3). The same trends holds true for the estimates of Panel B, although strictly statistically speaking the values appear to be less robust than their baseline estimate of Table 4.3, column (2).

Lastly, since the time frame of analysis spans over more than 35 years (1980-2016) and unobservables may change over time, there are possible unobserved time trends that are not captured by the year fixed effects but are correlated with the treatment effect. To address this issue, *NUTS-3-decade trends* are used as controls by computing an interaction term of region fixed effects for each decade. The results in column (6) indicate that the effect of regional airports on regional economic activity is about 1.35% under the unrestricted specification of Panel A, statistically significant at the 10% significance level, and about 1.23% under the military airports specification of Panel B, albeit statistically insignificant. The effect for both is lower than their baseline, which is not entirely unexpected, given that part of the treatment effect is thought to be absorbed by the *NUTS-3-decade trends* fixed effects.

Table 4.6: Robustness checks

	Dependent variable: Log(GDP)					
	Passenger threshold			*Distance buffer*		*Timing*
	(1) 1mn pax threshold	(2) 2mn pax threshold	(3) 3mn pax excl. <200k pax	(4) 75 km restriction	(5) 100 km restriction	(6) Region x decade FE
Panel A: Unrestricted specification						
Regional airport presence	0.0445***	0.0242***	0.0353	0.0248***	0.0264***	0.0134*
	(0.0086)	(0.0085)	(0.0041)	(0.0108)	(0.0089)	(0.0072)
Distance nearest non-regional airport	yes	yes	yes	yes	yes	yes
Employment shares	yes	yes	yes	yes	yes	yes
Elevation x Year	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Observations	31,812	33,162	33,864	26,460	30,662	33,857
Adjusted R^2	0.9841	0.9845	0.9846	0.9862	0.9856	0.9967
Panel B: Military airports specification						
Regional airport presence	0.0124	0.0125	0.0346***	0.0367***	0.0159	0.0122
	(0.0120)	(0.0121)	(0.0041)	(0.0153)	(0.0129)	(0.0096)
Distance nearest non-regional airport	yes	yes	yes	yes	yes	yes
Employment shares	yes	yes	yes	yes	yes	yes
Elevation x Year	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Observations	24,939	25,268	26,336	12,882	17,531	26,330
Adjusted R^2	0.9857	0.9857	0.9853	0.9877	0.9875	0.9968

Notes: Based on 25 km buffer zone and EC regional airport passenger definition.
Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

4.5 Conclusion

Summarizing and taking up the main objectives of this research: Firstly, how and to what extent do regional airports affect their adjacent local economy? This paper investigated the effect of regional airports on local economic development, expressed by GDP, population, and employment levels. The results show that, on average, local economic activity increases between 2 and 6 percent due to the presence of a regional airport. Also, population and employment levels are both positively affected by regional airports. An additional IV estimation points towards a similar outcome, with each additional 1 million passengers yielding to a positive effect between 2 to 3 percent on GDP, yet no effects on population or employment levels are found. A variety of sensitivity checks suggest that the effect strongly depends on the size definition of a regional airport. Moreover, it can be shown that the effect is somewhat local and declines with increasing distance. It is further demonstrated that anticipatory effect likely did not play a significant role although the effect on GDP intensifies over time. Secondly, the overall economic impact of regional airports is examined by contextualizing these findings. Using a back-of-the-envelope calculation, the total economic impact of regional airports is estimated to range between 41 and 60 billion Euros, or between 146 and 214 million Euros on a per NUTS-3 level, depending on the employed regression specification.

The European Commission has recently announced its plans cut subsidies for regional airports, which merely exist due to substantial public funding. Since most regional airports in the EU are subsidized and the ramifications of the prevailing global COVID-19 pandemic further aggravates their situation, many regional airport operators are set to face existential problems soon. Against this backdrop, this paper is the first to find supporting evidence in favor of the existence of regional airports in the European Union, in line with findings from other countries and regions. This research has also significant implications for policymakers: while traditional subsidies violate EU competition law, local governments that seek to reap the local economic benefits of their regional airports, need to create a more beneficial environment to attract more traffic. For instance, this could be achieved via strategic route development partnerships with airlines, which could eventually boost traffic and therefore overall profitability of the airport. While this paper has shown overall positive effects of regional airports in the EU, future research should further focus on the true net effects by focusing on potential regional disparities.

4.6 Appendix

Buffers

Table 4.7, provides additional justification for the preferred and employed 25 km distance specification to the nearest regional airport. A simple calculation indicates that average size of NUTS-3 units significantly differs between countries. For instance, for Germany the implied average NUTS-3 unit has a radius of approximately 15 km while for Sweden it takes the value of almost 74 km. Yet, the preferred 25 km specification is in fact very close to the mean implied radius for the EU-15.

Table 4.7: Comparison of NUTS-3 sizes per country

	Implied average radius in km				
	min	mean	max	sd	n
Austria	11.45	26.84	38.33	6.67	35
Belgium	5.69	14.39	22.55	3.90	44
Denmark	7.64	31.14	52.49	17.53	11
Finland	21.10	67.53	177.49	34.02	19
France	5.78	41.94	163.27	15.99	101
Germany	3.37	15.37	41.83	6.86	402
Greece	4.64	26.44	45.97	10.47	52
Ireland	17.17	50.62	67.44	15.91	8
Italy	8.21	28.28	48.51	8.41	110
Luxembourg	28.74	28.74	28.74		1
Netherlands	6.39	16.35	27.70	5.55	40
Portugal	15.96	32.97	52.15	9.37	25
Spain	2.04	48.04	83.23	20.73	59
Sweden	31.07	73.56	183.58	38.39	21
United Kingdom	2.53	16.94	68.00	12.80	173
EU-15	2.04	24.81	183.58	18.60	1101

Notes: To estimate the implied average radius per NUTS-3 region, I assume that each NUTS-3 is a perfectly circular shape. Hence, the radius can be expressed by $r = \sqrt{\frac{A}{\pi}}$

Sample specification

Figure 4.6 breaks down the composition of NUTS-3 areas in the EU. First and foremost, NUTS-3 areas can be distinguished between *airport areas*, these are areas with an airport or in close proximity (25 km) to one, versus *no-airport areas*. These airport areas can be broken down by their purpose type. Firstly, *regional airport areas* are airports and their surroundings below an annual passenger threshold of 3 million. Secondly, airports and their adjacent areas above that passenger threshold are considered to be *non-regional airport areas*. Thirdly, some areas inhibit airport infrastructure, which is solely used by the military. These *military air base areas* are therefore considered to be commercially inactive. Finally, *regional airport areas* can further be broken down by their original construction purpose: that is construction occurred either because of *military origin* or because of a *non-military origin*, such as a planned airport by the local government. Figure 4.6 also illustrates the composition of NUTS-3 areas into treatment and comparison groups. For the unrestricted sample of section 4.4.1, the treatment group comprises of all commercially active *regional airport areas*, while the comparison group consists of *no-airport areas*. For the restricted sample of section 4.4.1, the treatment group is defined as commercially-active regional airport areas of only *military origin* versus the comparison group of commercially-inactive airport areas containing *military air bases*.

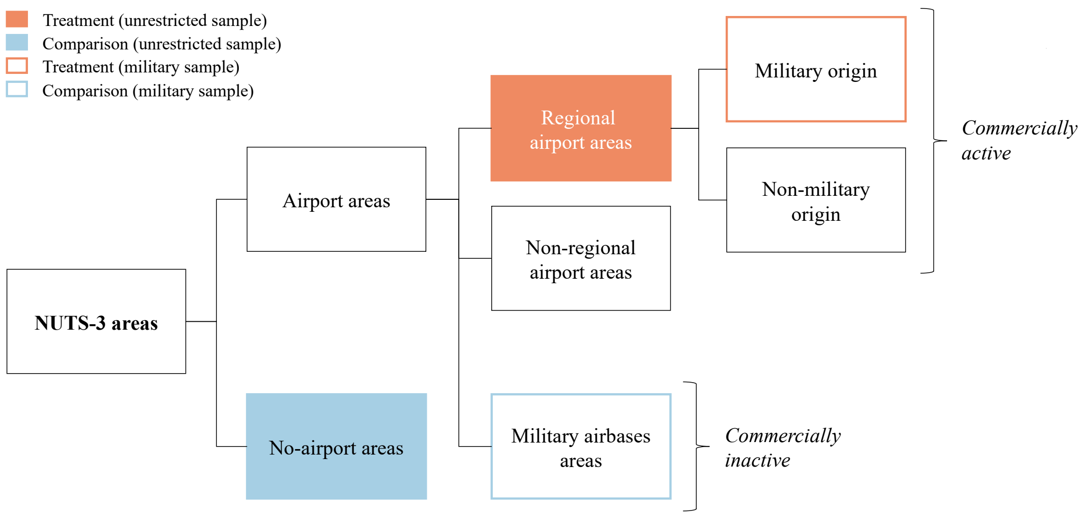
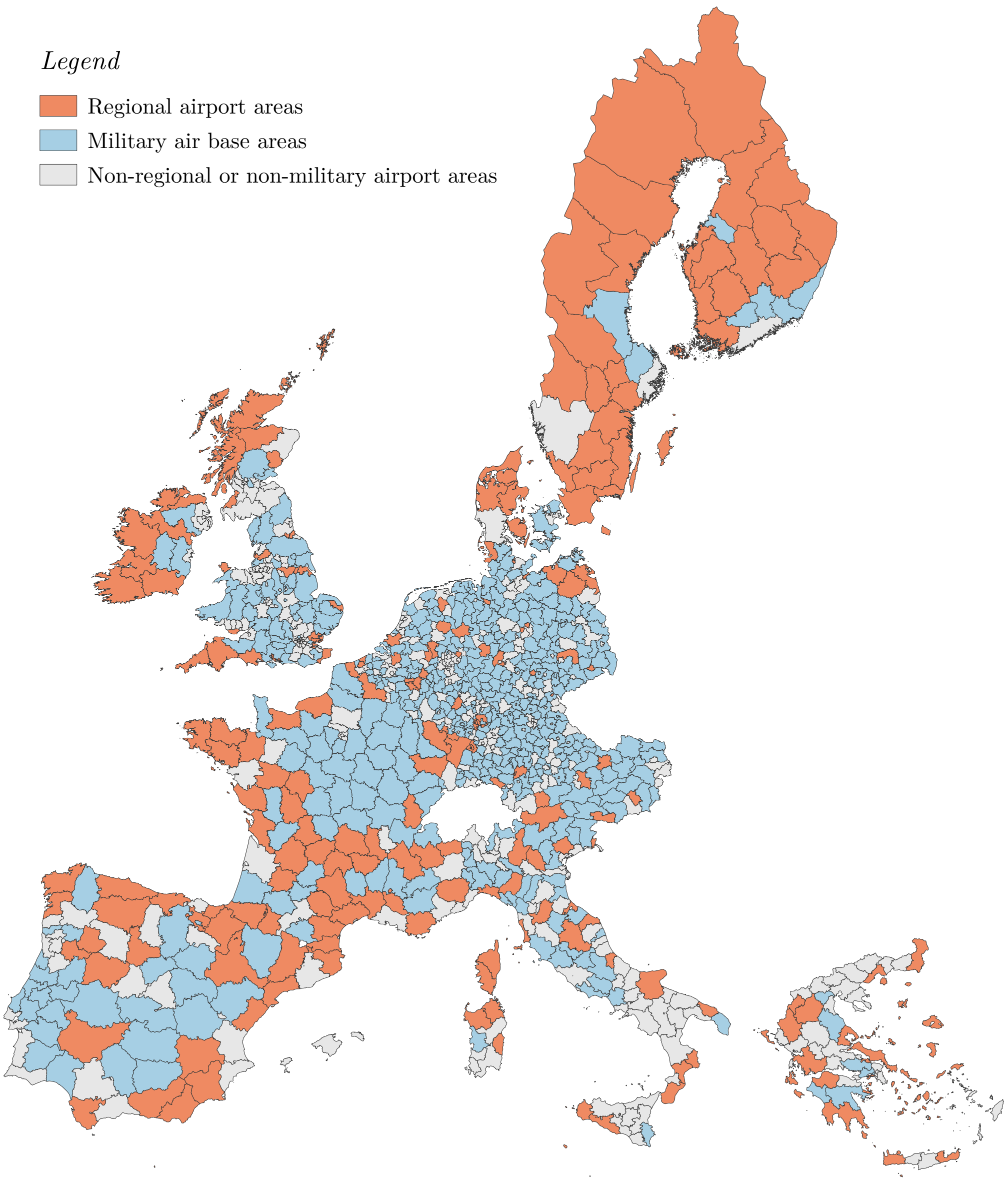


Figure 4.6: NUTS-3 decomposition into treatment and comparison groups

Military airport sample specification

Analogously to Figure 4.2, Figure 4.7 below depicts the classification of NUTS-3 areas into treatment and comparison group, based on the military sample specification.

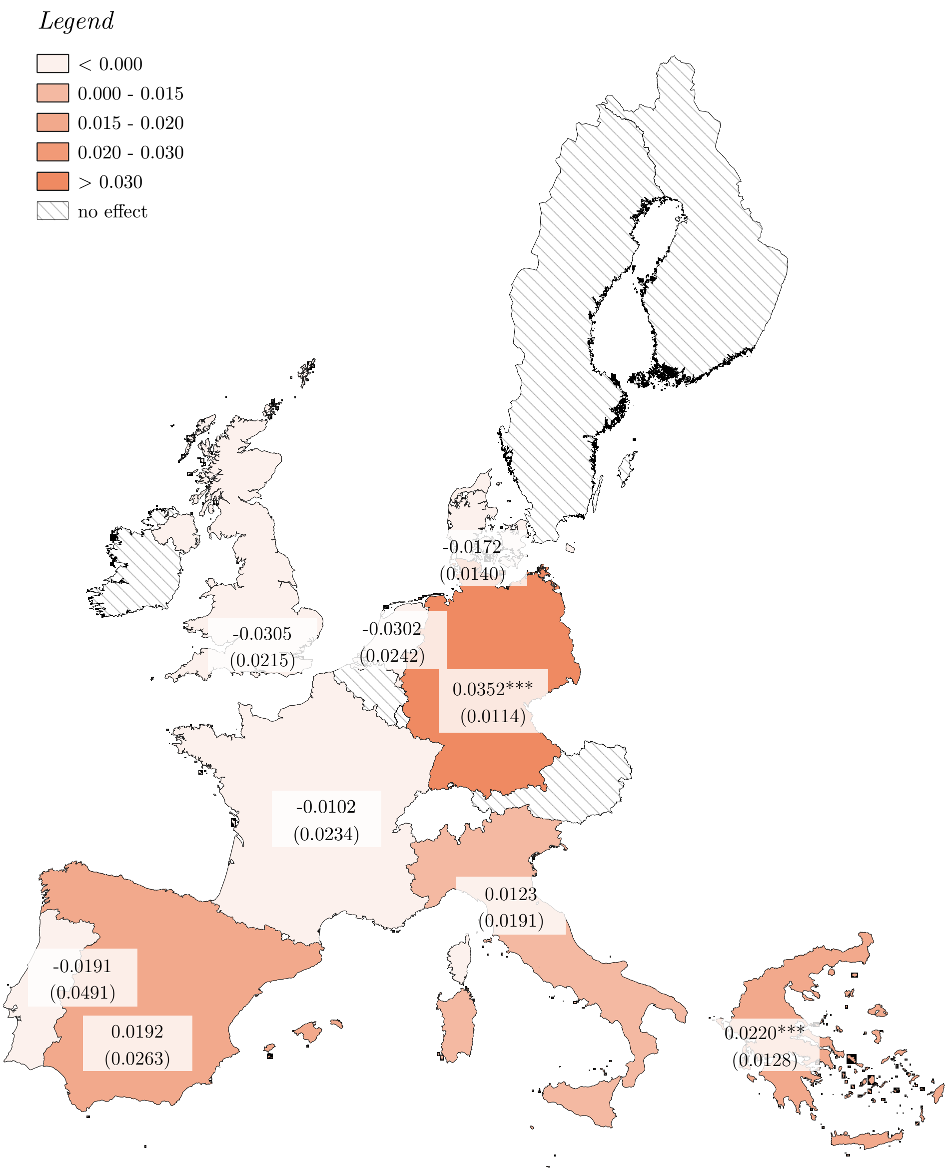


Notes: Based on 25 km buffer zone and EC regional airport passenger definition in 2016.

Figure 4.7: Military airports: EU-15 NUTS-3 treatment vs. comparison

Spatial heterogeneity

There might be considerable regional heterogeneity in the effect of regional airports on the local economy. To check to what extent such heterogeneity might be present, an estimation strategy of equation (4.3) is applied for each country individually. The results are summarized in Figure 4.8. Referring to Table 4.7, this might already be problematic due to the mere set-up of spatial units. NUTS-3 regions are by far not evenly distributed among countries. Small countries such as Luxembourg inhibit only one NUTS-3, leading to too few observations to estimate an effect. But also, larger countries such as Sweden or Finland only inhibit relatively speaking few but large NUTS-3 regions with very little regional variation. In fact, for Sweden, Finland, Austria, and Ireland, the presence of a regional airport, appears to be collinear with the region fixed effects, leading to an omission of the coefficient of interest. Yet, for the remaining countries effects are found with considerable heterogeneity. The effect in Germany is statistically significant and appears to be somewhat higher than the average treatment effect of Table 4.2. On the other hand, for Greece the magnitude of the estimate is closely aligned to the average treatment effect and statistically significant. Also a positive relationship is found, although smaller in magnitude and statistically insignificant at the 5% level, for Italy and Spain. The coefficients of the remaining countries (France, the United Kingdom, the Netherlands, and Denmark) are negative but statistically insignificant, which can be attributed to random variation. Although this exercise revealed considerable spatial heterogeneity (with certain limitations), arguably the *average treatment effect* at the Pan-European level is more informative in order to draw conclusions with regard to the economic significance of regional airports in the EU.



Notes: Country-level estimates are obtained from applying the estimation strategy of equation (4.3) for each country in isolation. Based on 25 km buffer zone and EC regional airport passenger definition. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 4.8: Treatment effect at the country level

Shift share analysis: first-stage results

This part of the Appendix focuses on the first-stage results by checking whether the instrument is sufficiently correlated with the variable 'number of passengers'. The first-stage results are reported in Table 4.8. The coefficient of interest represents the Bartik instrument, that is the interaction between the local base year's passenger number with the EU-level air passenger growth rate. In both the baseline specification of column (1) and the restricted sample of column (2), a strong and statistically significant correlation is found between the 'Bartik IV' and the dependent variable. Also, the test results for both weak identification (F-Statistic) and underidentification (χ^2), using the method of Sanderson and Windmeijer (2016), implies that the Bartik IV neither suffers from a weak-instrument problem nor from underidentification. However, for the specification in column (3), the test results suggest that the Bartik IV is likely neither a strong nor a valid instrument, possibly due to the much more restricted sample.

Table 4.8: Number of Passengers: First-Stage Results

	Number of Passengers		
	(1)	(2)	(3)
	Region fixed effects	Restricted sample	Military & aero club
Bartik IV	1.4710***	1.3044***	-0.0553
	(0.1389)	(0.0951)	(0.2301)
Distance non-regional airport	yes	yes	yes
Employment shares	yes	yes	yes
Elevation x Year	yes	yes	yes
Year FE	yes	yes	yes
Region FE	yes	yes	yes
Observations	7,937	7,469	4,049
S-W F-Statistic	112.22	188.10	0.06
S-W χ^2	112.33	188.31	0.06

Notes: The Bartik IV represents the interaction between the base year's number of passengers and the EU-level passenger growth rate. Based on 25 km buffer zone and EC regional airport passenger definition. Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Additional robustness checks

For sake of completeness, the same robustness checks of Table 4.6 are also performed on population and employment levels as the dependent variable of interest. Table 4.9, summarizes the robustness of the results for population levels as the dependent variable. First, when decreasing the passenger threshold, the magnitude of the coefficient of interest appears to decrease as well, indicating that the effect on population level grows when considering larger airports in the specification. Also, the second main sensitivity check is in line with previous findings, that is increasing the distance buffer decreases the effect on the dependent variable, highlighting the local nature of the response. However, for the last check, using region-decade fixed effects, no statistically significant results are found. The robustness checks of the effect on employment levels in Table 4.10 follow the same logic, albeit the results appear to be less robust.

Table 4.9: Robustness check: Population

	Dependent variable: Log(population)					
	Passenger threshold			*Distance buffer*		*Timing*
	(1) 1mn pax threshold	(2) 2mn pax threshold	(3) 3mn pax excl. <200k pax	(4) 75 km restriction	(5) 100 km restriction	(6) Region x decade FE
Panel A: Unrestricted specification						
Regional airport presence	0.0152***	0.0005	0.0129***	0.0117**	0.0145***	0.0004
	(0.0037)	(0.0036)	(0.0017)	(0.0046)	(0.0037)	(0.0024)
Distance nearest non-regional airport	yes	yes	yes	yes	yes	yes
Employment shares	yes	yes	yes	yes	yes	yes
Elevation x Year	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Observations	32,639	34,024	34,746	27,110	31,433	34,739
Adjusted R^2	0.9957	0.9959	0.9959	0.9963	0.9962	0.9995
Panel B: Military airports specification						
Regional airport presence	0.0010	0.0010	0.0183***	0.0147**	0.0222***	0.0017
	(0.0052)	(0.0052)	(0.0018)	(0.0065)	(0.0056)	(0.0031)
Distance nearest non-regional airport	yes	yes	yes	yes	yes	yes
Employment shares	yes	yes	yes	yes	yes	yes
Elevation x Year	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Observations	25,593	25,593	27,015	13,195	17,973	27,009
Adjusted R^2	0.9961	0.9961	0.9960	0.9969	0.9967	0.9995

Notes: Based on 25 km buffer zone and EC regional airport passenger definition. Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.10: Robustness check: Employment

	Dependent variable: Log(employment)					
	Passenger threshold			*Distance buffer*		*Timing*
	(1) 1mn pax threshold	(2) 2mn pax threshold	(3) 3mn pax excl. <200k pax	(4) 75 km restriction	(5) 100 km restriction	(6) Region x decade FE
Panel A: Unrestricted specification						
Regional airport presence	0.0326***	0.0036	0.0055*	0.0061	-0.0063	-0.0016
	(0.0062)	(0.0061)	(0.0030)	(0.0078)	(0.0063)	(0.0050)
Distance nearest non-regional airport	yes	yes	yes	yes	yes	yes
Elevation x Year	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Observations	31,844	33,150	33,821	26,248	30,545	33,815
Adjusted R^2	0.9875	0.9880	0.9883	0.9896	0.9891	0.9979
Panel B: Military airports specification						
Regional airport presence	-0.0176**	-0.0176**	0.0115***	-0.0050	0.0031	-0.0021
	(0.0083)	(0.0083)	(0.0030)	(0.0111)	(0.0094)	(0.0066)
Distance nearest non-regional airport	yes	yes	yes	yes	yes	yes
Elevation x Year	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Region FE	yes	yes	yes	yes	yes	yes
Observations	24,835	24,835	26,212	12,642	17,274	26,207
Adjusted R^2	0.9901	0.9901	0.9892	0.9909	0.9906	0.9980

Notes: Based on 25 km buffer zone and EC regional airport passenger definition.
Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

5
Summary and Conclusions

This dissertation provides a collection of three distinct empirical analyses on the effects of travel and transportation on urban and regional economies. With these analyses, I contribute to the current debate on short-term rentals on a single residential market, the implications of low emission zones on a nationwide housing market, and the economic effects of regional airports on regional economies in a pan-national context.

Chapter 2 studies the regulation effects of the online short-term rental platform Airbnb on the housing market, using a quasi-experimental research design. In Los Angeles County, 18 out of 88 cities have severely restricted short-term rentals by adopting Home Sharing Ordinances. I apply a panel regression-discontinuity design around the cities' borders and find that home sharing ordinances reduced listings by 50 percent and housing prices by 2 percent, on average. Additional difference-in-differences estimates show that ordinances reduced rents also by 2 percent. These estimates imply large effects of Airbnb on property values in areas attractive to tourists (e.g., an increase of 15 percent in house prices within 2.5km of Hollywood's Walk of Fame).

Chapter 3 analyzes whether people's perceptions of improvements in local air quality are reflected in the housing market. Using comprehensive data on real estate prices from Germany, I apply a quasi-experimental research design by exploiting the staggered introduction of Low Emission Zones (LEZs) across German cities. I find that residents value the presence of LEZs, reflected by roughly 2 percent 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.

Chapter 4 exploits market changes induced by the Single European Aviation market liberalization initiative to bring new evidence on the link between regional airports and economic development. Using administrative level data for the EU-15, I apply a difference-in-differences research design to identify the causal effects and spillovers of regional airports on local economic activity, population, and employment. The results suggest that the effect of the presence of regional airports on economic activity in the EU is positive, ranging between 2 and 6 percent. An additional instrumental variable estimation points towards a similar outcome, with each additional 1 million passengers yielding a positive effect on local GDP of between 2 and 3 percent.

In addition to each essay's estimation results, this dissertation also contains a compilation of studies based on quasi-natural experiments. The treatise demonstrates how researchers can benefit from quasi-natural experimental research settings where the collection of traditional experimental data would be unfeasible. For example, it would be discriminatory to set up an experimental setting where certain, selected short-term rentals are excluded from a city-wide legislation while others are not.

Finally, each chapter addresses an important research question with a relevant application. Firstly, short-term rentals are an undeniable advancement in terms of the way we travel, however they also impact urban areas on a different scale. We therefore need to understand how policies work in curbing its negative externalities. Secondly, Low Emission Zones are a frequently used policy tool to curb emissions and will intensify with the introduction of ultra-low and even zero emission zones across major cities. Therefore, it is necessary to understand the implications on other markets, such as the housing market. Finally, the funding of regional airports in Europe is part of an ongoing and controversial debate, not least because of the COVID-19 pandemic. Thus, it is imperative to understand its economic relevance to align future funding of critical infrastructure.

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Hiermit versichere ich, dass ich die vorliegende Dissertation selbständig und ohne unerlaubte Hilfe angefertigt und andere als die in der Dissertation angegebenen Hilfsmittel nicht benutzt habe. Alle Stellen, die wörtlich oder sinngemäß aus anderen Schriften entnommen sind, habe ich als solche kenntlich gemacht.

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