UNIVERSITAT DE BARCELONA

# Essays on Urban Economics

Filippo Tassinari

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PhD in Economics | **Filippo Tassinari**

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PhD in Economics

# Essays on Urban Economics

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PhD in Economics

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**Thesis title:**

# Essays on Urban Economics

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UNIVERSITAT DE BARCELONA

*Alla nonna Maddalena*
*e al nonno Mario*

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# Contents

# 1 Introduction

In the last sixty years, the share of World's population living in cities increased tremendously. According to the World Bank data[1], the share of people living in urban areas rose from 34% in 1960 to 56% in 2020. Furthermore, this figure is predicted to increase up to 68% by 2050 (United Nations, 2019). Focusing on some of the most developed areas of the Globe, North America and Europe, the share of population living in urban areas in 2020 is even more astonishing: 83% and 77% respectively. Given this descriptive evidence, it is clear that cities play a central role in the economy and in people's lives, and their role will be even more influential in the future. This could explain why over the years the interest in urban topics rapidly increased, both in academical and non-academical environments.

Given this evidence, why do people concentrate across space? The economic literature identifies three main factors as possible determinants of the geographic distribution of the population and, consequently, economic activities in cities: the geographical characteristics (first nature factors) that endorse some places with better conditions to be inhabited; agglomeration and history (second nature factors).

In the current century, given the already existing cities, the present and future success of a city seems to be determined by agglomeration economies more than geography or history. Borrowing the words of Edward Glaeser, agglomeration economies *"are the benefits that come when firms and people locate near one another together in cities and industrial clusters"* (Glaeser, 2010). According to Duranton and Puga (2004), those benefits are based on sharing, matching, and learning mechanisms. In recent years, most of the urban literature focused on this aspect, and the literature on agglomeration economies is now well established (see Rosenthal and Strange (2004); Combes et al. (2010); Combes and Gobillon (2015) for detailed reviews).

This literature uses to look at cities from the production side. However, I could also think about cities as places of consumption. This less developed but growing strand of the literature focuses on other reasons to explain why people locate, or not, in urban areas. Depending on their personal preferences, people might decide where to locate depending on the availability of different type of restaurants, shops,

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[1] `https://data.worldbank.org/indicator/SP.URB.TOTL.IN.ZS`

cinemas, and sport facilities, in order to make some example. For example, Couture and Handbury (2020) find that *"high initial density of non-tradable service consumption amenities like restaurants and nightlife plays a more important role than other commonly-cited* (production-related) *factors"* to explain the current century's urban revival in the United States driven by young college graduates.

Duranton and Puga (2020) overcome the distinction between cities as production or consumption by focusing on concept of urban density. With this notion, they focus instead on benefits and costs entailed in the definition of cities. On the one hand density *"boosts productivity and innovation, improves access to goods and services, reduces travel needs, encourages more energy efficient buildings and forms of transport, and allows broader sharing of scarce urban amenities"* (Duranton and Puga, 2020). On the other hand, however, density leads to crowding and its related costs: higher land values, and thus housing prices, traffic congestion, pollution, scarcer green space, increased inequalities, and higher crime and mortality rates (Ahlfeldt and Pietrostefani, 2019). Fujita and Thisse (2013) define this tension between agglomeration and and urban costs as the "fundamental trade-off of spatial economics". With this, they refer to the fact that urban density leads to a faster accumulation of benefits, but at a certain point the costs - *"demons of cities"* (Glaeser, 2011) - overcome them.

In this dissertation I aim at documenting costs and benefits of cities through the lenses of transport economics. The organization of economic activity across space depends crucially on the transportation of goods and people. As a consequence, it should not surprise if the transport sector as a whole represents about five percent of gross domestic product (GDP) in Western countries, and if transport networks account for some of the public largest investments ever made (Redding and Turner, 2015). The benefits that come along with better connected places can justify this public expenditure. Faster connections and lower transportation costs boost productivity. Within the view of agglomeration economics, the first models of the new economic geography emphasised the importance of transport costs as one of the main determinants of the distribution of economic activities and population across space. I can use a broad definition of transportation costs, including exchange of goods, people and ideas, and not just the closeness between workers and firms. The main idea of agglomeration economies is that, when transportation costs are high, companies and consumers do concentrate themselves across space, determining the growth of cities (Fujita et al., 1999). Redding and Turner (2015) provide a review of the way that transportation costs and infrastructures shape the organization of economic activity across space.

At the same time, higher availability of transport infrastructure also comes with some costs. Motorized traffic generates some of the heaviest urban costs: traffic congestion, air pollution, and noise. They represent transport-related negative externalities and have negative health consequences. This PhD dissertation seeks to contribute and quantify both benefits and cost related to transportation.

With an infra-city perspective, I can think about transportation infrastructure as a way to shape the spatial distribution of people and economic activity across space. In this sense, one can think about transportation costs in terms of movement of goods and people between cities. For a specific city, the accessibility to road or railway infrastructure determines the long-term growth or decline, highly likely through market or migration mechanisms (Jedwab and Storeygard, 2022; Berger and Enflo, 2017). Indeed, infrastructure investments have been, and still are, used as political instruments to foster economic growth of specific areas with high future potential or lagging-behind ones.

Differently, in a within city perspective, transportation add to the benefits also non-pecuniary costs. On the one hand urban density allows a more efficient public transport provision and fosters the development of alternative commuting modes. On the other hand, however, scarcity of space and crowding of people lead to urban costs such as traffic, air pollution, and noise. Those are recognised as some of the most severe urban costs, and lead to urban negative externalities. They are transport-related non-pecuniary costs and impose on urban residents severe negative health consequences. Given that, policymakers started to worry about those urban costs. As a consequence, different policies oriented to innovative car-less urban mobility and ecological footprint of cities have been implemented. The aim of those mobility policies is to make less room for cars and make urban areas more livable and enjoyable.

In my thesis I focus both on the intra-city benefits and the within-city costs related to transportation. In the second chapter of this dissertation, I analyse on the long-term economic effects of transportation infrastructure on the reorganization of economic activity and the growth of cities. Differently, in the third and fourth chapters, I focus on two of the negative externalities that within city transportation imposes on urban residents: traffic and noise.

In chapter two, titled *"Paving the way to modern growth: the Spanish Bourbon roads"*, I analyse the local impact of increasing accessibility through transport networks. Specifically, I aim at estimating the potential impact that the improvements in accessibility associated to the construction of the new road network had on the population growth of Spanish municipalities between 1787 and 1857. One of the main challenges of this type of analysis is endogeneity. Generally, transport infrastructures are not randomly assigned across territories but are often correlated to specific local characteristics related to potential economic growth. Therefore, the assignment of infrastructures to territorial units is potentially non-random. In order to retrieve causality, an exogenous selection of municipalities which get access to roads is required. The advantage of the Spanish road network is that it did not seek specifically for routes with high potential trade, but was instead explicitly designed to directly connect Madrid to the main cities of the country. Therefore, this case study would be an ideal natural experiment for this type of analysis. However, to reinforce my case, I also adopt an inconsequential-unit approach, removing from the analysis the termini and other cities targeted in the network design, and considering only the municipalities which got accessibility due to their random location in the middle of two important cities. As a robustness check, I also adopt an instrumental variable strategy based on a geographical instrument (least cost paths between targeted nodes) and an historical instrument (Roman roads).

I find that the increase in market access associated to road accessibility had a substantial effect on local population growth. The impact was substantially higher on the municipalities that had a more diversified occupational structure. By contrast, the effect of the new network on population growth was negative in municipalities close but without direct access to the roads. I interpret these findings as evidence of a process of rural-to-rural migration due to the new roads. The changes introduced by the new paved roads in the structure of transport costs were sufficient to provoke a new population equilibrium, in line with the predictions of the urban economics literature. Municipalities with higher market access were able to draw population from neighbouring areas, probably attracted by the capacity of the roads to stimulate the development of new activities along the route. This would indicate that roads triggered a process of spatial reorganization of population, with short-distance movements of population towards the vicinity of the new roads.

In the third chapter of this dissertation, titled *"Low emission zones and traffic congestion: evidence from Madrid Central"*, I focus on one of the most severe urban costs: traffic. Traffic impose non-pecuniary costs on cities due to the generation of air pollution, accidents and fatalities (Li et al., 2012; Green et al., 2016), delays, stress and road rage, and economic losses (Centre for Economics and Business Research, 2014). Traffic calming policies (i.e. congestion tolls and low emission zones) have been implemented to deal with traffic and pollution in urban areas. This chapter aims to exploiting the implementation a low emission zone (LEZ) in Madrid - i.e. *Madrid Central* - to ascertain whether LEZs have an effect on traffic. Low emission zones are areas to which the access is restricted for the most polluting vehicles. More precisely, it is a quantity measure tackling the externalities through the extensive margin (type of car driven). This measure has been extensively adopted in Europe and has been found to be effective in reducing pollution (Wolff, 2014; Ellison et al., 2013; Malina and Scheffler, 2015; Boogaard et al., 2012; Gehrsitz, 2017; Sarmiento et al., 2021). However, there is a lack of exhaustive evidence about their effect on traffic and car use.

To quantify the causal effect of LEZs on traffic, I develop two alternative empirical strategies. Firstly, I benefit from the exogeneity of the implementation timing to traffic dynamics to develop a pre/post panel fixed-effects analysis. As alternative strategy, I combine the causal impact analysis (Brodersen et al., 2015) with a meta-regression analysis to infer a causal effect exploiting the huge amount of time data available.

Results suggest that the implementation of *Madrid Central* led to an overall small increase in traffic for the whole city of Madrid. Nevertheless, this average result hides important spatial patterns in terms of traffic dynamics. In fact, the implementation did reduce traffic in the restricted area. The time-based model shows an average reduction of around 8.1% in the number of vehicles per sensor/hour and around 8.7% of traffic load in the restricted area. This traffic relief for the treated district is offset by an overall increase in transit in the other areas of the city, which I interpret as displacement effect. Using heterogeneity analyses, I further identify which of the city's streets are most negatively affected by the displacement, as well as showing that the reduction in the city centre gradually decreases over time and eventually disappears seven months after the implementation. I find different reasons to explain this temporal evolution, ranging from announcements by local politicians to the renewal's of the vehicles fleet triggered by the policy, with a shift towards cleaner and exempted cars. Finally, I look at potential changes in commuting and I identify a switch to public transport for commutes directed to the restricted area and rerouting of trips for destinations outside *Madrid Central* as two of the possible mechanisms explaining these results. Overall, the most important result of the chapter is the displacement effect towards unrestricted areas, a relevant and undesired consequence of the policy implementation. My results suggest that spatial spillovers should be considered when designing such schemes, in order to ensure that the whole city benefits from the measure, and not just the restricted area itself.

In the fourth chapter, titled *"The price of silence"*, I study whether street noise is capitalised into housing prices, using housing transactions and noise data from Barcelona. Noise pollution is one major urban cost. The European Environmental Agency estimates that long-term exposure to environmental noise causes 12,000 premature deaths and contributes to 48,000 new cases of ischaemic heart disease per year in Europe.

The economic literature tried to quantify the cost of this negative externality. Most of the studies did so through hedonic price models. In those models, the observed price difference comparing houses with similar characteristics allows to value the cost of non-market factors, such as noise pollution, in monetary terms.

Most of the estimates coming from those models suggest a negative relationship between noise and housing prices, however they tend to fail in providing causal evidence. In fact, the main empirical problem when it comes to estimate the effect of interest is omitted variable bias. On the one hand, a higher noise level would reduce the price of the house, as noise pollution is a disamenity. However, on the other hand, a higher noise level might be related to higher accessibility, thus pointing towards an increase in the the price of the dwelling. In other words, noise levels are correlated to road traffic. Streets with more traffic are likely to represent connection axis between important areas of the city. Thus, noise represents a negative externality, but it also reflects the connectivity of the area. Both aspects are likely to affect housing prices in opposite directions. Thus, disentangling the different effects and isolating the real effect of noise on housing prices is not straightforward.

To the best of my knowledge, my paper is one of the first ones estimating the capitalization of street noise into housing prices in a causal way. Specifically, I combine hedonic price estimates with a fixed-effects model and I benefit from a peculiar architectural feature of my framework, to isolate the effect of unobservable and precisely estimate the effect of interest.

My aim is to exploit the spatial variation in very granular data on street noise and housing transactions data. The grid pattern and the square blocks of the Eixample district of Barcelona provide a good setting to exploit variation in traffic noise intensity between properties belonging to the same building block but exposed to different noise levels. Specifically, I exploit variations in noise levels within 150x150 square meters area. I do so by including in my model building blocks fixed effects. Focusing on variations at a really granular level, I can ensure that all the unobserved characteristics which are simultaneously correlated with noise and influencing property values remain constant. The underlying hypothesis is that properties within a same block enjoy the same local characteristics, but are exposed to different noise levels. In other words, by including block fixed effects, I can keep accessibility, amenities/disamenities constant, and exploit only spatial - within the four street segments included in one block - and temporal variation in noise.

In addition, the residents in the district are quite homogeneous in terms of socio-demographic characteristics. This allows us to assume no systematically differences between people living into different street segments within one building block. Thus, I can exclude the existence of sorting effects, i.e. neighboorhod composition, on housing prices in my set-up(Bayer et al., 2007; Guerrieri et al., 2013; Diamond, 2016).

I do find the *price of silence* to be sizable in the Eixample district of Barcelona. Specifically, my results suggest that street noise leads to a price depreciation of 1.6% for sales' posted prices. In other words, referring to the average price in my estimation sample, I find that moving from one category to another of noise exposure (i.e. increase of 5db) induces a price reduction of about 7,250€. Differently, I do not find an effect for rents. I believe different reasons could explain this difference between sales and rents. First, the people buying or renting might be systematically different. Second, in a real estate market as the one of Barcelona, rents are much more flexible than sales, so that renters move much more often than buyers. Bought flats are much less liquid than other assets compared to other countries, so that buying is usually a long-term decision. Finally, the rental market in Barcelona is really tight, meaning that renters do not have much bargain power to lower the price in case of high noise exposure.

Furthermore, looking at heterogeneous results I find that evening and night, more than daily, are driving the price depreciation. Those results are in line with the second heterogeneity analysis I perform, according to which the negative effect on posted prices for sales is eight times larger for recreational noise, i.e. nightlife noise, than the one related to traffic. Differently, pedestrian noise is found to have a positive effect of housing prices.

My analysis suggests that policies directed to reduce noise in dense urban areas could lead to important welfare benefits. I could have direct benefits through reductions in negative health effects, as well as monetary benefits through an increase in the value of housing assets.

# 2 Paving the way to modern growth: the Spanish Bourbon roads[1]

## 2.1 Introduction

In the central decades of the 19th century, railways brought about an unprecedented productivity boost in the European countries' inland transport sector. The railway represented the first massive application of the new technologies of the Industrial Revolution to transportation, with groundbreaking consequences in all areas, from the scale of infrastructure investment to the volumes of freight and passenger traffic. The contrast between this revolutionary change and the much less sophisticated preindustrial transport technologies explains the widespread belief that inland transport was stagnant before the arrival of the railway. Leaving aside the "canal revolution", which was limited to a small number of countries, this interpretation would describe early 19th century domestic freight and passenger trade as burdened by the high cost and lack of technological change in the inland transport sector. This widespread view was only challenged for England, where the combination of canal investment, a geography quite adapted to coastal navigation and a growing network of turnpike roads allowed the English domestic markets to be close to full integration already in the mid-18th century (Smith, 1776; Granger and Elliott, 1967; Shiue and Keller, 2007; Jacks, 2011; Uebele, 2013; Brunt and Cannon, 2014; Bateman, 2015; Bogart et al., 2022).[2]

The contrast between railway dynamism and earlier stagnation, however, has been questioned by recent research, and is currently being replaced by an alternative picture of slow but gradual long-term improvements in domestic transport productivity, starting well before the arrival of the railway. An early warning against an excessive simplification of the history of 19th century transport and the assumption that railways were superior to any previous transport mode can be found in Robert Fogel's words: *"It is a misleading oversimplification to identify wagons, waterways*

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[1] Paper coauthored with Miquel-Àngel Garcia-López, Alfonso Herranz-Loncan and Elisabet Viladecans-Marsal.

[2] The Netherlands was another case of early domestic market integration, although not as advanced as England in the 18th century (Bateman 2015).

*and railroads with a sequence of temporal stages in which each was predominant (...). The transportation system that evolved during the nineteenth century embraced all three modes. The quantities of service delivered by each mode increased throughout the nineteenth century, although at unequal rates. Each mode was more productive than the other two in some domain, and this pattern of specialized preeminence continued to the end of the nineteenth century"* (Fogel, 1979).

Fogel clearly rejected the idea of a stagnant road transport sector during the era of railway dominance. Recent research has provided evidence that road transport was quite dynamic also before the construction of the first railways. Early experiences of Smithian economic growth, starting before the 19th century, were by definition linked to the growth of *"the extent of the market"* (Smith, 1776), which was in turn associated to gradual improvements in transport technology, including roads. Such improvements have been clearly illustrated by Yrjö Kaukiainen, who provided evidence on the remarkable increase in the speed of information transmission in Europe well before the railway and the electric telegraph. He estimated that dispatch times decreased by two thirds between ca. 1820 and the 1850s and that the reduction in the time that information took to move was much larger before than after the introduction of the telegraph. At least in part, those early gains were the result of the increase in the speed of overland transport. While by 1820, road transport could not cover more than 100 km per day, except for Britain, northern France and, maybe, the Low Countries and part of Germany, by 1840 speed was higher than 200 km per day on many roads out of those areas, thanks to the better quality of infrastructure, better carriages and more efficient organization of coach lines (Kaukiainen, 2001).

Road transport improvement was parallel to substantial progress in domestic market integration across Europe before the generalization of the railway technology. In many European economies price dispersion was rather low already by 1830 (Federico and Persson, 2010), and Chilosi et al. (2013) and Uebele (2013) have shown that most of the progress in the integration of the European wheat market took place before 1850. This literature indicates that, in the 19th century, European countries' market integration was not specifically associated to the construction of railways, but was to a large extent the result of organizational and technological improvements in other segments of the transport sector (water and road transportation). The fact that most advances in market integration took place before 1850 clearly reduces the role of steam technologies in the process (Uebele, 2013).

This paper aims at contributing to the debate on the impact of pre-railway transport by analysing the case of Spanish roads before the first wave of massive railway construction in 1855-65. The Spanish road network before the railways has been the object of quite contradictory assessments. Some researchers have described Spanish pre-railway inland transport as rather inefficient and costly, and one of the structural obstacles preventing market integration and economic development (Gómez Mendoza, 1989; Ringrose, 1970). Spain would therefore be an example of late economic integration, specially compared with the most developed European economies, such as Britain or the Netherlands (Jacks, 2005; Uebele, 2013). Integration would have only advanced in the second half of the 19th century, largely due to railway construction (Rosés et al., 2010). By contrast, other authors have observed substantial progress in market integration since early modern times and, specially, in the decades before 1850 (Barquín Gil, 1997; Martinez Vara, 1999; Reher, 2001; Llopis and Sotoca, 2005; Grafe, 2012; Nogués-Marco et al., 2019). This would be consistent with a gradual reduction in transport costs starting at least in the last decades of the Ancien Régime, which would be reflected, for instance, in the significant decrease in travel times through the road network, as observed by Madrazo (1991), Grafe (2012) and Nogués-Marco, Herranz-Loncán, and Aslanidis (2019). It would also be consistent with López-Cermeño and Santiago-Caballero (2020) that states that, already in the mid-18th century, transport networks were allowing to overcome the obstacles to the integration of Spanish markets. The available evidence on the gradual development of the transport sector and markets in pre-railway Spain has driven Frax Rosales and Madrazo (2001) to drastically reject the long standing view of a stagnant traditional transport sector which had reached its limits in the 18th century.

Instead of looking at market integration gains, here I approach the improvements in pre-railway inland transport by analysing the growth impact of road construction between the late 18th and the mid-19th centuries. Starting in the late 1740s, the Spanish monarchy launched a program of construction of new, high-quality paved roads. Construction was extremely slow for some decades but accelerated since the late 18th century, and by 1855 a network of 8,324 km. had been built (Uriol Salcedo, 1992). The construction of paved roads went hand-in-hand with the reduction of travel times and increasing market integration during the early 19th century, being arguably one of their explanatory factors. In order to study the potential growth effects of these new roads, in this paper I study to what extent the increase in market access associated to the new network that took place between 1787 and 1855 translated into higher demographic dynamism at the local level. This analysis is based on the authors' construction of a new georeferenced database of the evolution of paved roads during the period under study.

My work is related to previous research on the local impact of increasing accessibility through transport networks. This type of analysis has been carried out for several countries both for railway infrastructure in the 19th and early 20th century (Atack et al., 2010; Hornung, 2015; Jedwab et al., 2015; Donaldson and Hornbeck, 2016; Berger and Enflo, 2017; Bogart et al., 2022; Berger, 2019; Banerjee et al., 2020; Büchel and Kyburz, 2020) and for motorways in more recent times (Chandra and Thompson, 2000; Baum-Snow, 2007; Michaels, 2008; Duranton and Turner, 2012; Garcia-López et al., 2015; Jaworski and Kitchens, 2019; Baum-Snow et al., 2020; Bird and Straub, 2020; Jedwab and Storeygard, 2022; Herzog, 2021). However, this is one of the first applications of this approach to measuring the effects of pre-railway road infrastructure. The main precedent to my research is Bogart et al. (2020), which study the impact of multi-modal transport on local growth in England and Wales between 1680 and 1830. In addition, a number of recent papers analyse the impact of Roman roads on historical or present-day development levels and current infrastructure location (Wahl, 2017; Dalgaard et al., forthcoming; De Benedictis et al., 2018; Flückiger et al., 2022), although they differ from my study because they focus on very long-term persistence, while I analyse the short/medium-run effects of the new infrastructure.

One of the main challenges of this type of analysis is endogeneity. Generally, transport infrastructures are not randomly assigned across territories but are often correlated to specific local characteristics related to potential economic growth. Therefore, the assignment of infrastructures to territorial units is potentially non-random. In order to retrieve causality, an exogenous selection of municipalities which get access to roads is required. The advantage of the Spanish road network is that it did not seek specifically for routes with high potential trade, but was instead explicitly designed to directly connect Madrid to the main cities of the country. Therefore, my case study would be an ideal natural experiment for this type of analysis, in which the underlying hypothesis (no economic criteria behind network design and exogenous selection of the treated areas) would hold. However, to reinforce my case, I also adopt an inconsequential-unit approach, removing from the analysis the termini and other cities targeted in the network design, and considering only the municipalities which got accessibility due to their random location in the middle of two important cities. As a robustness check, I also adopt an instrumental variable strategy based on a geographical instrument (least cost paths between targeted nodes) and an historical instrument (Roman roads).

My results provide evidence of a significant positive effect of road accessibility on the population growth of the treated municipalities. The size of the effect was substantially higher in municipalities that had a higher presence of manufacturing and commercial activities in their occupational structure. By contrast, the construction of the new roads decreased population growth in those municipalities close but without direct access to the network. These findings would be consistent with the new network of paved road provoking significant population displacement effects between municipalities well before the arrival of the railway. Given the average size of my sample of municipalities, those movements were predominantly of a rural-to-rural character. My results therefore confirm that the Spanish transport sector was far from stagnant before the mid 19th century. Road infrastructure investment was transforming the territory before the railway era and, together with other improvements in the transport sector, would to some extent account for the slow but gradual process of Smithian growth that was taking place in the Spanish economy at the time (Álvarez-Nogal et al., 2022).

## 2.2 Road transport and the Spanish economy before the railways

The Spanish inland transport system of the early 18th century was largely based on roads. The massive shape of the country limited the reach of coastal navigation, and its rough geography reduced waterways to just ca. 300 km of canals and some stretches of rivers Ebro and Guadalquivir. Under those circumstances, most traffic within the country relied on a dense but very low quality network of traditional roads and paths. Lack of investment in medieval and early modern times meant that part of that network, specially the entrances to cities, bridges, and mountain passes, was still based on ancient Roman infrastructure (Pablo-Martí et al., 2020) and that many roads were not well adapted to wheeled traffic or were only passable in certain seasons, due to the low quality of their surface, the absence of bridges and other shortcomings (Dirección General de Obras Públicas, 1856; Alzola y Minondo, 1899; Madrazo, 1984). Municipalities were in charge of the construction and maintenance of the roads that crossed their territory, mainly through taxes on entrance and exit (*portazgos*). Limited trade and the resulting scarcity of municipal fiscal resources largely explain the state of disrepair of the network. Low quality seems to have been a characteristic shared by the whole road system, even the most important routes, such as those included in the main itineraries published during the period (Pablo-Martí, Alañón-Pardo, and Sánchez, 2020).

The consolidation of the new Bourbon dynasty after the War of Succession (1701-1713) brought with it a project of administrative modernisation of the country, which included significant changes in transport and communication policies. Already in 1716, the government nationalized and centralized the postal service and, four years later, completely reorganized it with a new regulation which included a catalogue of "post roads" of predominantly radial character. These post routes were probably better maintained than the rest of the network, given their specific interest for the Crown, and the presence of postal stops and frequent lodges would likely increase their safety and traffic. However, these improvements did not involve any significant infrastructure investment yet, and post roads remained largely unpaved and of relatively low quality (Pablo-Martí, Alañón-Pardo, and Sánchez, 2020).

This started to change in 1747, when the Spanish government, aware of the insufficiency of municipal budgets, entrusted the construction and maintenance of roads to the Director of the postal services, and established a regular funding mechanism by the Royal Treasury for the development and conservation of the main road network (Pablo-Martí, Alañón-Pardo, and Sánchez, 2020). Construction achievements during Fernando VI's reign (1746-1759), however, were just limited to the Santander - Reinosa road in the North and the Guadarrama pass near Madrid. A systematic construction policy was only adopted in 1761, when a royal decree passed by Carlos III approved the construction of a new road network. This construction plan should start with the roads that connected the Court with the provinces, following the model of the "post roads". As in the case of the postal system, this illustrates the administrative objectives of the road construction plans, clearly aimed at a better communication between Madrid and the rest of the territory in order to ease the administration and control of the country.

The new roads were characterized by its good quality, fully adapted to the requirements of high-speed wheeled transport. They had modern roadbeds, were equipped with bridges (a total of 1,074 had been built by 1868), had moderate gradients (ideally under 6%, although this limit was not always respected) and often allowed the crossing of mountain passes throughout the year (Madrazo, 1984; Uriol Salcedo, 1992). The first finished route, from Reinosa to Santander, was a modern paved road 7.8 m wide, and the highways built thereafter were generally between 7 and 12 m, although some of them reached even 20 m (Madrazo, 1984). The progress of construction was rather slow until 1799, due to the shortage of adequately trained technicians, insufficient funding and organizational weaknesses. As a result, only ca. 2,000 km. of roads had been finished by the end of the century. In fact, this figure included a few hundred km that had not been built by the central government but by the Basque provincial institutions, which enjoyed a substantial fiscal autonomy at the time (Alzola y Minondo, 1899). Thanks to organizational changes and additional training efforts, road construction accelerated in the first years of the 19th century, and the network reached 3,409 km in 1808. The Napoleonic Wars interrupted construction and provoked the abandonment and deterioration of some of the new roads, but works were slowly resumed since 1816, and the resources invested in the network gradually increased during the following decades. Funds for road construction rose from 7.2 million reales per year between 1816 to 1833 to 8.3 million in 1834-40, 11.5 million in 1841-46 and 45.5 million in 1847-55. As a result, the length of the network reached 8,324 km in 1855, and by that year Madrid was already connected with the main cities of the country with high quality paved roads (Dirección General de Obras Públicas, 1856; Uriol Salcedo, 1992). Although this was a small network, compared, for instance, with the 45,000 km of French roads at the time, it completely changed the conditions of transport in the main routes of the country.

The new roads, combined with the improvement in the organization of the postal system and the development of regular passenger transport services during the first half of the 19th century, had far-reaching effects. Organized passenger stagecoach services, limited before 1808 to the route Madrid-Cadiz and some short-distance lines around the capital, were established in 1816 between Madrid, Barcelona and Valencia and expanded to many other routes thereafter, gradually increasing their reliability, safety and speed and reducing their price. These advances represented a revolutionary change in the movement of people (Alzola y Minondo, 1899; Madrazo, 1984). Between 1822 and 1854 the price of stagecoach services decreased by 57 percent (Madrazo, 1991) and this was accompanied by a dramatic reduction in travel times. For instance, in the routes from Madrid to Barcelona, Cadiz or Santander, the speed of the movement of passengers and information increased from ca. 50 km per day in the 1820s to between 150 and 200 km per day in the 1850s, a figure that was close to the best standards in the most developed European economies. The core of the railway network, which would be built between 1855 and 1865 and gave service to a large extent to the same routes as the new paved road system, would obviously increase this figure significantly. However, the time saved thanks to the improvement in road transport between the 1820s and the 1850s (from ca. 13 to 3-4 days between Madrid and Barcelona, for instance) would be much higher than the additional 1-2 days savings provided by the railways (Nogués-Marco, Herranz-Loncán, and Aslanidis, 2019). In the case of freight, although the available information is much less abundant than for passengers, Madrazo (1984) provides some scattered data on the impressive traffic growth that took place in the century before 1850, such as the almost 20-fold increase in the number of vehicles using the Reinosa road between 1750 and 1850. The high efficiency of freight road transport in the mid 19th century has also been stressed by Barquín Gil (1997).

These transformations in the road transport sector took place in an economy that was far from stagnant before the arrival of the railways. The most recent long-term estimates of Spanish economic growth (Prados de la Escosura, 2017; Álvarez-Nogal, Prados de la Escosura, and Santiago-Caballero, 2022) show an increase of ca. 60% in both population and GDP per capita in the century before 1856, when massive railway construction started. The potential progress in market integration and traffic that was partly associated to the new road transport infrastructure would be consistent with a slow but sustained process of economic growth of Smithian character which, despite the series of catastrophic events that affected the Spanish economy (the Napoleonic invasion, the loss of the empire and the civil war of 1833-

40), brought about significant changes in all aspects of economic life. The railways, therefore, did not arrive to a stagnant economy or replace a lethargic road sector, but would have represented the continuation of a long-lasting dynamic trend.

To illustrate such economic dynamism and the role of the transport sector, in this paper I aim at estimating the potential impact that the improvements in accessibility associated to the construction of the new road network had on the population growth of Spanish municipalities. The next section describes the data on which my estimation is based.

## 2.3 Data

In order to estimate the impact of road accessibility on local population growth between the late 18th and the mid-19th century, I have retrieved indicators of the development of the road network and population size at the municipal level, as well as a number of control variables.

As is usual in this type of research, and given the lack of alternative economic indicators at the municipal level, population is the outcome variable which I use to proxy local growth. Population data for the late 18th century come from the so-called "Floridablanca Census". Published in 1787 and then expanded in 1789, it is the first Spanish census carried out with modern statistical techniques and allows, for all the municipalities at the time, to disaggregate population figures by sex, civil status, age and occupation.

In the case of the mid-19th century, I use the historical population data provided by the Spanish Statistical Institute (INE) website.[3] The period covered by this source starts in 1842. Figures for 1842, though, were not collected with homogeneous techniques all over the territory, and this population head count (usually known as the *"Censo de la Matrícula Catastral"* – Census of the Cadastre Register) is not considered as reliable or precise as the following ones. Thus, in my analysis I focus on data from the first modern census, carried out in 1857, discarding also later censuses due to the quick development of the railroad network after that date.

The administrative units employed for the analysis (municipalities) changed over time due to mergers and divisions. Since I do not have a map of municipality boundaries for the period under study, I had to rely on present-day maps and match the historical municipalities to the current ones using different sources. For those changes that took place after 1842, the INE website provides a complete catalogue (Ministerio de Administraciones Públicas, 2008). By contrast, the match between some

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[3] http://www.ine.es/intercensal/

of the municipalities included in the Floridablanca Census and the current ones is more complicated and not always possible, and a small share of the Spanish population (amounting to 1.13% in 1787 and 3.26% in 1857) could not be matched.[4]

As for road development, one of the strengths of my paper is represented by the quality and precision of the digitization of the new paved road network. Applying GIS techniques to information provided in Madrazo (1984) and Dirección General de Obras Públicas (1856), I have digitized the map of the network for four different years along the period under study. First, I digitized the map of roads in 1855 that is included in Dirección General de Obras Públicas (1856) and is my baseline map[5]. It reports the names of more than 500 municipalities crossed by the paved roads, which helped us to correct for the effects of the spatial distortions of the map on the specific location of each route. I then used information included in a series of hand-drawn maps in Madrazo (1984) to obtain digitized maps of the network in 1778, 1808 and 1840 by removing from my baseline map the road stretches that were still unfinished in those dates. Those maps have been used to compute the municipal accessibility measure I use in my analysis (see Figure 2.1). As it is described in detail below, the latter has been calculated on the basis of the least cost path, which has required the use of travel time and cost parameters obtained from the careful analysis of several sources that report average speeds for passenger transport for different years in km per day in the main routes (Madrazo, 1984; Grafe, 2012; Nogués-Marco, Herranz-Loncán, and Aslanidis, 2019). In order to approach the changes in accessibility associated to new roads, I employ the digitalized map of early 18th century network of old, low quality roads, which has been kindly provided by Federico Pablo-Martí (Figure 2.3).[6] Finally, I also use the digitalised map of the main Roman roads in Spain (McCormick et al., 2013) as an instrument in my IV strategy.

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[4] This only refers to peninsular municipalities; the islands and the North African cities of Ceuta and Melilla are not included in the analysis. I thank David Reher for providing us with the digitized version of the 1787 census, and Alfonso Díez-Minguela and Julio Martínez-Galarraga for their invaluable help for the matching process.

[5] I also check for the more precise 1861 map (Dirección General de Obras Públicas, 1861) to correct for some road segments which are not correctly classified in the 1856 map.

[6] Given the absence of investment in the old roads, I assume this network to be representative of the road system by 1787.

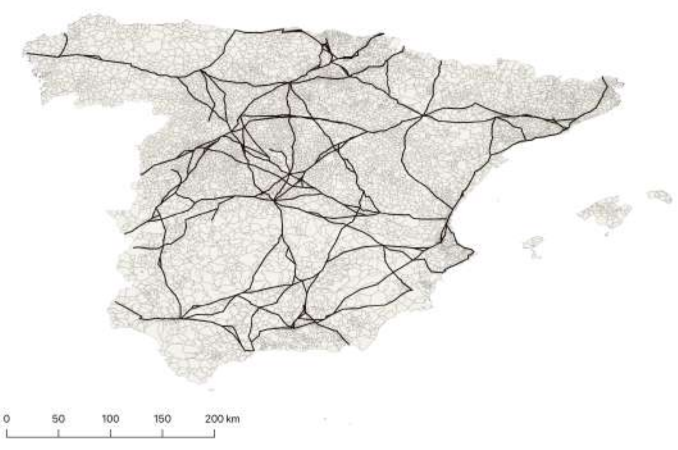
Figure 2.1: The Spanish paved road network, 1778, 1808, 1840 and 1855.

(a) 1778 (b) 1808

(c) 1840 (d) 1855

*Source*: Own digitization based on Madrazo (1984); Dirección General de Obras Públicas (1856)

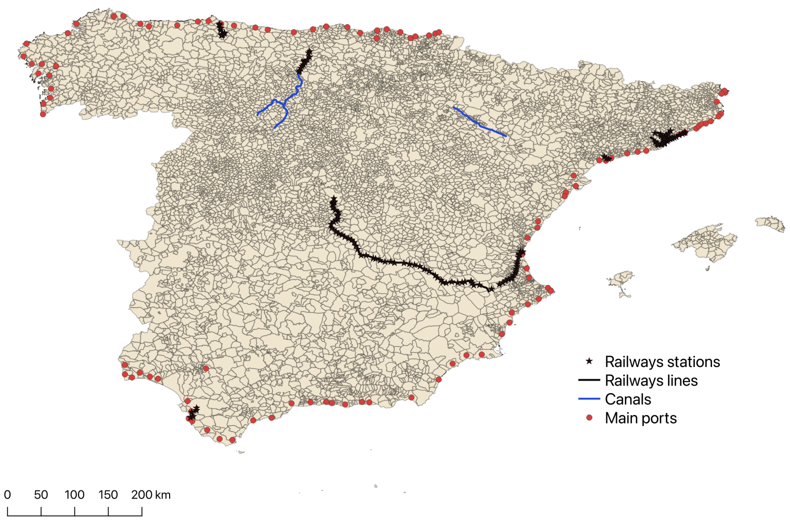
Figure 2.3: The Spanish road network before the construction of the paved roads.



*Source*: Map provided by Federico Pablo-Martí

My analysis also includes a full set of controls to identify potential confounding factors. I account, first, for access to railroads in each municipality. The first Spanish railroad, opened in 1848, was a short (28 km) line between Barcelona and Mataró. By the end of my period of analysis (1857) the network had reached a length of 635 km. I have digitized those early railway lines, starting from the shapefile of current railways and following the chronological description of the construction process provided by García Raya (2006). I specifically control for the exact location of railways' stations operating by the end of the year 1857.[7] I also account for the presence of the two navigable canals that were constructed in the 18th century: the Canal de Castilla and the Canal Imperial de Aragon, which have been digitized using information in Google Maps. Finally, I account for the 104 main Spanish ports at the time, selected on the basis of data at Dirección General de Aduanas (1857). Railway stations, canals and the main ports are represented in Figure 2.4. I have also gathered information on distance to the coast, presence of rivers and closeness to the Portuguese and French borders for each municipality using the shapefile of rivers and coastlines from Instituto Geografico Nacional (IGN). Finally, using the information provided by the Floridablanca Census, I have collected information on the population employed in a number of non-agrarian occupations (merchants, blacksmiths, craftsmen, lawyers and notaries) in each municipality at the end of the 18th century, and I have also gathered information on the main post offices in the late 18th century, taken from Ita (1789).

Figure 2.4: Railways, canals and main ports in 1857.



*Source*: Own digitization based on García Raya (2006); Dirección General de Aduanas (1857); Google Maps

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[7] I thank Guillermo Esteban for providing us the precise location of those stations.

## 2.4 Empirical framework

As already mentioned, endogeneity is one of the main concerns when it comes to analyze the effects of transport infrastructure. Often, transport infrastructure investment is not randomly assigned across territories, but is correlated to specific local characteristics related to potential economic growth[8]. In order to retrieve causality, an exogenous selection of municipalities which got access to roads is required. To deal with this, I exploit a historical quasi-natural experiment and I combine this with an inconsequential unit approach.

In this regard, the advantage of the Spanish road network is that it did not seek specifically for routes with high potential trade, but it was instead explicitly designed to connect Madrid to the main cities in the Peninsula. However, to further reinforce my case against endogeneity, I complement this with an inconsequential unit approach. This strategy, firstly adopted by Chandra and Thompson (2000), is based on the idea that the municipalities lying between two important cities (main nodes) might have got exogenous access to a new road because of their random location on the path between the two targeted cities. The idea behind this approach is to identify all the cities targeted by the infrastructure investment planner and remove them from the analysis. The underlying hypothesis is that all the remaining municipalities got access to the network only due to their geographical position. In order to define the targeted nodes that are excluded from the analysis for the inconsequential unit approach, I collect information from the 1720 general regulation of post services, whose main routes largely inspired the priorities in road construction as defined in the 1761 Royal Decree. As a result, I get a list of 57 targeted nodes, which are the cities and junctions mentioned in the description of those main post routes[9].

A second crucial issue is the definition and measurement of road accessibility. A binary definition of accessibility has often been used in the literature (Chandra and Thompson, 2000; Michaels, 2008), which differentiates between those municipalities that were crossed by a road and the rest. Binary accessibility indicators allow estimating the average impact of roads on local growth, but ignore the fact

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[8] Redding and Turner (2015) provide an exhaustive review of the current literature dealing with the endogenous placement of transport infrastructure

[9] See Grimaldo (1720). The targeted nodes are: Corunna, Alcalá de Henares, Alcántara, Alicante, Aranjuez, Arévalo, Badajoz, Barcelona, Baztán, Benavente, Burgos, Cadiz, Carmona, Cartagena, Ciudad Real, Ciudad Rodrigo, Córdoba, Denia, San Sebastián, Écija, El Escorial, El Puerto de Santa María, Fraga, Getafe, Guadalajara, Irún, La Junquera, Lleida, Madrid, Medina del Campo, Medinaceli, Mérida, Molinaseca, Murcia, Ourense, Pamplona, Pontevedra, Salamanca, San Clemente, Santiago de Compostela, Seville, Soria, Talavera de la Reina, Tarancón, Tarragona, Teruel, Toledo, Torrelodones, Tortosa, Trujillo, Tudela, Valencia, Valladolid, Vitoria, Zafra, Zamora and Zaragoza.

that the effect of the new infrastructure might have been very different depending on the location of each municipality and its distance to other population centres through the network. Towns close to big cities, for example, might have benefited more from increasing accessibility than more remote places that were located far from the most developed and diversified areas, and this difference would not be captured by an analysis based on a mere binary accessibility variable. Binary indicators also ignore the fact that some crossed municipalities might have had better accessibility than others before the construction of the new infrastructure, which would affect the size of the benefits received from the latter. Taking these aspects into account requires using an indicator of the variation of market access instead of local accessibility. Market access indicators have been widely applied in the more recent literature on the impact of infrastructure (Bogart et al., 2020; Donaldson and Hornbeck, 2016; Gibbons et al., 2019; Jaworski and Kitchens, 2019; Jedwab and Storeygard, 2022; Herzog, 2021). They capture the increase in local accessibility associated to all changes in the road network, and not just the average effect of the mere presence of a road. For those reasons, I use as indicator of the improvement in local accessibility the variation in market access between 1787 and 1855 associated to new road investment.

I compute market access as the weighted sum of inverted travel times (used as a proxy of transportation costs) to all other connected municipalities through the road network, using as weights the initial population at destination, which I take as indicator of economic activity. In the calculation for 1787, I use the previous system of low quality roads, while market access at the end of the period (1855) is based on the integration of the new high-quality network with those preexisting roads, deleting the old segments over which some new roads had been built (see Figure 2.5). In the case of those municipalities that were only crossed by the new network I added, for the 1787 calculation, the shortest segment between the existing network and those locations. The municipalities included in the calculation are 2,221, which accounted for ca. 50% of the Spanish population both in 1787 and 1857 and between 81% and 86% of the urban population of the country (defined as people living in cities with more than 10,000 inhabitants). Detailed information on the construction of those networks and the cost parameters imputed for the the market access computations are provided in the Appendix methodological note.

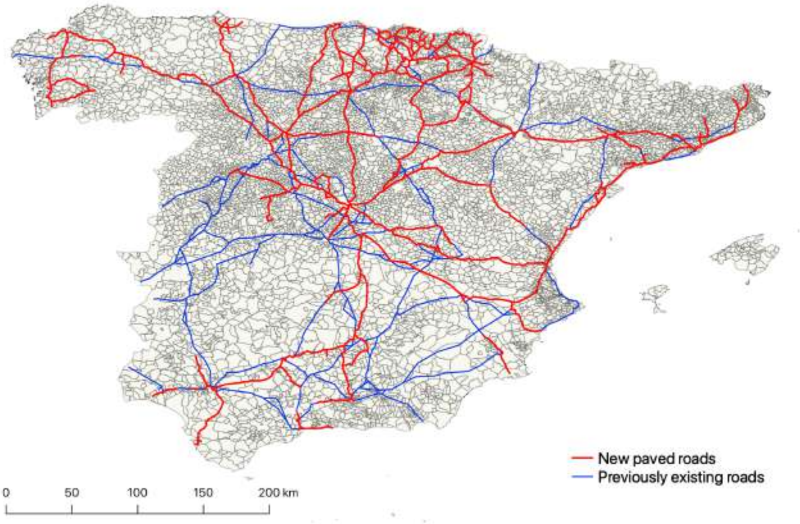
Travel times are computed with a least coast path algorithm, imputing different speeds to each type of road segment depending on its quality. My measure of market access is given by the following equation:

$$MA_o^t = \sum_d (t_{o,d}^t)^{-\theta} * W_d \tag{2.1}$$

Where $MA_o^t$ is the market access measure for municipality $o$ in year $t$; $t_{o,d}^t$ is the travel time between the municipality of origin $o$ and each municipality of destination $d$ in year $t$; $\theta$ is the elasticity to travel times; and $W_d$ are weights to capture the size of economic activity at destinations (usually measured at *t-1* to avoid endogeneity). In my case, I set $\theta = 1$ and $W_d = pop_{d,1787}$, and the market access indicator for each municipality is therefore calculated as:

$$MA_{o,t} = \sum_{d=1}^{M-1} \frac{pop_{d,1787}}{t_{o,d}} \tag{2.2}$$

Figure 2.5: Network employed for 1855 market access computation



In order to estimate the effect of increased road accessibility on local population growth, I regress the logarithm of municipal population on the logarithm of market access, a list of controls and historical judicial district fixed effects[10]. I cluster the standard error at the judicial district level and, consistently with the inconsequential unit approach I remove the 57 targeted nodes. My analysis is restricted to the remaining 2,164 municipalities. The model I estimate is as follow:

$$ln(pop_{m,t}) = \alpha_0 + \beta_1 ln(1 + MarketAccess_{m,t}) + \beta_2 X_m + \theta_p + \varepsilon_{m,t} \tag{2.3}$$

Where $pop_{m,t}$ is the population for municipality $m$ in year $t$ (1787 and 1857 respectively); $MarketAccess_{m,t}$ is the accessibility indicator for municipality $m$ and year $t$

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[10] Judicial districts were the smaller supramunicipal territorial divisions at the time. I thank Francisco Beltrán-Tapia for sharing with us the shapefile of the Spanish judicial districts in 1860. Figure A2.1 in the Appendix B shows those administrative units.

$X_m$ is a vector of time-invariant control grouped in three categories: geographical, accessibility and others[11]; and $\theta_p$ are fixed effects for the 459 historical judicial districts.

With data representing a panel of municipalities, I am able to partition $\varepsilon_{m,t}$ into permanent and time-varying components. In fact, after estimating the model in a pooled OLS framework, I can remove the time-invariant municipal effects by estimating equation 2.3 in a panel or first-difference version.

My historical context, combined with the removal of the main nodes, should ensure the exogeneity of local accessibility variables. However, it is possible that the planner, while designing the road network, deliberately established detours from the most direct line connecting two targeted cities, for example to connect a third municipality with high growth potential. Those detours might induce selection effects and violate the underlying hypothesis. Thus, as robustness check I also adopt an instrumental variable approach, following the so-called inconsequential unit IV approach (Banerjee, Duflo, and Qian, 2020; Büchel and Kyburz, 2020). I employ two different instruments in order to ensure the random allocation of local access to transport infrastructure. More precisely, I use both a historical and a geographical variable to instrument for market access. The former is based on the market access computed through the old Roman roads in Spain (Garcia-López et al., 2015), while the latter is constructed with least-cost paths between the targeted nodes. I compute my geographical instrument based on least cost paths considering the roughness of the terrain, calculated through GIS techniques on the basis of the elevation raster of the Spanish geographical institute (IGN). Specifically, the algorithm calculates the optimal path to minimize the connection cost between the selected targeted nodes. I then use this cost-minimizing network as instrument for the real one. Figure 2.6 shows the 1855 paved road network, the least-cost paths and the targeted nodes. Differently, for the historical IV I employ directly the digitalised network of the main important Roman roads constructed in Spain (McCormick et al., 2013). This second instrument is presented in Figure 2.7.

The model I estimate is as follow:

$$ln(1+MarketAccess_{m,t}) = \alpha_0 + \beta_1 ln(1+InstrumentMA_m) + \beta_2 X_m + \theta_p + \varepsilon_{m,t} \tag{2.4}$$

$$ln(pop_{m,t}) = \alpha_0 + \beta_1 ln(1+\widehat{MarketAccess}_{m,t}) + \beta_2 X_m + \theta_p + \varepsilon_{m,t} \tag{2.5}$$

Where $MarketAccess_{m,t}$ is market access for municipality $m$ in year $t$. $InstrumentMA_m$ is the time-invariant instrument defined in two different ways: market access com-

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[11] Geographical controls include distance to the coast and presence of rivers; Accessibility controls account for port, canals, railroads stations, and being within 5km to the French or Portuguse border; Other controls include the population share employed in the secondary sector in 1787 and having a main post office.

puted through the least cost paths between nodes (geographical instrument), and market access computed through the old Roman roads (historical instrument); $X_m$ is a vector of control variables and $\theta_p$ are fixed effects for the 459 historical judicial districts. Table A3.1 presents summary statistics of each variable.

Figure 2.6: Geographical instrument: digitized roads and least cost path lines

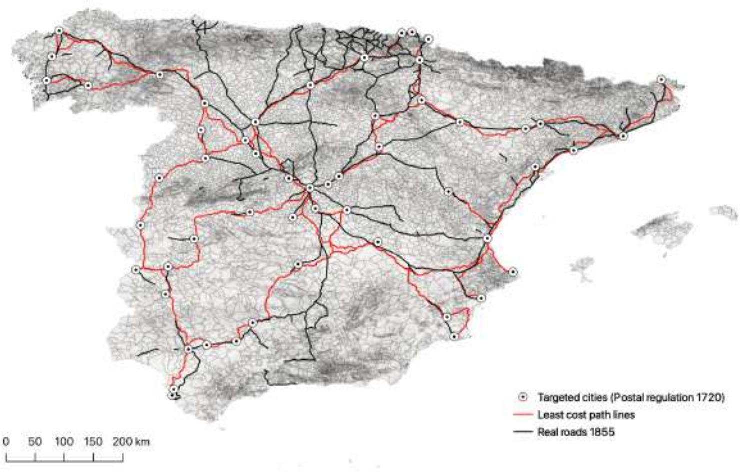

Figure 2.7: Historical instrument: digitized roads and old Roman network

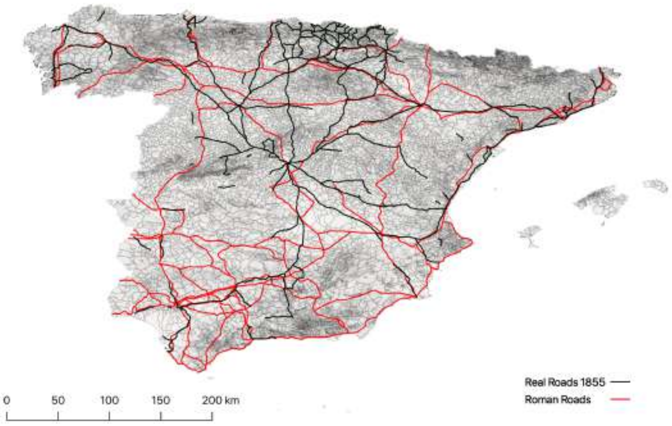

Table 2.1: Summary statistics

| | count | mean | sd | min | max | p25 | p50 | p75 |
|---|---|---|---|---|---|---|---|---|
| Population 1787 | 2,164 | 1,608.49 | 2,889.75 | 13.00 | 52,375.00 | 356.50 | 767.00 | 1,795.00 |
| Population 1857 | 2,164 | 2,350.15 | 4,342.75 | 100.00 | 97,777.00 | 548.50 | 1,120.00 | 2,463.50 |
| Market access old network | 2,164 | 27,635.03 | 7,341.39 | 12,262.74 | 102,862.43 | 23,259.37 | 27,628.38 | 31,036.12 |
| Log market access old net. | 2,164 | 10.19 | 0.25 | 9.41 | 11.54 | 10.05 | 10.23 | 10.34 |
| Market access 1855 | 2,164 | 81,723.67 | 30,431.95 | 26,606.62 | 567,789.49 | 65,358.95 | 81,343.60 | 95,060.53 |
| Log market access 1855 | 2,164 | 11.26 | 0.33 | 10.19 | 13.25 | 11.09 | 11.31 | 11.46 |
| Distance to coast (km) | 2,164 | 131.21 | 99.60 | 0.08 | 357.87 | 37.62 | 117.83 | 212.57 |
| River accessibility (0/1) | 2,164 | 0.09 | 0.28 | 0.00 | 1.00 | 0.00 | 0.00 | 0.00 |
| Port (0/1) | 2,164 | 0.02 | 0.13 | 0.00 | 1.00 | 0.00 | 0.00 | 0.00 |
| Canal (0/1) | 2,164 | 0.01 | 0.11 | 0.00 | 1.00 | 0.00 | 0.00 | 0.00 |
| Railroad’s stations 1857 (0/1) | 2,164 | 0.03 | 0.17 | 0.00 | 1.00 | 0.00 | 0.00 | 0.00 |
| 5km from Portuguese border | 2,164 | 0.01 | 0.07 | 0.00 | 1.00 | 0.00 | 0.00 | 0.00 |
| 5km from French border | 2,164 | 0.01 | 0.09 | 0.00 | 1.00 | 0.00 | 0.00 | 0.00 |
| Main Post station (0/1) | 2,164 | 0.07 | 0.26 | 0.00 | 1.00 | 0.00 | 0.00 | 0.00 |
| Pop. share in secondary sector | 2,164 | 2.12 | 2.64 | 0.00 | 24.57 | 0.31 | 1.36 | 2.84 |

*Notes:* The sample of municipalities included is the sample employed for regressions. That is, all the Spanish municipalities connected by at least one of the two road networks, for which I am able to compute a market access indicator.

## 2.5 Results

Table 2.2 presents my baseline results for equation 2.3[12]. In Columns 1 to 5 I follow a pooled strategy, while Column 6 presents panel results. Specifications 1-5 include historical judicial district fixed effects and distinguish themselves by the gradual inclusion of controls: geographical controls in Col. 2, accessibility controls in Col. 3, other controls in Col. 4, and all together in Col. 5. The small differences between the estimates reported in all columns suggest that the results are not driven by the controls. In Columns 6, I remove the time-invariant municipal effects in order to apply a panel framework. Since my fixed effects and controls are time-invariant I cannot include any of them in this specification. The standard errors are clustered at the historical judicial district fixed effects. Panel results are consistent with the pooled ones[13].

Column 5 is my preferred specification. I find a positive and statistically significant coefficient, suggesting a positive effect of the increase in market access associated to the new roads on city growth. According to the estimate, a 1% increase in market access translated into 0.397 additional percentage points of local population growth between 1787 and 1857. In other words, doubling the market access led to an average increase in municipal population of 39.7%. The table also provides the standardized (beta) coefficient of the market access variable which indicates, in my preferred specification, that one standard deviation increase in market access was associated to around one sixth (0.16) of a standard deviation increase in population growth.

These results are robust to the implementation of different elasticities to travel times in the market access computation: using values from 0.5 to 4 I always obtain positive and statistically significant coefficients, although of decreasing magnitude (Table 2.3). elasticities give more weights to closer municipalities, while lower values give increasing importance to more distant destinations. An elasticity of 1 would give equal importance to all travel distances. I use the Akaike's and Bayesian information criterions to decide between the different elasticities. As shown in the table, the lowest values are reported for the elasticities of 0.5 and 1, which should then be preferred. Since the standardised coefficients of those regressions are not different, I choose an elasticity of 1 as reference value, to avoid the weights of travel costs between origins and destinations to follow a quadratic process.

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[12] Table A2.1 in the Appendix reports also the estimated coefficients for the control variables.

[13] I also test a model in first differences and I find, as expected, the same coefficient of Col. 6.

Table 2.2: Market access and local population growth

| | (1)<br>POLS | (2)<br>POLS | (3)<br>POLS | (4)<br>POLS | (5)<br>POLS | (6)<br>Panel OLS |
|---|---|---|---|---|---|---|
| Log Market Access | $0.408^{***}$ | $0.403^{***}$ | $0.409^{***}$ | $0.413^{***}$ | $0.397^{***}$ | $0.343^{***}$ |
| | [0.164] | [0.163] | [0.164] | [0.168] | [0.169] | [0.187] |
| | (0.087) | (0.072) | (0.086) | (0.084) | (0.061) | (0.018) |
| Geographical controls | | ✓ | | | ✓ | |
| Accessibility controls | | | ✓ | | ✓ | |
| Other controls | | | | ✓ | ✓ | |
| Judicial district FE | ✓ | ✓ | ✓ | ✓ | ✓ | |
| R2 | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 | 0.43 |

*Notes:* 4,328 observations (2,164 municipalities × 2 years (1787-1857)) in each regression. The dependent variable is the log of municipal population. Fixed effects are taken at the historical judicial district level. Standard errors are clustered by historical judicial districts and are in parenthesis, and the standardised beta coefficients are within square brackets. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

Table 2.3: Market access and local population growth. Pooled OLS. Different elasticities

| | (1)<br>e=1 | (2)<br>e=0.50 | (3)<br>e=1.50 | (4)<br>e=2 | (5)<br>e=4 |
|---|---|---|---|---|---|
| Log Market Access | $0.397^{***}$ | $0.638^{***}$ | $0.168^{***}$ | $0.068^{***}$ | $0.014^{***}$ |
| | [0.169] | [0.180] | [0.130] | [0.090] | [0.049] |
| | (0.061) | (0.026) | (0.035) | (0.015) | (0.004) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| Fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| R2 | 0.9899 | 0.9902 | 0.9893 | 0.9889 | 0.9887 |
| AIC | 9256.66 | 9114.63 | 9527.63 | 9671.06 | 9751.59 |
| BIC | 9320.39 | 9178.36 | 9591.35 | 9734.79 | 9815.32 |

*Notes:* 4,328 observations (2,164 municipalities × 2 years (1787-1857)) in each regression. Pooled OLS regressions. The dependent variable is the log of municipal population. $e$ is the elasticity to travel distances employed in the market access computation. AIC and BIC are the Akaike's Information Criterion and Bayesian information criterion respectively. Fixed effects are taken at the historical judicial district level. Standard errors are clustered by historical judicial districts and are in parenthesis, and the standardised beta coefficients are within square brackets. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

As a robustness check, I present IV results in Table 2.4. Specifically, I instrument my market access indicator with market access computed through different networks. In Panel A, I present results for my historical instrument: market access computed through the ancient Spanish Roman roads. Differently, in Panel B I use the market access of an artificially network generated with least cost paths between the targeted nodes (geographical instrument). As the first-stage statistics above the critical values suggest, both instruments are good. In line with specifications 1-5 of Table 2.2, I run pooled IV models where I gradually include the different groups of controls. Again, the inclusion of the controls do not change the results. In my preferred specification (Col. 5), the two models provide similar results. According to those estimates, a 1% increase in market access translated into 0.647 (panel A) or 0.670 (panel B) additional percentage points of local population growth between 1787 and 1857. Alternatively, doubling the market access led to an increase in municipal population of 64.7%/67.0%.

Table 2.4: Instrumental variable approach

| | Panel A - Roman roads instrument | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Log Market Access | $0.662^{***}$ | $0.693^{***}$ | $0.663^{***}$ | $0.668^{***}$ | $0.647^{***}$ |
| | (0.068) | (0.073) | (0.067) | (0.055) | (0.058) |
| Geographical controls | | ✓ | | | ✓ |
| Accessibility controls | | | ✓ | | ✓ |
| Other controls | | | | ✓ | ✓ |
| Judicial district FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| First-Stage F-Statistic | 1526.68 | 1465.77 | 1478.88 | 1503.76 | 1415.08 |
| | Panel B - Least cost path instrument | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Log Market Access | $0.672^{***}$ | $0.706^{***}$ | $0.674^{***}$ | $0.681^{***}$ | $0.670^{***}$ |
| | (0.060) | (0.063) | (0.058) | (0.045) | (0.043) |
| Geographical controls | | ✓ | | | ✓ |
| Accessibility controls | | | ✓ | | ✓ |
| Other controls | | | | ✓ | ✓ |
| Judicial district FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| First-Stage F-Statistic | 36.39 | 38.38 | 36.32 | 36.23 | 38.76 |

*Notes:* 4,328 observations (2,164 municipalities × 2 years (1787-1857)) in each regression. Pooled IV regressions. The dependent variable is the log of municipal population. Log market access (and its relative interactions) is instrumented in Panel A with the market access of Roman roads (and its relative interactions) and in Panel B with the market access computed through the least cost path networks (and its relative interactions). Fixed effects are taken at the historical judicial district level. Standard errors are clustered by historical judicial districts and are in parenthesis, and the standardised beta coefficients are within square brackets. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

The previous results may be hiding a significant heterogeneity of effects across different municipalities. For instance, cities which got access to the road network at the beginning of the period might have grown more than those connected in the latter years. Alternatively, the growth effect might have been different for those municipalities whose new roads were not finished yet in 1855 and thus remained unconnected to the main network by the new paved roads (see Figure A2.2 in Appendix B). And, finally, the demographic impact of increasing market access might have changed depending on the occupational structure of the population.

In order to test these hypotheses, I successively add to the model of Panel A in Table 2.4 different interaction terms composed by the market access indicator and a variable that captures the effect I want to analyse. In addition, I incorporate in the regression the specific variable I want to test for and I add a second instrument (the interaction between the Roman roads market access indicator and the variable of interest). I present the results in Table 2.5.

In Columns (1) I test for the effect of early versus late accessibility, adding an interaction term that indicates whether the municipality got access to the road before or after 1808, based on information reported in Madrazo (1984). My results suggest that the average effect presented above is not different between the municipalities connected in earlier or later years. This would indicate that the impact of road accessibility improvement on local growth materialized quite soon after the connection to a new paved road. However, the first stage statistics suggest that the two instruments might be weak, which indicates that the estimates must be taken with some caution.

In Column (2) I account for the fact that some of the roads built in 1855 were not finished yet and remained unconnected to the main network (Figure A2.2). Again, the coefficient of the interacted parameter is not statistically significant, which would indicate that those municipalities crossed by the new roads started benefiting from them even before the completion of the routes. However, albeit the instrument seems to be good, those results might be affected by the small number of municipalities that were crossed by those unfinished roads.

Finally, in Column (3) I analyse whether the positive effect of the market access increase on growth was higher in municipalities with a larger percentage of the total population working in manufacturing and commerce-related occupations in 1787, including merchants, blacksmiths, craftsmen, lawyers and notaries. The positive and statistically significant coefficient for the interaction term in Column (3) suggests that, within the crossed municipalities, the higher the share of population working in manufacturing and commerce, the higher the growth impact of increasing market access. Accordingly, the new roads benefited relatively more those municipalities with a more diversified economic structure and higher presence of secondary and tertiary activities, compared to those in which the primary sector was predominant.

Table 2.5: Market access and local population growth. Heterogeneity analyses

| | Pooled IV | | |
|---|---|---|---|
| | (1)<br>Time 1808 | (2)<br>Unlinked roads | (3)<br>Secondary sector |
| Log Market Access | $0.674^{***}$ | $0.666^{***}$ | $0.652^{***}$ |
| | (0.034) | (0.043) | (0.059) |
| Log MA * Post1808 | -0.911 | | |
| | (0.570) | | |
| Log MA * IsolatedNew | | -2.324 | |
| | | (1.663) | |
| Log MA * Secondary sector share | | | $0.270^{*}$ |
| | | | (0.144) |
| Controls | ✓ | ✓ | ✓ |
| Fixed effects | ✓ | ✓ | ✓ |
| First-Stage F-Statistic | 5.02 | 38.59 | 4.18 |

*Notes:* 4,328 observations (2,164 municipalities × 2 years (1787-1857)) in each regression. Pooled IV regressions. The dependent variable is the log of municipal population. Log market access (and its relative interactions) is instrumented with the market access of Roman roads (and its relative interactions). I include in the regressions the second terms of the interactions, even if I omit them from the table. Fixed effects are taken at the historical judicial district level. Standard errors are clustered by historical judicial districts and are in parenthesis, and the standardised beta coefficients are within square brackets. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

## 2.6 Mechanisms: growth vs. relocation

My results indicate that the increase in market access provided by the new high quality Spanish roads had a positive impact on municipal population growth. Such higher population growth might have been the result of either higher natural growth through fertility increases and mortality reductions or migrant attraction. In the context of the late 18th and early 19th centuries, when the demographic transition had not started in Spain[14], the second mechanism is much more likely than the first one. In addition, it is also consistent with predictions by theoretical models from regional and urban economics (see a summary in Redding and Turner (2015)), which show that better transport infrastructure generates a new population equilibrium through differential changes in market access. This new equilibrium is associated to the relocation of labor towards those places that benefit from larger reductions in trade

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[14] In Spain, both mortality and fertility remained at very high levels during the period under study. Still in the third quarter of the 19th century, Spanish life expectancy at birth was lower than 30, and the reduction of fertility remained at typical pre-industrial levels, except for some regions (Nicolau, 2005)

costs.

The context of my research, though, is very different from the usual one to which those models are applied. Before 1857 Spain was still a predominantly rural country, and the extent of structural change and agglomeration economies was very limited. Given the low average size of the municipalities in my sample[15], my results would probably be reflecting rural-to-rural movements, rather than rural-urban migration. In addition, although, as indicated in the previous section, the impact of roads was higher in those municipalities with more presence of secondary and tertiary activities, even those places with higher capacity of attraction were still, on average, relatively small and predominantly agrarian. This differentiates my study from most analyses for the railway era (Atack, Bateman, Haines, and Margo, 2010; Berger and Enflo, 2017; Bogart, You, Alvarez-Palau, Satchell, and Shaw-Taylor, 2022), with the main exception of Büchel and Kyburz (2020), which also largely captures movements from rural to rural municipalities associated to railway construction in Switzerland. However, even in this case the context was much more industrialized than my own.

How likely were such rural-to-rural migration flows in the Spanish economy before 1857? The literature has often stressed that preindustrial societies could be highly mobile, although most movements were temporary and short-distance (Silvestre, 2002). There were also important rural-to-urban flows, although they hardly increased urban centers' population due to their comparatively high mortality rates. However, in Spain, as in other European economies, this situation was changing since the late 18th century, with an acceleration of urban growth, based on short-to-medium distance immigration in the most industrialized regions (Cura, 1993), and a widespread increase of long-distance movements in the 1850s (Santiago-Caballero, 2021). Thus, population mobility of an increasingly permanent character seems to have been growing substantially in Spain during the period under study. On the other hand, although the available evidence for Spain is mainly associated to rural-urban migrations, some studies on other countries have reported the increasing scope of rural-to-rural permanent movements. In addition to Büchel and Kyburz (2020), who show that the railways acted as a pull factor for migrants in rural Swizerland, Dribe (2003) has also shown the importance of short-distance permanent rural-to-rural mobility in mid-19 century Sweden. This author insists on the need to consider migration patterns that cannot be accounted for by the most traditional explanations of rural-urban movements, like wage differentials, and shows that those migrants stayed close to their place of origin and tended mostly to remain in typically rural activities.

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[15] The population of the median municipality in my sample was 778 inhabitants in 1787 and 1,187 in 1857

My results would indicate that changes in trade costs associated to the construction of the new network of paved roads might have fostered short-distance, largely permanent, rural-to-rural movements in Spain. The new roads increased certain municipalities' market access and offered as a consequence, specially to the most diversified populations, new opportunities for the growth of traditional sectors or the development of new specializations. Given the paucity of statistical information at the local level for the period under study, it is difficult to find evidence on these changes and their association with improvements in the transport network.

Nevertheless, for the case of the medium sized town of Monforte, for instance, Dubert (1998) has explicitly linked the arrival of the road network to an increasing ability to attract migrants, despite the extremely slow transformation of the town's socioeconomic structure. From a different perspective, Madrazo (1984) has illustrated the way in which the expansion of passenger travel that followed the construction of paved roads could have transformed the economy of many municipalities that gained access to the new network. This author estimated that the yearly amount of travellers using regular stagecoach services in Spain increased from 2,000 in 1818 to 825,000 in 1850 (p. 534). Such a huge rise translated into new demands of lodging and catering services along the route as well as into the growing presence of certain jobs associated to the road: *"together with the staging post and the inn, resources at the service of traffic include the helper, ready to cover the hoofs of the horses, the blacksmith and wagoner to mend the cart axles, rods and wheels; the leather craftsman that sells or fixes harnesses, whips, collars and other tack..."* (p. 563). According to Madrazo (1984) these effects were not confined to passenger traffic. As has been pointed out, the increase in freight transport during the period under study, although much more difficult to estimate, would have also been impressive and, for instance, in the case of the Reinosa road, the aforementioned 20-fold increase in freight traffic would have stimulate the establishment of new industries along the route (pp. 689-90).

In order to capture the potential relocation effects associated with rural-to-rural migration, I analyse the average impact of the new road network, not only on those municipalities crossed by a road but also on those located at certain distance. Thus, I regress in a pooled OLS framework the log population of all Spanish municipalities on the shortest linear distance from each municipality to the network, a list of controls, and historical judicial district fixed effects. The model I estimate is:

$$ln(pop_{m,t}) = \alpha_0 + \beta_1 Distance_{m,t} + \beta_2 X_m + \theta_p + \varepsilon_m \tag{4}$$

Where $Distance_{m,t}$ is, for each municipality m, the straight line distance in kilometers from its centroid to the closest road in year t; $X_m$ is a vector of control variables and $\theta_p$ are fixed effects for the 459 historical judicial districts. As in previous analyses, I cluster the standard errors at the historical judicial district level and exclude the 57 targeted cities, in line with the inconsequential-units approach. I compute the distance in two different ways. In a first analysis the distance to the closest road is defined as the distance to the network available at each point of time, i.e. the old network of low quality roads for 1787 and the network combining old and new -paved- roads for 1855. In a second analysis I use the 1855 network (including old and new roads) to define the distance to the network in both periods. I compute distances through GIS techniques considering all roads, even those partially constructed (not connected to the main network). In addition, to account for the fact that the centroid of the geometrical borders of each municipality did not always coincide with the exact economic and social centre of the town, in the case of the municipalities crossed by a road I set the distance at 0, and accept a small bias for all the others.

Table 2.6 presents the results for both the first (Panel A) and second (Panel B) approaches. In both panels, column 1 shows the average effect of distance for the full sample of Spanish municipalities. The coefficient for the distance variable is negative (although non-significant in Panel B), suggesting that local population growth decreased with distance from the network. However, the reduction in the population growth might not have been linear with distance. To test for this, in columns 2-5, I gradually restrict the sample to municipalities within a specific distance from the network (20km, 15km, 10km, 5km). The coefficients reported in those columns indicate that the effect of road infrastructure on long-term population growth tended to fade away with distance. Thus, while in the closer vicinity to the roads (column 5) the impact of each additional kilometre of distance from the roads was sizeable, this effect decreased substantially (eventually losing significance in Panel B) in more distant areas. Given the different definition of distance, the two panels provide us with three pieces of information. First, as above mentioned, both panels describe a geographical feature: population growth decreased with distance to the road network. Second, since the distance variable is time-variant in Panel A, their negative estimated coefficient suggests that municipalities that decreased their distance to the road network, grew more. Third, since the (absolute value of the) estimated coefficient is higher for the final 1855 network, Panel B shows that the growth mainly took place on those municipalities that eventually ended up close to the final road network.

Table 2.6: Road accessibility and population growth: distance to the roads

| | (1)<br>All | (2)<br><20km | (3)<br><15km | (4)<br><10km | (5)<br><5km |
|---|---|---|---|---|---|
| | | | Panel A | | |
| Distance to roads (km) | -0.004*** | -0.006*** | -0.007*** | -0.009*** | -0.010*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.002) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| Fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| R2 | 0.9877 | 0.9875 | 0.9877 | 0.9877 | 0.9889 |
| N | 14432 | 11054 | 9810 | 7848 | 4906 |
| | | | Panel B | | |
| Distance to roads (km) | -0.002 | -0.007*** | -0.012*** | -0.027*** | -0.079*** |
| | (0.001) | (0.002) | (0.003) | (0.004) | (0.008) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| Fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| R2 | 0.9877 | 0.9874 | 0.9877 | 0.9878 | 0.9891 |
| N | 14432 | 11054 | 9810 | 7848 | 4906 |

*Notes:* POLS regressions. The log of population is regressed on the linear distance (km) to the closest road. In columns (1) I present the baseline results including all the Spanish municipalities for which I have population data in both periods. In the other columns, I restrict the sample to municipalities whose centroids are within a specific distance from the 1855 network, including old and new roads. In Panel A, distance to the closest road is defined as distance to the pre-existing network for period one, and distance to the final network for period two. Differently, in Panel B I use the final network to define distance to the network in both periods. Fixed effects correspond to the historical judicial districts. Standard errors are clustered by historical judicial districts and are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

Next, in Table 2.7, I compare the impact of road accessibility on the municipalities crossed by the new paved roads with its effects on the neighbouring towns. In line with the methodology proposed by Büchel and Kyburz (2020), I modify my baseline equation above by replacing the distance indicator with a series of mutually exclusive binary distance bin dummies, defined by how far the centroid of each municipality was from the closest new paved road. As before, distances are calculated through GIS techniques considering all the new paved roads. In the case of the municipalities crossed by a new road I set the distance to 0. This way I ensure that all the municipalities crossed by a road are included in the first distance bin. As in previous regressions, I add the same controls and historical judicial district fixed effects. I cluster the standard errors at the historical judicial districts level and remove the main nodes from the sample. I perform four regressions, with 1, 2, 3 and 4km bins respectively. In each specification, the coefficient of the first dummy bin is positive and statistically different from 0, reflecting the positive effect of road accessibility on local population. Then, in all columns the coefficients for the second distance bins (and in columns 1 and 2 also the third) are negative and significant. These results suggest that the growth of the crossed municipalities came partially at the expense of the neighboring areas, since the construction of paved roads negatively affected the demographic growth of those municipalities located at a small distance from the network. This was arguably the outcome of short distance, largely rural-to-rural migration movements, and would be consistent with the idea of the new road network fostering a demographic relocation process. So, to sum up, rather than being part of a stagnant transport sector, by transforming the economies of the small rural municipalities they crossed and their neighbouring areas the new roads might have brought about a significant spatial reorganization of rural Spain.

Table 2.7: Road accessibility and population growth: distance to the roads

| | (1)<br>bins=1km | (2)<br>bins=2km | (3)<br>bins=3km | (4)<br>bins=4km |
|---|---|---|---|---|
| 1st distance bin | 0.250*** | 0.182*** | 0.124*** | 0.068* |
| | (0.030) | (0.031) | (0.035) | (0.036) |
| 2nd distance bin | -0.452*** | -0.279*** | -0.223*** | -0.139*** |
| | (0.133) | (0.041) | (0.036) | (0.037) |
| 3rd distance bin | -0.305*** | -0.204*** | -0.053 | -0.051 |
| | (0.055) | (0.034) | (0.036) | (0.033) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| Fixed effects | ✓ | ✓ | ✓ | ✓ |
| R2 | 0.51 | 0.51 | 0.50 | 0.50 |
| N | 14432 | 14432 | 14432 | 14432 |

*Notes:* POLS regressions. The log of population is regressed on a series of mutually exclusive distance bins indicating the distance between the muncipality's centroid and the closest new paved road. For the municipalities crossed by a road I replace the distance from the centroid to the road by 0, so that they are all included in the first bin. The bins are defined as follow: Column (6) 0-1km, 1-2km, 2-3km; Column (7) 0-2km, 2-4km, 4-6km; Column (8) 0-3km, 3-6km, 6-9km; Column (9) 0-4km, 4-8km, 8-12km. Fixed effects correspond to the historical judicial districts. Standard errors are clustered by historical judicial districts and are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

## 2.7 Conclusions

This paper analyses the effects of the construction of a new network of paved roads in Spain before the first wave of massive railway construction of 1856-66. Previous research was split between those scholars that stressed the stagnation and inefficiency of the Spanish pre-railway transport system and a number of works that highlighted the progress in market integration that took place in the century before 1855. Here I adopt a different approach. My research is inspired by a large body of recent literature that estimates the impact of new transport infrastructure on local growth using GIS techniques. Following this type of analysis, I generate a new georeferenced database of the paved road network and its evolution over time, and estimate the impact of the increase in market access associated to those new roads on the population growth of a large sample of Spanish municipalities between 1787 and 1857. To deal with potential endogeneity, I take advantage of the fact that the initial network design was largely inspired by administrative reasons, rather than economic ones. In addition, I adopt an inconsequential unit strategy in order to remove further endogeneity concerns. As a robustness check, I also present IV results based on a geographical instrument (least-cost paths between the targeted nodes) and an historical one (Roman roads).

My estimation results show that the increase in market potential associated to the construction of the new paved roads translated into a significantly higher population growth along the 70 years of the period under study. I also observe that the benefits of the new roads were higher in those municipalities whose occupational structure was more diversified and included a higher share of manufacturing and commerce-related activities in 1787. These results indicate that the new infrastructure had a substantial transformative capacity of local economies. Although road transport was indeed much more costly than railway transport, the investment effort carried out in the Spanish road system in the century before 1855 had persistent effects on the demographic dynamism of those locations which enjoyed a higher increase in market access. Those effects are consistent with the available evidence on the growth in transport flows and market integration that took place in the decades before the mid-19th century and do not give support to the idea of a stagnant economy constrained by stationary transport costs before the railways.

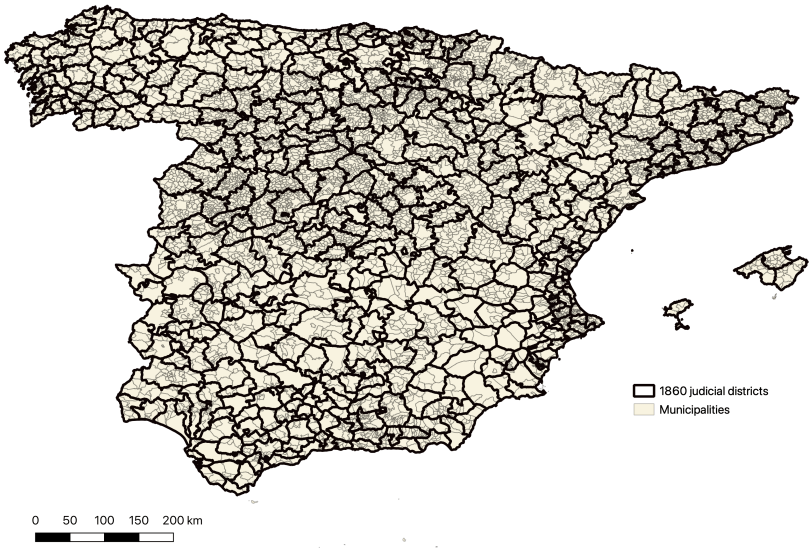
In fact, the changes introduced by the new paved roads in the structure of transport costs were sufficient to provoke a new population equilibrium, in line with the predictions of the Urban Economics literature. Municipalities with higher market access were able to draw population from neighbouring areas, probably attracted by the capacity of the roads to stimulate the development of new activities along the route. Such attraction would be consistent with the results of a regression analysis in which I divide municipalities among different bins, defined by their distance to the roads. While those locations that were crossed by a road enjoyed significant positive effects, the impact of road construction was negative on the adjacent bins. This would indicate that roads triggered a process of spatial reorganization of population, with short-distance movements of population towards the vicinity of the new roads. Given the short distances involved and the low average size of the municipalities in the sample, such movements would have had a predominantly rural-to-rural character, in contrast with the rural-to-urban exodus that would characterize later periods.

In a country like Spain, with very small possibilities to use water transport for domestic trade, the railways represented a revolutionary change with long lasting effects on the integration and spatial reorganization of the economy. However, my results provide evidence on the relevance of earlier progress in the transport sector. Although the potential impact of the new paved roads would never be comparable to the effects of the railway system, their transformative capacity is an additional illustration of the dynamism of the Spanish economy in the century before 1855. Rather than awakening a dormant economy, the railways reinforced a process of change that, despite the difficulties associated to a difficult geography and recurrent political conflict, was well under way in the mid-19th century.

## 2.8 Appendix

### 2.8.1 Additional figures and tables

Figure A2.1: Spanish judicial districts in 1860.



*Source:* Map kindly provided by Francisco Beltrán-Tapia

Figure A2.2: Municipalities crossed by a new paved road but not connected to the main network

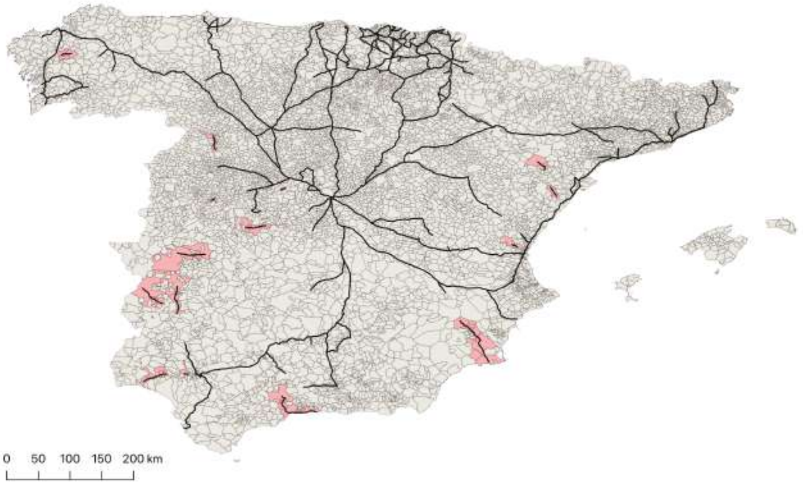


Table A2.1: Market access and local population growth

| | (1)<br>POLS | (2)<br>POLS | (3)<br>POLS | (4)<br>POLS | (5)<br>POLS | (6)<br>Panel OLS |
|---|---|---|---|---|---|---|
| Log Market Access | 0.408$^{***}$ | 0.403$^{***}$ | 0.409$^{***}$ | 0.413$^{***}$ | 0.397$^{***}$ | 0.343$^{***}$ |
| | [0.164] | [0.163] | [0.164] | [0.168] | [0.169] | [0.187] |
| | (0.087) | (0.072) | (0.086) | (0.084) | (0.061) | (0.018) |
| Log distance to coast | | 0.047 | | | 0.166 | |
| | | (0.123) | | | (0.109) | |
| River accessibility (0/1) | | 0.272$^{**}$ | | | 0.210$^{**}$ | |
| | | (0.107) | | | (0.091) | |
| Port (0/1) | | | 1.139$^{***}$ | | 0.998$^{***}$ | |
| | | | (0.200) | | (0.169) | |
| Canal (0/1) | | | 0.296 | | 0.259 | |
| | | | (0.337) | | (0.247) | |
| Railroad's stations 1857 (0/1) | | | 0.823$^{***}$ | | 0.537$^{***}$ | |
| | | | (0.137) | | (0.116) | |
| 5km from Portuguese border | | | 0.156 | | 0.205 | |
| | | | (0.123) | | (0.127) | |
| 5km from French border | | | 0.415$^{*}$ | | 0.265 | |
| | | | (0.216) | | (0.316) | |
| Main Post station (0/1) | | | | 0.462$^{***}$ | 0.453$^{***}$ | |
| | | | | (0.069) | (0.067) | |
| Pop. share in secondary sector | | | | 0.136$^{***}$ | 0.127$^{***}$ | |
| | | | | (0.014) | (0.014) | |
| Geographical controls | | ✓ | | | ✓ | |
| Accessibility controls | | | ✓ | | ✓ | |
| Other controls | | | | ✓ | ✓ | |
| Judicial district FE | ✓ | ✓ | ✓ | ✓ | ✓ | |
| R2 | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 | 0.43 |

*Notes:* 4,328 observations (2,164 municipalities × 2 years (1787-1857)) in each regression. The dependent variable is the log of municipal population. Fixed effects are taken at the historical judicial district level. Standard errors are clustered by historical judicial districts and are in parenthesis, and the standardised beta coefficients are within square brackets. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

## 2.8.2 Methodological note

In this section, I present a detailed description of all the procedures I followed for cleaning and preparing the data, together with the information on the construction of the instrument and the market access indicator.

### Administrative units (municipalities) and population data

One of the biggest challenges while cleaning and preparing the data for the analysis has been the fact that the administrative units (municipalities) that I use changed over time due to mergers and divisions. This would already be a challenging issue for the changes occurred between the 18th and 19th centuries, within the 70-years of analysis. Furthermore, things get even more complicated since a historical map of municipality boundaries for the period under study is not available, and thus I had to rely on the maps of current municipalities[16]. To deal with these issues, I match the historical municipalities to the present ones using different sources. Specifically, after dropping non-peninsular municipalities, I observe 18,151 entries in the 1787 Floridablanca census, 10,146 in the first census carried whose data are reported by the Spanish Statistical Institute (the 1842 *Censo de la Matrícula Catastral*) and the current 7,676 municipal administrative units. For those changes that took place after 1842, the INE website provides a complete catalogue (Ministerio de Administraciones Públicas, 2008) and following those information I can assign the population data to the right municipalities and correct most of the bias. By contrast, the match between some of the municipalities included in the Floridablanca Census and the current ones is more complicated and not always possible due to the lack of information[17]. Besides aggregating the population to the right administrative units, I also needed to make a few changes - i.e. merging two or more municipalities - in the current shapepefile. The reason is that the administrative geographical borders refer to a more recent period than the last population census (2011). I make some changes to take into account those units which have been created after 2011. In particular, Dehesas Viejas, Iznalloz and Domingo Pérez de Granada have been merged in a single administrative unit; the municipality of Balanegra has been incorporated into Berja; Jatar into Arenas del Rey; Montecorto and Serrato into Ronda; Pueblonuevo de Miramontes into Talayuela; and Valderrubio into Pinos Puente.

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[16] The shapefile of the Spanish municipalities is available on the website of the Centro Nacional de Informacion Geografica (CNIG)

[17] I thank Alfonso Díez-Minguela and Julio Martínez-Galarraga for their invaluable help for this matching process.

### Historical roads digitalisation

One of the strengths of my paper is the quality and precision of the digitization of the new paved road network. Applying GIS techniques to information provided in Madrazo (1984) and Dirección General de Obras Públicas (1856), I have digitized the map of the network for four different years along the period under study (see Figure 2.1). The digitization process has been quite complicated since the original maps were hand-drawn and not always precise. Thus, the classical method of geo-referencing scanned maps could not be used. As a consequence, I also employ the shapefile of current Spanish roads as reference. I started with the map of roads in 1855 that is included in Dirección General de Obras Públicas (1856). This map reports the names of more than 500 municipalities crossed by the paved roads, which helped us to correct for the effects of the spatial distortions of the map on the specific location of each route. Thanks to this process I obtained the shapefile of my baseline map for the year 1855. I conclude the digitalisation process by checking the more precise 1861 map (Dirección General de Obras Públicas, 1861) and by correcting for some road segments which are not correctly classified in the 1856 map. I then used information included in a series of less precise hand-drawn maps in Madrazo (1984) to obtain digitized shapefiles of the network in 1778, 1808 and 1840 by removing from my baseline map the road stretches that were still unfinished in those dates.

### Least cost path network

As already mentioned, as robustness check I run an analysis based on an instrumental variable approach. Specifically, I instrument accessibility to the new paved roads with accessibility to an ideal cost-minimizing network within a group of municipalities intentionally connected by the designer of the new network. In order to define the targeted nodes I collect information from the 1720 general regulation of post services, whose main routes largely inspired the priorities in road construction as defined in the 1761 Royal Decree. The targeted nodes are: Corunna, Alcalá de Henares, Alcántara, Alicante, Aranjuez, Arévalo, Badajoz, Barcelona, Baztán, Benavente, Burgos, Cadiz, Carmona, Cartagena, Ciudad Real, Ciudad Rodrigo, Córdoba, Denia, San Sebastián, Écija, El Escorial, El Puerto de Santa María, Fraga, Getafe, Guadalajara, Irún, La Junquera, Lleida, Madrid, Medina del Campo, Medinaceli, Mérida, Molinaseca, Murcia, Ourense, Pamplona, Pontevedra, Salamanca, San Clemente, Santiago de Compostela, Seville, Soria, Talavera de la Reina, Tarancón, Tarragona, Teruel, Toledo, Torrelodones, Tortosa, Trujillo, Tudela, Valencia, Valladolid, Vitoria, Zafra, Zamora and Zaragoza (Source: Grimaldo (1720)). I then generate an hypothetical least cost path network connecting those municipalities by considering the roughness of the terrain. Specifically, I take two of those municipalities - having in mind the design of the network - at the time and I run the Dijkstra's algorithm to get optimal paths between their centroids. Based on the costs derived from the elevation raster of the Spanish geographical institute (IGN), the alghoritm selects pixel by pixel the cost-minimizing route between them. I repeat the process for all the couple of nodes I am interested in according to the design of the network. I then use accessibility measured through this cost-minimizing network as instrument for accessibility computed through the new paved network. Figure 2.6 shows the 1855 paved road network, the least-cost paths and the targeted nodes.

### Market access computation

I want the market access indicator to vary over time to capture the changes in the network caused by the development of new and better quality roads. To do so, I keep in my sample all the municipalities connected by at least one of the two networks employed in the analysis (the new paved network and the pre-existing low quality roads). Some of those municipalities are crossed by both networks, while others are crossed only by one. To deal with this, I impute different travel times to each segment of the network, depending on the specific case.

I compute for each municipality of this sample an indicator of market access as the weighted sum of inverted travel times (as a proxy of transportation costs) to all other municipalities through the road network, using as weights the initial population at destination. Travel times are computed with a least coast path algorithm, assuming different speeds throughout the network. Specifically, I employ the old network to compute the market access for the year 1787. In addition, I add to the existing roads the shortest straight segments connecting to this network the centroids of the municipalities crossed by the 1855 paved network but not previously connected. Based on different historical sources I impute to the roads of the pre-existing network a speed of 2 km/h (cost parameter of 0.5). Differently, for the artificially added segments connecting the municipalities not crossed by the old roads I impute half of the speed (cost parameter of 1). To compute the 1855 market access, I integrate the two networks. The resulting network for the 1855 computations is composed by the pre-existing low quality roads and the new paved roads. I delete the old segments for which the path coincide with the new ones, with the idea that the new roads have been built on the new ones. To consider the fact that the two networks had different quality, I impute different cost parameters. Specifically, I impute a speed of 6.66 km/h (cost parameter of 0.150) based on different historical sources to the newly constructed paved roads. Differently, I keep the speed of 2km/h (cost parameter of 0.500) for the pre-existing network.

The resulting sample of crossed municipalities for the computation of the 1855 market access is of 2,164 (excluding the targeted nodes removed from the empirical analysis). Due to this high number, the computational task to recover all the travel times is really demanding. To deal with this, I employed the software GRASS GIS[18] which has the advantage of calculating those travel distances without building new topologies. Specifically, after importing the shapefiles of roads and the centroids of the connected municipalities (with the command *v.import*), I prepare the network by connecting the two topologies with the command *v.net* (option connect). In this way, I am able to split the different segments at each centroid intersection. After this, I employ the command *v.net.allpairs* to compute for each municipality the travel time value to reach all other municipalities through the network that I then employ in the market access formula. I import those paramethers in STATA and I run a code to compute the market access index for each single municipality based on equation 2.2.

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[18] Geographic Resources Analysis Support System (GRASS) is a Geographic Information System (GIS) technology built for vector and raster geospatial data management, geoprocessing, spatial modelling and visualization. Source: https://grass.osgeo.org/learn/overview/

# 3 Low emission zones and traffic congestion: evidence from Madrid Central

## 3.1 Introduction

Traffic congestion and pollution represent two of the most severe urban costs. The World Health Organisation[1] estimates that 91% of the World's population is exposed to harmful pollution levels and about 4.2 million people die yearly due to ambient air pollution. According to the European Environmental Agency (2020b), the transport sector is responsible for more than 40% of emissions related to air pollution, with negative effects on health (Anderson, 2020; Chay and Greenstone, 2003; Currie and Walker, 2011; Knittel et al., 2016) and climate change. This percentage is likely to be even higher in urban centres. As well as air pollution, cities bear another important cost related to the transport sector: traffic congestion. Beside the direct consequences on air pollution[2], traffic is responsible for accidents and fatalities (Li et al., 2012; Green et al., 2016), delays, stress and road rage, and economic losses (Centre for Economics and Business Research, 2014).

Traffic calming policies have become a popular measure implemented for dealing with these urban costs[3]. The two most common measures differ in terms of the type of negative externality targeted: pollution in the case of low emission zones (LEZs), and traffic in the case of urban congestion tolls. While urban congestion tolls impose a fee on all vehicles that want to access a specific street or area of the

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[1] `https://www.who.int/airpollution/ambient/en/`

[2] 25% of air pollution is caused by traffic according to the Joint Research Center of the European Commission & the World Health Organisation

[3] Other policies have been demonstrated to be ineffective to tackle traffic congestion. Duranton and Turner (2011) provide evidence for the US that increasing road capacity leads to an increase in traffic, due to an induced extra demand. The same result holds for European cities (Garcia-Lopez et al., 2021). Not even road closure seems to solve the problem: Bou Sleiman (2021) identifies a traffic displacement to the outskirts of Paris caused by a pedestrianisation in the city centre. Moreover, imposing fuel taxes has also been shown to be ineffective in reducing traffic (Anas and Lindsey, 2011). Differently, Blaudin de Thé et al. (2021) suggest that urban form, i.e. density, design and diversity, has an impact on car dependency in cities.

city, LEZs are areas to which the access is restricted for the most polluting vehicles. More precisely, a congestion toll is a price measure targeting the intensive margin (number of miles driven), while a low emission zone is a quantity measure tackling the externalities through the extensive margin (type of car driven) (Barahona et al., 2020). Congestion tolls are effective in internalizing the external cost of traffic (Keat Tang, 2021; Herzog, 2020; Carnovale and Gibson, 2015; Börjesson et al., 2012). However the acceptability of this measure is low because drivers, who fail to forecast the traffic relief they will benefit from, perceived it as an individual welfare loss (Lindsey and Verhoef, 2008). Yet tolling is rarely used in practice and low emission zones are more commonly implemented, due to their higher acceptability (Fageda et al., 2020).

This paper aims to exploiting the implementation a low emission zone in Madrid to ascertain whether LEZs have an effect on traffic. LEZs have been extensively adopted in Europe[4] and have been found to be effective in reducing pollution (Wolff, 2014; Ellison et al., 2013; Malina and Scheffler, 2015; Boogaard et al., 2012; Gehrsitz, 2017; Sarmiento et al., 2021)[5]. However, there is a lack of exhaustive evidence about their effect on traffic and car use. It is true that the restrictions forbid access to some drivers, but LEZs has a non-trivial effect on traffic because of people's behavioural responses. In fact, the policy might lead to a fleet renewal (Wolff, 2014; Börjesson et al., 2012; Isaksen and Bjorn, 2021; Percoco, 2014), making the number of affected drivers lower than expected. Furthermore, if the area of implementation is small, it is not time-consuming for people to avoid the restricted area and just drive a bit longer, and thus overall traffic in the city might even increase.

From an economic perspective, the main issue with car use is that it generates external costs which are usually higher than the private cost bore by the driver. In an optimal framework, car users would pay the social marginal cost of use, which would compensate for the negative externalities generated (i.e. pollution and congestion). Russo et al. (2021) show that the marginal external cost of traffic congestion is about two thirds of the private time cost of travel. This substantial effect implies that policies implemented with the aim of abating road congestion and would lead to significant welfare gains. Indeed, Hall (2018) and Hall (2020) show that judiciously designed road pricing could lead to notable Pareto improvements and thus social welfare gains.

*Madrid Central* is a LEZ of about 5 square kilometres that has been in operation in the central district of the Spanish capital since 30th November 2018. Despite its small size (less than 1% of the total area of Madrid) and its primary intention of

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[4] `https://www.urbanaccessregulations.eu/userhome/map`

[5] Related to the LEZ-induced reduction in pollution, LEZs have also been proved to be effective in improving health outcomes (Margaryan, 2021; Pestel and Wozny, 2021).

targeting pollution, this policy represented a step towards more sustainable urban mobility. It was conceived in line with a new idea of urban mobility that removes cars from the street and that promotes public transport use, shared mobility, cycling and walkability (Madrid city council, 2018). Madrid represents an interesting setting for this analysis due to its considerable traffic dynamics. According to the 2018 TomTom traffic report[6], people lost an average of 17 minutes on a 30-minutes trip during rush hours, which is equivalent to 4 days and 16 hours of extra time spent driving in rush hours over the course of the year for each driver. Furthermore, 74.4% of the total local emissions in the city is estimated to be produced by road traffic (Madrid city council, 2017).

By exploiting this policy, I answer the following research questions: 1. Are LEZ schemes effective in reducing traffic within the area of implementation? 2. Do LEZs cause traffic displacement? In other words, did *Madrid Central* cause an increase in traffic levels outside the restricted area? To do so, I make use of different traffic-related geolocated open data collected from around 4,000 magnetic sensors within the city of Madrid. For each measuring point, I observe different variables at a 15-minute intervals, providing more than 280 millions observations for the 25 months of interest (Dec. 2017 - Dec. 2019).

To quantify the causal effect of LEZs on traffic, I develop two alternative empirical strategies. Firstly, I benefit from the exogeneity of the implementation timing to traffic dynamics to develop a pre/post panel fixed-effects analysis. As alternative strategy, I combine the causal impact analysis (Brodersen et al., 2015) with a meta-regression analysis to infer a causal effect exploiting the huge amount of time data available.

Results suggest that the implementation of *Madrid Central* led to an overall small increase in traffic for the whole city of Madrid. Nevertheless, this average result hides important spatial patterns in terms of traffic dynamics. In fact, the implementation did reduce traffic in the restricted area. The time-based model shows an average reduction of around 8.1% in the number of vehicles per sensor/hour and around 8.7% of traffic load in the restricted area. This traffic relief for the central district is offset by an overall increase in transit in the other areas of the city, which I interpret as displacement effect.

Using heterogeneity analyses, I further identify which of the city's streets are most negatively affected by the displacement, as well as showing that that the reduction in the city centre gradually decreases over time and eventually disappears seven months after the implementation. I find different reasons to explain this temporal evolution, ranging from announcements by local politicians to the renewal's

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[6] `https://www.tomtom.com/en_gb/traffic-index/madrid-traffic/`

of the vehicles fleet triggered by the policy, with a shift towards cleaner and exempted cars. Finally, I look at potential changes in commuting and I identify a switch to public transport for commutes directed to the restricted area and rerouting of trips for destinations outside *Madrid Central* as two of the possible mechanisms explaining these results.

Overall, the most important result of the paper is the displacement effect towards unrestricted areas, a relevant and undesired consequence of the policy implementation. My results suggest that spatial spillovers should be considered when designing such schemes, in order to ensure that the whole city benefits from the measure, and not just the restricted area itself.

This paper mainly contributes to two strands of the literature. Firstly, it builds up the literature on urban traffic and more specifically, the sub-strand focusing on the effects of traffic calming policies. Within this sub-strand of the literature, almost all the studies focus on urban congestion tolls and tend to show a reduction in urban traffic. Keat Tang (2021), while estimating the willingness to pay to avoid traffic using the housing market, finds significant improvements in traffic caused by the implementation of the London Congestion Charge (LCC). The same congestion charge is studied by Herzog (2020), who analyses its effects on regional traffic and commuting. Among the many findings, he shows that the LCC reduced traffic on roads leading downtown and had a positive welfare effect for commuters. Börjesson et al. (2012) analyse the Stockholm's congestion charge and suggest that its implementation generated traffic reductions and an increase in the use of exempted vehicles. Finally, Carnovale and Gibson (2015) exploit an unanticipated court injunction of Milan's road pricing scheme and found a substitution of trips towards unpriced times and unpriced roads, as well as a reduction in air pollution.

With respect to LEZs, most of the studies have looked at pollution-related outcomes (Wolff, 2014; Sarmiento et al., 2021; Malina and Scheffler, 2015; Gehrsitz, 2017; Ellison et al., 2013; Boogaard et al., 2012), suggesting a causal improvement on air quality due to the implementation. Regarding traffic, Borger and Proost (2013) is the first theoretical attempt to model LEZs. Two other empirical analysis study at the effect of LEZs on traffic, and these are perhaps the two papers closest to mine. Focusing on a panel of European cities and using city-level traffic observations, Bernardo et al. (2021) finds no effect of LEZs on congestion (while they document a reduction in pollution). My paper extends their results by looking beyond an average effect and by exploiting spatial variations across the city. My findings suggest the importance of focusing at a spatially detailed level of analysis to evaluate the true effect of similar policies. In fact, with areas being made better off and other being made worse off by the policy, there may be important welfare considerations concealed behind a single (null) result. Galdon-Sanchez et al.

(2021) also analyse *Madrid Central* and their main outcome of interest is consumption spending. Nevertheless, in a preliminary section they also look at traffic and pollution. Using a different empirical strategy (i.e. difference-in-differences with unrestricted areas used as controls), they look at relative changes in traffic dynamics between different areas of the cities. Their results point towards a reduction in traffic in the restricted area, but they cannot ascertain how much of the traffic reduction is attributable to an increase in transit elsewhere or to the policy itself. Differently, by looking at each area separately, I am able to detect the direct effect of the policy on traffic for the restricted area and elsewhere.

The second group of papers to which my study contributes focuses on the displacement effect of place-based policies. Neumark and Simpson (2015) suggest that spatial spillovers between areas are a serious threat to identifying the real effect of a specific policy. Studies not accounting for spatial spillovers might overestimate or underestimate the effect of the policy itself. Moreover, not considering these undesired effects may lead to a misjudgment of the policy itself[7]. Few studies focus on the displacement of traffic or pollution driven by traffic calming policies. Bou Sleiman (2021) documents a displacement of traffic and pollution caused by a road closure in the city centre of Paris. Analysing German LEZs, Sarmiento et al. (2021) find displacement of pollution outside the zone's borders. With respect to congestion charges, Carnovale and Gibson (2015) and Keat Tang (2021) find an increase in traffic in unrestricted areas. Percoco (2020) finds the same for pollution when analysing the London congestion charge. To the best of my knowledge, my paper is the first one to look at the possible traffic displacement caused by LEZs.

This paper has different contributions. Firstly, it sheds light on the effect of LEZs on traffic in an urban environment, filling the existing gap in the literature. Secondly, I provide evidence of the existence of traffic displacement as an undesired consequence of LEZs implementation. My results suggest the importance of analysing spatial spillovers to fully evaluate LEZs and how important this consideration is when designing new schemes. Thirdly, as the policy has been an issue of political debate, my results can help the policy makers to intervene where necessary to attenuate the undesired consequences. Finally, I adopt an innovative framework of analysis based on an approach not yet established in the field of economics (causal impact analysis), which represents an useful and straightforward tool that can be further implemented to get a better understanding of traffic-related phenomena.

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[7] A clear example of this is represented by the analysis of Bou Sleiman (2021) about a pedestrianization policy in the centre of Paris. An evaluation of the policy in the restricted area only would lead to a positive evaluation, since residents of the area are exposed to less traffic and pollution. However, as response to the policy, traffic has been displaced to a peripheral area of the city where more people than those who benefited by the closure end up being exposed by the displaced traffic and pollution.

The remainder of the paper is organised as follows. I begin by presenting the institutional framework of the policy. Then, Section 3 presents a description of the data. The empirical framework is explained in Section 4. The results suggesting a traffic reduction in the city centre and a displacement to all the other areas of the city are described in Section 5. In the same section I also present robustness checks and heterogeneity analysis results. In Section 6 I look at the mechanisms explaining the main results. Section 7 concludes.

## 3.2 Institutional framework

In February 2017, the European Commission warned Spain about its continue air pollution breaches, looking at possible monetary sanctions if no actions were taken[8]. In response to the European Commission's request, different reforms have been adopted to reduce air pollution and avoid sanctions. One of the best known is the implementation of the *Madrid Central* LEZ on 30th November 2018. The LEZ covers an area of about 5 square kilometres[9] in the central district of the Spanish capital, restricting access for the most polluting vehicles. The system is active 24 hours a day (all weekdays) and entries are controlled by cameras with plate recognition. A monetary fine of 90 euros, reduced by half if paid in the first 21 days after the sanction, is imposed on drivers accessing the LEZ without authorisation.

The restrictions are based on what are known as ecological labels, the Spanish system for distinguishing between different levels of polluting vehicles. At a general level, access is forbidden to vehicles without an ecological label. B- and C-type vehicles can access the zone only if they park in off-street parking spaces, while ECO-vehicles can enter without sanction if they park in on-street car parks. Finally, no restrictions apply to 0-emission vehicles. Furthermore, certain permits are granted based on the type of vehicle and area of residence. Residents in the area of implementation can always access, and are also entitled of 15 monthly passes for visitors. The rules do not apply either to motorbikes, taxis, emergency and commercial vehicles. I made computations of potentially affected drivers based on the 2017 vehicle fleet in circulation in the city of Madrid[10]. About 70% of the drivers circulating inside or close to the city centre (inside the M-30 ring-road) were affected by the implementation of the LEZ if they were not residents in the area[11].

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[8] `https://ec.europa.eu/commission/presscorner/detail/en/IP_17_238`

[9] It is important to notice that the restricted area represents less than 1% of the around 600 km2 of Madrid's area.

[10] *Estudio del parque circulante de la ciudad de Madrid (2017)*

[11] 70,13% of those vehicles were cars. Of those, 13.41% and 83,39% were without ecological label and with B- or C-type label respectively. Those figures suggest that 9.67% (72,13% x 13,41%)

In addition to the set of rules based on vehicle types, the implementation of the LEZ has not been homogeneous over time. Until 31st December 2018, the cameras controlling access were not active and the rules were enforced by police officers. Between 1st January and 15th March 2019, the City Hall opted for a transitionary period in which, rather than fines, letters were sent to the drivers entering without permission to inform them that, in the following months, they would be fined for entering the restricted area. In the same way, since 15th March 2019, the system has been fully operative. However, after the local elections on 15th June of the same year, a new mayor who was clearly opposed to *Madrid Central* scheme took office. Consequently, on 1st July, the sanctions were suspended, but the suspension only lasted a week as a court reinstated the system of control from 8th July onwards. Despite the fact that the new local government kept its negative view about the program, *Madrid Central* is still in operation, albeit with a few small changes from 1st January 2020, with a couple of streets being opened to traffic. This is why my analysis stops at the end of 2019, as well as the unusual traffic dynamics in 2020 due to the lockdown implemented to tackle the spread of COVID-19. Figure A3.1 in the Appendix shows a timeline of the main events related to the project.

To complete the description of the affected traffic and the policy itself, it is useful to look at how and why people commuted to the restricted area before the implementation. To this end, I look at the 2018 household mobility survey (Encuesta de Movilidad 2018) of the Autonomous Community of Madrid (whole region). About 64% of the trips to the city centre are made by walking. However, if I focus only on trips longer than 2km, about a third of commutes are made by car, only slightly fewer than commutes by public transport (39,5%). Finally, looking at the reason for the trip to the city centre in the case of car commuters, the survey suggests quite a balanced situation between work and leisure. Surprisingly, only 3,8% of the respondents identify shopping as the main reason of the trip, a percentage even lower than for studying. In any case, it is worth stressing that these frequencies only represent trips with city centre as the destination, and do not reflect trips that cross the restricted area without origin or destination there.

## 3.3 Data

To properly measure the impact of the LEZ, I make use of different granular and detailed time-varying datasets. Traffic data comes from the Open Data website of

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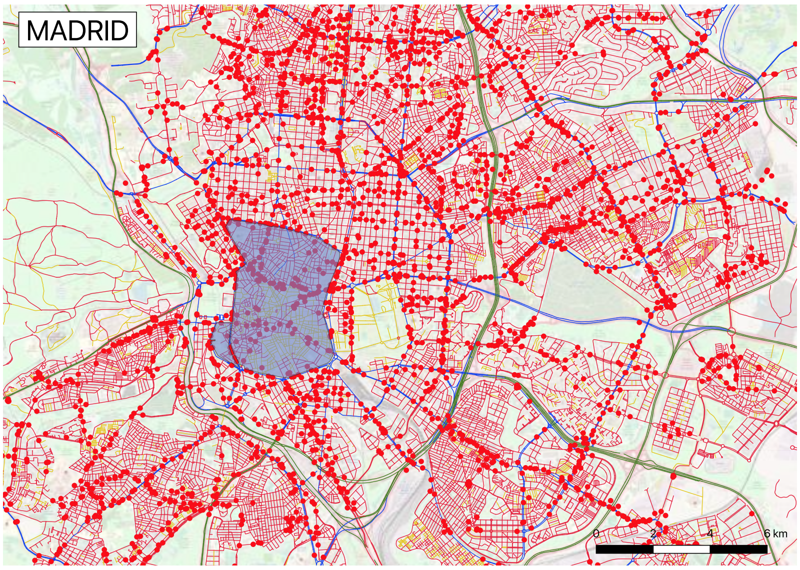
of all the vehicles circulating around the city centre in 2017 were directly affected by the policy, if not residents in the newly restricted area. Furthermore, 60,15% (72,13% x 83,39%) of the total vehicles were forbidden for through traffic (trips crossing the area without origin and destination there) across the area, again if not residents.

the city of Madrid[12]. Specifically, starting from the year 2013, data on traffic are recorded with a frequency of every 15 minutes by around 4,000 magnetic buried sensors across the city. This really high number of detectors is the result of the innovative, integrated system of traffic monitoring (SICTRAM), which provides around 280 million observations over the 25 months of interest (Dec. 2017 - Dec. 2019). Each sensor records route direction, intensity (number of cars passing over the detector every 15 minutes), occupancy (percentage of time in which the sensor is covered within the 15 minutes period) and a parameter of traffic load (degree of use - 0/100 - of the street taking into account intensity, occupancy, road capacity, traffic lights and other relevant characteristics of the street)[13]. Figure 3.1 in the Appendix, shows the location of these measurement points, as well as the restricted area. It also gives an idea of how granular the level of analysis is.

Figure 3.1: Madrid Central area and traffic measurement points.



The huge amount of data (over 280 million observations for each variable of interest) is a great resource, but it also poses a challenge in terms of computation. For this reason, I made some decisions in order to reduce the number of observations without undermining the representativeness of the results. Firstly, I collapse the quarterly of hour frequency to an hourly interval, reducing the size of the data to a quarter[14]. Secondly, I only keep observations between 7am and 10pm. Then, to ensure a more balanced panel dataset, I only keep sensors with at least 2,000

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[12] `https://datos.madrid.es/portal/site/egob`

[13] Only for a subset of measuring points classified as non-urban and placed along the freeway ring road *M-30*, the average speed is registered instead of the load indicator

[14] For occupancy and traffic load the hourly value is the mean of the four sub-periods, while for intensity the hourly value is calculated summing up the 15-minutes data.

time observations and I remove sensors that have only been operative a few months. In addition, I discard all the measurement points along the central street *Gran Via*, as public works reduced accessibility to this street in the months right before the implementation and the structure of the street (i.e. number of lanes and width of sidewalks) actually changed between the pre and post periods. Despite all these decisions, I am able to work with around 43 million hourly observations, over a 25-months period (Dec. 2017 - Dec. 2019) gathered by 3,640 sensors.

Figure 3.2 plots the temporal evolution of traffic load inside the restricted area for the whole period of interest. Traffic data are really noisy and reflects seasonalities: it is normal to observe significant variations between different hours of the day, between weekdays and weekends, and also between different months of the year. Without cleaning for this noise, it would be almost impossible to get any information out of a temporal plot. As it is common in the literature, I plot daily averages rather than raw data to clean the noise from daily hourly patterns, and I compute a moving average of 14 days before and after the specific day[15]. In this way I am able to smooth the data and have a look at the trend. This Figure suggests that traffic load is decreasing after the implementation of the policy (first red vertical line), as well as when the fines started to be sent out (second vertical line). However, this evidence is descriptive and, therefore, I am not able to account for seasonalities (day of the week and month of the year). The two big lower peaks in correspondence of the Summer months are a clear example of this issue. Figure A3.2 in the Appendix presents a similar plot where observations for weekends and holidays are removed[16], to account for it. The evolution is now smoother, and even accounting for this, the graph suggests a reduction in traffic load right after the implementation of the policy on 30th November 2018.

I further collect data from different sources to be used as controls. Firstly, I collect daily weather conditions (average rainfall and temperature) from the OpenData portal of the Spanish National Meteorological Agency (AEMET)[17]. I also gather information on average daily petrol prices in the city of Madrid from the Spanish Ministry for Ecological Transition and Demographic Challenge[18]. Table A3.1 in the Appendix shows descriptive statistics for the dependent variables and non binary controls. Furthermore, I explore open data on the monthly metro station access (Dec. 2017 - Dec. 2019) from the Consortium of Regional Transport of Madrid[19].

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[15] A moving average of 14 days means that the plotted value is an average considering the 14 time periods - days in this case - before and after the selected one.

[16] in the empirical framework I directly control for those and other factor to isolate the effect of seasonalities in traffic patterns

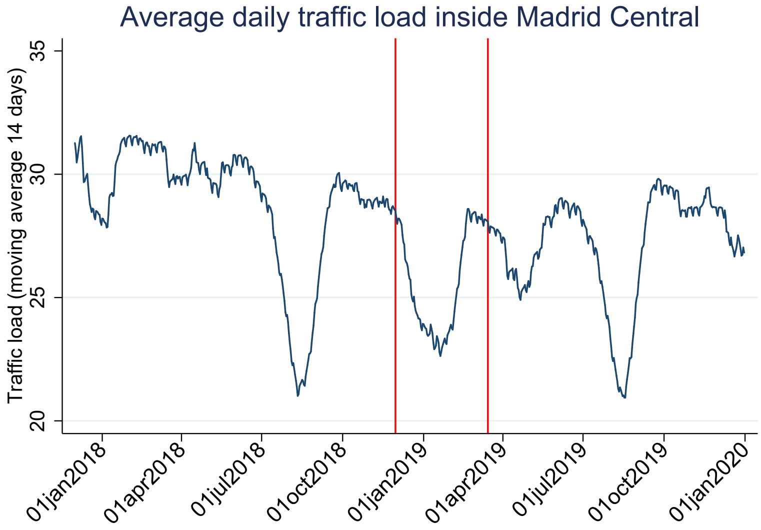
[17] `http://www.aemet.es/es/datos_abiertos/AEMET_OpenData`

[18] `https://sedeaplicaciones.minetur.gob.es/shpcarburantes/`

[19] `https://opendata.esri.es/maps/crtm::datos-abiertos-elementos-de-la-red-de-metro/`

In addition, from the General Traffic Directorate (DGT) of the Spanish Ministry of the Interior, I obtained the monthly municipal new vehicle registrations by type of ecological label from 2017 to 2019, for each municipality in the Autonomous community of Madrid.

Figure 3.2: Temporal evolution of traffic load within *Madrid Central*.



*Note*: The graphs plots 14 days moving averages for daily traffic load inside *Madrid Central*. The first vertical line represents the day of implementation, while the second indicates the day in which fines started to be sent out.

## 3.4 Empirical Framework

Traffic-related data, especially in an urban-integrated context, are quite difficult to deal with, due to high frequencies, seasonality and spatial patterns. In addition, traffic is highly endogenous, since it is really sensitive to people's commuting behaviour. One may think of the many potential commuting options for an individual as a choice between different modes[20] and also route selection[21]. Moreover, a within-city analysis is even more complicated by the fact that an urban road network is highly interconnected, meaning that the implementation of a restricted area in the

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`about`

[20] Percoco (2014) focuses on the behavioural responses of users, which could switch commuting mode to non-banned vehicles such as motorbikes or taxis.

[21] In this sense emblematic is the example of live GPS navigation systems, which constantly update the suggested route based on traffic conditions.

city centre would alter the whole network and not only the specific area. Thus, comparing comparing sensors inside and outside *Madrid Central* in a difference-in-differences analysis would lead to biased estimates, as the controls would also be affected by the policy (i.e. contaminated control group). In fact, if drivers take detours further away due to the implementation of the low emission zone, then also the sensors outside the area of implementation are actually treated[22]. This strategy would only enable the quantification of the relative changes between areas (Neumark and Simpson, 2015). It would be impossible to ascertain whether a possible reduction of traffic in the restricted area would be attributable to an increase in transit elsewhere or to the policy itself.

To estimate the causal effect of the *Madrid Central* LEZ on traffic, I combine two different empirical strategies. I describe them in the following sections.

### 3.4.1 Time-based panel fixed effect model

Firstly, I perform a pre/post implementation analysis using a time-based panel fixed effect model. The pre/post analysis has the advantage of presenting clear and straightforward results. At the same time, it may be weak in terms of providing causal evidence with respect to whether the underlying hypothesis is violated. This identification strategy is based on the idea that the timing of the implementation of the low emission zone, conditional on a rich set of time and seasonal fixed effects, temporal controls and flexible time trends, is exogenous with respect to traffic dynamics. Based on different facts, I argue that the timing of the implementation of the policy is exogenous to traffic dynamics. Firstly, the policy was implemented on push of the European Commission and LEZs aim at targeting pollution rather than traffic. In addition, I show evidence on the Google searches[23] suggesting that, only in the two days preceding the implementation, there was an increase in searches. In any case, even though people were aware of the implementation, the only element potentially affecting the time-based strategy is whether or not they adapted their commuting behaviours before 30th November 2018. It is hard to imagine that people stopped crossing the city centre or change commuting mode before the actual implementation of the policy. Another important assumption that needs to hold in order to interpret the results as causal, is that the policy of interest is the only main change between the two periods. In other words, I need to isolate the effect of other potential confounding factors that possibly occurred during the same period (e.g.

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[22] Carnovale and Gibson (2015); Kreindler (2020) show that drivers switched to un-tolled roads or un-tolled hours as a consequence of traffic calming policies' implementations.

[23] Figure A3.3 in the Appendix plots the Google searches in the area of Madrid for the string *"Madrid Central"*

variation in the number of parking spaces or changes in public transport frequencies). I cannot account for them directly though the inclusion of day, week or month fixed effects, since they would be nested in the treatment variable (only defined as post). To account for this, I control for as many observables as possible, I include year fixed effects, as well as flexible area-specific time trends to capture changes in unobservables that are not already partialled out by controls and fixed effects. In addition, as robustness check I run a regression in which I include area-specific year fixed effect, to capture for location specific shocks occurring over time[24]. Similar approaches have been used to evaluate the effect of different traffic calming policies on pollution and traffic (Davis, 2008; Carnovale and Gibson, 2015; Percoco, 2014, 2020).

I start by quantifying the global effect on traffic for the whole city by estimating the following panel fixed-effects model:

$$log(Y_{m,t}) = \alpha_0 + \beta_1 Implementation_t + \beta_2 X_t + \beta_3 timetrend_t + \theta_m + \tau_t + \varepsilon_{p,t} \quad (3.1)$$

where: $Y_{m,t}$ is the dependent variable which in turn is intensity, occupancy and traffic load for sensor $m$ and period $t$; $t$ is an indicator for year-month-day-hour; $Implementation_t$ is a dummy variable which assumes value 1 for periods after the day of implementation; $X_t$ contains time-varying controls such as holidays, announcement (binary indicator assuming value 1 between the day in which the implementation is officially announced - 23rd October 2018 - and the last day before the implementation - 29th November 2018), suspension (binary variable assuming value 1 for time units within the period 1st-7th July 2019), peak hours[25], daily rainfall, average daily temperature, daily petrol prices; $timetrend_t$ is a linear daily time trend to capture changes in unobservables that are not partialled out by the fixed effects. $\theta_m$ represents the sensors fixed-effects and $\tau_t$ contains time (year) and seasonal fixed-effects (week of the year, day of the week, and hour of the day). The standard errors are clustered at the commuting area-day level[26].

In this way, I can estimate the effect for the average sensor in the city, without distinguishing between restricted and unrestricted areas, and this would provide an indication on how overall traffic in the city is affected by the policy.

Considering how the empirical framework is constructed, the local average treatment effect is the result of combining together many different estimated coefficients.

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[24] Results hold.

[25] I define peak hours 7-10h and 17-20h from Monday to Friday, based on pre-implementation traffic dynamics for the whole city of Madrid.

[26] There are 141 commuting areas in Madrid, which are similar to the neighbourhoods (131 in Madrid) and have been defined by the Spanish Institute of Statistics, with the idea of being specifically suitable to study daily commuting.

Specifically, the seasonal fixed effects allow me to compare the same week of the year, day of the week, and hour of the day variations pre and post implementation. Taking the average of all estimated effects enables me to attenuate the effect of anomalous days if the framework cannot capture specific-occurring events.

The global effect is an average effect which might hide different spatial patterns across different areas of the city. To check for this and properly answer my research questions, I estimate a differ model in which I distinguish the sensors in different groups based on their location. More specifically, I estimate the following panel fixed-effects model:

$$\begin{aligned} log(Y_{m,t}) = &\alpha_0 + \beta_1(Implementation_t * Area_m) + \beta_2 X_t + \\ &+ \beta_3 timetrend_t * Area_m + \theta_m + \tau_t + \varepsilon_{p,t} \end{aligned} \tag{3.2}$$

where: $Area_m$ is a categorical variable classifying the around 4,000 traffic measuring points in four different groups based on their location (sensors within the restricted area; those between its border and the *M-30* ring-road freeway; non-urban sensors on the *M-30* freeway and measurement points outside the ring-road). Figure 3.3 distinguishes between the sensors in the four groups[27]. By interacting this variable with the $Implementation_t$ dummy, I am able to directly estimate the effect of the policy implementation in each specific area. In addition, by interacting $timetrend_t$ and $Area_m$, I allow for area-specific flexible time trends. As before, the standard errors are clustered at the commuting area-day leves, but results hold also when I cluster at bigger or smaller spatial units.

Overall, this approach represents a straightforward way to check whether or not the implementation of the restricted area had an impact on traffic in the city centre and its surrounding areas.

### 3.4.2 Causal impact analysis and meta-regression

An alternative approach for testing for the causal effect in a context of high frequency and spatially interconnected traffic data is the causal impact analysis. This strategy was first developed by Brodersen et al. (2015). The main idea is to construct a counterfactual for a selected treated unit (a specific sensor in the restricted area) in two steps, based on pre-treatment evolution of the data. Firstly, from among the list of all potential controls (sensors outside *Madrid Central*), the algorithm finds a pool of sensors which showed a similar time-series evolution to the selected sen-

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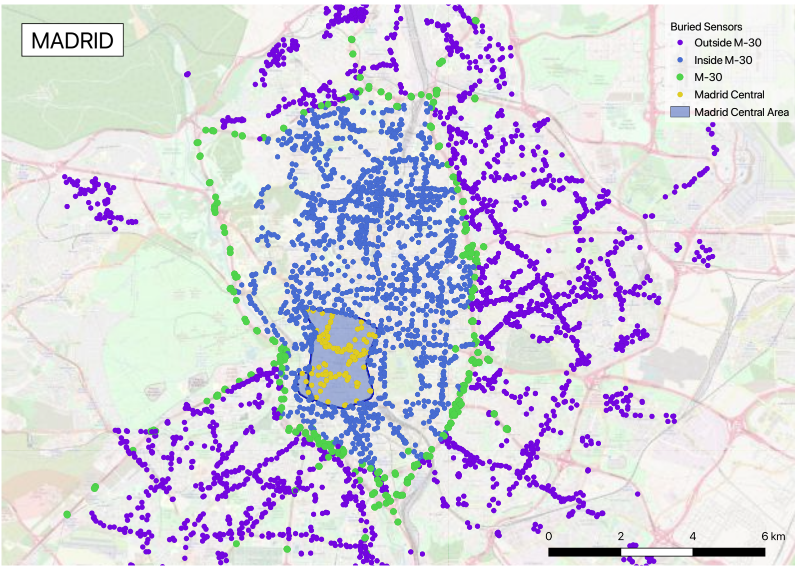
[27] In a robustness check, I also replicate the analysis with dummy bins of distances from the border of the restricted area.

Figure 3.3: Visualisation of the four groups of the variable *Area*.



*Note*: The four groups are: inside Madrid Central, between Madrid Central and the M30 freeway ring road, on the M30 freeway, and outside M30.

sor in the pre-treatment period[28]. Then, based on a Bayesian structural time-series estimation, the selected controls are used to construct a post-treatment counterfactual time-series for the matched control group itself. This synthetic counterfactual describes how traffic dynamics around the selected controls would have evolved in the absence of the policy intervention. In this way, the control group will not be contaminated by spatial spillovers induced by the policy, and thus would be a good control group. Therefore, the impact of the treatment is obtained by taking the difference between the observed real time-series for the selected sensor inside the restricted area and the simulated synthetic counterfactual generated for the matched controls.

The algorithm allows me to estimate the effect for daily frequencies, so I run it keeping observations recorded between 12pm and 1pm only, as this was the hour with the heaviest traffic in the restricted area before implementation. Estimations based on different selected hours provide consistent results. I set to three the number of matched controls to be used to construct the post-implementation counterfactual time series. I implement the algorithm for both intensity and traffic load.

For each selected sensor, the method gives me the absolute and relative effects and their relative confidence intervals, as well as certain statistical tests. The indi-

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[28] The user can decide the number of matched controls to be included to construct the post-treatment counterfactual.

vidual results are not representative of the overall effect I am quantifying[29]; however, they might already suggest some features. Thus, following an approach previously adopted by Schmitt et al. (2018), I perform a meta-regression analysis to obtain an aggregate estimation of the effects obtained for each sensor alone. Specifically, to account for the fact that my inputs come from different estimates, I weight each estimation by the inverse of its previously estimated variance. By doing so, I give more importance to the better predicted counterfactual post-intervention time series.

With this method, I can obtain an average estimation of the causal effect of LEZ implementation on traffic in the restricted area.

## 3.5 Results

### 3.5.1 Time-based panel fixed effect model

Before presenting the estimations of equation 3.2, which specifically looks at the effect on traffic inside the restricted area, it is interesting to analyse the global effect of the LEZ implementation for the overall transit in the whole city. I present the results for the model of equation 3.1, in which the implementation variable is not interacted with the spatial indicator and, thus, I do not distinguish between restricted and unrestricted areas. The results are presented in Table 3.1. I focus on logarithmic transformations only, in order to have more normally distributed dependent variables and to deal with their different scales of measurement. However, the results obtained by employing dependent variables expressed in absolute values are consistent with those presented here. The coefficient of interest, identifying the effect of the implementation of the LEZ on traffic for the average sensor in Madrid, is positive and statically different from 0 in all columns. In this respect, in Column 1, I observe that the LEZ induced an average increase of about 4.0% of vehicles per hour per sensor. At the same time, also the elasticities estimated for occupancy and traffic load also show an average hourly increase of 3.4% and 3.6% respectively. Overall, these results suggest an increase in global traffic for the whole city of Madrid after the implementation of the LEZ. In fact, all parameters show a small but significant increase during the implementation period compared to the pre-implementation values. These initial results highlight why it is important to investigate the effect of low emission zones on traffic. LEZs are intended to deal with pollution, so the effect on overall traffic is difficult to be predicted *a priori*

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[29] The estimated sensor-specific effect would be indicative of the effect if and only if the selected measuring point is representative for all the others within the area of implementation.

due to people's behavioural responses. As a matter of fact, different reasons may explain those results. Firstly, if drivers take detours to avoid crossing the restricted areas, they may face longer commutes and thus overall traffic would increase. Secondly, the implementation might lead to a renewal of the vehicles' fleet and so, once again, may induce people to drive more (while polluting less).

Table 3.1: Overall effect of *Madrid Central* on traffic in Madrid

| | (1)<br>log intensity | (2)<br>log occupancy | (3)<br>log load |
|---|---|---|---|
| Implementation | 0.040$^{***}$ | 0.034$^{***}$ | 0.036$^{***}$ |
| | (0.005) | (0.006) | (0.005) |
| Suspension | -0.003 | 0.030$^{***}$ | 0.010 |
| | (0.006) | (0.008) | (0.006) |
| Announcement | -0.041$^{***}$ | 0.004 | -0.035$^{***}$ |
| | (0.003) | (0.004) | (0.003) |
| Peak hours | 0.476$^{***}$ | 0.542$^{***}$ | 0.499$^{***}$ |
| | (0.001) | (0.001) | (0.001) |
| Holiday | -0.508$^{***}$ | -0.674$^{***}$ | -0.544$^{***}$ |
| | (0.005) | (0.006) | (0.005) |
| Precipitation | -0.000 | 0.001$^{***}$ | 0.000$^{***}$ |
| | (0.000) | (0.000) | (0.000) |
| Average temperature | 0.002$^{***}$ | 0.002$^{***}$ | 0.002$^{***}$ |
| | (0.000) | (0.000) | (0.000) |
| Daily gasoline price | 0.042$^{*}$ | 0.235$^{***}$ | -0.004 |
| | (0.024) | (0.030) | (0.026) |
| $N \times T$ | 43,145,211 | 42,036,903 | 38,567,144 |
| R2 | 0.90 | 0.71 | 0.78 |
| Mean Y | 5.74 | 1.63 | 2.97 |
| Sensor FE | ✓ | ✓ | ✓ |
| Time FE | ✓ | ✓ | ✓ |

*Notes:* Hourly panel fixed effect model. A parameter of traffic for sensor $m$ and year-month-day-hour $t$ is regressed on a binary time-series indicator for policy implementation, a list of controls, a daily linear time trend and time, seasonal and location fixed effects. The dependent variables are in turn log intensity (Column 1), log occupancy (Column 2) and log traffic load (Column 3). Standard errors are clustered at the commuting area-day level and are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

After seeing these results, the importance of focusing on a granular urban setting is even more justified. In fact, it is highly likely that commuters decide to take detours to avoid the restricted area, and thus the effect of the policy may be different in different areas of the city (i.e. reduction of traffic within the restricted area and generation of an undesired displacement of traffic to other areas of the city). With this in mind, in Table 3.2, I present the results of equation 3.2 which looks for spatial variation across the city. By interacting the variable *Area* with the implementation dummy, I can analyse the impact of the policy in different areas of the city. Since the traffic load parameter is not available for non-urban *M-30* sensors, I add the average speed as fourth dependent variable. All elasticities are negative and statistically significant for the interaction with the variable indicating sensors within *Madrid Central*, indicating a reduction in traffic in the area with limited access. In contrast, all the other coefficients for the remaining zones show an increase in traffic[30]. Thus, even though the policy induced an average decrease of 8.1% hourly vehicles per sensor within *Madrid Central*, the results also highlight average increases of 2.2%, 1.3% and 6.4% for circulating vehicles over the other groups of sensors (Column 1). Referring to the absolute number of average vehicles in the period previous the implementation in the restricted area, the 8.1% reduction translates into 29.49 less vehicles passing over the average sensor within Madrid Central. Likewise, for the other areas the percentages suggest +12.14, +23.25, and + 23.32 more vehicles passing over the average sensor in the relative area. Nevertheless, the absolute values for the non restricted areas might be misleading, since those borders are artificially designed in this study. Drivers might drive back and forth between the borders of those areas, since those borders do not define different driving restrictions. Consequently, summing together the three values would not be really informative and it would likely overestimate the average displacement of vehicles, due to double counting in different areas. To get an indicative idea of the average increase in the number of vehicles per sensor in the whole non restricted area, I run a similar model in which I distinguish only between sensors inside or outside *Madrid Central.* The estimated elasticity for the non restricted area is +4.2%, which translates into an increase of 25 vehicles per hour at the average sensor.

Similar patterns of results are also obtained for occupancy and traffic load. With respect to traffic load, for example, the implementation of the LEZ caused an average reduction of 8.7% for the sensors inside the restricted area and an increase of 2.0% and 5.4%, respectively inside and outside the M-30 freeway[31]. I interpret these results for the unrestricted areas as spatial spillovers induced by the policy. Consequently, those results provide evidence on why comparing different areas of the city in a difference-in-differences set up would be misleading in terms of identifying the real effect, due to the contaminated control group problem mentioned above.

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[30] The negative coefficient for average speed goes actually in this direction: a lower speed corresponds to a less fluid traffic situation.

[31] Together with a 0.7% decrease in the average speed on the M-30 freeway.

These results confirm existing evidence. Looking at German LEZs, Sarmiento et al. (2021) find an increase of two pollutants (03 and CO) outside the zone's borders, suggesting as explanation a rerouting of traffic flows around the border of the restricted areas. To the best of my knowledge, this is the only other study that looks directly at LEZs. However, other works focusing on congestion tolls highlight spatial spillovers of pollution and traffic. Carnovale and Gibson (2015) analyse a congestion toll implemented in Milan (*Area C*) and find an increase in traffic in unpriced roads, suggesting that some drivers respond to the policy by driving around the area. Keat Tang (2021) and Percoco (2020) examine the London congestion charge and find evidence of a displacement effect for traffic and pollution respectively.

To avoid any concern regarding the fact that the results do not depend on the classification of the different areas, I perform a robustness check based on distances from the border of the specific area. Specifically, I estimate a model in which I interact the implementation dummy with 2 km distance dummy bins from the border of Madrid Central. In this way, I can ensure that my results do not depend on the definition of the groups of sensors. The results hold, as it can be seen in Table A3.2 in the Appendix.

As second robustness check, I perform a placebo analysis in which I artificially set the implementation date to 1, 2, and 3 years before the real date. To this end, I extend the time span of the analysis as much possible adding data starting from December 2014. I use daily sums for intensity to facilitate the computational process. Table A3.3 in the Appendix shows that the reduction in the number of circulating vehicles within the restricted area is actually driven by the policy. In fact, none of the coefficients for the falsification tests are different from zero.

Taken together, my results suggest that, overall, the implementation of the LEZ generated a small average increase in traffic for the whole city. However, this average net result hides different patterns across the city: *Madrid Central* did reduce traffic in the central district, at the expenses of all the other areas of the city, for which I observe a displacement of traffic induced by the policy. It is worth noting that Madrid's LEZ covers less than 1% of the city's total area. Therefore, it is not very time-consuming for people to reroute their trips to unrestricted paths. This might explain the high displacement of traffic to the unrestricted areas.

Table 3.2: Effect of *Madrid Central* on traffic by areas of the city

| | (1)<br>log intensity | (2)<br>log occupancy | (3)<br>log load | (4)<br>log speed |
|---|---|---|---|---|
| Impl. × MC | -0.081$^{***}$ | -0.177$^{***}$ | -0.087$^{***}$ | |
| | (0.012) | (0.016) | (0.013) | |
| Impl. × InsideM30 | 0.022$^{***}$ | 0.026$^{***}$ | 0.020$^{***}$ | |
| | (0.006) | (0.007) | (0.006) | |
| Impl. × M30 | 0.013$^{**}$ | 0.019$^{***}$ | | -0.007$^{***}$ |
| | (0.005) | (0.007) | | (0.003) |
| Impl. × OutsideM30 | 0.064$^{***}$ | 0.052$^{***}$ | 0.054$^{***}$ | |
| | (0.005) | (0.006) | (0.006) | |
| Suspension | -0.003 | 0.030$^{***}$ | 0.010 | 0.010$^{**}$ |
| | (0.006) | (0.008) | (0.006) | (0.004) |
| Peak hours | 0.475$^{***}$ | 0.542$^{***}$ | 0.499$^{***}$ | -0.082$^{***}$ |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| Announcement | -0.041$^{***}$ | 0.004 | -0.035$^{***}$ | 0.004$^{*}$ |
| | (0.003) | (0.004) | (0.003) | (0.002) |
| Holiday | -0.508$^{***}$ | -0.674$^{***}$ | -0.544$^{***}$ | 0.075$^{***}$ |
| | (0.005) | (0.006) | (0.005) | (0.002) |
| Precipitation | -0.000 | 0.001$^{***}$ | 0.000$^{***}$ | -0.002$^{***}$ |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Average temperature | 0.002$^{***}$ | 0.002$^{***}$ | 0.002$^{***}$ | 0.001$^{***}$ |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| Daily gasoline price | 0.043$^{*}$ | 0.234$^{***}$ | -0.003 | 0.058$^{***}$ |
| | (0.023) | (0.030) | (0.026) | (0.014) |
| N $\times T$ | 43,145,211 | 42,036,903 | 38,567,144 | 4,580,168 |
| R2 | 0.90 | 0.71 | 0.78 | 0.59 |
| Sensor FE | ✓ | ✓ | ✓ | ✓ |
| Time FE | ✓ | ✓ | ✓ | ✓ |
| Seasonal FE | ✓ | ✓ | ✓ | ✓ |

*Notes:* Hourly panel fixed effect model. A parameter of traffic for sensor $m$ and year-month-day-hour $t$ is regressed on a binary time-series indicator for policy implementation interacted with a categorical variable indicating the location of the sensor, a list of controls, area-specific daily linear time trends and time, seasonal and location fixed effects. The dependent variables are in turn log intensity (Column 1), log occupancy (Column 2), log traffic load (Column 3), and log average speed (Column 4). Standard errors are clustered at the commuting area-day level and are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

### 3.5.2 Causal impact analysis and meta-regression

An additional empirical strategy involves estimating a causal effect for each buried sensors within the restricted area with the causal impact algorithm[32] and aggregating them together through a meta-regression analysis[33]. The forest plot (Figure 3.4) presents the individual and global results for intensity. Figure A3.4 in the Appendix does the same for traffic load. Looking at the sensor-specific results, most of the estimations are negative and statistically significant, as expected, suggesting a reduction in traffic for most of the measuring points within the restricted area. The aggregated results indicate a significant reduction for both variables: about -15.7 hourly vehicles per sensor (corresponding to a relative effect of -6.5%) and -1.58 for traffic load (-5.3%). To further understand the results and to find possible spatial patterns, in Figures A3.5 and A3.6, I map the sensor-specific estimated causal impacts for intensity. Figure A3.5 shows a spatial distinction between positive, null and negative effects. Consistently with the forest plot, it shows a prevalence of negative impacts, suggesting a reduction in the number of vehicles driving over the specific sensor caused by the implementation of the LEZ.

Along the same line, Figure A3.6 distinguishes the same effects by magnitude. It is worth noting that, the positive impacts, implying an increase in traffic compared to the pre-implementation situation, are not only fewer in number, but they are also lower in absolute values compared to the negative ones[34]. Based on these figures, I cannot identify a specific spatial pattern for the previously partially restricted "residential priority areas" (APR) or other areas of the city centre. Nevertheless, I am able to identify the pivotal areas of the city centre, which despite the new restrictions did not register a reduction in transit. An increase in transit for some sensors within the restricted area might be driven by a latent demand effect: since there are less cars in the city centre, some unrestricted vehicles (e.g. taxi or motorbikes) might drive there more or longer as it is now faster than before.

Overall, this second strategy suggests a causal reduction in traffic within the restricted area due to the LEZ implementation. In addition, the estimated effects confirm the previous results, with relative reductions really close to the previous findings (-8.1% for intensity in the first strategy and -6.5% here). The small difference in magnitude might be driven by the different estimations procedures used, as well as by the fact that, in this case, I keep only one hourly observation per day

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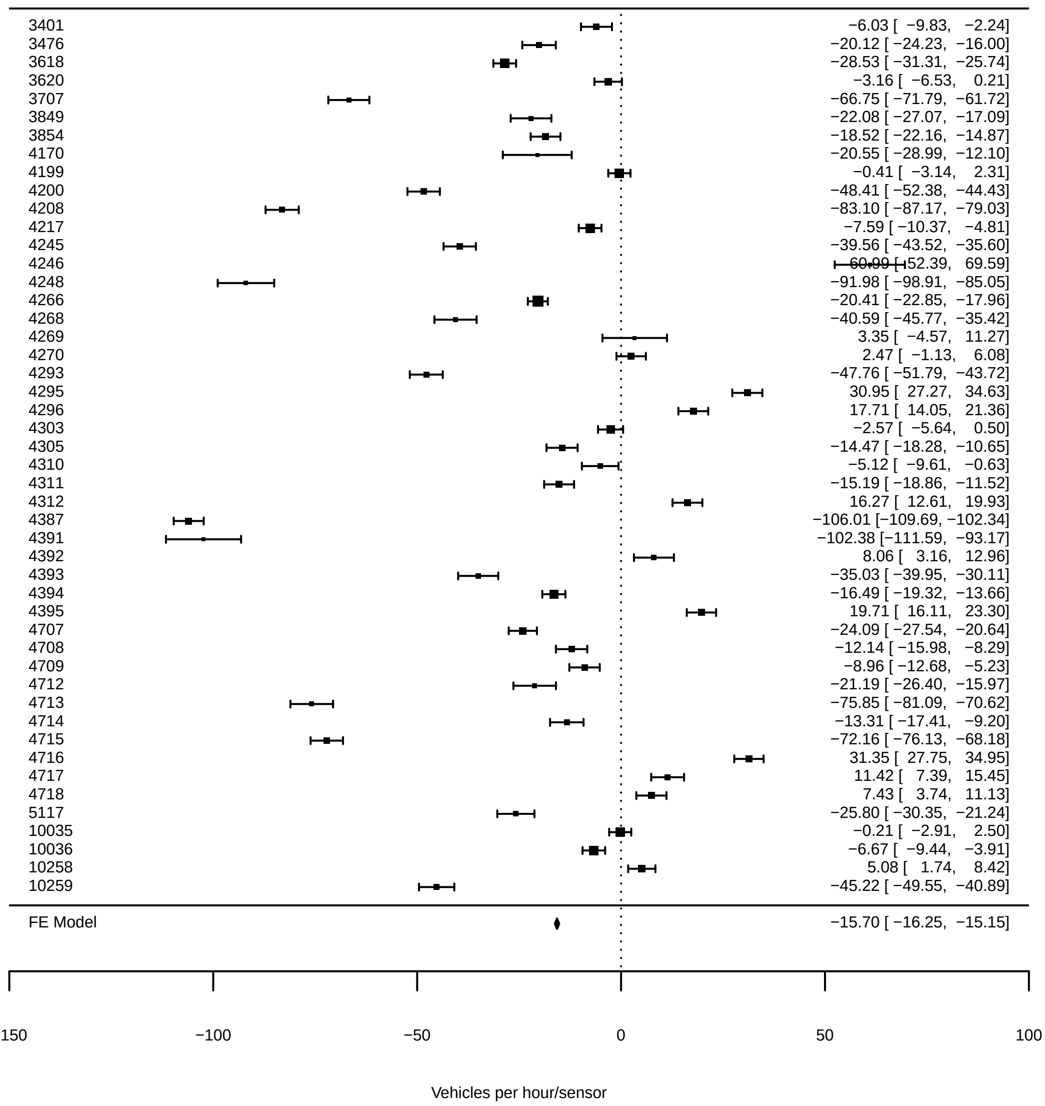
[32] Specifically I use the *MarketMatching* package in R.

[33] I perform the meta-regression with the *metafor* package in R

[34] With respect to the dark point located on the East side of the restricted area, at the *Calle Alcalá* entrance, it is interesting to see how the increase in vehicles driving over this sensor correspond to one of the cameras recording most fines for forbidden access to the restricted area (more than 87,000 fines from this specific access point between mid-March and December 2019).

instead of all those between 7am and 10pm.

Figure 3.4: Results of meta-regression for causal impact estimations of intensity



*Note*: The column on the left includes the id number of the sensors, while on the right the estimated impacts and their relatives confidence intervals are reported. The last line present the result of the meta-regression aggregating together all the sensor-specific impacts.

### 3.5.3 Heterogeneous analysis

As mentioned earlier, the results presented in Table 3.1 are an average effect hiding different spatial patterns. In fact, the results of Table 3.2 suggest a reduction in transit within the restricted area and a displacement to all other areas of the city. However, the results by area are also still averages of all the sensors included in the specific group and might hide further different spatial patterns. In this section, I expand these results by means of an heterogeneity analysis. Specifically, I develop a street-level analysis, running a sensor-specific time-series regression from which I obtain one effect for each measuring point comparing the pre and post traffic situation. More formally, I replicate the city-wide model developed in the first strategy for each magnetic sensor. The only difference from Equation 3.1 is the omission of the sensor fixed effects, which cannot be included in a time-series regression[35]. The resulting evidence gives me the opportunity to further exploit the previously obtained average results and gain a better understanding of the displacement effect.

More formally, for each measuring point I estimate the following time-series model:

$$log(Y_t) = \alpha_0 + \beta_1 Implementation_t + \beta_2 X_t + \beta_3 timetrend_t + \tau_t + \varepsilon_t \tag{3.3}$$

where $t$ is an indicator for year-month-day-hour, and controls, daily time trend and time and seasonal fixed effects are defined as before. Thus, the vector $\beta_1$ of estimated coefficients summarises the change in traffic conditions for each specific sensor between the two periods.

Figures 3.5 in the text and A3.7 in the Appendix present the results for intensity and traffic load respectively. In both maps, for each sensor, I represent the estimated value for the variable implementation, obtained from the time-series sensor-specific regressions. I distinguish between negative, null and positive effects, representing respectively reductions, no differences and increases in traffic for the specific measuring point with respect to the pre-implementation period. Table 3.2 highlights an average increase in the number of circulating vehicles of 6.4% for the area outside the *M-30* ring-road freeway. The main contribution of this heterogeneity analysis is that it goes further than average results and look at traffic patterns at a really granular level. In particular, with respect to both intensity and traffic load, the maps show how most of this displacement is mainly concentrated in the South-Western area of the city, while the other parts of this specific area are not very negatively affected by the policy. There might be different reasons explaining this result, however the concentration of population is for sure an important factor. Out of the 21 districts

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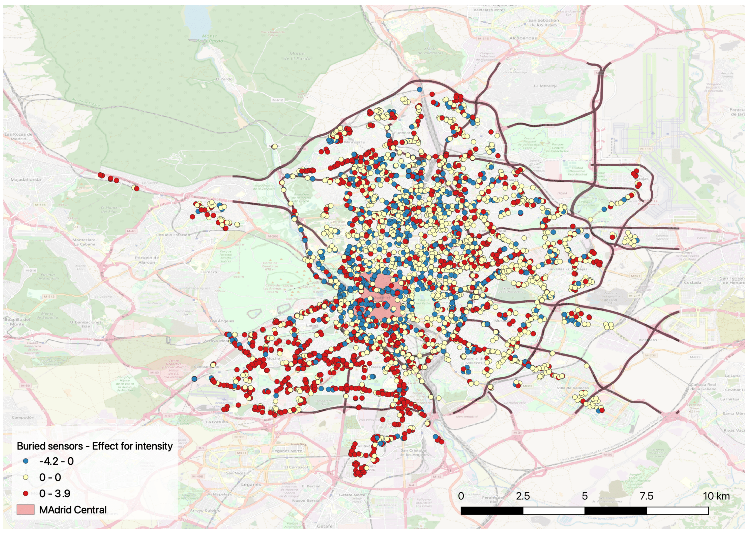
[35] I also only cluster the standard errors only over time, without considering the spatial dimension.

of the city, the four ones in which I find the higher traffic increase (*Carabanchel*, *Latina*, *Usera*, and *Villaverde*) account together for almost one quarter of the total population of the city. Furthermore, the neighbouring municipalities in the South-Western area (*Alcorcon*, *Fuenlabrada*, *Getafe*, *Leganes*, and *Mostoles*) are the most populated ones within the entire region of Madrid (excluding Madrid), summing up almost 1 million people. This would explain why those areas are experiencing the highest concentration of increases in traffic compared to the pre implementation period.

Figure 3.5: Sensor-specific time-series regression results for log intensity.



*Note*: For each sensor, the map represents the estimated coefficients in the time-series regressions for the variable *Implementation*. Blue dots represent negative and statistically significant coefficients (i.e. reductions in traffic for the specific sensor with respect to the pre-implementation period); yellow dots represents non-statistically significant coefficients (i.e. no differences in traffic); and red dots stands for positive and statistically significant coefficients (i.e. increases in traffic).

The average results might also hide temporal heterogeneity. It may be the case that the reduction in traffic within the restricted area was more or less intense in different months. This could be explained by the transitionally implementation phase, the initial exemption from fines, the week of suspension, or also the attitude of the policy-maker with regard to the policy itself. To further develop the analysis in this respect, I estimate the following panel model for the sensors in the restricted area only:

$$\begin{aligned} log(Y_{m,t}) =& \alpha_0 + \beta_1(Implementation_t * Month_t) + \beta_2 X_t + \\ & \beta_3 timetrend_t + \theta_m + \tau_t + \varepsilon_{p,t} \end{aligned} \tag{3.4}$$

where controls are the same as in the previous models, and fixed effects include year, week of the year, day of the week, hour of the day and specific sensor. The standard errors are clustered at the commuting area-month level[36]. The interacted term $Implementation_t * Month_t$ provides the monthly effect of the policy. I restrict the sample to sensors inside the restricted area, thus I analyse at the temporal evolution of the average reduction in transit shown in the first line of Table 3.2[37]. Figure 3.6 plots the estimated coefficients (and their relative 1% confidence intervals) for the intensity variable. The months are ordered after the implementation date (30th November 2018), so that December 2018 is the first month of implementation, January 2019 the second and so on. The plot shows a 6% non statistically significant reduction for December 2018. After this, the coefficients are negative and high in magnitude (-23% in January 2019) and slowly decrease month by month until July, when the coefficients become non statistically different from zero.

Different reasons might explain these results. Firstly, in the first month (until January 1st 2019) no cameras were installed and fines were issued by police officers randomly stopping vehicles in the restricted area. Another factor that may explain the non-immediate reduction in transit is the possibility that drivers where not fully informed about the policy (they did not know about its implementation or did not know whether or not their vehicles were actually affected). Even if the effect would be statistically significant, these two reasons would still explain why the estimated coefficient for December is much smaller in magnitude than in the following months.

January 2019 registers the highest reduction in the number of vehicles driving over the average sensor in the restricted area (-23%). This sharp drop might be explained by the physical installation of the cameras at each access point of the restricted area.

The slow reduction in the effect over the following the months may be the result of people becoming more aware of the complicated set of rules of the policy. It may be the case that, in the beginning, a person who was unsure whether or not her vehicle was affected, while sawing the cameras, decided not to take the risk of entering the restricted area. Over time, people get better informed and may realise that their vehicles were actually allowed to access the restricted area. Alternatively, they may have managed to take advantage of the monthly passes for residents or the parking exemptions.

Interestingly, the reduction in transit disappears from July 2019 onwards. On

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[36] Clustering at different temporal level (day or year) only changes the significativity of the estimated effect for August.

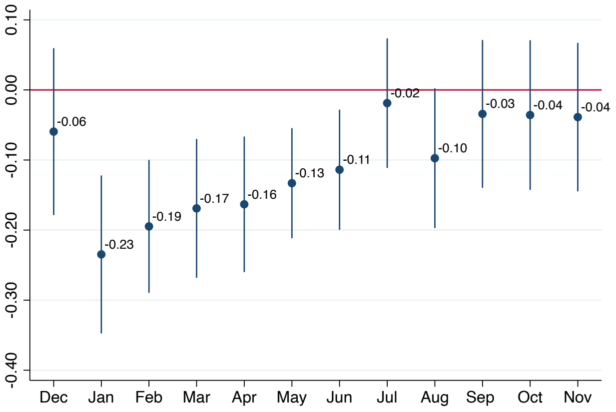
[37] I do not consider observations for December 2019, so that the estimated effect for the December dummy would not represent the average effect between the two Decembers in the post period, and would only consider the first month in which the policy was implemented (the implementation was effective starting from 30th November 2018).

15th June, a new mayor who was clearly opposed to *Madrid Central* took office. Throughout the electoral campaign, had clearly stated his opposition to the implementation of the policy and, once elected, he publicly announced the suspension of fines for entering the restricted area without permission from 1st July 2019 onwards[38]. It seems that people believed him and the announcement was more effective than the actual law, as no year-on-year traffic reductions were registered from July 2019 onwards.

Another factor that may explain the gradual lessening of the effect and its eventual disappearance is the possibility that people bought exempted vehicles. People may have reacted by replacing their older, banned vehicles with non restricted ones. If this is the case, it is realistic to suppose that this change would not be immediate and for sure the overall effect on traffic in the restricted area would be cumulative over time. In fact, each month more people might buy unrestricted vehicles, adding to those already bought in the previous months. The cumulative effect of this, considering that new vehicles can drive within *Madrid Central*, would lead to a gradual reduction over time in the negative effect on traffic. I test for this possibility in the next section.

Figure A3.8 in the Appendix shows exactly the same temporal evolution also for traffic load.

Figure 3.6: Effect by month within the restricted area. Intensity



*Note*: Monthly effect of *Madrid Central* implementation on log intensity in the restricted area. Months are ordered by time after the implementation: Dec represents the first month of implementation, Jan the second one, and so on. 1% confidence intervals are reported together with the point estimates.

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[38] Newspaper article in Spanish: https://www.20minutos.es/noticia/3673957/0/madrid-central-reconvertira-moratoria-multas/

## 3.6 Mechanisms

The main results highlight a reduction in traffic within the restricted area and a displacement to all other areas of the city. In this section, I provide potential explanations to understand why this happened, by focusing on the mechanisms behind the results. I will mainly focus on three potential mechanisms explaining those results.

### 3.6.1 Changes in commuting modes

The first potential explanation is linked to people's behavioural responses. It is likely that, due to the policy implementation, some people made some changes to their daily commutes. On the one hand, some drivers may just have changed the route of their trip and would therefore drive on unrestricted roads after the policy was implemented. On the other hand, other drivers might have changed their commuting mode itself, such as switching to public transport or bicycle. To test for these possibilities, I gathered data on public transport use in the city of Madrid. By comparing these data in different areas of the city (restricted and not restricted) and by running a pre/post implementation time-based analysis again, I can draw some conclusion on this matter. Specifically, I exploit data on monthly metro stations' accesses between December 2017 and December 2019 for the city of Madrid[39]. For each metro station, I have the number of passengers entering each month. The idea is to ascertain whether there might have been an increase in these accesses after the policy was implemented and to look at where such increases were concentrated. The data refers to metro accesses and not exits. Thus, those data do not refer to destinations of the commutes but only to the origin of the specific trip. I am interpreting them as return-trips, which I imagine to be complementary to the way into the city centre.

To test this potential mechanism, I estimate the following monthly panel fixed-effects model:

$$\begin{aligned} log(Y_{m,t}) =& Accesses_{s,t} = \alpha_0 + \beta_1 (Implementation_t * Area_s) + \\ & + \beta_2 timetrend_t * Area_s + \theta_s + \tau_t + \varepsilon_{s,t} \end{aligned} \tag{3.5}$$

where: $Accesses_{s,t}$ is the monthly number of passenger accesses to metro station $s$ and period $t$; $t$ is an indicator for year-month; $Implementation_t$ is a dummy variable which assumes value 1 for months after the implementation[40]; $Area_s$ is a

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[39] Those are open data provided by the Consortium of Regional Transport of Madrid.

[40] *Madrid Central* was implemented on 30th November 2018, so the first treated month is December 2018.

categorical variable distinguishing the metro stations in the four areas previously used; $timetrend_t$ is a linear monthly time trend (by interacting $timetrend_t$ and $Area_s$ I allow for area-specific flexible time trends); $\theta_s$ represents the metro station fixed-effects and $\tau_t$ contains time (year) and seasonal (month of the year) fixed-effects.

Table 3.3 shows the estimated results. The dependent variable is the absolute value of monthly accesses, so that the estimated coefficients present the average increase/decrease in the period in which the policy is in place. I find evidence of a positive and statistically significant increase of 24,005 monthly accesses at the average metro station within the restricted area. Comparing this figure with the average number of entries in the pre-implementation period in the *Madrid Central area* (441,980.2), it represents a 5,4% increase. In contrast, the results for the average stations in the other non-restricted areas are not statistically different from zero. I interpret these results in the following way: some of the people commuting to the city centre do switch to public transport, while others reroute their trips to avoid the city centre when the origin or destination of their trip is not in that area. Rerouted trips would explain the generation of displacement to non-restricted areas. As I am not measuring the volume, this increase can be also provoked by the same drivers spending more time on the road (i.e. longer commutes to avoid the city centre), rather than having more drivers overall. However, the increase in accesses to the stations in the central district suggests a change in commuting mode for trips to the city centre, which would also partially explain the reduction in traffic there. If the trips are made by public transport, I would also expect an increase in trips directed to the city centre (thus an increase in metro accesses outside). One reason that may potentially explain the absence of an increase in the other areas is that the people commuting to the centre are spread all over the city, so the average increase in passengers per station is not relevant because it is spread over many more stations in the non-restricted areas. However, as all those trips are directed to a specific point, the increase is much bigger there because it is dived between a lower number of stations.

Table 3.3: Mechanism. Public transport use

| | (1)<br>Metro monthly accesses |
|---|---|
| implementation=1 × MC | $24005.101^{**}$ |
| | (9363.738) |
| implementation=1 × Inside M30 | 5120.127 |
| | (4810.362) |
| implementation=1 × Outside M30 | 2815.578 |
| | (4081.571) |
| N×$T$ | 5,564 |
| R2 | 0.98 |
| Station FE | ✓ |
| Seasonal FE | ✓ |

*Notes:* Monthly panel fixed effect model. Monthly metro station accesses for station $s$ and year-month $t$ is regressed on a binary time-series indicator for policy implementation interacted with a categorical variable indicating the location of the metro station. Area-specific monthly time trends are also included together with year, month of the year and metro station fixed effects. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

### 3.6.2 Renewal of the vehicle fleet

The second possible explanation is also related to people's behavioural responses. In this case, however, I do not look at responses in terms of daily commutes, but rather I test for a fact well documented in the literature as a response to driving restrictions: renewal of the vehicle fleet. Some people may decide to buy newer cars for which the access to the city center is not restricted. Previous evidence suggest that this is the case. Analysing German LEZs, Wolff (2014) finds that drivers increase the adoption of low-emission vehicles the closer they live to the area. Börjesson et al. (2012) analyse the Stockholm congestion charge and show that, after 2008 when some vehicles were exempted from the toll, the sales of these vehicles increased. Focusing on Bergen, Isaksen and Bjorn (2021) find that commuters who were more exposed to the congestion charge on their way to work were more likely to purchase an electric vehicle. In addition, Percoco (2014) shows that Milan's Ecopass led to the same result. All these results are in line with the theoretical framework of Barahona et al. (2020), which suggests that, if vintage-specific restrictions (i.e. restrictions that differentiate vehicles by their pollution rates) are designed to work exclusively through the extensive margin (type of car driven) and not the intensive one (number of miles driven), they can yield important welfare gains by shifting the fleet composition toward cleaner cars.

In the context of my paper, this explanation would be more a consequence of the policy (still very relevant) than a mechanism potentially explaining the results. However, this can also be seen as a reason explaining the results in the sense that, for example, the reduction of transit in the city centre might have been even higher without this change. This would also explain why the reduction in traffic observed in the city centre gradually decreased month by month (as shown in the heterogeneity analysis). If people buy exempted cars, every month there will be a higher number of vehicles allowed to access the restricted area and thus, the reduction in traffic in that area would gradually reduce.

To test for this, I obtained from the General Traffic Directorate (DGT) of the Spanish Ministry of the Interior, the municipal monthly new vehicles' registrations by type of ecological label from 2017 to 2019, for each municipality in the Autonomous Community of Madrid. Therefore, for each of the 179 municipalities in this region, I have the monthly[41] number of new vehicles registered by vehicle type (e.g. car, motorbike, bus, truck, etc.) and by type of ecological label. Thanks to this information, I can test whether the number of newly registered restricted vehicles fell after the implementation and whether registrations of the non-restricted vehicles actually increased.

Firstly, I provide descriptive evidence of the temporal evolution of the new registrations in the city of Madrid of cars without any ecological label (strictly affected by the policy) and motorbikes (exempted). In Figure A3.9 in the Appendix, the first red vertical line represents the implementation date, while the second shows the point at which fines started to be issued. The descriptive evidence seems to suggest that, after both events, the number of monthly registrations of cars decreased, while motorbike registrations spiked upwards. However, this graph does not take into account seasonalities or other potentially confounding factor related to the new vehicle market. To test more formally for a potentially renewal of the vehicle fleet, I focus on the car market only and look at differences between types of ecological label, with the idea that different labels are subject to different restrictions. Specifically, I estimate the following monthly panel fixed effect model:

$$\begin{aligned} log(Y_{m,t}) =& log(Registrations_{l,t}) = \alpha_0 + \beta_1 (Implementation_t * Label_l) + \\ &+ \beta_2 timetrend_t + \theta_l + \tau_t + \varepsilon_{l,t} \end{aligned} \tag{3.6}$$

where: $Registrations_{l,t}$ is the number of new cars with ecological label $l$ registered in period $t$; $t$ is an indicator for year-month; $Implementation_t$ is a dummy variable which assumes value 1 for months after the implementation[42]; $Label_l$ is a categor-

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[41] I have data from January 2017 until December 2019

[42] *Madrid Central* was implemented on November 30th 2018, so the first treated month is December 2018.

ical variable distinguishing the five different ecological label types; $timetrend_t$ is a linear monthly time trend; $\theta_l$ represents label-specific fixed-effects and $\tau_t$ contains time (year) and seasonal (month of the year) fixed-effects.

Table 3.4 presents the results. In Column 1, I include all car registrations in the city of Madrid, while, in Column 2, the sample is composed of neighbouring municipalities only. The dependent variable is expressed in logarithms, so that the coefficients can be interpreted as elasiticities. For Madrid (Column 1), the new monthly registrations of cars without the ecological label dropped by half in the post implementation period. No differences in new registrations are observed for B- and C- type labels (partially affected by the policy), while an increase in the least affected (ECO) or exempted (0) cars is registered. This evidence seems to suggest a renewal of the vehicles' fleet induced by the policy, with a shift towards cleaner and non-restricted vehicles. In Columns 2, I present results for the registrations in municipalities neighbouring Madrid. Here I do observe as well an increase in registrations of cars with ecological label ECO or 0, while no differences are registered for the strictly forbidden cars without ecological label. Those results seems to suggest that people living in neighbouring municipalities are not directly affected by the policy, highly likely because they do not commute to the restricted area by car. I test this through a simple difference in difference setup, where I compare the monthly registrations of cars without ecological label in Madrid (treatment) and the neighbouring municipalities (control group) and I do find a monthly average treatment effect of 72 less cars registered in the capital with respect to the control group (corresponding to a -29% reduction in the logs model).

The results of this section are in line with the previously mentioned literature and suggest that the low emission zone did induce a renewal of the vehicles feet in Madrid. This would also explain the gradual reduction in the magnitude of the effect within the restricted area and its final disappearance. Indeed, if every month more people buy less restricted cars and more exempted ones, the number of total vehicles affected by the policy is reducing over time and thus it is normal to see an increase of circulating vehicles in the restricted area over time. The gradual reduction of traffic due to the replacement of old vehicles is in line with the results of Beria (2016) for the Milan's congestion pricing.

Table 3.4: Mechanism. New cars' registration

| | (1)<br>Madrid | (2)<br>Neighboring municipalities |
|---|---|---|
| Implementation × No ecological label | -0.502$^{**}$ | -0.347 |
| | (0.209) | (0.281) |
| Implementation × Label B | -0.314 | -0.436 |
| | (0.209) | (0.281) |
| Implementation × Label C | -0.102 | -0.021 |
| | (0.209) | (0.281) |
| Implementation × Label ECO | 0.623$^{***}$ | 0.614$^{**}$ |
| | (0.209) | (0.281) |
| Implementation × Label 0 | 0.572$^{***}$ | 0.657$^{**}$ |
| | (0.209) | (0.281) |
| N×$T$ | 180 | 180 |
| R2 | 0.96 | 0.95 |
| Fixed Effects | Yes | Yes |
| Date trend | Linear | Linear |

*Notes:* Monthly panel fixed effect model. Monthly new cars' registrations with ecological label $l$ and year-month $t$ is regressed on a binary time-series indicator for policy implementation (assuming value 1 starting from December 2018) interacted with a categorical variable indicating the type of ecological label. A monthly time trend is included together with year, month of the year and label type fixed effects. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

### 3.6.3 Commutes from outside Madrid and cruising for parking

One additional factor that may explain the reduction inside the area of implementation and the displacement outside, especially for the areas further away from the city centre, is the commute of people not living in Madrid. This explanation would be consistent with both the high displacement found for the areas further away from the city centre (Outside M30) and the increased use of public transport towards the central district. In this case, I am referring to drivers coming from outside Madrid who might decide to park their banned cars in the outskirts and then switch to public transport for the last part of their commute. This explanation would refer to drivers of cars with no ecological label, who cannot enter the city centre, as well as the owners of vehicles with B- or C- type labels, who may not want to pay for expensive private parking slots in the central area. The absence of reduction in the new monthly registrations of cars without ecological labels in municipalities neighbouring Madrid (Column 2 in Table 3.4) would be consistent with this explanation. As a consequence, the increased average traffic for sensors outside the *M30* ring-road might be driven by drivers cruising for on-street parking slots around areas with good public transport connection to the restricted area. Unfortunately, due to the lack of data, I cannot test empirically for this third mechanism.

## 3.7 Conclusions

In the last few decades, local governments adopted traffic calming policies (i.e. low emission zones and congestion tolls) to tackle urban disamenities such as traffic and pollution. LEZs have been found to be effective in reducing pollution. However, their impact on traffic has only been partially analysed. With this paper, I aim to fill this current gap in the literature, by analysing the effect of LEZs on traffic and car use in an urban environment. I do so by using high-frequency granular data recorded by about 4,000 magnetic sensors around the city of Madrid and by exploiting the implementation of the *Madrid Central* LEZ. I argue that it is really important to focus on a granular urban environment, as average results may hide different spatial patterns. Place-based policies are known to generate spillover effects that should be considered in order to fully evaluate a policy.

I show evidence of a small global average increase in traffic levels for the city of Madrid caused by the implementation of the LEZ. In other words, without distinguishing between restricted and unrestricted areas, my results suggest an average increase of 4.0% for the number of circulating vehicles and 3.6% for traffic load for the city overall. Nevertheless, this global result hides opposite patterns in different areas of the city. Combining different empirical strategies, I show evidence of a reduction in traffic levels within the restricted area, at the expense of other areas of the city which show a higher level of circulating vehicles and load. Specifically, I find an average hourly decrease of 8.1% in the number of vehicles circulating over the average sensor and of 8.7 percentage points for traffic load in the restricted area. The traffic relief documented for the restricted area, is more than offset by an increase in traffic in all the areas outside the city centre, which I interpret as displacement effect induced by the policy.

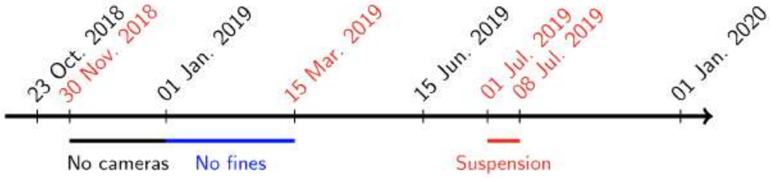
I further expand those results by means of an heterogeneity analysis. Firstly, I decompose the results to a really granular level of detail, identifying the streets of the city most negatively affected by the displacement of traffic. Secondly, I look at the temporal evolution of the reduction in transit within the restricted area. In this regard, I suggest that the gradual reduction of the effect over time (and its eventual disappearance) may be explained by announcements by local politicians and the renewal of the vehicle fleet induced by the policy.

My results are consistent with previous research analysing people's behavioural responses to driving restrictions. Specifically, it has been proven that drivers may switch to untolled roads or untolled hours due to changes in the road network. This would be consistent with the displacement of traffic triggered by the rerouting of trips not directed to the city centre. Secondly, people may also react by changing commuting mode (e.g. switching to public transport) or buying exempted vehicles. I provide evidence of both mechanisms. On the one hand, I find that some commutes to the restricted area are substituted by public transport, while drivers reroute their trips to avoid the city centre neither the origin nor destination is in that area. On the other hand, in line with previous results I find that the policy resulted in a renewal of the vehicle fleet towards less restricted and cleaner cars.

Overall, my findings suggest that *Madrid Central* has been successful in reducing the number of vehicles in the city centre. It has also been an incentive to renew the vehicle fleet with cleaner vehicles. As such, the policy has probably achieved its original aim of abating pollution. Nevertheless, the most important result of the paper is the displacement effect towards unrestricted areas, a relevant and undesired consequence of the implementation. This factor should be considered when designing such schemes, in order to ensure that the whole city benefits from the measure, rather than just the restricted area itself. In the specific case of Madrid, the negative spatial spillover is likely to be driven by the size of the restricted area. *Madrid Central* takes up less than 1% of Madrid's total area, so it is not very time-consuming for people to reroute their trips to unrestricted paths. If this is the case, it also lowers the incentive for people to change commuting modes or buy less-polluting cars, unless their trips are not directed to the restricted area. Further research is needed to show whether the magnitude of the reduction depends on the dimension of the restricted area, or whether larger LEZs cause lower (or no) traffic displacement. An interesting case study to make a comparison is the city of Barcelona, where the local government implemented a LEZ that covers the whole municipal area (95 km2). This will add more evidence on whether LEZs can be designed in order to avoid regressive welfare impacts.

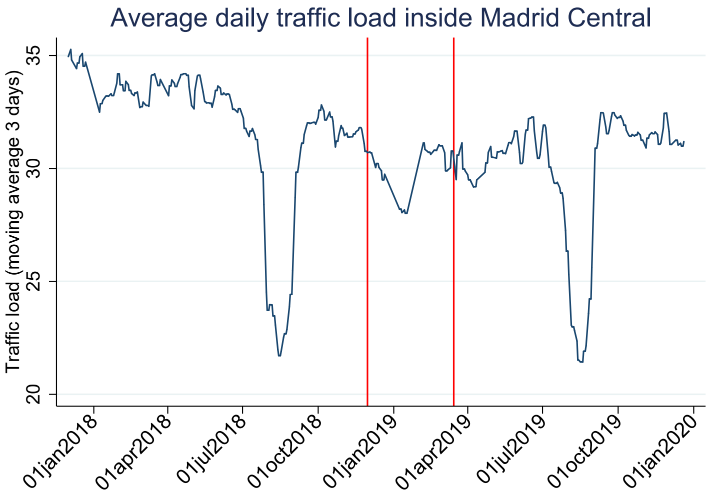
## 3.8 Appendix

Figure A3.1: Timeline of Madrid Central implementation.



- 30th November 2018: Official date of implementation;
- 15th March 2019: Beginning of real sanctions;
- 15th June 2019: New mayor (against MC) in charge;
- 1st-7th July 2019: Suspension of MC, protests and judgement;
- 1st January 2020: less stringent rules.

Figure A3.2: Temporal evolution of traffic load inside *Madrid Central*.



*Note*: The graphs plots 3 days moving averages for daily traffic load inside *Madrid Central*. The first vertical line represents the day of implementation, while the second indicates the day in which fine started to be sent out. Weekends and holidays are removed from the sample to attenuate the effect of seasonalities.

Table A3.1: Summary statistics

| | Full Sample (N=3,640) | | | | | | |
|---|---|---|---|---|---|---|---|
| | mean | sd | min | max | p25 | p50 | p75 |
| Intensity | 578.20 | 753.53 | 0.00 | 91194.50 | 147.50 | 328.50 | 704.50 |
| Occupancy | 8.91 | 11.72 | 0.00 | 100.00 | 2.50 | 5.00 | 9.75 |
| Traffic load | 25.52 | 16.69 | 0.00 | 100.00 | 12.25 | 22.50 | 36.00 |
| Log intensity | 5.74 | 1.19 | -1.39 | 11.42 | 5.00 | 5.80 | 6.56 |
| Log occupancy | 1.63 | 1.10 | -1.39 | 4.61 | 0.92 | 1.66 | 2.30 |
| Log traffic load | 2.97 | 0.82 | -1.39 | 4.61 | 2.53 | 3.11 | 3.58 |
| Average temperature | 15.53 | 7.86 | 1.60 | 32.90 | 8.50 | 14.20 | 22.40 |
| Precipitation | 1.33 | 4.05 | 0.00 | 38.40 | 0.00 | 0.00 | 0.10 |
| Daily gasoline price | 1.31 | 0.06 | 1.18 | 1.41 | 1.27 | 1.34 | 1.35 |

| | Sensors outside Madrid Central (N=3,566) | | | | | | |
|---|---|---|---|---|---|---|---|
| | mean | sd | min | max | p25 | p50 | p75 |
| Intensity | 584.54 | 759.06 | 91194.50 | 0.25 | 149.25 | 332.75 | 713.25 |
| Occupancy | 8.82 | 11.55 | 100.00 | 0.00 | 2.50 | 5.00 | 9.75 |
| Traffic load | 25.48 | 16.69 | 100.00 | 0.00 | 12.25 | 22.25 | 35.75 |
| Log intensity | 5.74 | 1.20 | 11.42 | -1.39 | 5.01 | 5.81 | 6.57 |
| Log occupancy | 1.62 | 1.10 | 4.61 | -1.39 | 0.92 | 1.61 | 2.30 |
| Log traffic load | 2.97 | 0.82 | 4.61 | -1.39 | 2.51 | 3.1 | 3.58 |

| | Sensors inside Madrid Central (N=74) | | | | | | |
|---|---|---|---|---|---|---|---|
| | mean | sd | min | max | p25 | p50 | p75 |
| Intensity | 320.83 | 292.32 | 6305.00 | 0.75 | 128.75 | 248.00 | 398.75 |
| Occupancy | 11.68 | 12.06 | 100.00 | 0.00 | 4.50 | 7.75 | 14.00 |
| Traffic load | 27.91 | 15.29 | 100.00 | 0.00 | 16.25 | 25.00 | 37.50 |
| Log intensity | 5.42 | 0.87 | 8.75 | -0.29 | 4.86 | 5.51 | 5.99 |
| Log occupancy | 2.05 | 0.93 | 4.61 | -1.39 | 1.50 | 2.05 | 2.64 |
| Log traffic load | 3.16 | 0.62 | 4.61 | -1.39 | 2.79 | 3.22 | 3.62 |

| | Pre implementation | | Post implementation | |
|---|---|---|---|---|
| | mean | sd | mean | sd |
| Intensity | 339.42 | 306.53 | 302.40 | 276.27 |
| Occupancy | 12.49 | 12.71 | 10.87 | 11.33 |
| Traffic load | 29.29 | 15.68 | 26.54 | 14.76 |
| Log intensity | 5.48 | 0.87 | 5.36 | 0.87 |
| Log occupancy | 2.13 | 0.9 | 1.98 | 0.95 |
| Log traffic load | 3.21 | 0.61 | 3.11 | 0.63 |

*Notes:* The descriptive statistics presented here are calculated over the sample employed for estimations (N=43,145,221). I collapse the quarterly of hour frequency to an hourly one and I maintain only sensors with at least 2,000 time observations over the period.

Table A3.2: Robustness check. Sensors classified in 2 km dummy bins based on their distance from the border of *Madrid Central*

| | (1)<br>log intensity | (2)<br>log occupancy | (3)<br>log load |
|---|---|---|---|
| Implementation × MC | -0.081$^{***}$ | -0.177$^{***}$ | -0.087$^{***}$ |
| | (0.012) | (0.016) | (0.013) |
| Implementation × 0-2km | 0.011$^{**}$ | 0.011 | 0.010 |
| | (0.006) | (0.007) | (0.006) |
| Implementation × 2-4km | 0.041$^{***}$ | 0.046$^{***}$ | 0.039$^{***}$ |
| | (0.006) | (0.007) | (0.006) |
| Implementation × 4-6km | 0.055$^{***}$ | 0.053$^{***}$ | 0.051$^{***}$ |
| | (0.006) | (0.007) | (0.006) |
| Implementation × 6-8km | 0.061$^{***}$ | 0.052$^{***}$ | 0.060$^{***}$ |
| | (0.006) | (0.007) | (0.007) |
| Implementation × >8km | 0.068$^{***}$ | -0.010 | 0.046$^{***}$ |
| | (0.012) | (0.013) | (0.011) |
| N×$T$ | 43,145,211 | 42,036,903 | 38,567,144 |
| R2 | 0.90 | 0.71 | 0.78 |
| Sensor FE | ✓ | ✓ | ✓ |
| Time FE | ✓ | ✓ | ✓ |
| Seasonal FE | ✓ | ✓ | ✓ |

*Notes:* Hourly panel fixed effect model. A parameter of traffic for sensor $m$ and year-month-day-hour $t$ is regressed on a binary time-series indicator for policy implementation interacted with a categorical variable indicating the location of the sensor, a list of controls (suspension, peak hour, announcement, holiday, precipitation, average temperature, daily gasoline price), area-specific daily linear time trends and time, seasonal and location fixed effects. The coefficients for the controls are estimated but omitted from the table. The dependent variables are in turn log intensity (Column 1), log occupancy (Column 2), log traffic load (Column 3), and log average speed (Column 4). Standard errors are clustered at the commuting area-day level and are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

Table A3.3: Placebo analysis for intensity within *Madrid Central*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Implementation × MC | -0.094$^{***}$ | -0.024 | -0.072 | 0.074 |
| | (0.022) | (0.054) | (0.047) | (0.063) |
| Implementation date | 30/11/2018 | 30/11/2017 | 30/11/2016 | 30/11/2015 |
| N $\times T$ | 2,805,859 | 5,078,724 | 5,078,724 | 5,078,724 |
| R2 | 0.92 | 0.82 | 0.82 | 0.82 |
| Sensor FE | ✓ | ✓ | ✓ | ✓ |
| Time FE | ✓ | ✓ | ✓ | ✓ |
| Seasonal FE | ✓ | ✓ | ✓ | ✓ |

*Notes:* Daily panel fixed effect model. Log intensity for sensor $m$ and year-month-day $t$ is regressed on a binary time-series indicator for policy implementation interacted with a categorical variable indicating the location of the sensor, a list of controls, area-specific daily linear time trends and time, seasonal and location fixed effects. Only the coefficients for the restricted area are presented. Daily intensity is the sum of hourly values. In Column 1, the implementation date is the real one (30/11/2018), while in the following columns I simulate the implementation to be 1, 2, or 3 years before the real date respectively. In order to do so, for those columns I use data between December 2014 and November 2018. Differently, for Column 1, I employ the same time framework of the analysis (December 2017-December 2019). Standard errors are clustered at the commuting area and are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

Figure A3.3: Google searches of *"Madrid Central"* in Madrid for October/December 2018.

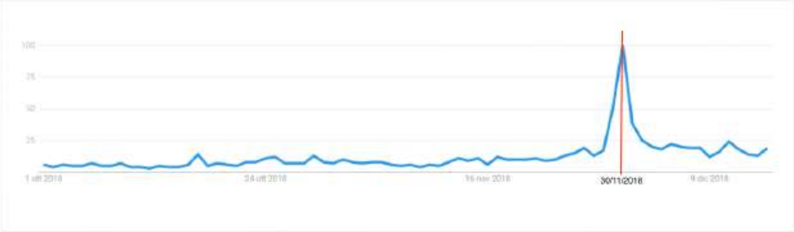
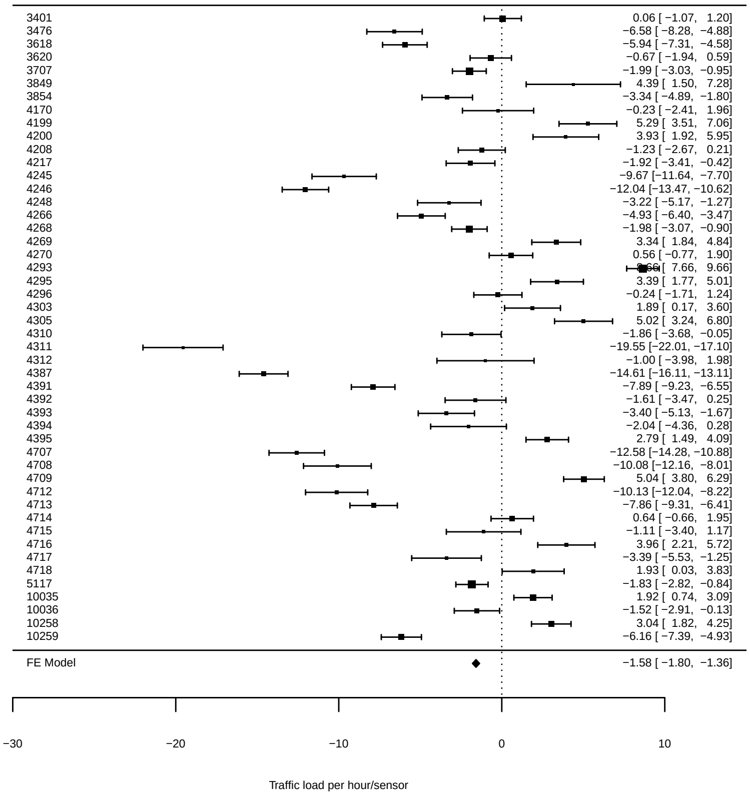


Figure A3.4: Results of meta-regression (FE model) for causal impact analysis of traffic load



*Note*: The column on the left includes the id number of the sensors, while on the right the estimated impacts and their relatives confidence intervals are reported. The last line present the result of the meta-regression aggregating together all the sensor-specific impacts.

Figure A3.5: Causal impact analysis - Sensor-specific results for intensity.

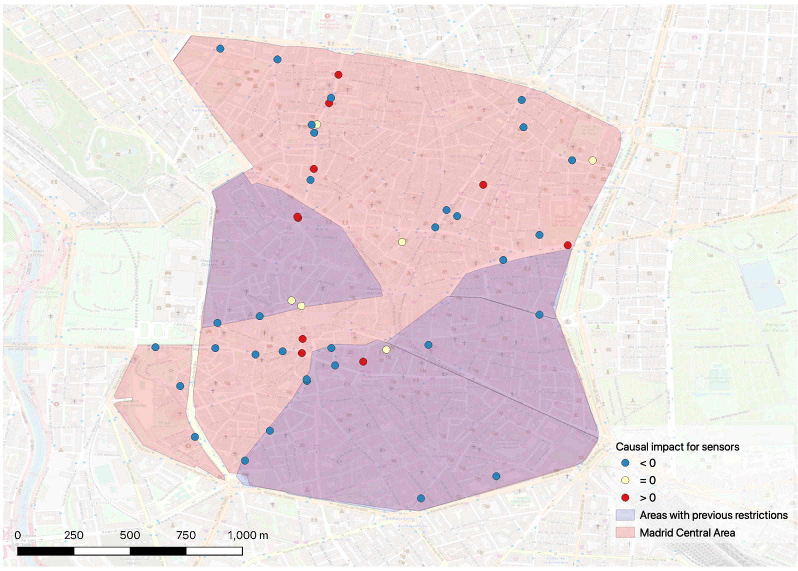


Figure A3.6: Causal impact analysis - Sensor-specific results for intensity.

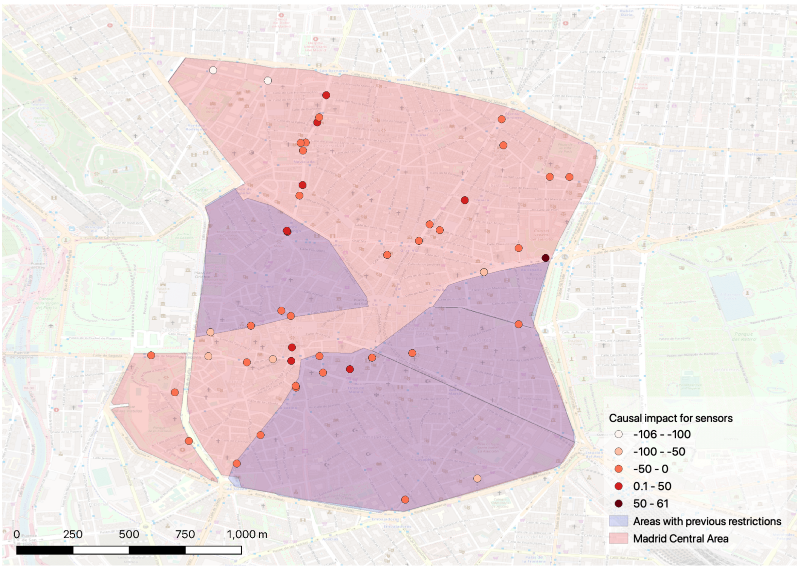
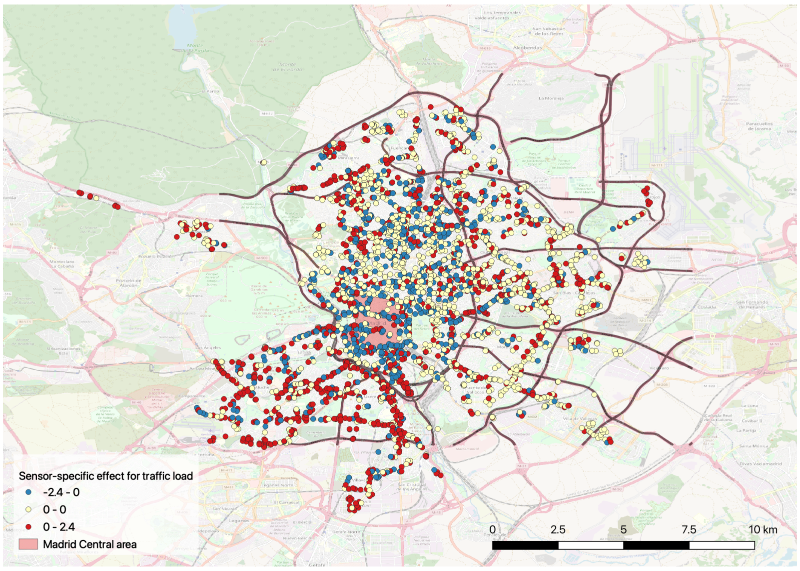
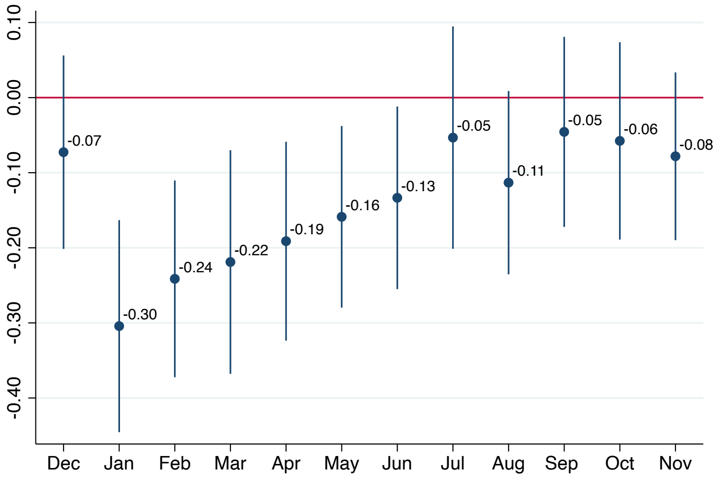


Figure A3.7: Sensor-specific time-series regression results for log traffic load.



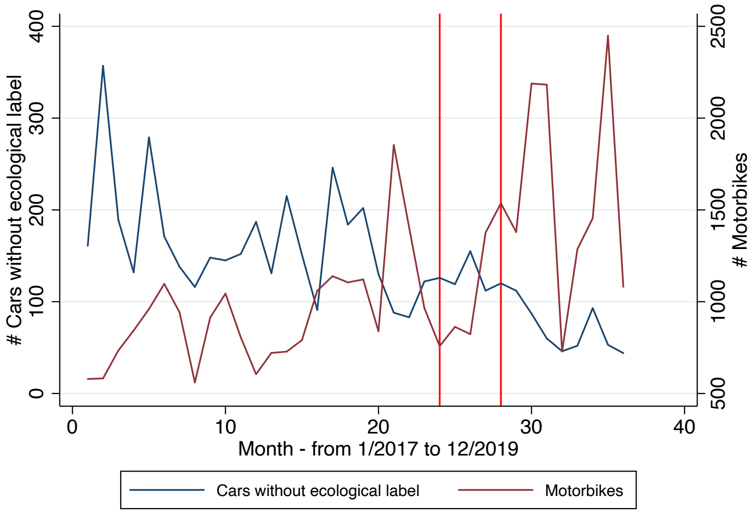
*Note*: For each sensor, the map represents the estimated coefficients in the time-series regressions for the variable *Implementation*. Blue dots represent negative and statistically significant coefficients (i.e. reductions in traffic for the specific sensor with respect to the pre-implementation period); yellow dots represents non-statistically significant coefficients (i.e. no differences in traffic); and red dots stands for positive and statistically significant coefficients (i.e. increases in traffic).

Figure A3.8: Effect by month within the restricted area. Traffic Load



*Note*: Monthly effect of *Madrid Central* implementation on log traffic load in the restricted area. Months are ordered by time after the implementation: Dec represents the first month of implementation, Jan the second one, and so on. 1% confidence intervals are reported together with the point estimates.

Figure A3.9: New monthly vehicles' registration in Madrid.



# 4 The price of silence[1]

## 4.1 Introduction

Together with air pollution and congestion, noise pollution is one major consequence of motorized traffic. The European Environmental Agency (2020a) estimates that in 2019, at least 20% of the EU population was living in areas with high noise levels[2] More specifically, 113 million people are estimated to be affected by long-term day-evening-night traffic noise levels of at least 55dB. In addition, 22 million are exposed to high levels of railway noise, 4 million to high levels of aircraft noise and less than 1 million to high levels of noise caused by industries[3]. Long-term exposure to noise can lead to stress, headache and sleep disturbance (Boes et al., 2013). In addition, it contributes to higher risk for type 2 diabetes (SØrensen et al., 2013) and has an impact on birth weights (Argys et al., 2020). Furthermore, epidemiological studies have found that environmental noise is associated with an increased incidence of arterial hypertension, hearth failure and stroke (Barregard et al., 2009; Bodin et al., 2009; Munzel et al., 2018). As a result, the European Environmental Agency estimates that long-term exposure to environmental noise causes 12,000 premature deaths and contributes to 48,000 new cases of ischaemic heart disease per year in Europe[4].

This evidence has motivated the economic literature to incorporate the externality of noise pollution in hedonic price models. Proposed by Rosen (1974), an hedonic price model assumes that the price of property depends on its characteristics and the amenities surrounding it. The observed price difference comparing houses with similar characteristics allows to value the cost of non-market factors in monetary terms. Examples of studies employing hedonic price models to estimate the correlation between noise pollution and housing prices are Ozdenerol et al. (2020); Swoboda et al. (2015); Weinhold (2013); Brandt and Maennig (2011); Wilhelms-

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[1] Paper coauthored with Marianna Magagnoli

[2] For Europe, high noise levels are defined by the Seventh Environment Action Programme as above 55 decibels (dB) $L_{den}$ (day-evening-night noise level) and 50 dB $L_{night}$ (night noise level).

[3] The European Environmental Agency (EEA) stresses that these numbers are likely to underestimate the actual exposure.

[4] 22 million people suffer chronic high annoyance, while 6.5 million people suffer from chronic sleep disturbance.

son (2000). All these studies suggest a negative relationship between street noise and housing prices.

However, hedonic price models do not establish causalities. They can be interpreted as the marginal willingness to pay for the amenity, conditional on observable characteristics. The presence of unobservable characteristics correlated with the regressors would bias the result, making it difficult to disentangle whether the negative effect is fully driven by noise. It might be the case that it reflects also other amenities correlated with noise and not included in the model (i.e. accessibility).

The economic literature has thus exploited natural quasi-experimental designs with the aim of accounting for time-invariant unobservable characteristics (Kuminoff et al., 2010). One part of this literature focuses on noise caused by airports. For example, Nelson (2004) found that a 200,000$ house may sell for 20,000$ to 24,000$ less when exposed to airplane noise. More recently, Zheng et al. (2020); Boes and Nüesch (2011) have also documented the negative impact of aircraft noise on housing prices. Other studies have used variation in public transit lines such as buses (Fan et al., 2021), metro (Ahlfeldt et al., 2019), rail (Diao et al., 2021), or new roads (Ossokina and Verweij, 2015).

Whether noise is capitalised into housing prices is an open question. This paper contributes to the literature by studying the causal impact of street noise on housing prices. Specifically, I combine hedonic price estimates with a fixed-effect model and I benefit from a specific architectural feature of my framework, to isolate the effect of unobservable and precisely estimate the effect of interest. To the best of my knowledge, my paper is one of the first contributions estimating the capitalization of traffic noise into housing prices in a causal way. von Graevenitz (2018), which estimates the impact of traffic noise on housing prices in Copenhagen, is the most closely related paper. It uses fixed effects in a street border research design, with the aim of reducing the omitted variable bias from unobservable characteristics. However, as the author points out, these estimates are likely suffering from measurement error because of the relatively poor quality of the data on traffic noise.

Given this, I contribute to the existing literature by adding a precise and causal estimate of the effect of interest. An additional contribution is that I am able to distinguish between sales and rents, while previous evidence focused on sales only. Furthermore, I provide heterogeneous analysis by time of the day and noise source.

My aim is to exploit the spatial variation in very granular data on street noise and housing transactions data. The grid pattern and the square blocks of the Eixample district of Barcelona provide a good setting to exploit variation in traffic noise intensity between properties belonging to the same building block but exposed to different noise levels. Specifically, I exploit variations in noise levels within 150x150 square meters areas, and I compare properties sold or rented over time within the block. I do so by including in my model building blocks fixed effects. Focusing on variations at a granular level, I can ensure that all the omitted variables I cannot control for remain constant.

Furthermore, focusing on the Eixample district gives me another advantage. The people living in this district are quite homogeneous in terms of socio-demographic characteristics. This allows me to assume a negligible sorting effect into different street segments within one building block. Thus, for this neighbourhood I can exclude that a specific group of people would cluster in one street segment or the other, leading to price effects on housing related to neighborhood composition (Bayer et al., 2007; Guerrieri et al., 2013; Diamond, 2016).

Besides the availability of data and its suitability for the identification strategy, Barcelona is itself an interesting and relevant case study. It is one of the most polluted cities in Europe, both for air and noise pollution[5]. The last report of the Public Health Agency of Barcelona (APSB) shows that the 57% of the residents in in the city is exposed to high noise levels, 27% of which to a level higher than 65 dB. The same report suggests how traffic noise pollution is responsible for 130 yearly deaths (related to cardiovascular diseases). At the same time, 210,000 people suffer severe psychological or social affectation, and 60,000 people suffer severe sleep disorders.

My results suggest that street noise leads to a price depreciation of 1.6% on listed prices for sales. In other words, referring to the average price in my estimation sample, I find that moving from one category to another of noise exposure (i.e. increase of 5db) correspond to a price reduction of about 7,250€. I find this negative effect on listings for sales, but not for rents. Furthermore, looking at heterogeneous results I find that evening and night noise, more than the daily, are driving the price depreciation. Finally, for the 2017 data, I am also able to differentiate by noise sources. I do find a negative effect on sales' posted prices for traffic and recreational noise. Differently, I find a positive effect for pedestrian noise.

The remainder of the chapter is organised as follows. I begin by describing the data and the Eixample district of Barcelona. The empirical framework is explained in Section 3. Then, Section 4 presents the main results suggesting a price depreciation for sales' listings due to noise exposure. In the same section I also present results for rents and heterogeneity analyses. Section 5 concludes.

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[5] According the Mimi's Worldwide Hearing Index Mimi's Worldwide Hearing Index, Barcelona is the loudest European city (i.e. the city with the highest noise pollution score).

## 4.2 Geographical setting and data

For identification purposes, explained in detail in the next Section, my analysis will focus on the Eixample district. The Eixample is one of the ten districts of Barcelona. It lies exactly in the middle of the city[6], and it covers an area of 7.5 square kilometeres (7.32% of the area of the city). In 2021, 16% (about 270,000 people) of the city's population was living in this area. Table A4.1 in the Appendix presents the resident's socio-demographic characteristics of this district compared to the other neighborhoods of the city[7]. As those descriptive statistics suggest, the Eixample has a higher population density than the rest of the city (37,431 vs. 30,928 inhabitants per square kilometre). In addition, Eixample's residents are richer, younger, more educated, and less unemployed than the average resident in the rest of the city. Furthermore, looking at the substantially lower standard deviations, I can see how the people living in this district are more homogeneous in terms of socio-economic characteristics with respect to average residents of all the other basic statistical areas.

To identify the effect of interest, I benefit from a peculiar architectural feature of this district: its orthogonal structure and its homogeneous square blocks. Figure A4.2 and A4.3 in the Appendix show from different perspectives[8] this architectural feature.

In order to estimate the effect of street noise on housing prices, I have retrieved geolocalised and granular data on noise exposure and housing transactions, as well as a number of control variables, for the city of Barcelona.

Barcelona's City Hall releases strategic noise maps (SNM) at the street and acoustic zone levels. Those maps are developed every five years and serve to *"globally assess the population's exposure to noise produced by different noise sources in a given area"* (Barcelona's City Hall). The strategic noise maps at the street sections level are openly available[9] for the years 2009, 2012 and 2017. The assessment of the noise level is carried out employing two different methods: real measurements and simulations. The real measurements are realised with 109 fixed and 2,309 mobile sensors[10] (Lagonigro et al., 2018). Specifically, fixed and mobile sensors regis-

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[6] Figure A4.1 in the Appendix shows the position of this district within the city.

[7] The statistics are computed by averaging together the values of each basic statistical area (AEB) composing the specific district. Basic statistical areas (AEB) are administrative units slightly bigger than census tracts. There are 233 AEB in the city of Barcelona, of which 26 in the Eixample district.

[8] Map and bird-eye's view respectively.

[9] The maps are downloadable from the Open Data website of Barcelona's city hall: https://opendata-ajuntament.barcelona.cat/data/en/dataset/tramer-mapa-estrategic-soroll

[10] Figure A4.4 and A4.5 in the Appendix present an example of those two types of sensors.

ter respectively long-term and short-term readings[11]. Long-term measures (at least 24 hours long) establish the temporary evolution of noise during the day. Short-term readings (at least 15 minutes long) assess the daily noise level and its source (Barcelona City Council, 2017). Starting from those real readings, and following recommendations from the European Union (EU Monitor), computation methods (i.e. propagation models) are applied to obtain the noise level in the whole street network.

With the combination of the two methods, real measurements and simulations, a noise measure is obtained for each of the 14,343 street sections of the city. A street section is a portion of street in between two crossroads, with an average length of about 150 metres. Each street section is classified into one of eight different noise levels. A level is 5 decibels (dB) wide and each street segment can be classified into one of the following categories: <45dB, 45-50dB, 50-55dB, 55-60dB, 60-65dB, 65-70dB, 70-75dB, or 75-80dB. To give an idea of what those numbers mean, it is worth to remember that high noise level are defined as greater than 55dB for day and 50dB for night noise (European Environmental Agency, 2020a). Figure A4.6 in the Appendix presents the distribution of street sections into the different categories for daily noise , both for the whole city and for the Eixample district, on which my empirical analysis is based. Besides the overall noise levels, the data allow me to distinguish noise by time of the day (day, evening, night) and for the 2017 values also by source (street traffic, railways, industrial, recreational, and pedestrian). Thus, this data provide me one noise classification for each street segment for the years 2009, 2012, and 2017. To understand how those data look like, I present the 2017 maps for daily and night noise in Figures A4.7 and A4.8 in the Appendix.

Data on housing prices are gathered from *Idealista*, the major real estate website used in Spain. Specifically, I have all listings, both for rentals and sales, in the city of Barcelona posted on the website in the month of December of each year between 2007 and 2017. Specifically, for the 10-years period I have 126,520 sales and 119,939 rentals for the whole city. The listings within the Eixample district are 24,224 and 27,098, respectively for sales and rents. The distinction between rent and sales available in the dataset, might be informative about the heterogeneous preferences for noise avoidance between buyers and renters. Posted prices are available, together with the exact location of the dwelling and the characteristics of the house such as number of rooms, presence of air conditioning, lift and boxroom and type of property (studio, penthouse, duplex). Posted prices might differ from transaction prices, i.e. the real price of the transaction, due to bargaining that comes along with the process. However, transaction prices are usually available

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[11] Both measures are realised taking into account weather conditions, wind, and other events that might interfere with proper noise measurement.

from tax records, and thus could have some limitations (Garcia-Lopez et al., 2021). On the one hand, there might be time lapse between the date of the transaction and the date in which the tax is paid. On the other hand, tax records might be subject to price under-reporting to pay lower taxes. Previous studies suggested that posted prices, especially for rents, are a good proxy for transactions prices.

Table 4.1 presents the number of observations for housing transactions, distinguishing them by type of transaction (sales vs. rents) and year. I report statistics both for the whole city of Barcelona and the Eixample District, on which my empirical analysis will be focused on. Similarly, figures A4.9 and A4.10 in the Appedix show the spatial distribution of listings, for sales and rents respectively, by basic statistical areas.

Housing transactions are matched to the shapefile of the cadastre, which provides a map of all the 70,221 buildings of the city. In this way, besides adding to my data the age of the building, I can identify the block each property belongs to, which is essential in my identification strategy.

Table 4.1: Number of observations of housing transactions by year, type, and source

| | Idealista - Sales | | Idealista - Rents | |
|---|---|---|---|---|
| Year | Barcelona | Eixample | Barcelona | Eixample |
| 2007 | 607 | 143 | 197 | 50 |
| 2008 | 968 | 202 | 559 | 111 |
| 2009 | 1,437 | 277 | 1,134 | 241 |
| 2010 | 4,239 | 789 | 2,328 | 495 |
| 201 | 12,126 | 2,465 | 9,925 | 2,340 |
| 2012 | 12,826 | 2,438 | 16,135 | 3,886 |
| 201 | 13,941 | 2,480 | 18,124 | 4,206 |
| 2014 | 16,791 | 3,184 | 17,570 | 3,781 |
| 2015 | 20,999 | 3,818 | 15,609 | 3,363 |
| 2016 | 20,839 | 3,882 | 16,586 | 3,567 |
| 2017 | 21,747 | 4,546 | 21,772 | 5,058 |
| Total | 126,520 | 24,224 | 119,939 | 27,098 |

I further collect data in order to control for characteristics of the block which influence housing prices and are correlated to noise. For example, trees help mitigate noise and make a street more enjoyable. The geolocation of trees is provided by Barcelona's City Hall from 2018 until 2021[12], and I use those data to compute the

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[12] In my analysis, I will use the information of the year 2018, being the closest one to my noise

number of trees for each street segment.

Amenities also play a crucial role in determining housing prices of an area. I gather information on restaurants, bars, cultural and touristic amenities both from the Commercial Census of the city of Barcelona and by web-scraping Tripadvidor. I will use this information to control for the number of each kind of amenities in each street segment.

The last data source I use, from which I construct different variable, is Open Street Map (OSM). Starting from the topology of the map of the city, I am able to compute the street gradient and street width for each street segment. Those parameters are relevant for traffic, and thus indirectly for noise. In addition, I believe street width, together with floor, helps me to deal with a potential omitted variable bias not included directly in the model: flat's view and light exposure. From OSM, I gather information also on the position of traffic lights, and public transportation stops, and I compute the square meters of urban parks within 500 metres from each street segment.

## 4.3 Empirical framework

The main threat to the identification of the causal effect of noise on housing prices is an omitted variable bias problem. On the one hand, a higher noise level would reduce the price of the house, as noise pollution is a disamenity. However, on the other hand, a higher noise level might be related to higher accessibility, thus pointing towards an increase in the the price of the dwelling. In other words, noise levels are correlated to street traffic. Streets with more traffic are likely to represent connection axis between important areas of the city (for example, between residential areas and the central business district). The same intuition can be extended to public transport accessibility, i.e. proximity to a train station. Thus, noise represents a negative externality, but it also reflects the connectivity of the area. Both aspects are likely to affect housing prices in opposite directions. Thus, disentangling the different effects and isolating the real effect of noise on housing prices is not straightforward.

My strategy hinges on the variation in noise intensity at very granular levels. Specifically, I benefit from the orthogonal structure and the homogeneous square blocks of the Eixample district, by looking at variations within the same building block (i.e. areas of about 150x150 square meters).

I exploit variations between properties belonging to the same building block but

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measures.

exposed to different noise levels. I do so by including in my model building blocks fixed effects. Focusing at variations at a granular level, I can ensure that all the unobservable characteristics which are simultaneously correlated with noise and influencing property values remain constant within each building block. The underlying hypothesis is that properties within a same block enjoy the same local characteristics, but are exposed to different noise levels. Specifically, I can safely assume that street and public transport accessibility is constant for all the properties, whatever is the street segment of the four surrounding the building block. At the same time, amenities and disamenities also play a crucial role in determining housing prices of an area. Within those areas, differences in school access, crime rates, shops and restaurants availability, closeness to touristic attractions, and any other element which might directly effect housing prices, are negligible. All those variables would increase or decrease the housing value. However, if the area is small enough, I can ensure that the price effect would be the same for all the properties and it will be captured by the building block fixed effect. In other words, by including block fixed effects, I can keep accessibility, amenities/disamenities constant, and exploit only spatial - within the four street segments adjacent to one block - and temporal variation in noise.

Furthermore, focusing on the Eixample district only, gives me another advantage. The people living in this district are quite homogeneous in terms of socio-demographic characteristics[13]. This makes me confident that people living on one street segment or the other within the building block are not sistematically different. Thus, for this neighbourhood I can exclude that a specific group of people would cluster in one street segment or the other, leading to sorting effects on housing prices (Bayer et al., 2007; Guerrieri et al., 2013; Diamond, 2016).

Figure 4.1 visually shows the orthogonal structure of the Eixample district and it helps understanding the empirical strategy I rely on.

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[13] See the previous section for a description of this, as well as Table A4.1 in the Appendix reporting the socio-economic descriptives for the residents in this district

Figure 4.1: Identification strategy for the orthogonal structure of the Eixample district

*Source*: Cadastre, Idealista, City Council of Barcelona, Open Street Map.

*Note*: The map shows few building blocks of the Eixample district. Lines are the street segments, their color reflects the 2017 noise range at daytime. The squares represent the properties listed between 2007 and 2017.

Thanks to the main data sources, I have information on noise exposure for the years 2009, 2012 and 2017 and housing transactions between 2007 and 2017. I need to assign the correct level of noise to each housing transaction, depending on the year in which the house has been sold or rented. Here there is a trade-off between measurement error and statistical power. On the one hand, I should keep only the listings posted in the years in which I observe noise to reduce the measurement error. On the other hand, by doing so I would reduce enormously the housing data I have available, thus loosing statistical power for my empirical analysis. As a middle point, I decided to keep in my analysis only the housing transactions registered in the exact, previous or following year in which I observe noise[14]. Specifically, I assign the 2009 noise values to the housing transactions occurred in 2008, 2009, and 2010; the 2012 values for those of 2011, 2012, and 2013; the 2017 values for those of 2016, and 2017. In this way, I might introduce some measurement error in my analysis, but I believe it is negligible and I can increase the statistical

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[14] Having noise values for 2009, 2012, and 2017, I drop from the analysis housing transactions from 2007, 2014, and 2015.

power of my empirical analysis. The underlying assumption is that noise exposure for each street segment does not vary drastically one year to the other. Looking at the temporal evolution of noise values, it seems a plausible hypothesis. In fact, noise levels by street sections are be highly correlated accross time. In particular, the noise levels of 2009 and 2012 are correlated with a coefficient of 0.97, while between 2012 and 2017 the coefficient is 0.74.

Given this framework, and considering that I am keeping only transactions for which the full set of house characteristics is available[15], I end up with a regression sample of 16,331 and 19,187, respectively for Idealista listings for sales and rents. As already mentioned, I exploit variations in noise levels within building blocks to identify the effect of interest. In the Eixample district there are 404 of those spatial units. Thus, one might think that reducing the sample of housing transactions would reduce the statistical power of my model. Nevertheless, even with all the expedients I took, I end up with an average of 40 sales and 47 rents within each building block of the Eixample district for the period of interest.

Specifically, to estimate the effect of street noise on housing prices I estimate the following repeated cross-sections fixed-effect model:

$$log(Price_{isjt}) = \beta_0 + \beta_1 Noise_{st} + \beta_2 X_i + \beta_3 Z_s + \omega_j + \tau_t + \varepsilon_{isjt} \tag{4.1}$$

Where $log(Price_{ijst})$ is the log of the transaction price of unit $i$, block $j$, and street segment $s$ at time $t$. $Noise_{st}$ is the noise range of the street segment $s$ on which the property belongs. Specifically, this variable is classified into different bins of 5dB each, so that the coefficient $\beta_1$ would capture the price effect related to a 5dB increase in noise exposure. $X_i$ is a vector controlling for unit $i$ characteristics such as floor, size (m$^2$), number of rooms, presence of air conditioning, lift and boxroom, type of property, age of the building and its quality of construction. $Z_s$ controls for street segment characteristics[16], such as street gradient and street width, number of trees, restaurants, bars, cultural and touristic amenities. It also includes information such as the square metres of urban parks within a 500 meters distance. Finally, $\tau_t$ is a year fixed effect, and $\omega_j$ is the block fixed effect which allow me to compare transactions within the same block.

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[15] I also exclude from the analysis the outlier observations. Specifically, for sales I drop the listings with posted price lower than 10,000€ and smaller than 20 square metres as size. For rentals, I drop ads with monthly rents below 100€ or above 30,000€. This is in line with previous studies using the same data source (Garcia-Lopez et al., 2021)

[16] Given my empirical framework, I argue that it not necessary to control for public transport accessibility, school accessibility, crime, and similar variables. In fact, all those variables would be constant within the 150x150 metres building blocks where I identify the effect of interest.

## 4.4 Results

### 4.4.1 Baseline results

The results of the estimation of equation 4.1 are reported in Table 4.2. Specifically, this regression is run on the sample of Idealista listings for sales posted in the Eixample district of Barcelona between 2007 and 2017.

In columns from 1 to 5, I gradually include time fixed effects, block fixed effects, flat controls, and street controls. Specifically, in Column 1 I simply regress the noise level on the log listed price, and I do find a positive and statistically significant effect of noise on housing prices. This results would suggest that, the higher the noise, the higher the price. I present this counterintuitive result, because I believe it is showing the above-mentioned omitted variable bias problem. Without controlling for any other effect, this relationship is simply capturing the fact that higher noise is positively correlated with higher accessibility, thus reflecting a factor pushing up the price.

As soon as I add block fixed effects (together with year fixed effects) in column 3, the effect reduces to about one tenth of the previous coefficient. Focusing on variations within building blocks I can consider constant all the other factors that influence housing prices and net them out. In column 3, I do no include any control, and that is probably the reason why the effect is still slightly positive. In fact, by controlling for flat and street segment characteristics in my preferred specification (column 5), I do find a negative and statistically significant coefficient. The higher the level of noise exposure, the lower the price of the house. Specifically, I find that moving from one category to another of noise exposure (i.e. increasing noise level by 5db) induces a price depreciation of 1.6%. Referring to the average posted price in my estimation sample, this would correspond to a reduction of about 7,250€. Finally, in column 6 I present an even more stringent model, in which I interact block and year fixed effects, to look only at variations within building blocks in each specific year. The results are identical to those of the previous model.

Taken together, those results suggest that, once I net out the effect of confounding factors and I control for flat and street characteristics, I do find a negative effect of noise exposure on posted prices for sales. I find this effect not only to be statistically significant, but also meaningful in monetary terms. On average, flats located in street segments just one category above the average value in my estimation sample, are listed for 7,250€ less just for the higher noise exposure. This value would be much higher by comparing properties exposed to extremely low or high categories of noise, meaning that the *price of silence* in the Eixample district of Barcelona is substantial.

Table 4.2: Main results - Noise and posted price for sales. Idealista dataset.

| *Y = log price* | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Noise | 0.106$^{***}$ | 0.129$^{***}$ | 0.017$^{*}$ | -0.004 | -0.016$^{***}$ | -0.015$^{**}$ |
| | (0.007) | (0.007) | (0.007) | (0.006) | (0.006) | (0.007) |
| Floor | | | | 0.011$^{***}$ | 0.010$^{***}$ | 0.009$^{***}$ |
| | | | | (0.002) | (0.002) | (0.002) |
| Floorspace | | | | 0.001$^{***}$ | 0.001$^{***}$ | 0.001$^{***}$ |
| | | | | (0.000) | (0.000) | (0.000) |
| Quality | | | | 0.124$^{***}$ | 0.125$^{***}$ | 0.112$^{***}$ |
| | | | | (0.009) | (0.009) | (0.009) |
| N. rooms | | | | 0.054$^{***}$ | 0.054$^{***}$ | 0.049$^{***}$ |
| | | | | (0.002) | (0.002) | (0.002) |
| Air Conditioning | | | | 0.078$^{***}$ | 0.078$^{***}$ | 0.064$^{***}$ |
| | | | | (0.007) | (0.007) | (0.007) |
| Studio | | | | -0.350$^{***}$ | -0.351$^{***}$ | -0.307$^{***}$ |
| | | | | (0.030) | (0.030) | (0.032) |
| Penthouse | | | | 0.215$^{***}$ | 0.218$^{***}$ | 0.213$^{***}$ |
| | | | | (0.013) | (0.013) | (0.014) |
| Duplex | | | | 0.106$^{***}$ | 0.101$^{***}$ | 0.088$^{***}$ |
| | | | | (0.026) | (0.026) | (0.028) |
| Lift | | | | 0.208$^{***}$ | 0.202$^{***}$ | 0.192$^{***}$ |
| | | | | (0.014) | (0.014) | (0.015) |
| Boxroom | | | | 0.049$^{***}$ | 0.045$^{***}$ | 0.057$^{***}$ |
| | | | | (0.011) | (0.011) | (0.012) |
| Year construction | | | | 0.001$^{***}$ | 0.001$^{***}$ | 0.001$^{***}$ |
| | | | | (0.000) | (0.000) | (0.000) |
| Flat controls | | | | ✓ | ✓ | ✓ |
| Street controls | | | | | ✓ | ✓ |
| Block FE | | | ✓ | ✓ | ✓ | |
| Year FE | | ✓ | ✓ | ✓ | ✓ | |
| Year × Block FE | | | | | | ✓ |
| R2 | 0.015 | 0.080 | 0.470 | 0.584 | 0.588 | 0.673 |
| N | 16,331 | 16,331 | 16,331 | 16,331 | 16,331 | 16,331 |

*Notes:* The dependent variable is the logarithm of posted prices for sales listed on the Idealista website in December. The different columns include or not flat controls, street controls, year fixed effects, and block fixed effect respectively. Standard errors are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

Differently, in Table 4.3, I look at the same relationship for rentals. Here, in columns 5 and 6, including all fixed effects and controls, I do not find a statistically significant result. According to those results, street noise does not seem to be capitalised into listed prices for rents. I believe this might be due to different reasons. First, the group of people renting and buying might be different. I expect young people and students, much more likely to rent than buying, to be less worried about noise exposure. Taking the example of students, it is likely that they would prefer to live in areas attractive for leisure and nightlife, which are also more noisy[17]. In addition, in a real estate market as the one of Barcelona, rents are much more flexible than sales, so that renters move much more often than buyers. Bought flats are much less liquid than other assets compared to other countries, so that buying is usually a long-term decision. Finally, the rental market of Barcelona is really tight, meaning that renters have less bargain power to lower the price since supply of rents tend to be much lower than demand. All these possible explanations for whether rental prices does not seem to reflect noise exposure can be complemented with the fact that my data reflect posted prices and not transaction prices.

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[17] In the next subsection I will show that my results seems to be highly driven by nightlife noise.

Table 4.3: Main results - Noise and posted price for rentals. Idealista dataset.

| *Y = log price* | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Noise | $0.048^{***}$ | $0.076^{***}$ | $0.019^{***}$ | 0.005 | 0.005 | 0.003 |
| | (0.005) | (0.004) | (0.004) | (0.003) | (0.003) | (0.004) |
| Floor | | | | 0.001 | 0.001 | 0.001 |
| | | | | (0.001) | (0.001) | (0.001) |
| Floorspace | | | | $0.005^{***}$ | $0.005^{***}$ | $0.005^{***}$ |
| | | | | (0.000) | (0.000) | (0.000) |
| Quality | | | | $0.172^{***}$ | $0.172^{***}$ | $0.158^{***}$ |
| | | | | (0.016) | (0.016) | (0.017) |
| N. rooms | | | | $0.013^{***}$ | $0.013^{***}$ | $0.016^{***}$ |
| | | | | (0.002) | (0.002) | (0.003) |
| Air conditioning | | | | $0.132^{***}$ | $0.132^{***}$ | $0.125^{***}$ |
| | | | | (0.004) | (0.004) | (0.004) |
| Studio | | | | $-0.205^{***}$ | $-0.206^{***}$ | $-0.194^{***}$ |
| | | | | (0.012) | (0.012) | (0.012) |
| Penthouse | | | | $0.054^{***}$ | $0.054^{***}$ | $0.051^{***}$ |
| | | | | (0.008) | (0.008) | (0.008) |
| Duplex | | | | $0.167^{***}$ | $0.166^{***}$ | $0.147^{***}$ |
| | | | | (0.021) | (0.021) | (0.022) |
| Lift | | | | $0.101^{***}$ | $0.100^{***}$ | $0.087^{***}$ |
| | | | | (0.007) | (0.007) | (0.007) |
| Boxroom | | | | $-0.018^{**}$ | $-0.018^{**}$ | $-0.017^{*}$ |
| | | | | (0.008) | (0.008) | (0.009) |
| Year construction | | | | $0.000^{***}$ | $0.000^{***}$ | $0.001^{***}$ |
| | | | | (0.000) | (0.000) | (0.000) |
| Flat controls | | | | ✓ | ✓ | ✓ |
| Street controls | | | | | ✓ | ✓ |
| Block FE | | | ✓ | ✓ | ✓ | |
| Year FE | | ✓ | ✓ | ✓ | ✓ | |
| Year × Block FE | | | | | | ✓ |
| R2 | 0.006 | 0.237 | 0.473 | 0.720 | 0.720 | 0.772 |
| N | 19,187 | 19,187 | 19,187 | 19,187 | 19,187 | 19,187 |

*Notes:* The dependent variable is the logarithm of posted prices for rentals listed on the Idealista website in December. The different columns include or not flat controls, street controls, year fixed effects, and block fixed effect respectively. Standard errors are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

### 4.4.2 Heterogeneous analysis

To further extend my results, I present an heterogeneous analysis differentiating the noise by time of the day. Results are presented in Table 4.4. Here, I am estimating three versions of the model of equation 4.1 in which I replace the total noise with different variables indicating the noise level for each street segment at day (7am-9pm), evening (9-11pm), and night (11pm-7am) respectively[18]. Specifically, I am looking at sales, for which I find a negative effect in my baseline result.

As before, my preferred specification is the one in which I include year and block fixed effects, and the full set of flat and street controls (column 5). The results of the three different panels suggest that evening noise (9-11pm) and night noise (11pm-7am) drive the effect. In fact, I do not find an effect for daily noise, while I do find negative coefficient for noise registered between 9pm and 7am. I believe that a buyer, especially in a period of time in which working from home was not common, would care more about noise when she is actually at home. I can think about workers, who are out all day, but care about noise when they get back home to rest at evening and night hours. In general this can apply also for non-workers, since it is likely that they spend more time in the dwelling at night compared to daytime.

In general those results can be explained by the fact that people tolerate daily noise better than night noise. Not coincidentally, the European Environmental agency defines different thresholds for noise to be considered high: 55dB for daily noise, and 50dB for night noise (European Environmental Agency, 2020a). This would reflect different perceptions, and thus tolerances, of people to noise exposure.

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[18] I estimate three separate regressions instead of including all the three variables together in the same regressions because the three different explanatory variables are highly correlated between each other.

Table 4.4: Heterogeneous results by time of the day

| *Y = log price* | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Panel A - Day (7am - 9pm)** | | | | | | |
| Noise | $0.080^{***}$ | $0.122^{***}$ | $0.023^{***}$ | 0.005 | -0.008 | -0.005 |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) |
| **Panel B - Evening (9-11pm)** | | | | | | |
| Noise | $0.113^{***}$ | $0.134^{***}$ | $0.022^{***}$ | 0.004 | $-0.014^{**}$ | -0.010 |
| | (0.007) | (0.007) | (0.007) | (0.006) | (0.006) | (0.007) |
| **Panel C - Night (11pm - 7am)** | | | | | | |
| Noise | $0.135^{***}$ | $0.141^{***}$ | $0.021^{***}$ | 0.003 | $-0.015^{**}$ | $-0.013^{*}$ |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) |
| Flat controls | | | | ✓ | ✓ | ✓ |
| Street controls | | | | | ✓ | ✓ |
| Block FE | | | ✓ | ✓ | ✓ | |
| Year FE | | ✓ | ✓ | ✓ | ✓ | |
| Year × Block FE | | | | | | ✓ |
| N | 16,331 | 16,331 | 16,331 | 16,331 | 16,331 | 16,331 |

*Notes:* The dependent variable is the logarithm of posted prices for sales listed on the Idealista website in December. The different panels refer to noise exposure at different hours of the day: 7am-9pm in Panel A, 9-11pm in Panel B, and 11pm-7am in Panel C. The different columns include or not flat controls, street controls, and year and block fixed effect respectively. Standard errors are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

In addition, I present results for a second heterogeneous analysis based on noise sources. Specifically, for the noise data of 2017, the total noise can be decomposed by the source type. I can distinguish between traffic, railway, industrial, recreational, and pedestrian noise. I exclude from the analysis railway and industrial noise, since I do not observe variations in the Eixample district and the noise exposure from those sources is negligible in this district. Differently from the previous results, since this information is available only for the year 2017, I am running a simple cross-sectional estimation[19]. Specifically, I estimate the following model:

$$\begin{aligned} log(Price_{isj}) =& \beta_0 + \beta_1 Noise_s^{Traffic} + \beta_2 Noise_s^{Pedestrian} + \\ &+ \beta_3 Noise_s^{Recreational} + \beta_4 X_i + \beta_5 Z_s + \omega_j + \varepsilon_{isj} \end{aligned} \tag{4.2}$$

Where $log(Price_{ijs})$, $X_i$, $Z_s$, and $\omega_j$ are defined exactly as in the previous equa-

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[19] Consistently as before, I include all posted sales for the years 2016 and 2017, assuming that the noise of each street segment in the Eixample district did not change systematically one year to the other.

tion 4.1. Differently than before, I include different variables of noise referring to different sources[20], to understand which source is driving the results. It is worth to notice that no year fixed effects are included, since I am estimating a cross-sectional model with 2017 noise values only.

The results are presented in Table 4.5. My preferred specification is that of column 4, in which the model is fully specified[21]. All the effects are statistically significant. Traffic and recreational noise lead to price depreciation in listed prices for sales. The coefficient for recreational noise, thus referring to nightlife, is about eight times higher than those of traffic. This result goes in line with those of the previous heterogeneous analysis, according to which evening and night noise are driving the baseline results. Also by performing a t-test I can consider those two effect to be different, with recreational noise to have a more negative effect on prices than traffic noise. On the one hand, keeping everything else constant, an increase of about 5dB in recreational noise exposure, i.e. moving from one category to the other, induces a 5.4% reduction in the posted prices for sales. On the other hand, the price reduction associated with traffic noise is just 0.7%.

Differently, the effect related to pedestrian noise is positive and statistically significant. Specifically, keeping everything else constant, an increase in pedestrian noise exposure would induce an increase in posted prices of about 3.8%. I believe this result is somehow expectable. Pedestrian noise can be considered more as an amenity than a disamenity. Furthermore, higher pedestrian noise might be correlated with less noise exposure from other unpleaseant sources. Finally, in my model this noise source might also reflect the fact that the flat is in a more people-friendly street segment, with less space for cars, and this could result as well in a price increase for the house (Koster et al., 2019; Yoshimura et al., 2022)[22].

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[20] Given that the correlations between the variables measuring the different sources are not that high, I can include them all together in the same regression.

[21] Here I include block fixed effects, flat and street segment controls.

[22] The two papers do not look directly at housing prices. However, their results suggest effects than can be indirectly capitalised into the real estate market for houses. Koster et al. (2019) shows that the more pedestrians lead to an increase in commercial rents and vacancies. Differently, Yoshimura et al. (2022) looks at the effect of converting street use from vehicles to a walkable environment on the revenues of retail stores, finding a positive relationship.

Table 4.5: Heterogeneous results by noise source

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Street traffic noise (Tot) | 0.027$^{***}$ | 0.010$^{**}$ | 0.001 | -0.007$^{*}$ |
| | (0.003) | (0.004) | (0.004) | (0.004) |
| Pedestrian noise (Day) | 0.102$^{***}$ | 0.034$^{**}$ | 0.032$^{**}$ | 0.038$^{***}$ |
| | (0.010) | (0.015) | (0.014) | (0.014) |
| Recreational noise (Night) | 0.094$^{***}$ | -0.063$^{***}$ | -0.059$^{***}$ | -0.054$^{***}$ |
| | (0.006) | (0.012) | (0.011) | (0.011) |
| Flat controls | | | ✓ | ✓ |
| Street controls | | | | ✓ |
| Block FE | | ✓ | ✓ | ✓ |
| R2 | 0.060 | 0.495 | 0.583 | 0.587 |
| N | 7,894 | 7,894 | 7,894 | 7,894 |

*Notes:* Cross-sectional regression with *Idealista* listings for sales posted in Decembers 2016 and 2017. I split the noise variable between street traffic noise, pedestrian noise, and recreational noise. I exclude railway and industry noise, since I do not observe variations in the Eixample district. Flat and street controls include the same variables of the previous estimations. Standard errors are in parenthesis. $^{*}$, $^{**}$ and $^{***}$ indicates significant at 1, 5, and 10 percent level, respectively.

## 4.5 Conclusions

Noise pollution is one major urban negative externality. Long-term exposure to noise is proven to have negative health effects. It leads to stress, headache, sleep disturbance, and by increasing blood pressure it rises the risk of arterial hypertension, hearth failure and stroke. Nevertheless, urban residents seem to be less worried about noise than about other negative externalities such as traffic and pollution. As a consequence, politicians tend to implement attenuation policies more than really try to solve the problem and internalize the external cost of noise. For example, changing street pavements or noise barriers are common policies implemented to reduce street noise.

I wonder whether people care or not about noise. I believe housing prices reflect residents' preferences. Thus, in my paper I investigate whether street noise is capitalised into housing prices. In other words, I am interested in quantifying the price discount needed for one person to accept a higher level of noise exposure.

I base my empirical analysis in Barcelona. Specifically, I take advantage of a peculiar architectural feature to solve an omitted variable bias problem that is common when quantifying the effect of interest. A priori, being noise a disamenity, I would expect a negative effect on house prices. Nevertheless, street noise is highly likely correlated with other variables which positively affect prices (i.e. accessibility). Thus, it is really challenging to isolate the effect of interest and quantify a causal effect.

The grid pattern and the square blocks of the Eixample neighborhood provide a good setting to exploit variation in street noise intensity between properties belonging to the same building block but exposed to different noise levels. By including building block fixed effects, I exploit variations in noise levels within 150x150 square meters areas, for which all other factors which influence house prices can be safely assumed to be constant.

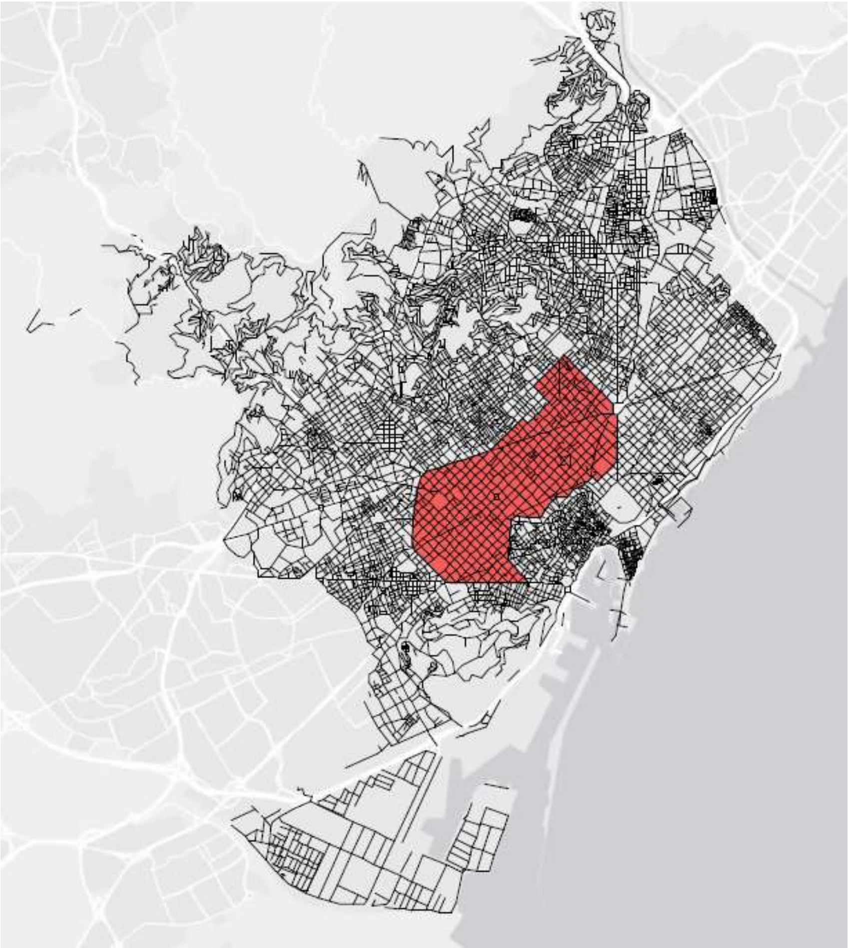
I do find the *price of silence* to be sizable in the Eixample district of Barcelona. Specifically, I find posted prices for sales to be 1.6% lower when exposed to a higher category of noise exposure, i.e. an increase of 5dB. In other words, for the average listing in my regression sample, I find that this noise increase leads to a reduction of about 7,250€ in posted prices for sales. Differently, I do not find an effect for rents. I believe different reasons could explain this difference between sales and rents. First, the people buying or renting might be systematically different. Second, in a real estate market as the one of Barcelona, rents are much more flexible than sales, so that renters move much more often than buyers. Bought flats are much less liquid than other assets compared to other countries, so that buying is usually a long-term decision. Finally, the rental market in Barcelona is really tight, meaning that renters do not have much bargain power to lower the price in case if high noise exposure.

I run a couple of heterogeneity analyses and I find that evening and night noise are more relevant than daily noise for the price depreciation. Those results are in line with the second heterogeneity analysis I perform, according to which the negative effect on posted prices for sales is eight times larger for recreational noise, i.e. nightlife noise, than the one related to traffic. Differently, pedestrian noise is found to have a positive effect of housing prices.

I conclude by stating that policies directed to reduce noise in dense urban areas could lead to important welfare benefits. On the one hand, given the negative health consequences of long-term noise exposure, reducing noise would reduce health disorders and thus the expenditures directed to them. On the other hand, in this chapter I show that higher noise exposure has a relevant monetary effect on housing prices. Thus, by reducing noise, a city could also increase the value of an important asset such as housing.

## 4.6 Appendix

Figure A4.1: Eixample District



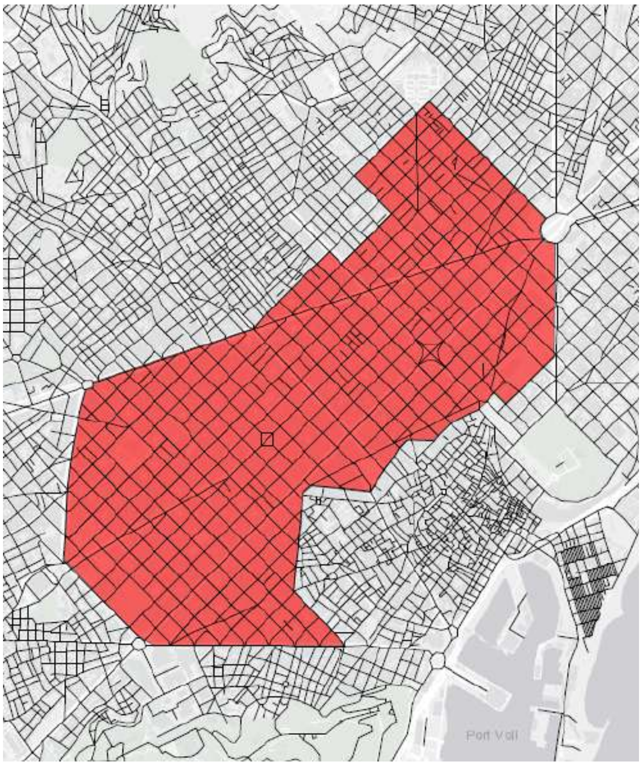
*Source*: Open Street Map.

Table A4.1: Residents' socio-economic characteristics: Eixample vs. rest of the city

| | Eixample | | | | Rest of the city | | | |
|---|---|---|---|---|---|---|---|---|
| AEB characteristics | Mean | Median | Sd | N | Mean | Median | Sd | N |
| Population density (2009) | 37,430.75 | 38,367.25 | 14,678.70 | 36 | 30,927.74 | 31,435.48 | 19,506.02 | 197 |
| Relative HH Income (2008) | 116.13 | 112.93 | 34.34 | 36 | 103.11 | 89.66 | 54.10 | 197 |
| % Foreign (2009) | 19.08 | 18.76 | 3.22 | 36 | 18.43 | 14.84 | 10.93 | 197 |
| % Pop. 15-44 yo (2009) | 42.27 | 42.38 | 2.26 | 36 | 43.28 | 42.01 | 5.75 | 197 |
| % Unemployed (2011) | 5.65 | 5.75 | 0.51 | 36 | 6.80 | 6.74 | 1.87 | 197 |
| % w/ Primary studies (2009) | 14.84 | 15.12 | 3.12 | 36 | 20.72 | 20.91 | 7.54 | 197 |
| % w/ University studies (2009) | 29.21 | 28.06 | 5.97 | 36 | 19.18 | 17.40 | 10.09 | 197 |
| % w/o Studies (2009) | 7.61 | 7.71 | 1.32 | 36 | 10.97 | 10.23 | 3.97 | 197 |

*Notes:* AEB stands for basic statistical areas. Those are units slightly bigger than census tracts. There are 233 AEB in the city of Barcelona. The Eixample district contains 36 different AEBs. The statistics in the table are computed as average of the specific indicator for all AEBs in the Eixample and in the rest of the city.

Figure A4.2: Orthogonal structure of roads in the Eixample district



*Source*: Open Street Map.

Figure A4.3: Bird's-eye view of the Eixample District

*Source*: Astelus.com

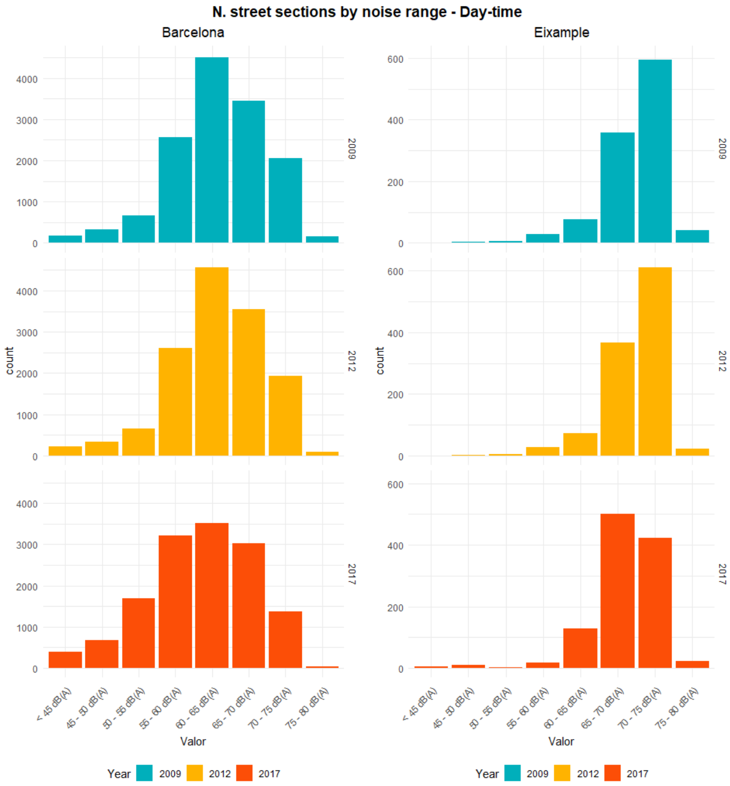
Figure A4.4: Example of movable sensor for short-term noise readings.

*Source*: Barcelona City Council (2017)

Figure A4.5: Example of fixed sensor for long-term noise readings.

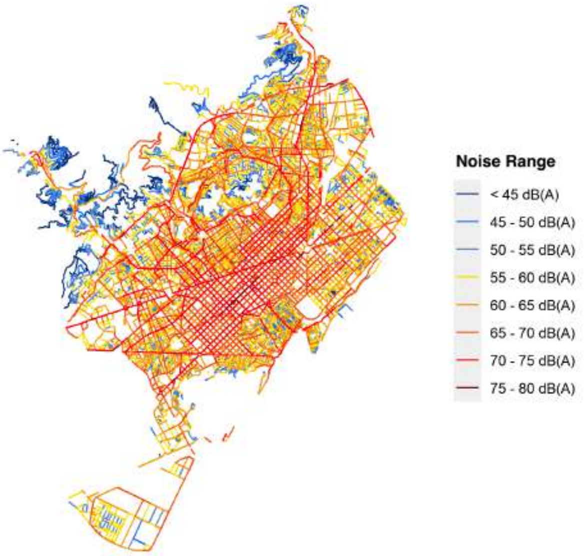
*Source*: Barcelona City Council (2017)

Figure A4.6: Distribution of street segments by categories of daily noise levels.



*Source*: own elaboration of Barcelona's City Council data.

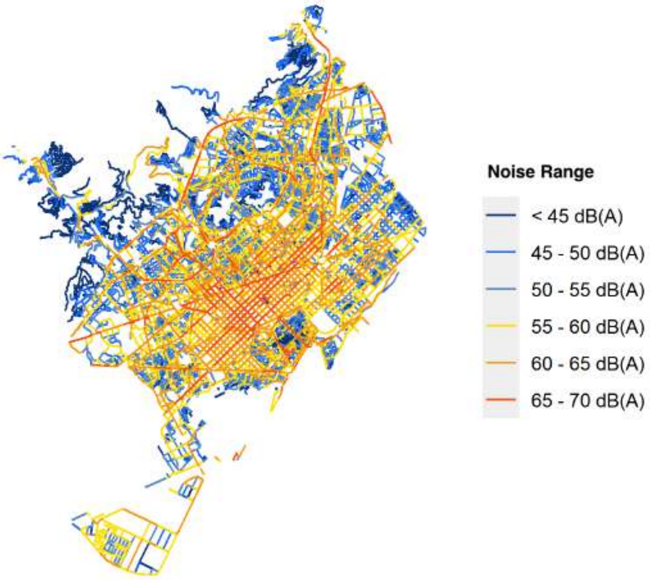
Figure A4.7: 2017 Daily noise



*Source*: City Council of Barcelona.

*Note*: The map shows the 2017 noise range of each street section during the day, from any source.

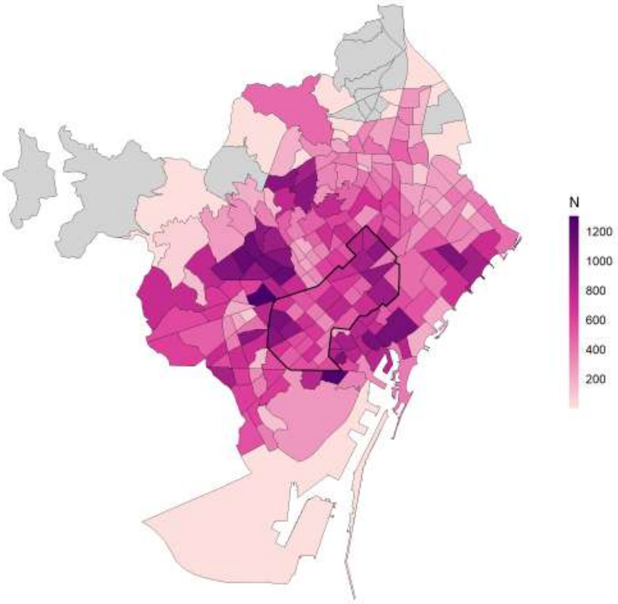
Figure A4.8: 2017 Night noise



*Source*: City Council of Barcelona.

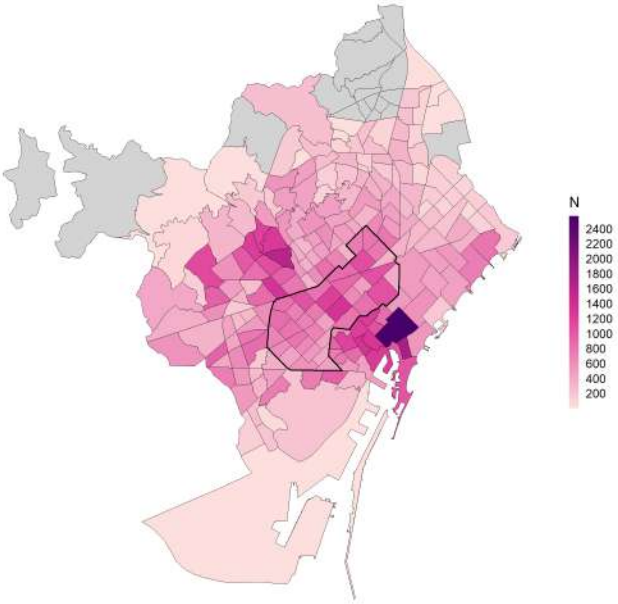
*Note*: The map shows the 2017 noise range of each street section at night, from any source.

Figure A4.9: Number of sales listed in Idealista



*Source*: Idealista data.

Figure A4.10: Number of rents listed in Idealista



*Source*: Idealista data.

# 5 Concluding remarks

Cities have been growing at an incredible peace during the last century. Cities attract people because they offer them both high production and consumption benefits. Nevertheless, denser urban areas are also characterised by crowding and urban costs. In this dissertation I aim at documenting costs and benefits of cities through the lenses of transport economics. The organization of economic activity across space depends crucially on the transportation of goods and people. As a consequence, it should not surprise if the transport sector as a whole represents about five percent of gross domestic product (GDP) in Western countries, and if transport networks account for some of the public largest investments ever made (Redding and Turner, 2015). The benefits that come along with better connected places can justify this public expenditure. Faster connections and lower transportation costs boost productivity. In addition, higher accessibility makes people more closer and foster agglomeration economics. At the same time, higher availability of transport infrastructure also comes with some costs. Motorized traffic generates some of the heaviest urban costs: traffic congestion, air pollution, and noise. They represent transport-related negative externalities and have negative health consequences. This PhD disseration seeks to contribute and quantify both benefits and cost related to transportation.

In the second chapter, *Paving the way to modern growth: the Spanish Bourbon roads*, I estimate the potential impact that the improvements in accessibility associated to the construction of the new road network had on the population growth of Spanish municipalities between 1787 and 1857. Besides contributing to the debate on whether the Spanish economy was stagnant or dynamic before the construction of the railway network, this paper contributes to the literature by being the first one quantifying the effect of interest in a pre-industrialized economy. Specifically, I study the new radial network of paved-roads constructed by the Bourbons' dynasty in order to connect Madrid with the most important cities in Spain. In order to study the potential growth effects of these new roads, I study to what extent the increase in market access associated to the new network that took place between 1787 and 1855 translated into higher demographic dynamism at the local level. I find that the increase in market access associated to road accessibility had a substantial effect on local population growth. The impact was substantially higher on the municipalities that had a more diversified occupational structure.

Determining the extent to which the estimated effect reflects changes in the level of economic activity versus a reorganization of the existing activity is a fundamental question. Besides the positive effect on population growth for the newly connected municipalities, I find the effect to be negative in municipalities close but without direct access to the roads. I interpret these findings as evidence of a process of rural-to-rural migration due to the new roads. The changes introduced by the new paved roads in the structure of transport costs were sufficient to provoke a new population equilibrium, in line with the predictions of the urban economics literature. Municipalities with higher market access were able to draw population from neighbouring areas, probably attracted by the capacity of the roads to stimulate the development of new activities along the route. This would indicate that roads triggered a process of spatial reorganization of population, with short-distance movements of population towards the vicinity of the new roads.

In chapter 3, titled *"Low emission zones and traffic congestion: evidence from Madrid Central"*, I study one of the major transport-related urban costs. Traffic impose non-pecuniary costs on cities due to the generation of air pollution, accidents and fatalities, delays, stress and road rage, and economic losses. Specifically, I analyze whether the implementation of a low emission zone in the city centre of Madrid had an effect of traffic dynamics in the city. Low emission zones are areas to which the access is restricted for the most polluting vehicles. More precisely, it is a quantity measure tackling the externalities through the extensive margin (type of car driven). This measure has been extensively adopted in Europe and has been found to be effective in reducing pollution. However, there is a lack of exhaustive evidence about their effect on traffic and car use. My paper is the first one estimating the effect of LEZ on traffic.

The main result of this chapter suggest that place-based policy should be carefully designed to avoid undesired consequences due to people's behavioural responses. Specifically, spatial spillovers should be considered when designing similar schemes, in order to ensure that the whole city benefits from the measure, and not just the restricted area itself. In fact, my results suggest that the implementation of *Madrid Central* led to an overall small increase in traffic for the whole city of Madrid. Nevertheless, this average result hides important spatial patterns in terms of traffic dynamics. In fact, the implementation did reduce traffic in the restricted area. The time-based model shows an average reduction of around 8.1% in the number of vehicles per sensor/hour and around 8.7% of traffic load in the restricted area. This traffic relief for the central district is offset by an overall increase in transit in the other areas of the city, which I interpret as displacement effect. Using heterogeneity analyses, I further identify which of the city's streets are most negatively affected by the displacement, as well as showing that that the reduction in the city centre gradually decreases over time and eventually disappears seven months after the implementation. I find different reasons to explain this temporal evolution, ranging from announcements by local politicians to the renewal's of the vehicles fleet triggered by the policy, with a shift towards cleaner and exempted cars. Finally, I look at potential changes in commuting and I identify a switch to public transport for commutes directed to the restricted area and rerouting of trips for destinations outside Madrid Central as two of the possible mechanisms explaining these results.

In the fourth chapter of my dissertation, *the price of silence*, I study street noise pollution. Specifically, I analyze whether street noise is capitalised into housing prices, using housing transactions and noise data from Barcelona. Previous evidence suggests a negative relationship between noise exposure and housing values. However, hedonic price models do not establish causalities. I combine hedonic price estimates with a fixed-effect model and I benefit from a specific architectural feature. With this empirical framework, I am able to solve an omitted variable bias problem. In fact, when it comes to estimate the effect of interest, the presence of unobservable characteristics correlated with noise would bias the result. My paper is one of the first causally estimating the capitalization of street noise into housing prices. Furthemore, I contribute to the literature by showing differences between sales and rentals markets, and by presenting interesting heterogeneous results by time of the day and noise source.

Barcelona is a relevant case study since it is one of the most noisy cities in Europe. The Public Health Agency of Barcelona (APSB) suggests that the 57% of the residents in the city is exposed to high noise levels. In addition, traffic noise pollution is responsible for 130 yearly deaths (related to cardiovascular diseases); 210,000 people suffer severe psychological or social affectation, and 60,000 people suffer severe sleep disorders. Besides this specific figures, I exploit a peculiar architectural feature of the city: the homogeneous and orthogonal structure of the buildings of its Eixample district.

My strategy hinges on the variation in noise intensity at very granular levels. Specifically, I benefit from the orthogonal structure and the homogeneous square blocks of the Eixample district, by looking at variations within the same building block (i.e. areas of about 150x150 square meters). I exploit variations between properties belonging to the same building block but exposed to different noise levels. I do so by including in my model building blocks fixed effects. Focusing on variations at a really granular level, I can consider constant all the other unobserved characteristics that influence housing prices and net their effect out. Furthermore, the people living in this district are quite homogeneous in terms of socio-demographic characteristics, thus I can exclude the existence of price effects on housing related to sorting and neighborhood composition.

I do find the *price of silence* to be sizable in the Eixample district of Barcelona. Specifically, my results suggest that street noise leads to a price depreciation of 1.6% for sales' posted prices. In other words, referring to the average price in my estimation sample, I find that moving from one category to another of noise exposure (i.e. increase of 5db) induces a price reduction of about 7,250€. Differently, I do not find an effect for rents. Furthermore, looking at heterogeneous results I find that evening and night, more than daily, are driving the price depreciation. Those results are in line with the second heterogeneity analysis I perform, according to which the negative effect on posted prices for sales is eight times larger for recreational noise, i.e. nightlife noise, than the one related to traffic. Differently, pedestrian noise is found to have a positive effect of housing prices.

To conclude, I want to summarise three main lessons than can be drawn from this dissertation.

(i) First, transportation investments should be thought having in mind all their potential consequences. For example, in the second chapter of this dissertation I show that the increased accessibility determined by the construction of a new roads network had a substantial effect on local population growth in $19^{\text{th}}$ century's Spain. I also show that this effect is the result of a relocation process more than general growth. In fact, the construction of the new radial road network determined the growth of the newly connected municipalities at the expenses of neighbouring ones. On the one hand it is true that increased accessibility lead to city growth. On the other hand, this local growth happened at the expenses of other municipalities. Thus, it is important to recognise that costly transport investments, i.e. the construction of a national road network, reshape the existing population equilibrium into a new one, and determine winners and losers.

(ii) The second lesson to be learned is that transportation policies should be carefully designed in order to minimize undesired consequences. Economists tend to evaluate a place-based policies on their impact on the area of interest. However, transport policies are likely to generate changes beyond the border of the treated area. Spatial spillovers between areas are a serious threat to identifying the real effect of a specific policy. Studies not accounting for spatial spillovers might overestimate or underestimate the effect of the policy itself. Moreover, not considering these undesired effects may lead to a misjudgment of the policy itself. In the third chapter of my dissertation, I evaluate a place-based policy restricting traffic to polluting vehicles in the city centre of Madrid. My findings suggest that Madrid Central has been successful in reducing the number of vehicles in the city centre. It has also been an incentive to renew the vehicle fleet with cleaner vehicles. As such, the policy has probably achieved its original aim of abating pollution. Nevertheless, the most important result of the paper is the displacement effect towards unrestricted areas, a relevant and undesired consequence of the implementation. This factor should be considered when designing such schemes, in order to ensure that the whole city benefits from the measure, rather than just the restricted area itself.

(iii) Finally, I suggest that reducing transport-related negative externalities in dense urban areas would lead to important welfare benefits. Recently, sustainability and well-being became central topics for cities' governors. There is a upsurge of policies directed towards innovative and sustainable urban mobility. The main target of this policies is to curb transport related negative externalities such as pollution, noise, and traffic. The question that economists should answer is whether those are good policies, or instead mayors are just thinking about their political support. The fourth chapter of this dissertation suggests that policies intended to reduce noise would lead to important welfare benefits. In this specific case, I can observe direct benefits through reductions in negative health consequences, as well as monetary benefits through an increase in the value of housing prices.

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