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LOW EMISSION ZONES AND TRAFFIC CONGESTION: EVIDENCE FROM MADRID CENTRAL [*]

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ABSTRACT: The aim of this paper is to shed light on the effect of Low Emission Zones (LEZs) on traffic. LEZs are areas in which access is restricted for the most polluting vehicles. They have been found to be effective in reducing pollution, while the expected effect on traffic is not clear. Using high-frequency granular data on traffic for the city of Madrid, I analyse whether LEZ schemes are effective in reducing traffic within the area of implementation and whether they generate a displacement effect. Taking advantage of the exogeneity of the implementation timing, I develop a pre/post analysis based on time. Results suggest a reduction in traffic inside the restricted area and a displacement to all the other areas of the city. I find a switch to public transport for commutes directed towards the restricted area and rerouting of trips for destinations outside Madrid Central to be two of the possible mechanisms explaining these results. The reduction in transit inside the restricted area gradually decreases over time and disappears after 7 months. This is consistent with the renewal of the vehicles' fleet with unrestricted and cleaner vehicles generated by the policy.

JEL Codes: R41; R48; H23
Keywords: Traffic calming policy, low emission zone, traffic, cities, displacement

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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 (2020), the transport sector is responsible for more than 40% of emissions related to air pollution, with negative effects on health (Anderson, 2020; Chay & Greenstone, 2003; Currie & Walker, 2011; Knittel, Miller, & Sanders, 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 (Green, Heywood, & Navarro, 2016; Li, Graham, & Majumdar, 2012), 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 ubran 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 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, Gallego, & Montero, 2020). Congestion tolls are effective in internalizing the external cost of traffic (Börjesson, Eliasson, Hugosson, & Brundell-Freij, 2012; Carnovale & Gibson, 2015; Herzog, 2020; Keat Tang, 2018). 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 & Verhoef, 2008). Yet tolling is rarely used in practice and low emission zones are more commonly implemented, due to their higher acceptability (Bernardo, Fageda, & Flores-Fillol, 2021).

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 (Boogaard et al., 2012; Ellison, Greaves, & Hensher, 2013; Gehrsitz, 2017; Malina & Scheffler, 2015; Sarmiento, Wagner, & Zaklan,

[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, Pasidis, & Viladecans-Marsal, 2022). 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 & Lindsey, 2011).

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

2021; Wolff, 2014)[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 (Börjesson et al., 2012; Isaksen & Bjorn, 2021; Percoco, 2014; Wolff, 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, Adler, Liberini, and van Ommeren (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 30$^{\text{th}}$ November 2018. Despite its small size (less than 1% of the total area of Madrid) and its primary intention of 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

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

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

impact analysis (Brodersen, Gallusser, Koehler, Remy, & Scott, 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 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 (2018), 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 (Boogaard et al., 2012; Ellison et al., 2013; Gehrsitz, 2017; Malina & Scheffler, 2015; Sarmiento et al., 2021; Wolff, 2014), 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, Gil, Holub, and Uriz-Uharte (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 (2018) 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

[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.

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.
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.

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 30$^{\text{th}}$ 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].

[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%) 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. Further-

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 A.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 we 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. 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 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

more, 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.

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

(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 1, 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.

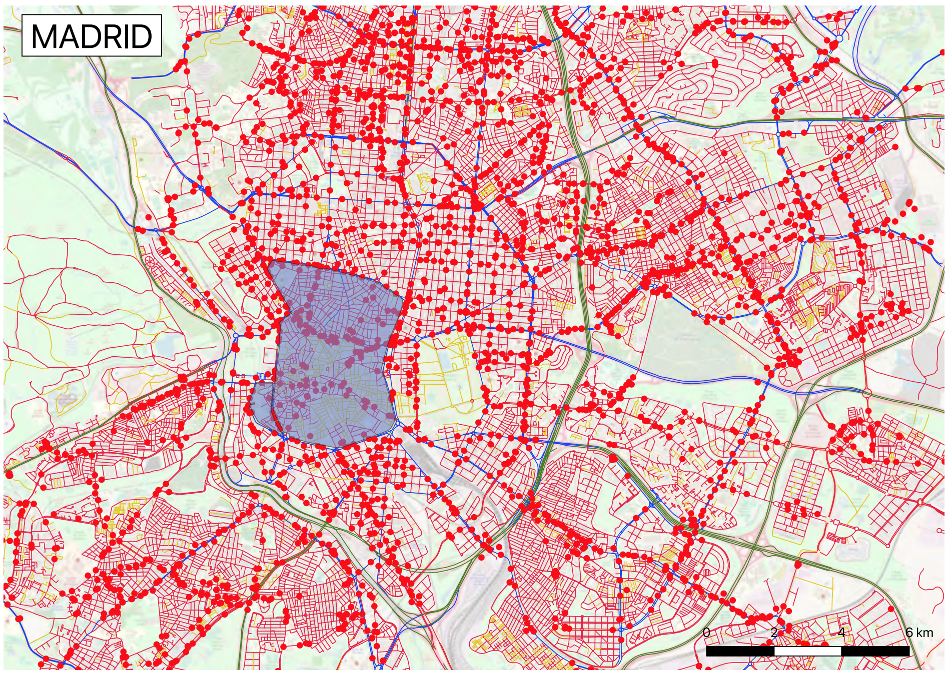

Figure 1: Madrid Central area and traffic measurement points.

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 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)

[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.

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 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 issure. Figure A.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 30$^{\text{th}}$ November 2018.

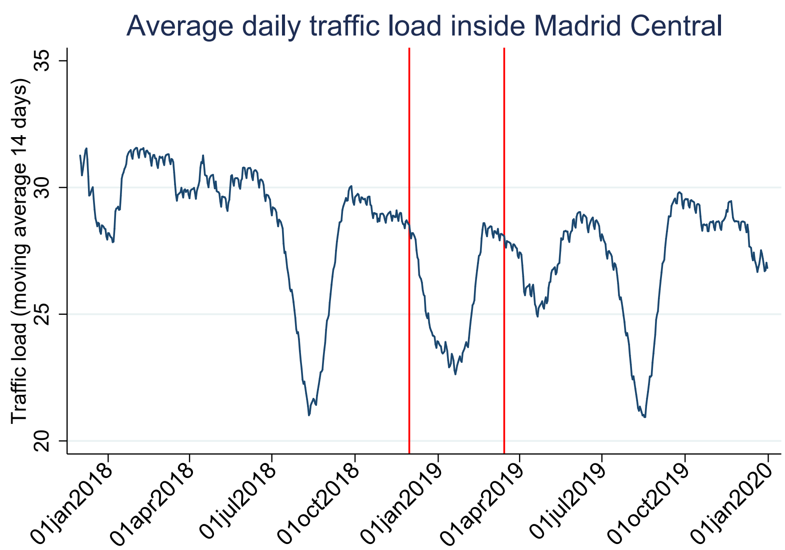

Figure 2: Temporal evolution of traffic load within *Madrid Central*.

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.

[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

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 A.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]. 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.

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 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 & 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.

[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/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.

[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.

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 30$^{\text{th}}$ 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. 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 (Carnovale & Gibson, 2015; Davis, 2008; Percoco, 2014, 2020).

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

$$log(Y_{m,t}) = \alpha_0 + \beta_1 Implementation_t + \beta_2 X_t + \beta_3 timetrend_t + \theta_m + \tau_t + \epsilon_{p,t} \tag{1}$$

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

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

[24]Results hold and are available upon request.

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. θ_m represents the sensors fixed-effects and τ_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. 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:

$$log(Y_{m,t}) = \alpha_0 + \beta_1(Implementation_t * Area_m) + \beta_2 X_t + \beta_3 timetrend_t * Area_m + \theta_m + \tau_t + \epsilon_{p,t} \quad (2)$$

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 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.

[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.

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

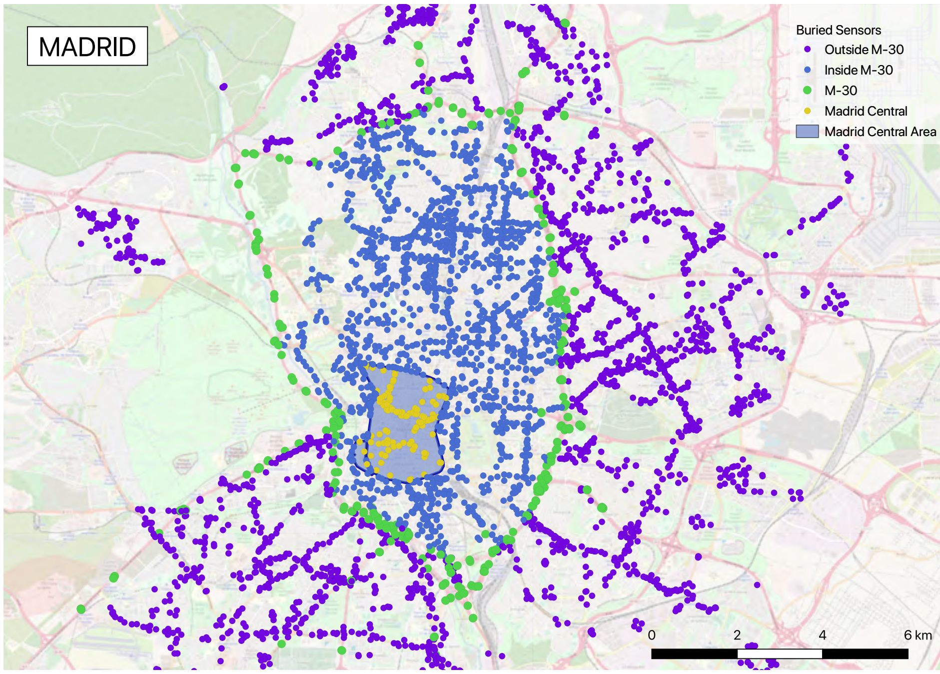

Figure 3: Visualisation of the four groups of the variable *Area*: inside Madrid Central, between Madrid Central and the M30 freeway ring road, on the M30 freeway and outside M30

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 sensor 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

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

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 individual 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, Tull, and Atwater (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.

5. Results

5.1. Time-based panel fixed effect model

Before presenting the estimations of equation 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 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 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, we 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 inves-

[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.

tigate 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* 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 1: Overall effect of *Madrid Central* on traffic in Madrid

	(1) log intensity	(2) log occupancy	(3) 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$\times T$	43145211	42036903	38567144
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 2, I present the results of equation 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.

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 (2018) and Percoco (2020) examine the London congestion charge and find evidence of a displacement effect for traffic and pollution respectively.

[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.

Table 2: Effect of *Madrid Central* on traffic by areas of the city

	(1) log intensity	(2) log occupancy	(3) log load	(4) log average speed
Implementation × MC	-0.081***	-0.177***	-0.087***	
	(0.012)	(0.016)	(0.013)	
Implementation × InsideM30	0.022***	0.026***	0.020***	
	(0.006)	(0.007)	(0.006)	
Implementation × M30	0.013**	0.019***		-0.007***
	(0.005)	(0.007)		(0.003)
Implementation × 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 $\times T$	43145211	42036903	38567144	4580168
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.

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 A.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 A.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 we 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.

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 4) presents the individual and global results for intensity. Figure A.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 A.5 and A.6, I map the sensor-specific estimated causal impacts for intensity. Figure A.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 A.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

[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).

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 instead of all those between 7am and 10pm.

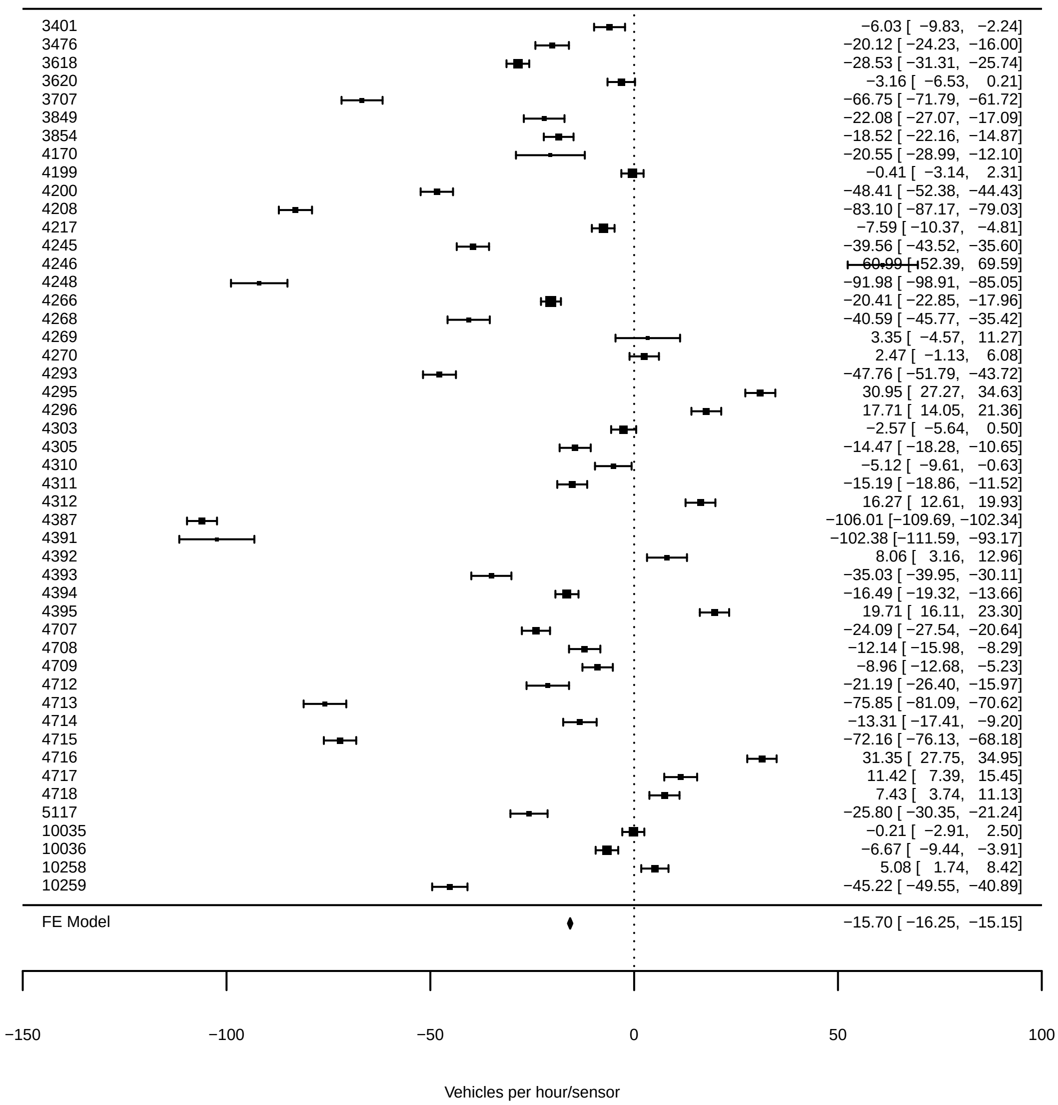

Figure 4: Results of meta-regression (FE model) for causal impact estimations of intensity

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.

5.4. Heterogeneous analysis

As mentioned earlier, the results presented in Table 1 are an average effect hiding different spatial patterns. In fact, the results of Table 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 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:

$$log(Y_t) = \alpha_0 + \beta_1 Implementation_t + \beta_2 X_t + \beta_3 timetrend_t + \tau_t + \epsilon_t \tag{3}$$

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 β_1 of estimated coefficients summarises the change in traffic conditions for each specific sensor between the two periods. Figures 5 in the text and A.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 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 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

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

concentration of increases in traffic compared to the pre implementation period.

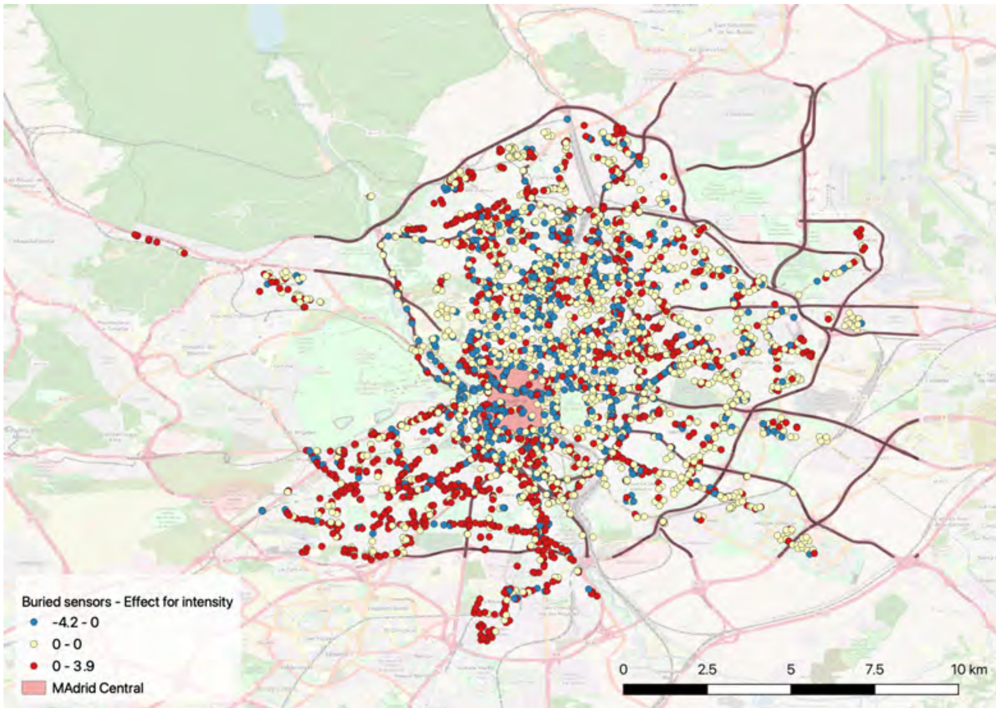

Figure 5: Sensor-specific time-series regression results for log intensity.

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:

$$log(Y_{m,t}) = \alpha_0 + \beta_1(Implementation_t * Month_t) + \beta_2 X_t + \beta_3 timetrend_t + \theta_m + \tau_t + \epsilon_{p,t} \quad (4)$$

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

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

first line of Table 2[37]. Figure 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 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

[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).

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

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 A.8 in the Appendix shows exactly the same temporal evolution also for traffic load. Finally, in Section B in the Appendix, I present results for an heterogeneity analysis by time of the day.

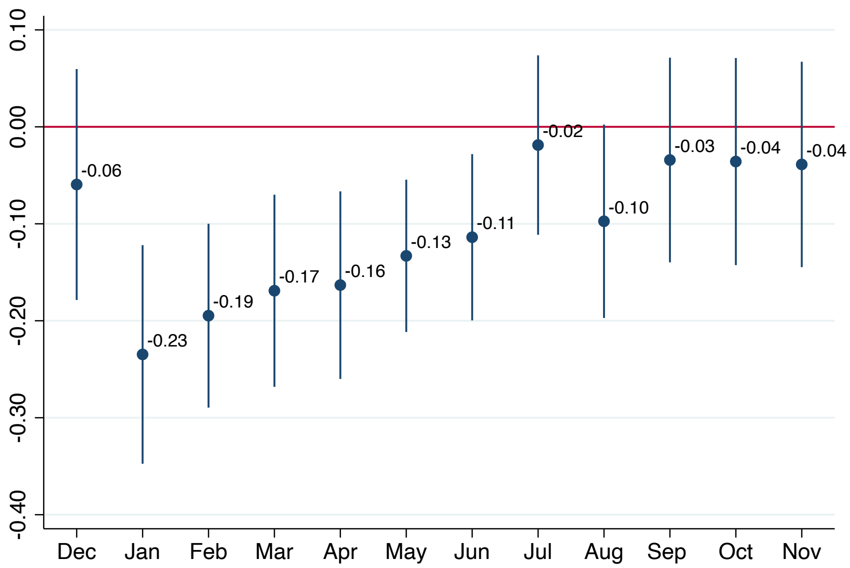

Figure 6: Effect by month within the restricted area. Intensity

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.

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.

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:

$$Accesses_{s,t} = \alpha_0 + \beta_1(Implementation_t * Area_s) + \beta_2 timetrend_t * Area_s + \theta_s + \tau_t + \epsilon_{s,t} \quad (5)$$

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 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); θ_s represents the metro station fixed-effects and τ_t contains time (year) and seasonal (month of the year) fixed-effects.
Table 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

[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.

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: Mechanism. Public transport use

	(1) 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	5564
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.

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 A.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

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

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:

$$log(Registrations_{l,t}) = \alpha_0 + \beta_1(Implementation_t * Label_l) + \beta_2 timetrend_t + \theta_l + \tau_t + \epsilon_{l,t} \quad (6)$$

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

Table 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)[43].

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

[42] *Madrid Central* was implemented on November 30$^{\text{th}}$ 2018, so the first treated month is December 2018.

[43] Results of this model are available upon request

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 4: Mechanism. New cars' registration

	(1) Madrid	(2) 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.

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 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.

7. Conclusion

In the last few decades, local governments adopted traffic calming policies (i.e. low emission zones and congestion tolls) to tackle urban dis-amenities 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.

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Appendix

A. Figures and tables

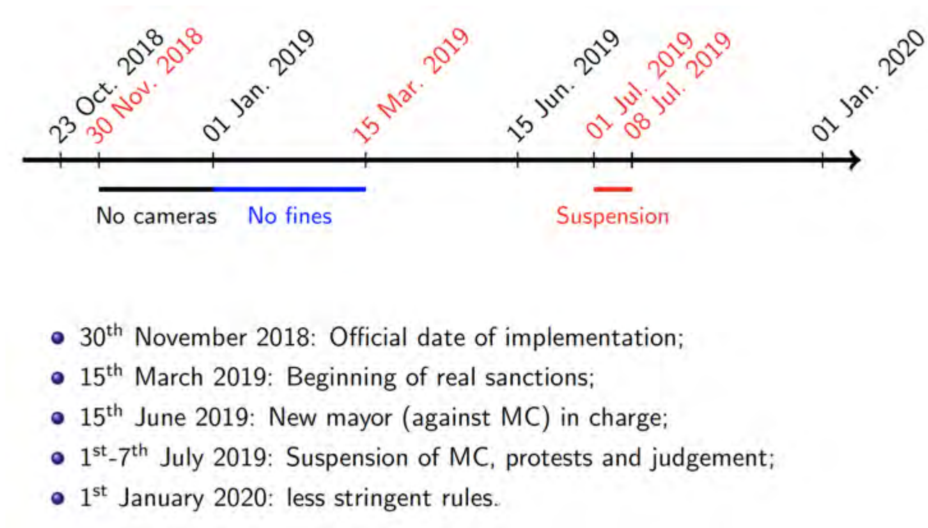

Figure A.1: Timeline of Madrid Central implementation.

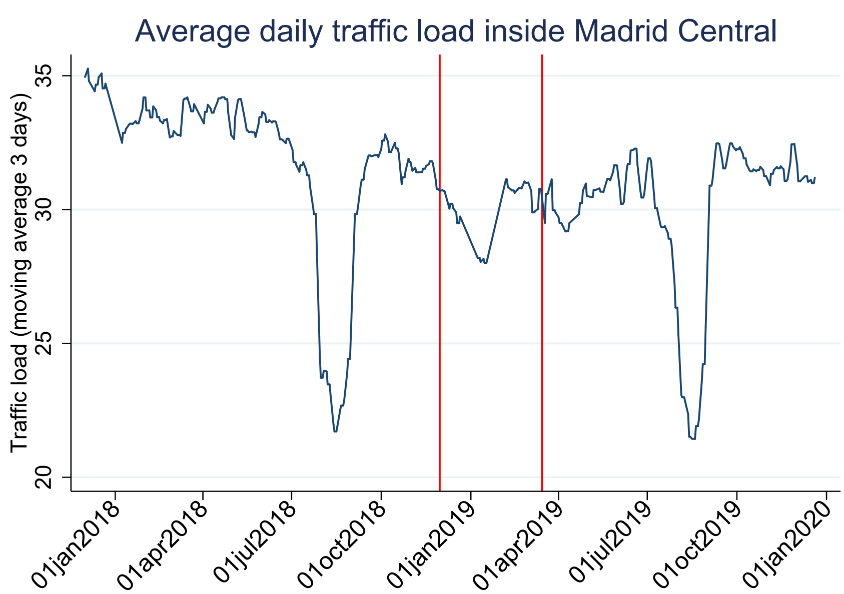

Figure A.2: Temporal evolution of traffic load inside *Madrid Central*.

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.

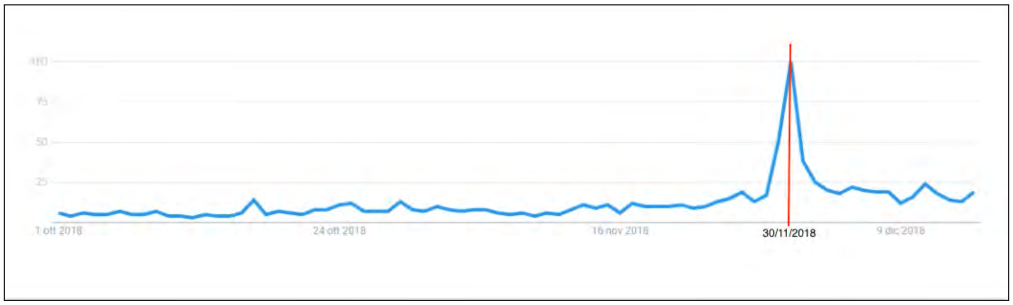

Figure A.3: Google searches of *"Madrid Central"* in Madrid for October/December 2018.

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

	(1) log intensity	(2) log occupancy	(3) 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)
Suspension	-0.003	0.030***	0.010
	(0.006)	(0.008)	(0.006)
Peak hours	0.475***	0.542***	0.499***
	(0.001)	(0.001)	(0.001)
Announcement	-0.041***	0.004	-0.035***
	(0.003)	(0.004)	(0.003)
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	0.043*	0.235***	-0.003
	(0.024)	(0.030)	(0.026)
N×T	43145211	42036903	38567144
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, 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.

Table A.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 $\times T$	2805859	5078724	5078724	5078724
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.

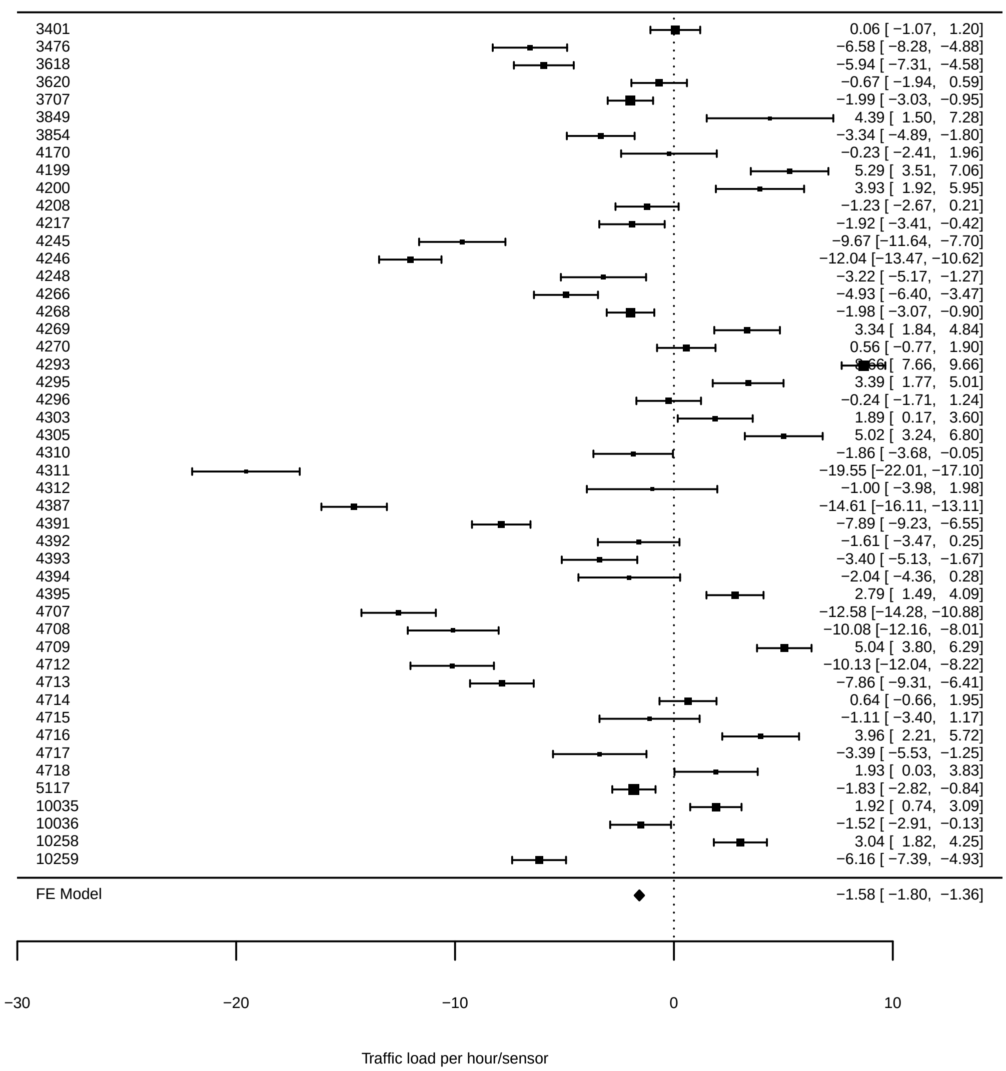

Figure A.4: Results of meta-regression (FE model) for causal impact analysis of traffic load

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.

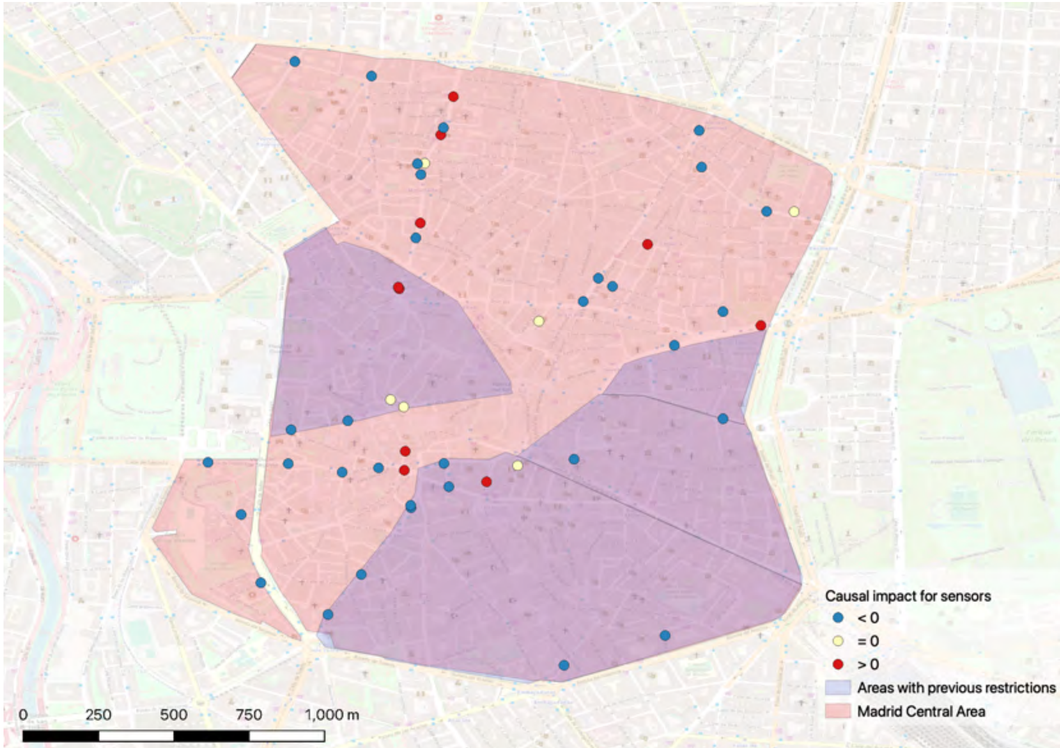

Figure A.5: Causal impact analysis - Sensor-specific results for intensity.

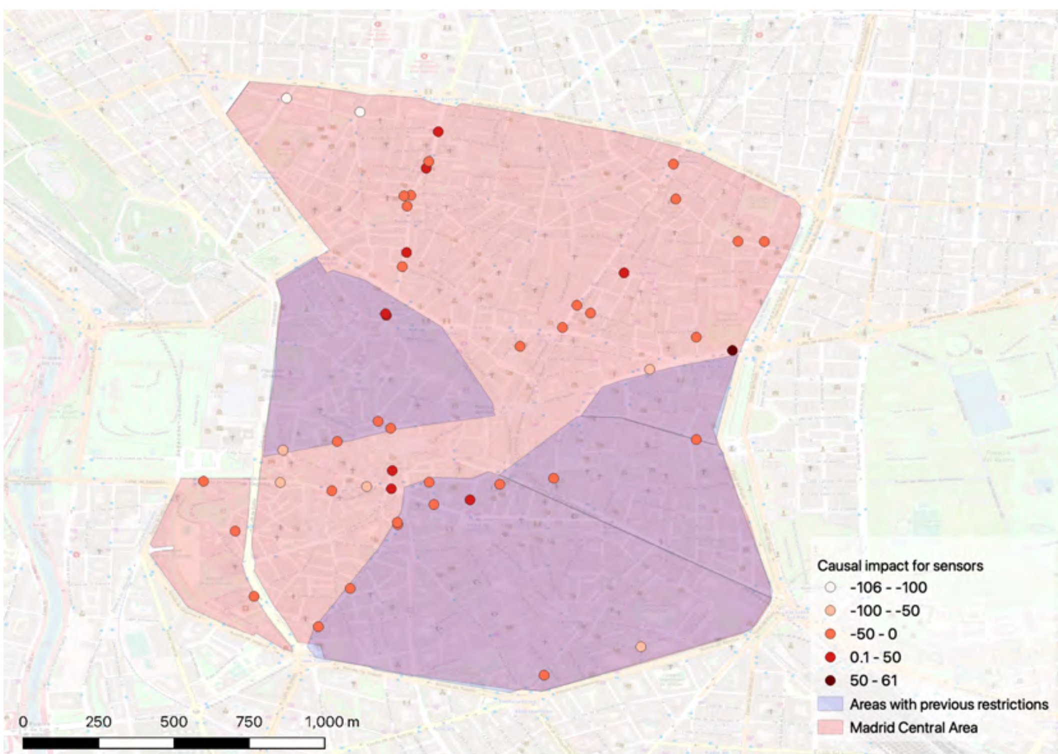

Figure A.6: Causal impact analysis - Sensor-specific results for intensity.

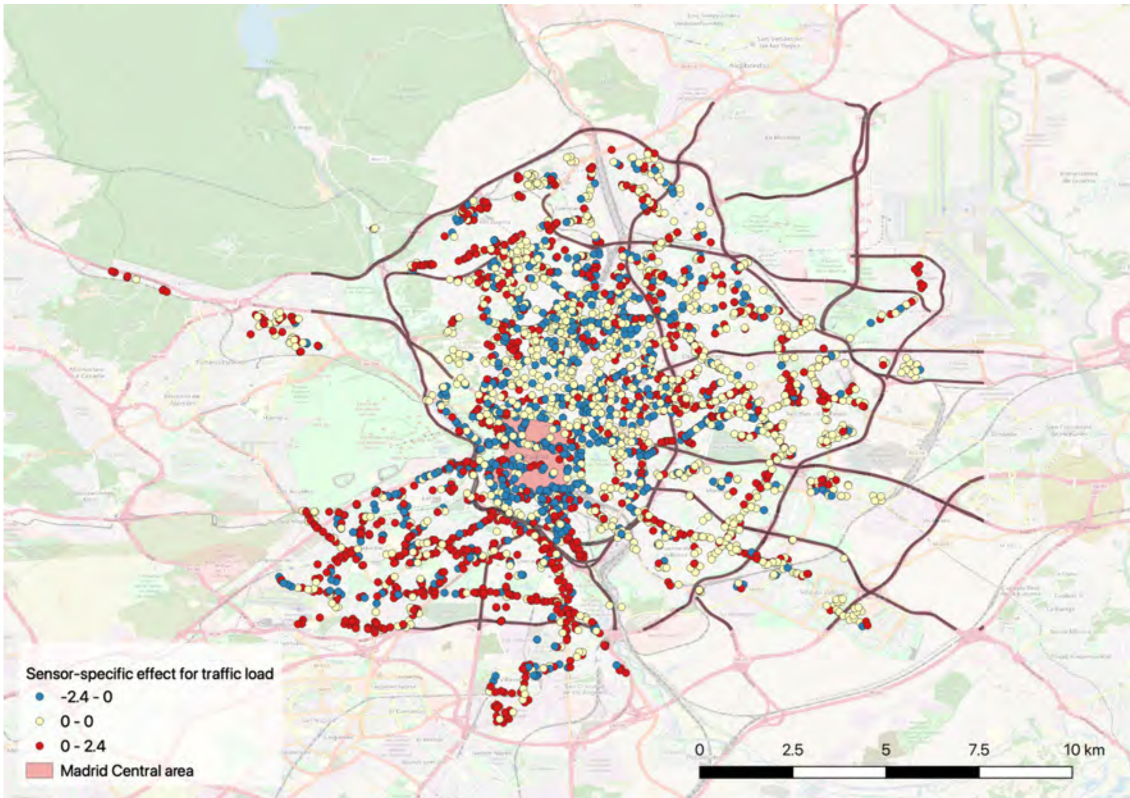

Figure A.7: Sensor-specific time-series regression results for log traffic load.

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).

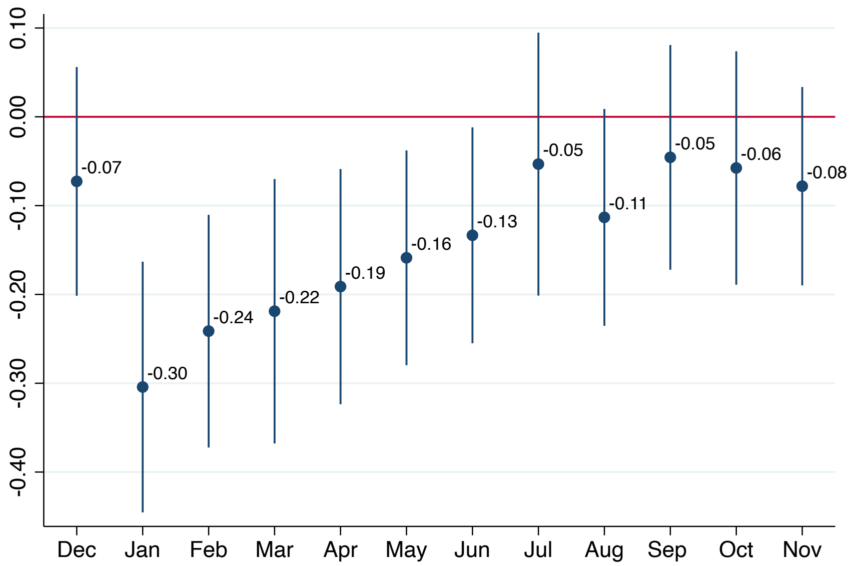

Figure A.8: Effect by month within the restricted area. Traffic Load

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.

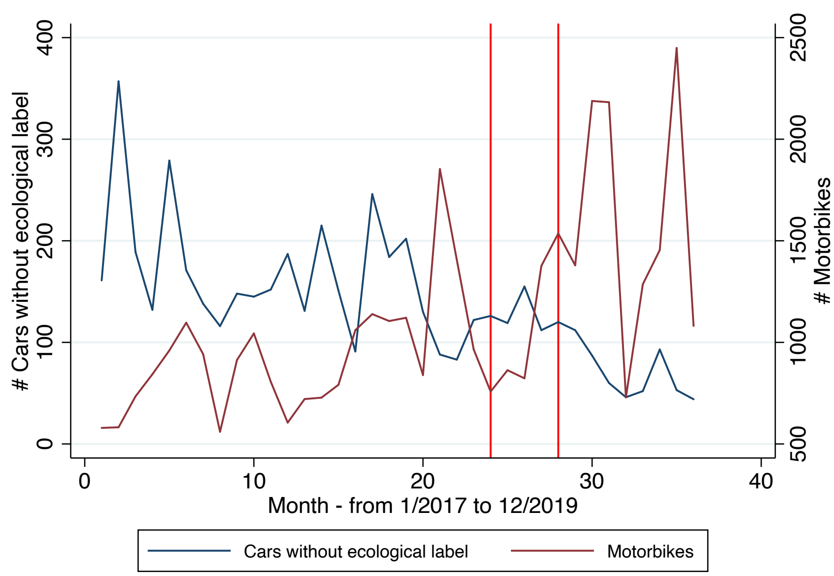

Figure A.9: New monthly vehicles' registration in Madrid.

B. Heterogeneity by time of the day

Besides the heterogeneity by month presented in the main text, another temporal-related heterogeneous analysis is worth to be analysed. Specifically, I am looking at weather the effects vary by time of the day. I divide the hours of a day into five time groups (7-10h, 10-14h, 14-17h, 17-20h, and 20-22h), and I run hourly panel fixed effect model over those subsamples. As before, controls include binary indicators for the suspension period, potential anticipatory effect, and holidays; together with average daily values for precipitations, temperature and gasoline prices. The time and seasonal fixed effects include year, week of the year, day of week, hour of the day. Sensors fixed effects are also included as well as time trends. Standard errors are clustered at the commuting area-day level. The results are presented in Table B.1, where the outcome variable is log intensity in Panel A and log traffic load in Panel B. Most of the models confirm the main results, with reductions in transit in the restricted area and displacement elsewhere. Interestingly, in the early morning 7-10h, we do not observe any reduction in transit for the restricted area. For intensity, the coefficient is not statistically different from zero, although the sign is positive, and for traffic load the coefficient is positive and statistically significant at the 10% confidence level. Those specific hours are those in which commercial vehicles deliver goods to shops and supermarkets. It can be the case the provider of those services reorganised their schedules of activities in order to take advantage of less crowded streets in the city centre after the implementation of *Madrid Central*. Another alternative explanation could be that people working inside the restricted area were already commuting by public transport or bike. This would explain the unchanged average number of vehicles circulating in the city centre, since even before most of those vehicles were represented by commercial deliveries and public transport.

Table B.1: Heterogeneity analysis by time of the day

	(1)	(2)	(3)	(4)	(5)
	h. 7-10	h. 10-14	h. 14-17	h. 17-20	h. 20-22
Panel A - Dep. variable: log intensity					
Implementation × MC	0.021	-0.062***	-0.118***	-0.155***	-0.124***
	(0.022)	(0.011)	(0.013)	(0.014)	(0.011)
Implementation × InsideM30	0.052***	0.024***	0.011**	0.007	0.019***
	(0.009)	(0.006)	(0.005)	(0.005)	(0.004)
Implementation × M30	0.047***	0.006	0.007	0.003	0.05
	(0.010)	(0.006)	(0.005)	(0.005)	(0.004)
Implementation × OutsideM30	0.087***	0.066***	0.070***	0.056***	0.037***
	(0.009)	(0.005)	(0.005)	(0.005)	(0.004)
N×T	8473621	11472805	8493848	8486872	5591459
R2	0.91	0.92	0.93	0.94	0.94
Panel B - Dep. variable: log traffic load					
Implementation × MC	0.039*	-0.082***	-0.111***	-0.145***	-0.116***
	(0.023)	(0.013)	(0.014)	(0.015)	(0.012)
Implementation × InsideM30	0.059***	0.011*	0.008	0.009	0.017***
	(0.010)	(0.007)	(0.005)	(0.006)	(0.005)
Implementation × OutsideM30	0.087***	0.048***	0.056***	0.048***	0.026***
	(0.009)	(0.006)	(0.005)	(0.006)	(0.005)
N×T	7556090	10251834	7582508	7573539	4981472
R2	0.81	0.81	0.83	0.84	0.85

Notes: Hourly panel fixed effect model. A parameter of traffic for buried 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 time trends and fixed effects. Each column represent a regression over a different subgroup of observations. Column 1 keeps only observations between 7-10h, 2 10-14h, 3 14-17h, 4 17-20h, and 5 20-22h. The dependent variable is log intensity in Panel A and log traffic load in Panel B. 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.

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