UNIVERSITÀ CATTOLICA DEL SACRO CUORE
MILANO

Dottorato di ricerca in Economia e Finanza
ciclo XXXVI
S.S.D.: SECS-P/01, SECS-P/03, SECS-P/05

THE ECONOMICS AND POLITICS OF ENVIRONMENTAL POLICIES

Tesi di Dottorato di: Francesco Nicolini
Matricola: 5007442

Anno Accademico 2023/2024

The Economics and Politics of Environmental Policies

Dissertation

submitted to

the Faculty of Economics of
Università Cattolica del Sacro Cuore

by

Francesco Nicolini

for the
Degree of Doctor of Philosophy

2024

Supervisors: Dr. Colussi Tommaso
Dr. Cipullo Davide

Acknowledgments

My first thanks go to Giovanni Petrella. Although he is a professor in a different field from my current research, he was the first to put his trust in me and support me during the difficult initial choices after my Master's degree. He has accompanied me at various times between my first job and my doctorate, offering advice and sometimes assistance, always with his usual kindness.

I have certainly been very fortunate to have had Tommaso Colussi and Davide Cipullo as supervisors and co-authors. Their direct yet always respectful approach, coupled with their heterogeneous perspectives and skills, have given me the opportunity not only to learn much more than expected but also to enjoy these four years of doctoral studies. I sincerely believe that without them, the chances of completing my PhD would have been infinitely lower, and my passion for research would not have been the same. I also have to admit my luck in having had Olmo Silva as local supervisor during my visiting period at LSE. In addition to his friendly manner and availability, the trust he placed in my ideas right away (much more than they deserved) pushed me to develop research that I would have probably abandoned otherwise.

Finally, I must thank my parents. They have always supported my ideas and projects (even when not shared), of which the doctorate is just one of the many attempts to increase the likelihood of a precarious and underpaid career. I cannot leave out who, in addition to supporting me in these choices, also put up with me along the way. My biggest thank goes to my girlfriend and partner of twelve years, Giulia, without whom very little of the little I have done would have been done.

Preface

This thesis comprises two essays that examine the effectiveness of urban environmental policies, their public perception, and their impact on regulated business activities. The essays focus on a specific policy: a pollution-based road pricing (i.e., charging zone) that the city of Milan implemented in 2008 in its city centre to reduce congestion and pollution caused by vehicles. This policy has unique features that make it significant. The charging zone is similar to a traditional congestion charge, but it includes fundamental environmental features. Access charges are determined by the pollution level of the vehicle. Additionally, the policy creates a sharp geographical discontinuity in traffic regulation, which asymmetrically affects residents living in the same neighbourhood and firms competing in the same local market.

Chapter 1 ("Pollution, traffic reduction and electoral outcomes") examines the political consequences of the policy. It shows that turnout at local elections has reduced in electoral precincts just outside of the charging zone. Indeed, individuals residing barely outside the regulated area, who are more likely to have to travel into the zone but who do not receive the discounts given to internal residents, are the most negatively affected. It also shows that those individuals do not tend to punish the incumbent party, due to the strong alignment between parties on the environmental policy. Dissatisfaction with local political representation leads to reduced engagement in municipal elections, which persists over time.

Chapter 2 ("Do pollution charges matter? Evidence from small businesses in Milan") investigates the effects of the charging zone on small businesses. Although some industries remain unaffected by the policy due to their revenue generation models, the pollution charge has led to a significant reduction in the revenues of retail businesses that are barely within the regulated area. The negative effect is likely to be caused by a reduction in customer flow due to the overlap of the enforcement period of the scheme with the working hours of businesses in this industry. This chapter also shows that the policy has generated no effects on business mobility and only a partial impact on local competition.

Contents

Chapter 1

Pollution, traffic reduction and electoral outcomes *

ABSTRACT

This paper investigates the political effects of environmental policies. Specifically, we study the impact of a pollution-based road pricing scheme on voter behaviour by exploiting the features of the charging zone (CZ) implemented in Milan. The policy resulted in unequal benefits and costs for individuals residing within and outside the CZ. To establish causality, we compare electoral outcomes at precinct-level on opposite sides of the CZ boundary by applying a spatial regression-discontinuity design. RD estimates show a lower turnout in local elections in precincts barely outside the CZ with respect to those barely inside, resulting in a sizeable discontinuity at the boundary (6-10 p.p.). We further investigate (i) to what extent the discontinuity depends on policy-induced urban mobility or on changes in individual voting behaviour and (ii) whether our results are consistent with a reward of CZ residents or a punishment of the individual residing outside of the area. To this end, we develop a novel methodology combining the regression discontinuity design with the synthetic control method. We find that the effect is mainly explained by changes in individual voting behaviour and driven by those residing barely outside the CZ who choose not to vote. The effects are stronger among precincts populated by voters whose costs to adapt to the change in traffic regulation are higher (e.g., car users, families with children). Our findings show that individuals adversely impacted by an environmental policy may exhibit a decline in their political engagement when all parties appear to agree on the policy in question.

Joint with *Davide Cipullo* and *Tommaso Colussi*

---

*We would like to thank Felipe Carozzi, Ludovica Gazzè, Marco Le Moglie, Stephan Schneider, Olmo Silva, Giovanni Ursino, and seminar participants at the London School of Economics and Università Cattolica del Sacro Cuore, and the conference participants at the JRC NetCIEx Workshop and European Public Choice Society 2024 Conference for comments and suggestions. We would also like to thank Milan's agency for mobility and the environment (AMAT) for providing us their proprietary data on traffic.

I Introduction

Tackling environmental issues is a challenging and controversial tasks in economic policy: supranational bodies and policy-makers act to commit seriously in the achievement of long-term goals, but national and local policies struggle to be implemented. One of the primary obstacles to the adoption of environmental policies is that they typically yield benefits in the long run, such as reduced greenhouse gas emissions and improved health outcomes of the population, while incurring short-term costs. When these costs affect only a specific segment of the population, they divide it into winners and losers, potentially creating political discontent (Cruz and Rossi-Hansberg, 2022; Conte, Desmet, and Rossi-Hansberg, 2022).

An additional challenge for implementation also comes from the difference between people's perception of environmental problems and their attitudes towards environmental policies (Dechezleprêtre et al., 2022). While most people pronounce concerned about environmental changes, their support for environmental policies strongly depends on their specific modalities and the perception of their efficacy. For instance, carbon pricing is viewed by economists or policy makers as key element for emissions' reduction (Stiglitz, Stern, and Others, 2017), but its mechanisms are not easily implemented and rarely well perceived (Douenne and Fabre, 2022).[1] Such complex inter-plays between environmental policies and their broader societal impacts have captured the attention of economists and policymakers (Besley and Persson, 2023).

In this paper, we provide quasi-experimental evidences of the impact of pollution taxes on political outcomes by studying the charging zone (CZ) introduced in the city centre of Milan. The charging zone is a pollution-based road pricing designed to reduce the external effects of traffic by charging road users directly in order to discourage the use of certain vehicles or fuel types in specific areas.[2] To study the political consequences of this policy, we compare electoral outcomes at precinct-level on opposite sides of the CZ boundary by employing a spatial regression-discontinuity design. We find that electoral precincts situated outside but in close proximity to the boundary of the charging zone have exhibited a decline in turnout in municipal elections. Individuals residing just outside of the area have a high probability of needing to reach locations within the road pricing area, yet they lack access to the fee discounts reserved for residents of the charging zone. Their discontent may have been manifested in a decreased interest in participating in municipal elections, rather than in a direct punishment of the incumbent party, probably due to the high alignment of opponent parties toward the policy.

The charging zone introduced in city centre of Milan exhibits several features that render it particularly suitable for identifying the effects of pollution taxes on electoral outcomes. Firstly, the policy, after its introduction in 2008, had a markedly disparate impact on citizens residing from one side or the other of the CZ, in terms of both benefits and costs. This enables us to leverage both the temporal and geographical variations for causal identification. Secondly, a

[1] Douenne and Fabre (2022) show how French people would reject a carbon tax mainly due to overestimated monetary losses and mis-perception about the effectiveness and the regressive distribution of the policy.

[2] Differently from carbon taxes, pollution taxes take into consideration not only green gas emissions, such as carbon dioxide, but also different air pollutants, such as nitrogen oxide and particulate matter emissions. The charging zone of Milan used European emission standards – which are based on the analysis of both pollutants and green gas emissions – to identify the level of pollution for each vehicle.

temporary suspension of the policy in 2012 due to a judicial appeal allows us to exploit an additional and unanticipated source of variation to study the effect on traffic and pollution. Finally, the city of Milan provides granular data, measuring political outcomes at a electoral precinct level, spanning periods before and after the implementation of the charging zone.

We estimate the political consequences of the charging zone by using data on municipal and national elections over the 2001-2021 period for approximately 1,200 electoral precincts located in Milan. We exploit the geographical variation of the policy through the use of a regression discontinuity design in which the running variable is the distance of the electoral precincts to the boundaries delimiting the charging zone. When pooling all municipal elections that took place after the policy implementation, we estimate a sizeable and significant discontinuity in voter turnout, i.e. 6-10 percentage points. Interestingly, we observe no statistically significant impact on vote shares for any party in municipal elections, and no sizeable effect on electoral outcomes at the national level. Furthermore, we address potential concerns about the robustness of the estimated result on municipal turnout by testing its sensitivity to the selected bandwidth, the electoral precincts in close proximity to the CZ boundary, and the treatment assignment using placebo boundaries. Lastly, we complement our preferred specification model with a weighted regression discontinuity design, accounting for the number of eligible voters in each electoral precinct, and a difference-in-discontinuities model, cleaning the estimates from any potential pre-treatment discontinuity in electoral outcomes at the threshold.

We consider a number of potential mechanisms that could explain the effect on local election turnout. First, the policy could have induced individual urban mobility and thus altered the voter base, by attracting or rejecting people in specific areas. But it could also have influenced individual voting behaviour, by affecting their decision to participate in local politics. Second, the observed effect could be driven by an increase in turnout in electoral precincts within the CZ or by a reduction in the precincts outside of the area. We document that (i) the effect on municipal turnout is only partially attributable to individual urban mobility, suggesting that it is primarily driven by a change in voting behaviour, and (ii) the effect is caused by those individuals residing in close proximity to the boundary but outside of the charging zone who choose not to vote. In order to achieve this latter result, we have developed a novel methodology, which combines the regression discontinuity design with the synthetic control method. This methodology allow us to to construct a counterfactual turnout for electoral precincts on either sides of the threshold based on electoral data from the municipalities of Naples and Turin, which held municipal elections at the same time of Milan and did not have any CZ in place at the time of the elections. Furthermore, we show that the reduction in municipal turnout is more pronounced in electoral precincts with a high number of families with children and homeowners, whose costs to adapt to the change in traffic regulation are higher.

Our main finding is that the introduction of the charging zone have induced individuals living just outside of the area to decrease their political participation in municipal elections. We provide evidence that the policy generated different benefits and costs for citizens residing within and outside of the CZ, supporting the conjecture that the latter are the most penalised. To this end, we exploit the judicial appeal against the policy in 2012, which resulted in the suspension of the road pricing for a period of two months. We employ a regression discontinuity design in which the running variable measures the time distance to the re-instalment of the CZ and compare the number of vehicles accessing the area and pollution levels in the hours just before and just after the policy change. For this analysis we leverage two main datasets: administrative data tracking vehicles accessing the CZ from the 43 gateways located on the CZ boundary and levels of local pollutants, such as carbon monoxide (CO) and nitrogen dioxide (NO2), detected by the 8 air quality stations located in Milan. Consistent with the policy's characteristics, which impose a lower fee on citizens residing within the CZ, we estimate a significantly larger impact on vehicles (subject to fee) owned by individuals residing outside of the regulated area (-28%) than those owned by CZ residents (-17%). We also show that the policy has positive effect on the air quality registered within the CZ, as evidenced by a decrease in both CO and NO2 levels. However, its impact on other areas of the city is less clear. Overall, the policy appears to have generated greater benefits at a lower cost for citizens residing within the CZ, while creating fewer benefits and imposing greater costs on those residing outside. These findings lend support to the hypothesis that those living barely outside of the regulated area are the most adversely affected.

This paper contributes to the literature in political economy estimating the electoral consequences of environmental policies. Stokes (2016) demonstrates that voters pay attention to climate policies and can retrospectively punish incumbents for nearby facilities they perceive as harmful to their communities. In particular, she shows that proposals for wind turbines result in a 5% decrease in the vote share of the incumbent provincial government, while operational wind turbines result in a 10% decrease. Colantone et al. (2024) provide survey evidence demonstrating that the ban on polluting car implemented in most of the territory of Milan may advantage parties contrasting it.[3] We add to this literature by studying the effect of a pollution tax on vehicles, initially introduced by the right-wing party and then confirmed by the left-wing, and by providing quasi-experimental evidence using extremely granular administrative data on electoral results. Our findings complement the literature by showing the potential political implications in case the opponent parties are aligned on the environmental policy. As the policy creates winners and losers, when those negatively affect do not see any political representation addressing their concerns, they become dissatisfied with local politics and choose not to participate in elections. Conversely, those who are positively affected by the policy may not necessarily reward local politicians for the implementation of this environmental policy.

Our study also relates to the urban and environmental economics literature on traffic reduction policies and their effects on car usage and pollution (Davis, 2008; Wolff, 2015; Viard and Fu, 2015; Green, Heywood, and Navarro, 2016).[4] We find that the effects of this policy on pollution and car traffic are similar to the ones estimated for similar policies adopted in London (Leape, 2006), thus strengthening the external validity of our results. Our results are also in line with findings by Gibson and Carnovale (2015) and Percoco (2013), which use the 2012 judicial

[3]In 2019, the municipality of Milan introduced a Low Emission Zone (called "Area B") which banned most polluting vehicles from almost 70 percent of the city's territory.

[4]Different types of policies have been implemented to reduce traffic congestion and road pollution in cities. Mexico City and Beijing introduced driving restrictions for vehicles based on their license plate number (Davis, 2008; Wang, Xu, and Qin, 2014; Viard and Fu, 2015); several German cities have implemented low-emissions zones banning more polluting vehicles (Wolff, 2015; Gehrsitz, 2017).

appeal to study the effect of the congestion charge in Milan on traffic congestion, accidents and pollution. We refine their analysis by employing a new administrative dataset on the number of vehicles entering the CZ that further distinguishes whether the car owner was a resident of the CZ or not. This distinction ultimately allows us to estimate the differential effect of the policy based on its cost.

The remainder of the paper is organized as follows. Section II provides some background on the charging zone of Milan and the political path that brought to its introduction. Section III describes the data sources and discusses the empirical strategy. Section IV presents the main results on the electoral outcomes. Section V discusses the potential mechanisms and explores heterogeneous effects. Section VI studies the differential impacts on traffic and pollution perceived by individuals residing within and outside of the regulated area. Section VII concludes.

II INSTITUTIONAL BACKGROUND

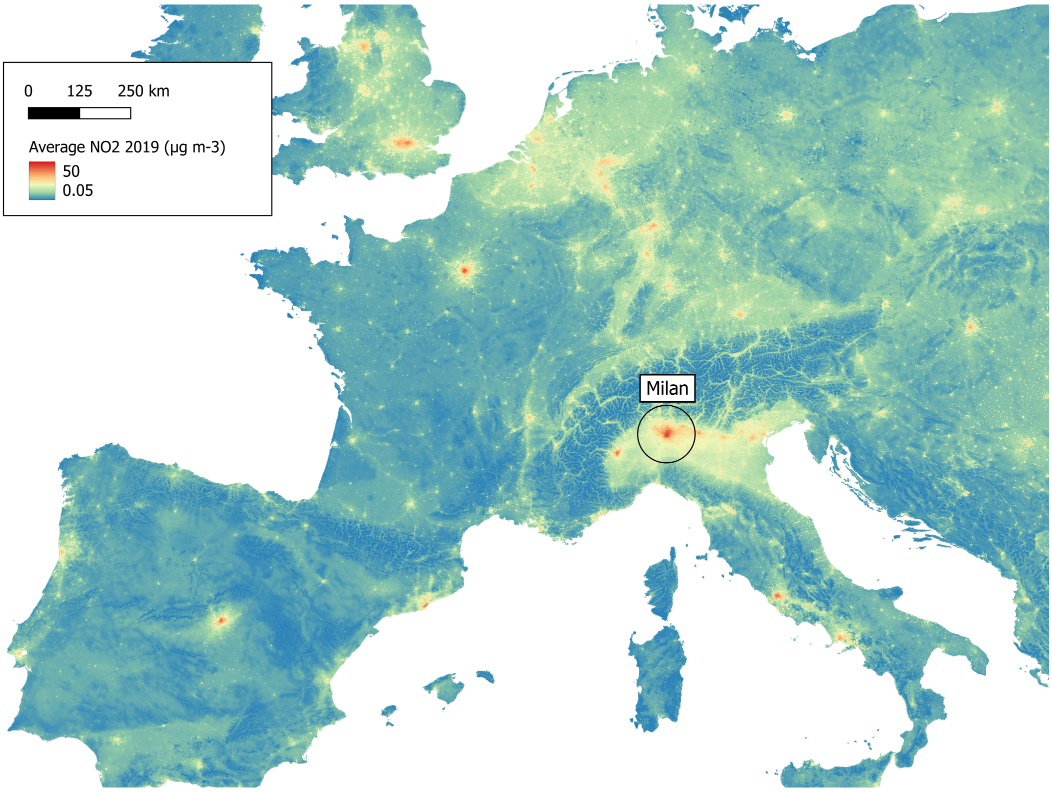
This section provides a brief introduction to the pollution concerns in the city of Milan, the history of the charging zone, and its functioning. For a more detailed analysis of the political path that led to the introduction of the charging zone and how the different schemes operate, please see Section C in the Appendix.

THE POLLUTION IN MILAN. The city of Milan in northern Italy exemplifies the intricate interplay between population dynamics, economic importance, and environmental concerns. As the second most populous city in Italy, with a population of 1.4 million within the city and over 3 million in the metropolitan area, Milan occupies a pivotal position within the national and European economic landscape. Milan is confronted with considerable environmental challenges, particularly in relation to its poor air quality standards. Firstly, the high population density of the city (7,500 inh./km^2) and the Lombardy region contributes significantly to the high pollution levels in the city. Such a concentration of human activities, including road transport, house heating, agriculture and industrial processes, has a serious impact on the emission of pollutants. Secondly, the city's geographical location within the Po Valley exacerbate pollution levels. The valley's distinctive topography, comprising a vast, flat lowland area encircled by physical barriers such as the Alps and the Appennines mountains, traps the air pollutants and gas emissions, leading to the formation of smog, particularly during periods of stagnant weather conditions. Consequently, the metropolitan area of Milan is one of the most polluted areas in Europe (EEA, 2020).[5] Figure 1 illustrates the elevated concentration of nitrogen dioxide (NO2) in Milan in comparison to the rest of Europe, suggesting also the significant impact of internal combustion engines in the pollution levels in Milan (EEA, 2018).

A BRIEF HISTORY OF THE CHARGING ZONE. The local government of Milan initiated efforts to mitigate the adverse effects of traffic congestion and pollution during the 1990s, with very lim-

[5] In 2007, the consulting firm Ambiente Italia ranked Milan as the third-highest concentration of particle matter (PM10) among large European cities, both in terms of average annual level and in terms of number of days above the limit of 50 μg/m^3.

FIGURE 1
POLLUTION LEVELS (NO2) IN MILAN



Notes: This picture represents an authors' elaboration on data from the European Environmental Agency representing average NO2 levels in 2019.

ited success. In the campaign approaching the 2006 municipal election, traffic reduction policies emerged as a key issue, prompting parties to debate alternative policies to limit traffic congestion and combat pollution, particularly in the city's central area. Specifically, the right-wing coalition advocated in favour of the introduction of a road pricing regime on the entry of polluting vehicles, whereas the left-wing coalition proposed the creation of large pedestrian areas. Overall, both coalitions seemingly agreed on the necessity of implementing traffic restrictions in the city centre.

In January 2008, the city of Milan implemented a charging zone in the city centre, known as the "Ecopass" scheme. This scheme, introduced by the right-wing local governing coalition, was a pollution-based road pricing system that required a fee on vehicles entering the city centre, based on their level of pollution.[6] Furthermore, individuals residing within the city centre were offered the option of purchasing discounted yearly subscriptions.[7] The initial design of the policy and the failure to update the definitions of polluting vehicles placed significant pressure

[6] Vehicles were charged four different fees, between 0 euros and 10 euros, based on their pollution levels. Electric and hybrid vehicles, bi-fuel vehicles, and recent petrol (Euro 3, 4, and 5) and diesel (Euro 4 and 5) vehicles were granted free access. Euro 1 and 2 petrol (diesel) cars were subject to 2 euros (5 euros) fee, while older petrol (diesel) vehicles were charged 5 euros (10 euros) per entry. Euro categories are the EU emission standards for vehicles' pollution, ranging from Euro 1 (vehicles matriculated not before 1992) to Euro 6 (vehicles matriculated not before 2014). See Figure A.1 in the Appendix for a summary of "Ecopass" fee scheme.

[7] Yearly subscriptions for CZ residents were 50/125/250 euros, depending on the pollution class of their vehicle.

on the financial sustainability of the scheme and on its political support. As the 2011 municipal elections approached, both coalitions criticised the charging zone and blamed the opponent party for its introduction.[8]

Following the 2011 municipal election, a referendum including a question regarding the charging zone was held. The results demonstrated that 80% of voters were in favour of the proposed traffic restrictions (with a turnout of 49%). The newly elected left-wing administration restricted the opportunities for free entrance into the CZ, reduced the number of fees to two, and introduced a ban on more polluting vehicles. Starting from January 2012, the "Area C" scheme replaced the former "Ecopass" operating with the same space-time restrictions.[9] Furthermore, the policy incorporated a rolling agenda of additional limitations on the number of vehicles granted free access and further expansions of the number of vehicles banned from the city centre, to be introduced over time.[10]

During 2012, the owner of a private parking space located inside the CZ initiated legal proceedings against the road pricing regime. The second instance judges (*Consiglio di Stato*) forced the municipality to suspend the road pricing until the final decision. The suspension became immediately effective upon the court's decision on 26th July 2012. Two months later, the court declared that the policy complied with national law and the CZ was restored on September 17th.

HOW THE CHARGING ZONE WORKS. The charging zone is a pollution-based road pricing that encompasses an area of 8.2 km^2 situated within the historic city centre, equivalent to 4.5% of the total municipal area. A total of 43 cameras with automatic number plate recognition are installed along the boundary monitoring all roads leading to the area. The road pricing system, which operates between 7.30 a.m. and 7.30 p.m. from Monday to Friday, varied fees between the the "Ecopass" (active from 2008 to 2011) and "Area C" (active since 2012) schemes. The first scheme comprises four fee levels (0, 2, 5 and 10 euros), while the second scheme restricts the fees to two levels (0 and 5 euros). Both schemes impose charges based on the vehicle's pollution levels, with access granted to less polluting vehicles at no cost. Furthermore, both schemes offer several payment methodologies (e.g., by debit and credit card, by a dedicated phone number, online or through an automatic bank payment) and significant discounts for individuals residing within the CZ. The charge is only applied to vehicles entering the zone, with free mobility within the area once inside. Figure A.3 in the Appendix illustrates the area covered by the charging zone with respect to the territory of the city of Milan.

[8]Interestingly, the incumbent right party – which introduced the "Ecopass" scheme – blamed the left coalition for its introduction and proposed to abolish it. Figure A.2 in the Appendix provides evidence of electoral signs posted in Milan just before the second ballot of the 2011 municipal elections pointing the left party as the real policy supporter.

[9]Electric, hybrid and bi-fuel vehicles were granted free access, while all other vehicles admitted into the CZ were subject to the payment of a 5-euros fee. Older than euros 1 petrol vehicles as well as euros 1 and 2 diesel vehicles were banned since the introduction of "Area C". See Figure A.1 in the Appendix for a summary of the fee and bans regime. Residents living within the CZ were granted 40 free daily entrances a year and charged a reduced fee of 2 euros on subsequent accesses.

[10]Since 2019, bi-fuel vehicles are subject to the 5-euros fee payment. Diesel Euro 3 vehicles belonging to non-residents individuals were banned starting from January 2016 while diesel Euro 3 vehicles belonging to residents were banned starting from October 2016. Petrol Euro 1 vehicles were banned starting from October 2019 and October 2020 for residents and non-residents, respectively.

FIGURE 2

ELECTORAL PRECINCTS, POLLING STATIONS, AND THE CHARGING ZONE

Notes: This picture represents the geographical position of the electoral precincts (computed as the average location of their eligible voters), the polling stations, parks (green areas), census blocks (grey lines) and the charging zone boundary (red line) based on an authors' elaboration on data from the municipality of Milan and the Italian Institute for Statistics (ISTAT).

III EMPIRICAL FRAMEWORK

A DATA

The official electoral data at precinct level are provided by the Municipality of Milan.[11] We collect data for around 1,200 electoral precincts covering five municipal elections of Milan (from 2001 to 2021) and five national elections of the Italian Parliament (from 2001 to 2018). For each election, our data contains the electoral results, the address of residence of the eligible voters and the address of the polling stations per election. Electoral results include the number of eligible voters, the valid and invalid votes, and the number of all valid votes received by each candidate, list, party and coalition.

In order to estimate the location of each electoral precinct, we compute the average position of the home addresses of its eligible voters. We then calculate the distance from each precinct to its polling station, the closest metropolitan line station, and the boundary of the charging zone. We excluded 8 electoral precincts having eligible voters both inside and outside the CZ and 157 electoral precincts whose eligible voters are not linked to them for residence reasons such

[11] See https://siel.comune.milano.it

as hospital, prisons and other special electoral precincts.[12] The map of the electoral precincts of the municipality of Milan, the charging zone and more relevant geographic references are represented in Figure 2.[13]

TABLE 1

ELECTORAL PRECINCT SUMMARY STATISTICS

	Obs	Mean	Std. Dev.	Min	Max
Panel A. Municipal elections.					
Eligible voters	5614	847.2	118.6	291	1450
Turnout	5614	0.640	0.131	0.214	0.970
Incumbent	5614	0.498	0.0983	0.148	0.772
Left-wing vote share	5614	0.447	0.110	0.146	0.772
Right-wing vote share	5614	0.450	0.108	0.159	0.760
Distance to metro station	5614	1.074	0.861	0.0224	4.771
Distance to polling station	5614	0.358	0.211	0.00960	2.394
Distance to CZ boundary	5614	-2.423	1.719	-7.782	1.224
Panel B. National elections.					
Eligible voters	5633	816.0	117.6	233	1449
Turnout	5633	0.794	0.0635	0.472	0.971
Incumbent	5633	0.391	0.0969	0.0331	0.726
Left-wing vote share	5633	0.390	0.0762	0.0331	0.697
Right-wing vote share	5633	0.444	0.107	0.164	0.726
Distance to metro station	5633	1.075	0.861	0.0224	4.771
Distance to polling station	5633	0.360	0.220	0.00960	2.379
Distance to CZ boundary	5633	-2.423	1.719	-7.782	1.220

Notes: The unit of observation is a electoral precinct-election year. Panel A collects pooled data for 2001, 2006, 2011, 2016 and 2021 municipal elections; panel B collects pooled data for 2001, 2006, 2008, 2013 and 2018 national elections. *Incumbent* represents the vote share received by the coalition that won the most seats in the municipal council (or Parliament respectively) in the previous election. Distance measures are reported in kilometres.

Table 1 reports summary statistics of electoral precincts during municipal (panel A) and national (panel B) elections. During the municipal elections held from 2001 to 2021, the average number of eligible voters at each polling station is approximately 850, with an average turnout of 64 percent. The left-wing and right-wing vote shares for mayoral candidate are both approximately 45 percent each. During the national elections held from 2001 to 2018, the electoral precincts have 816 eligible voters with a turnout of just below 80 percent on average.[14] The right-wing receives 44 percent of the vote share in Parliament elections, while the left-wing 39 percent. Statistics of the geographical variables reveal that the distance to the polling station ranges from few metres to more than 2 kilometres, while the distance to the CZ boundary ranges

[12]Since we have the full list of special electoral precincts only for 2016 municipal election, we make a number of assumptions: drop all precincts with less than 200 eligible voters which is the minimum required to be a standard precinct, drop all precincts with more voters than eligible voters, drop all precincts not present in all 5 elections, drop all list of precinct declared “special” in 2016.

[13]The average location of eligible voters is computed by using the residential address employed to deliver the electoral cards (i.e., "tessere elettorali").

[14]The number of eligible voters differs between municipal and national elections because of different voter bases. For instance, Italian citizens living abroad can vote in their precinct in municipal elections, but vote by mail in national elections.

TABLE 2

COVARIATE BALANCING (2001 CENSUS) AT THE CZ BOUNDARY

Variable	Conventional RD effect	Robust p-value	Robust 95% c.i.	MSE Bandwidth	Mean w/in bw	Effective Obs.	Total Obs.
Population	–0.053	0.974	[–0.407; 0.393]	0.439	4.988	674	5716
Female	–0.004	0.867	[–0.047; 0.040]	0.327	0.535	508	5716
Married	0.042	0.031	[0.004; 0.088]	0.370	0.421	569	5716
Divorced	–0.024	0.104	[–0.063; 0.006]	0.454	0.036	690	5716
Children	–0.004	0.666	[–0.031; 0.020]	0.350	0.081	540	5716
Under 30	0.004	0.929	[–0.031; 0.034]	0.335	0.277	523	5716
Over 60	–0.011	0.556	[–0.072; 0.039]	0.341	0.275	530	5716
Graduated	0.022	0.339	[–0.024; 0.069]	0.381	0.308	579	5716
Italians	–0.005	0.614	[–0.035; 0.021]	0.505	0.928	759	5716
Non-europeans	0.014	0.154	[–0.006; 0.038]	0.413	0.049	635	5716
Students	–0.001	0.625	[–0.018; 0.011]	0.441	0.071	674	5716
Commuters	–0.010	0.693	[–0.039; 0.026]	0.481	0.466	721	5716
Commuters into CZ	0.018	0.246	[–0.012; 0.045]	0.369	0.174	563	5692
Car users	–0.012	0.214	[–0.032; 0.007]	0.416	0.142	637	5692
Employed	0.006	0.711	[–0.020; 0.029]	0.385	0.958	580	5685
Household size	0.025	0.830	[–0.076; 0.094]	0.337	0.728	520	5683

Notes: The unit of observation is the census block of the 2001 Italian Population Census. *Population* and *household size* varibles are log tranformed. Some census block have no information on the commuting and household surveys. Results are estimated with local linear regressions, triangular kernel weights and MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). P-values are based on robust bias-corrected inference.

from -7.8 kilometres (at the margin of the city of Milan) to 1.2 kilometres (inside the city centre).[15]

B SPATIAL REGRESSION DISCONTINUITY DESIGN

The combination of data granularity and geographical variation of the CZ allows us to employ a (sharp) spatial regression discontinuity design to estimate the effect of the policy on electoral outcomes. This strategy exploits the discontinuity in the treatment of residents living within the CZ area with respect to the residents living outside of it, while all other factors vary smoothly at the boundary of the zone. Each electoral precinct receives the treatment depending on its latitude and longitude, given by the average location of its eligible voters. We collapse the two geographical dimensions into a single score described by the chordal distance $f_{\mathbf{b}}(\mathbf{P}) : \mathbb{R}^2 \rightarrow \mathbb{R}$ of the precinct $\mathbf{P} = (P_1, P_2)$ and the closest boundary point $\mathbf{b} = (b_1, b_2)$ of the CZ (i.e., $distance_{i,t}$). Formally, we estimate

$$y_{i,t} = \beta_0 + \beta_1 CZ_{i,t} + f(distance_{i,t}) + \delta_t + \varepsilon_{i,t}. \quad (1)$$

The coefficient of interest is β_1, which captures the difference in political outcomes at the CZ boundary between precincts located inside and outside of the are. We use local linear regressions following the MSE minimization (Calonico, Cattaneo, and Titiunik, 2014) for optimal bandwidth

[15] Distances to the polling stations can display very low values since they are calculated using the average location of their eligible voters. If voters are located around the assigned polling station, their average position can result very close to the one of the polling station.

choices. To improve precision, we add controls for the distance between each precinct and the assigned polling station and the closest metropolitan line station.

We start by providing evidence on the validity of the continuity of potential outcomes assumptions (see Hahn, Todd, and Van der Klaauw, 2001; Imbens and Lemieux, 2008). First, we run the estimating equation on a set of pre-determined covariates; these are variables from the 2001 Italian Population Census, which provides demographic data such as number of population, sex, marital status, nationality, age, graduates and household size for 5,716 census blocks in Milan. Table 2 shows that in 2001 the main demographic and commuting variables do not show any significant discontinuity at the threshold, ultimately reassuring us about the validity of our design.[16] In particular, the probability of commuting into the CZ exhibits a smooth variation across the boundary of the area prior to the implementation of the traffic regulation.[17] Second, in Table B.1, we assess that the outcome variables employed in this analysis used to be balanced across the threshold before the introduction of the road pricing scheme (i.e., 2001 and 2006 elections). Specifically, turnout and vote shares received by the incumbent, left and right parties evolve smoothly at the threshold both in municipal and national elections. Third, we assess the continuity of the density function of the running variable at the cutoff (McCrary, 2008). Although the municipality has no incentive in reshaping the electoral precincts to manipulate their distance to the CZ boundary since seats are assigned at large, we test for evidence of manipulation of the running variable around the threshold following Cattaneo, Jansson, and Ma (2020). Based on the results presented in Figure A.4 in the Appendix, we cannot reject the null hypothesis of no manipulation at conventional levels.

IV MAIN RESULTS

This section estimates the political effects of the adoption of the charging zone in Milan. We initially examine the political effect in the 2011, 2016 and 2021 municipal elections of Milan, as well as the 2008, 2013 and 2018 national elections of the Italian Parliament. Next, we conduct a number of robustness checks to test the sensitivity of the main result to alternative bandwidth selections and model specifications.

Table 3 reports the effect of the charging zone on the political outcomes. First, we investigate the effect of the CZ on turnout and vote shares for the incumbent, right- and left-wing for all municipal elections, pooling elections for 2011, 2016 and 2021 (panel A). The results show a lower turnout in local elections in precincts barely outside the CZ with respect to those barely inside, resulting in a sizeable discontinuity at the boundary around 6-10 percentage points depending on the employed kernel weights in the estimation (columns 1-2). Interestingly, the support for incumbent party does not show any discontinuity at the CZ boundary when using a uniform kernel (column 3) and a small but not statistically significant effect when using a triangular kernel

[16] We use census blocks as the units of observation as they are more granular then precincts (i.e., there are approximately 1200 precincts in Milan but 6000 census blocks). We use the centroids of each census block to compute the running variable employed in equation 1 when estimating the covariate balancing across the threshold on 2001 census variables.

[17] The probability of being married shows a statistically significant discontinuity at the threshold. The high number of tested covariates increases the probability to find a discontinuity statistically significant at the 5 percent level.

(column 4). Similarly, the vote shares for left and right coalitions behave relatively smoothly at the threshold, being the estimated coefficients close to zero (columns 5-6). We also find a lack of statistically significant effect on the three national elections (2008, 2013, and 2018). In particular, we observe a small and positive but not significant effect on electoral turnout, and estimates close to zero for the remaining outcomes (panel B).

TABLE 3

EFFECT OF CZ ON ELECTORAL OUTCOMES

Dep. var.	Turnout		Incumbent		Left-wing		Right-wing	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Kernel	Uniform	Triang	Uniform	Triang	Uniform	Triang	Uniform	Triang
Panel A. Municipal elections.								
RD Effect	0.062**	0.104***	-0.002	0.026	0.009	-0.005	-0.010	0.011
	(0.029)	(0.038)	(0.017)	(0.019)	(0.033)	(0.028)	(0.040)	(0.036)
Robust p-value	0.046	0.014	0.891	0.248	0.646	0.815	0.909	0.609
MSE bandwidth	0.264	0.243	0.245	0.232	0.286	0.350	0.286	0.312
Mean w/in bw	0.63	0.63	0.54	0.54	0.46	0.46	0.45	0.45
Observations	171	150	153	149	204	258	204	223
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Panel B. National elections.								
RD Effect	0.023	0.027	0.007	0.006	0.007	-0.001	0.018	0.003
	(0.028)	(0.025)	(0.009)	(0.008)	(0.040)	(0.026)	(0.043)	(0.032)
Robust p-value	0.389	0.214	0.413	0.268	0.944	0.874	0.678	0.866
MSE bandwidth	0.211	0.241	0.285	0.378	0.251	0.422	0.255	0.345
Mean w/in bw	0.81	0.81	0.41	0.41	0.40	0.41	0.45	0.44
Observations	121	153	207	285	162	320	165	254
Controls	✓	✓	✓	✓	✓	✓	✓	✓

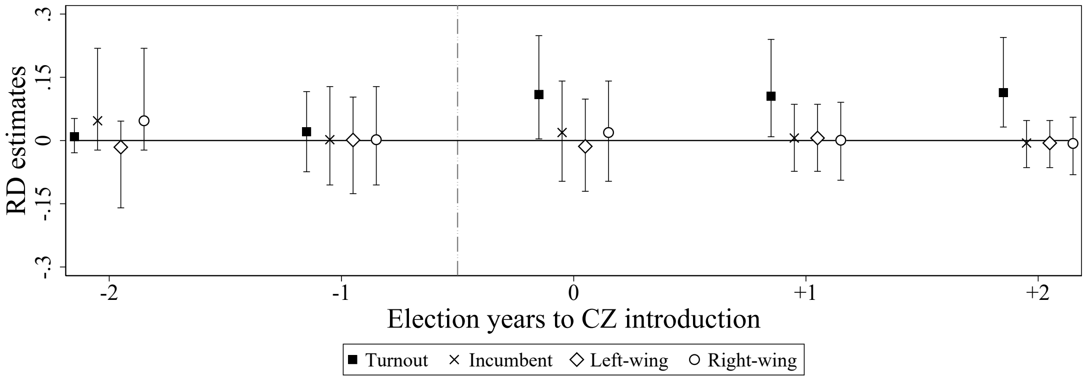
Notes: The unit of observation is a electoral precinct-election year. *Panel A* collects pooled data for 2011, 2016 and 2021 municipal elections; *panel B* collects pooled data for 2008, 2013 and 2018 national elections. *Incumbent* represents the vote share received by the coalition that won the most seats in the municipal council (or Parliament respectively) in the previous election. Estimation method: local-linear regression within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014) with uniform or triangular kernel weights. All specifications include controls for distance to the polling station, distance to the closest metropolitan line station and election year fixed effects. Inference is based on standard errors robust to clustering at the electoral precinct level. Conventional standard errors are shown in parentheses, also robust bias-corrected p-value and 95% confidence intervals are reported. *, **, and *** denote the 10%, 5%, and 1% significance levels, respectively.

Figure 3 illustrates the evolution of the discontinuity at the threshold over time in municipal electoral outcomes, when using triangular kernel weights in the estimation. In the years preceding the introduction of the charging zone, no discernible discontinuity on turnout and vote share for incumbent, left and right parties is evident. In the post-treatment election years, the turnout shows a statistically significant effect of 10 p.p., which remains constant over time. In contrast, all other outcomes remain unaltered. Figure A.5 in the Appendix provides graphical evidence of the RD effect on turnout, vote share for the incumbent, left- and right-wing parties for pooled municipal elections following the introduction of the charging zone (i.e., 2011, 2016 and 2021).

Overall, the policy appears to have affected electoral participation at the local level, while leaving municipal vote shares and national politics unaltered. On the one hand, mayors from

FIGURE 3

CROSS-SECTIONAL RD EFFECTS IN MUNICIPAL ELECTIONS



Notes: The figure illustrates the cross-sectional RD effects and 95 percent robust bias-corrected confidence intervals for pre- and post-treatment municipal elections. RD estimates are computed separately for each year. Estimation method: local-linear regression within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014) and with triangular kernel weights.

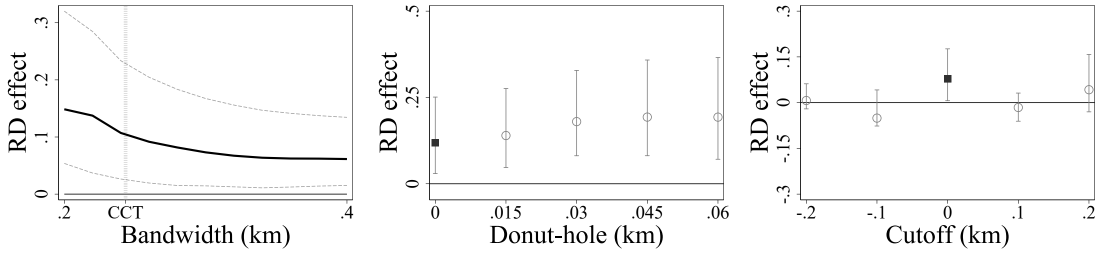
both the left and right wings have supported and criticised the presence of a charging zone in the city centre. Their behaviours and proposals towards the policy have often been similar. For instance, the initial charging zone scheme (i.e., "Ecopass") was introduced by a right-wing mayor, whereas the subsequent scheme (i.e., "Area C") was implemented by a left-wing mayor. Furthermore, during the electoral campaign for the 2011 municipal elections (after three years since the initial instalment), both candidates expressed concern about the potential political consequences of the policy. Consequently, both of them criticised the current road pricing scheme and attributed the support of the policy to the opponent coalition. Interestingly, the right-wing incumbent mayor, who introduced the CZ, also blamed the left-wing for its support to the policy.[18] This simultaneous alignment towards the policy could be a potential explanation for the absence of any discernible impact on the vote share of either the incumbent or the opposition. On the other hand, the implementation of local road pricing policies significantly affects local politics, but their influence on national politics is limited, as these policies often lack broader resonance and may not take up a substantial portion of the national political agenda.

ROBUSTNESS CHECKS. We then test the robustness of the obtained result, reported in Table 3, column 2 of panel A. First, Figure 4 documents that our main result on municipal turnout is robust to the bandwidth selection, the exclusion of observations close to the threshold and *placebo* treatment assignments. In the left panel, we show that the estimates are relatively stable in size and statistically significant across a wide range of bandwidths between 200 and 400 metres, around the Calonico, Cattaneo, and Titiunik (2014) MSE-optimal bandwidth equal to 243 metres. As expected, as the bandwidth increases, the bias of the local polynomial estimator increases and

[18] FigureA.2 in the Appendix represents a political sign of the right-wing candidate posted in the city of Milan just before the second ballot of the municipal elections in 2011. The sign states: *"The Ecopass scheme* (i.e., first scheme of the charging zone) *will cost 10 euro for everyone if the left-wing wins. Letizia* (i.e. the right-wing incumbent who has actually introduced the CZ) *has abolished it"*.

FIGURE 4

ROBUSTNESS CHECKS OF RD EFFECT ON MUNICIPAL TURNOUT



Notes: In the left panel, the horizontal axis represents the bandwidths (in km) used in the estimation of the local linear regression. The black solid line represents the estimated coefficients for β_1 in equation 1 as a function of the chosen bandwidth, while the dashed lines represent the 95 percent confidence intervals of each coefficient. The vertical line represents the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth. In the central panel, the horizontal axis represents the distance (in km) between the CZ boundary and the observations excluded by the sample at each iteration. In the right panel, the horizontal axis represents the distance (in km) between the CZ boundary and a variety of placebo thresholds. All specifications include controls for distance to the polling station, distance to the closest metropolitan line station and election year fixed effects. 95% confidence intervals are based on standard errors robust to clustering at the precinct level and to the uncertainty in the selection of the optimal bandwidth.

its variance decreases. In the central panel, we provide evidence that the result is robust to the exclusion of those precincts located very close to the CZ boundary (up to 60 metres). The results of this exercise demonstrate that the outcome is not solely influenced by the precincts situated in close proximity to the boundary of the charging zone. In the right panel, we demonstrate that the estimated results are capturing the effect of the charging zone since we cannot estimate statistically significant effects at placebo thresholds, respectively located 200 and 100 metres away from the original threshold at opposite sides of it.

Second, Table B.2 in the Appendix provides evidence of the robustness of the main result to different model specifications. Column 1 shows the result on municipal turnout is analogous to the preferred specification (reported in column 2) when the geographic covariates and year fixed effects are excluded. Columns 3-4 present the estimates from a weighted RD model, which accounts for the number of eligible voters in each electoral precinct. Columns 5-6 present the findings of a difference-in-discontinuities (*diff-in-disc*) model, as outlined in Lalive (2008) and Grembi, Nannicini, and Troiano (2016).[19] This approach integrates the regression discontinuity design with a before-after estimation methodology, thereby eliminating potential pre-treatment discontinuities at the threshold from the standard RD estimates. The estimated effects are highly consistent across all different specifications, ranging from 8.2 to 10.3 percentage points, and are statistically significant at the 1 to 5 percent levels.

V MECHANISMS

In this section, we explore the potential mechanisms underlying the CZ effect on municipal turnout. In particular, we investigate (i) to what extent the discontinuity depends on policy-

[19] The *diff-in-disc* model estimates the following equation $y_{it} = \delta_0 + \delta_1 x_{i,t} + CZ_i(\gamma_0 + \gamma_1 x_{i,t}) + Post_t\left[\alpha_0 + \alpha_1 x_{i,t} + CZ_i(\beta_0 + \beta_1 x_{i,t})\right] + \epsilon_{it}$, where $x_{i,t}$ represents the distance of each electoral precinct to the CZ boundary and β_0 captures the *diff-in-disc* estimator. The regression is estimated within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014) with triangular kernel weights.

induced urban mobility or on changes in individual voting behavior and (ii) whether the results are consistent with an increased political engagement of the residents living inside of the CZ or a decreased interest of those located outside of it. Finally, we show heterogeneous treatment effects based on pre-determined census variables measuring mobility and family preferences.

A Has the way people vote changed, or has the voter base changed?

The results presented in Table 3 may be interpreted in light of two potentially complementary dynamics. On the one hand, it could be argued that CZ changed the way of voting of residents who were mainly impacted by the policy. Those who derived the greatest benefits from the policy may have been encouraged to engage more actively in local politics. Conversely, those who borne the greatest costs may have been less inclined to participate in a political system that did not adequately represent their interests. We refer to this dynamic as the direct impact of the policy on individual voting behaviour. On the other hand, the results may be driven by urban mobility. Indeed, some individuals may have moved into or out of the area once the policy was introduced. It is reasonable to posit that individuals living just inside and just outside the CZ after the policy implementation may not necessarily be comparable in all characteristics. Readers should note that this dynamic would not challenge the validity of our estimates, since any differences would be an outcome of the policy itself. However, it does offer another potential mechanism at work. We refer to this phenomenon as the indirect impact of the policy on aggregate voting behaviour, which acts through changes in voters' characteristics.

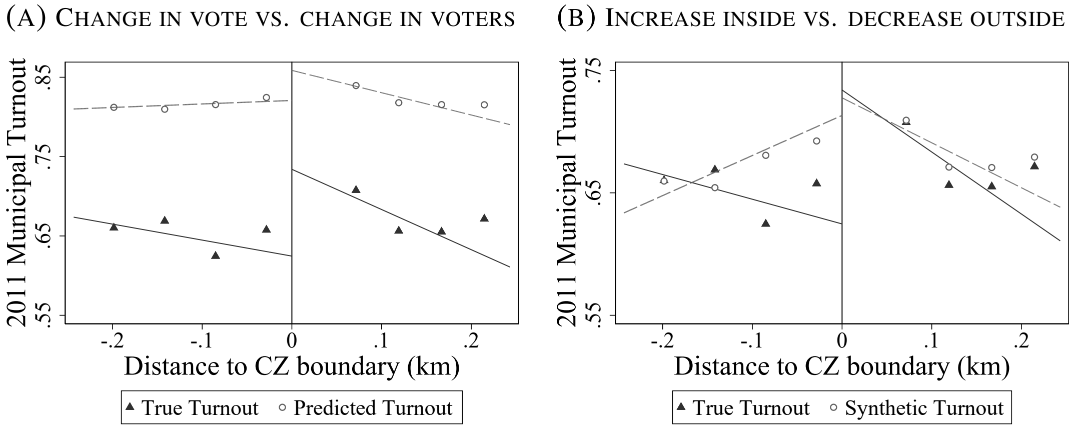
To understand the extent to which a change in the characteristics of the voters affects voter turnout, we conduct two exercises. First, we study the potential effect of the charging zone on the characteristics of the resident population. Similarly to the exercise on the pre-treatment covariate balancing, we estimate the CZ effect on the post-treatment population characteristics reported in the 2011 census, four years after the beginning of the program.[20] Table B.3 in the Appendix reports the estimates derived from the spatial RD, as specified in equation 1. Results demonstrate that no imbalances exist with regard to the demographic characteristics under examination suggesting that the discontinuity on municipal turnout are unlikely caused solely by demographic factors.[21] Nevertheless, a portion of the observed political effect may still be attributed to the interplay of diverse characteristics that exhibit a smooth variation across the threshold when considered individually but that may potentially exert an influence when considered collectively. Second, in order to consider the potential interplay between these variables, we conduct the following prediction exercise. We initially link the census variables to the electoral precincts.[22] We then regress turnout in each precinct in 2001 municipal election on several demographic characteristics measured in the same year's census, and use the estimated coefficients to predict each precinct's turnout in 2011 based on characteristics measured in the 2011 census. Figure 5a

[20]The "Ecopass" scheme was introduced on January 2^{nd} 2008 and data collection for the 2011 population census started in October 2011.

[21]In the 2011 population census, some information on mobility are no more available (i.e. preferred means of transportation, census block where the individual works or studies).

[22]For each electoral precinct, we compute the average values of the population census variables of those census blocks in which eligible voters reside.

FIGURE 5



Notes: Panel A shows observed turnout (black triangles) and predicted turnout based on the distribution of 2011 census characteristics at the district level and their association with turnout estimated in 2001. The running variable is the distance (in kilometers) to the closest point along the CZ boundaries. Panel B shows observed turnout (black triangles) and its synthetic counterfactual estimated following Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) using electoral precincts of Naples and Turin as donor pool. The running variable is the distance (in kilometers) to the closest point along the CZ boundaries.

shows the observed turnout in 2011 municipal election and the predicted counterfactual of the turnout. The intuition behind this empirical exercise is to provide a counterfactual scenario in which the composition of individuals living in the treated and control group changes while each individual's voting decision is based on the same strategy of the pre-reform period. The level difference between predicted (gray circles) and observed turnout (black triangles) on both sides of the threshold does not come as a surprise, as it reflects the reduction of voter turnout over time between 0.8 and 0.65 which is a well-known feature of the Italian municipal elections in those years. Interestingly, a small discontinuity at the threshold is observable for the predicted turnout. However, this only accounts for approximately one-third of the main estimated effect, suggesting that a significant portion of the effect reported in Table 3 is attributable to individuals modifying their turnout decision in response to the policy's adoption. Nevertheless, changes in the demographic composition of precincts situated just inside and just outside of the CZ contribute to explain why we document a small – albeit insignificant – effect also on national-level elections (the jump in predicted turnout is comparable in size to that in Table 3, column 2 of panel B).

B Have voters inside CZ increased turnout, or voters outside decreased it?

A limitation of the RD design in this setting is that the technique is only able to estimate the relative difference at the threshold between precincts located inside and outside the CZ. However, it is not possible to conclude whether the effect is driven by an increase in political participation by residents living inside the charging zone or by a disinterest of those residing outside of it. Indeed, the policy has had an impact on not only those living inside the regulated area but also on all individuals intending to access the city centre. Those residing barely outside of the charging zone may be significantly affected by the traffic regulation, due to the proximity of the CZ boundary, without benefiting from the decreased traffic in their neighbourhood and the discounted fee available to CZ residents only.

The inability to empirically distinguish the source of the effect represents a challenge in numerous RD applications in economics and other social sciences. For example, studies examining whether a party's success in previous elections confers an advantage in subsequent elections. In these studies, the causal effect of the observed discontinuity is typically attributed to the winning condition. However, it is possible that barely losing could also influence future outcomes. It is noteworthy that some recent studies have attributed the cause of the discontinuity to the losing condition (Bernharda and de Benedictis-Kessnerb, 2021; Wasserman, 2023). This highlights the absence of a genuine untreated counterfactual and a clearly defined methodology for disentangling the effects of the winning and losing conditions.

In order to distinguish between the two significant channels of political reward and punishment (Dechezleprêtre et al., 2022), we propose a novel methodology that combines a standard RD analysis with the synthetic control method (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010). The objective of this exercise is to provide a counterfactual turnout for observations on either sides of the CZ boundary that should illustrate how the turnout could have evolved in the absence of policy implementation. Specifically, we construct a 2011 synthetic municipal turnout for each electoral precinct in Milan using the electoral outcomes and demographic characteristics of the cities of Naples and Turin (i.e., the donor pool), which held their municipal elections on the same dates as Milan. In contrast to the electoral data for Milan, we do not know the addresses of the eligible voters of each precinct in Naples and Turin. Consequently, the electoral outcomes of the two donor cities are calculated at the polling station level, and their location is used to link them with the census variables of the closest census blocks.[23] Following Abadie, Diamond, and Hainmueller (2010), a synthetic counterfactual of the 2011 municipal turnout at precinct level can be computed as:

$$Y_i^{syn} = \sum_{j=1}^{n_0} W_{i,j}^* Y_j, \tag{2}$$

where $W_{i,j}^*$ is the weight given to the polling stations j, located in Naples and Turin, in the synthetic conterfactual unit corresponding to each electoral precinct i located in Milan. In particular, the n_0 vector of weights $W_i^* = \{W_{i,1}^*, ..., W_{i,n_0}^*\}$ solves:

$$\min_{W_i} \quad \|Z_i - \sum_{j=1}^{n_0} W_{i,j} Z_j\|^2 \quad \text{s.t.} \quad W_{i,1}, ..., W_{i,n_0} \geq 0 \quad \text{and} \quad \sum_{j=1}^{n_0} W_{i,j} = 1, \tag{3}$$

where Z_i and Z_j are vectors of the pre-treatment covariates (i.e., 2001 census variables, and 2001 and 2006 electoral outcomes respectively), for the precincts in Milan and the polling stations in the two donor cities respectively, used as predictors to find optimal weights in the post-treatment

[23] For each polling station, the demographic variables are computed as a weighted average of the values of all the census blocks whose centroids are within 300 metres the polling station.

synthetic outcomes (i.e., 2011 municipal turnout). [24]

Figure 5b presents the results of this analysis. The black triangles represent the observed average turnout of the electoral precinct within each bin, while the grey circles illustrate the average values of their synthetic counterfactuals. If the effect of CZ on turnout is predominantly attributable to the residents living within the CZ, then we will observe outcomes in precincts to the left of the threshold that mirror its synthetic counterfactual, and an observed outcome to the right of the threshold that, in contrast, is higher than its synthetic counterfactual. Conversely, if the effect of the CZ on turnout is primarily driven by those residing outside of the CZ, we will observe an inverse pattern: synthetic and observed turnout will be relatively similar to the right of the threshold, while the observed turnout will be lower than its counterfactual to the left of the discontinuity. This latter hypothesis is supported by the empirical evidence: the synthetic counterfactual closely resembles the observed turnout in precincts within the CZ, while it significantly exceeds the observed turnout just outside of the regulated area. It is noteworthy that the observed turnout exhibits the greatest divergence from the synthetic counterfactual in those precincts situated outside of the charging zone but in close proximity to the boundary, particularly within 150 metres. The results of this exercise support the conjecture that those individuals residing just outside the CZ boundary are the most negatively affected by the policy and that their participation in municipal elections is consequently reduced. In Section VI, we present further analysis to examine the different impact of traffic regulation on residents located within and outside of the CZ.

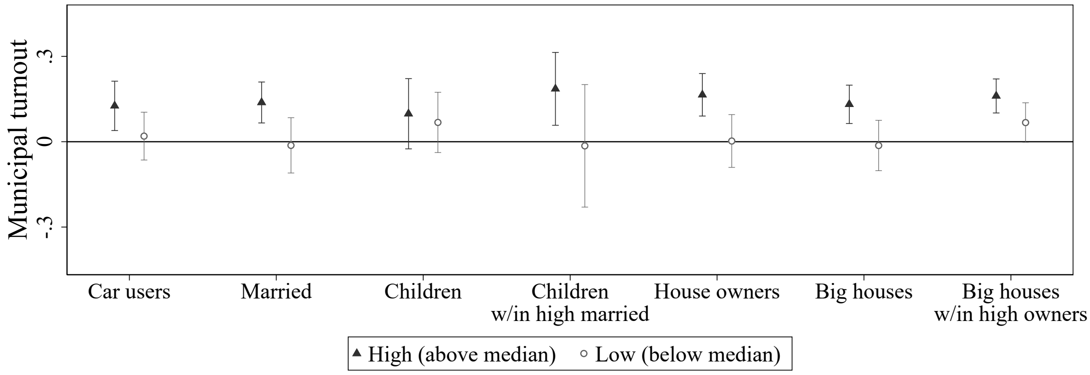
C Effects by demographic characteristics

In the previous subsections, we estimated that the pollution-based road pricing regime reduced turnout among individuals living just outsize the CZ. This effect is consistent with a perception of discontent by voters who appear to be mostly negatively affected by the policy. To further understand the mechanisms behind the estimated effect on turnout at the local level, we examine whether the magnitude and significance of the discontinuity vary across a range of population characteristics of each electoral precinct. For this heterogeneity analysis, we run separate regressions – as specified in equation 1 – for units below and above the median value for each of the demographic variables considered. In particular, we compute the median value within the optimal bandwidth of a set of variables from the 2001 census. Results are shown in Figure 6: the largest discontinuities are estimated when considering electoral precincts with shares of car users, married couples with children and house owners above the median, while no discontinuity

[24] In settings with a relatively large number of units (183 polling stations in Naples and 138 in Turin) in the donor pool, the minimization problem may have not a unique solution since multiple convex combinations of the predictors of non-experimental units could perfectly reproduce the values of experimental observations, generating biases and overfitting problems. To overcome this issue, we restrict the donor pool of each electoral precincts to those polling stations with an average pre-treatment turnout diverging 10 p.p at maximum from the one of the electoral precinct. We perform this analysis with the command `allsynth` in Stata described in Wiltshire (2022) to perform bias-corrected synthetic control estimates.
In addition to the average turnout in 2001 and 2006 municipal elections, we use the percentage of female, married, under 30, over 60, graduated, employed, italians, european immigrants, non european immigrants, and the average household size as pre-treatment covariates from the 2001 census.

FIGURE 6

EFFECTS ON MUNICIPAL TURNOUT BY DEMOGRAPHIC CHARACTERISTICS



Notes: The unit of observation is a electoral precinct-election year. Estimation method: local-linear regression with triangular kernel weights and MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). All specifications include controls for distance to the polling station, distance to the closest metropolitan line station and election year fixed effects. Black markers and spikes refer to coefficients and 95% confidence intervals from regressions in which the sample is restricted to values of each pre-determined variable reported in the horizontal axis above the median. Conversely, gray markers and spikes refer to coefficients and 95% confidence intervals from regressions in which the sample is restricted to values of each pre-determined variable reported in the horizontal axis below the median. Inference is based on standard errors robust to clustering at the electoral precinct level. Note that "Children w/in high married" is restricting the sample based on *children* being above or below the median value conditional on *married* being above the median value, while "Big houses w/in hih owners" is restricting the sample based on *big house* being above or below the median value conditional on *house owners* being above the median value.

is estimated when looking at those electoral precincts with these characteristics below the median. The results of the heterogeneity exercise seems to suggest that (i) the policy affects only families with children, with no discernible impact on childless couples, and (ii) all homeowners are impacted, with the effect increasing in proportion to the size of the house. These findings are strongly in line with the losers' discontent mechanism: the impact is visible in localities where families drive regularly and own the apartment in which they live. On the one hand, the policy requires either the payment of a high fee or the use of alternative means of transportation in order to enter the city centre of Milan. However, families with children may find it particularly costly to reduce car usage. On the other hand, relocating inside the CZ (or far away) can be particularly costly for house owners, thus further re-inforcing discontent.

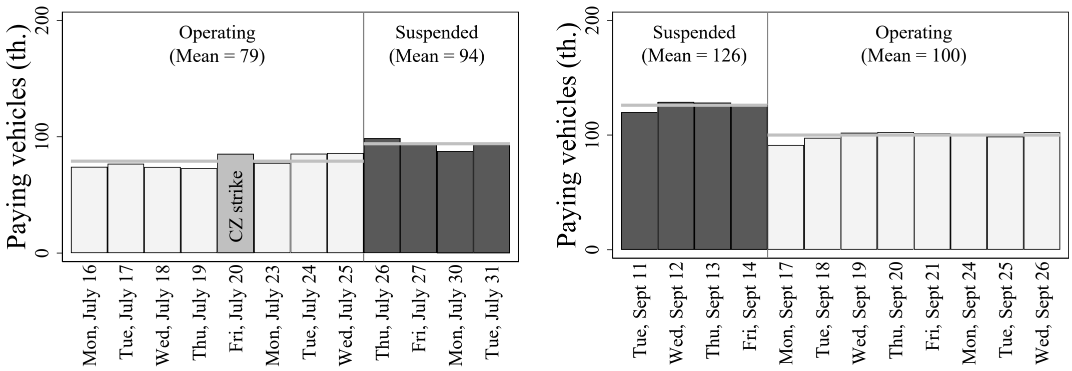
VI THE HETEROGENEOUS EFFECTS ON RESIDENTS INSIDE AND OUTSIDE OF THE CZ

This section presents evidence of the heterogeneous impact of the policy on residents living inside and outside the CZ. Differential benefits and costs experienced by individuals based on their location may be a significant contributing factor to the observed decline in electoral participation in precincts situated just outside the regulated area.

In order to study the differential impacts of the CZ, we exploit the sudden and temporary policy suspension that occurred between July and September 2012, described in Section II. A policy evaluation based on the analysis of this suspension has already been provided by Gibson and Carnovale (2015). With regard to this study, we are able to distinguish the impact of the

FIGURE 7

(A) CZ SUSPENSION (B) CZ RE-INSTALMENT



Notes: The figure illustrates the number of those vehicles accessing the CZ on a daily basis, which are required to pay the fee. Panel A depicts the number of entrances (in thousands) into the CZ on working days immediately preceding and following the suspension of the policy, which occurred on Thursday 26th July 2012. On Friday 20th July 2012, the CZ was suspended and vehicles were permitted to enter the area free of charge due to a national strike affecting public transport. Panel B focuses on the working days immediately preceding and following the re-installation of the policy, which occurred on Monday 17th September 2012.

CZ on vehicles belonging to residents living within the area and those belonging to others. We leverage administrative data from the municipality of Milan's agency for mobility and the environment (AMAT). Our data contains information on the number of vehicles entering the CZ from each of the 43 gateways on a hourly basis over the course of the year 2012. We also observe information on the pollution class of the vehicle and on whether the owner was a resident of the CZ or not. Furthermore, we collect data on pollution (i.e., hourly values of CO and NO2) from the Lombardy region's Agency for Environmental Protection (ARPA) to complement this heterogeneity analysis by studying the impact of the CZ on pollution, measured by 8 air quality stations (only 4 with CO sensors) distributed across the territory of Milan.

To study the effect of the policy on traffic and pollution, we focus our analysis on the re-introduction of the CZ, which occurred on Monday 17th September 2012. In particular, we deploy an RD design in which the running variable measures the time distance (in hours of working days) to the re-instalment of the road pricing scheme and compare vehicle entrances and pollution levels in the hours just before and just after the policy change. The decision to focus our estimates on the re-instalment of the policy – rather than the initial suspension – is based on two main reasons. Firstly, the sudden suspension of the policy occurred after more than four years of operating policy. After such a prolonged period of traffic regulation, citizens could have adopted structural changes to avoid (or decrease) the cost of accessing the charging zone (e.g., changing the children's school or family doctor, moving inside or far way from the area, etc.) making our short-term estimates a lower bound on long-run effects.[25] Furthermore, it is plausible that individuals may require some time to adapt to the renewed possibility of using a car to access the area at no cost (e.g., switching from public transport to private vehicles). Consequently, focusing

[25]Other structural changes in the long-run, such as buying a free of charge LEV, do not influence our estimates on traffic (but it does on pollution) since they were accessing the area free of charge before the policy implementation and after its introduction.

our analysis on the initial suspension may result in a further underestimation of the actual impact of the policy. Figure 7 illustrates the daily number of vehicles subject to fee accessing the area in time periods around the policy's initial suspension and its re-instalment. A change in volumes just before and just after the initial policy suspension is present but quite smooth (panel A), while the change after the CZ re-instalment is more substantial (panel B). Secondly, on Friday 20th July 2012, the CZ was suspended and vehicles were permitted to enter the area free of charge due to a national strike affecting public transport. The presence of this potential alteration to the volume of vehicles accessing the CZ (observable in Figure 7, panel A), only four working days before the initial suspension, makes the effect of the re-instalment of the policy less problematic to estimate. Formally, we estimate:

$$y_{i,h,d} = \alpha_0 + \alpha_1 \times ReInstall_d + f(time)_{h,d} + \theta_i + \lambda_h + \phi_d + \varepsilon_{i,h,d}. \tag{4}$$

In equation (4), the coefficient of interest is α_1, which estimates the causal effect of the re-instalment of the road pricing regime. θ_i is the unit fixed effect (i.e., gateway and air quality station, respectively), λ_h is the hour-of-day fixed effect, and ϕ_d is the day-of-week fixed effect. Our preferred specification is a local-linear regression with triangular kernel, estimated within the MSE-optimal bandwidth calculated according to Calonico, Cattaneo, and Titiunik (2014). To improve the precision of the estimates, we augment the empirical model with controls for hourly temperatures and rainy hours at the municipal level from ARPA. In the pollution analysis, we include also controls for hourly humidity levels, wind speed and wind direction fixed effects which could affect the level of pollutants.

TABLE 4

EFFECT OF CZ RE-INSTALMENT ON VEHICLE TRANSITS

Dep. var.: log(Vehicle transits)	Residents inside CZ				Residents outside CZ			
	Fee		No-fee		Fee		No-fee	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Kernel	Uniform	Triang	Uniform	Triang	Uniform	Triang	Uniform	Triang
RD Effect	-0.183***	-0.219***	0.003	0.004	-0.330***	-0.325***	0.007	-0.044
	(0.051)	(0.048)	(0.008)	(0.007)	(0.082)	(0.075)	(0.048)	(0.048)
Robust p-value	0.008	0.001	0.440	0.477	0.005	0.001	0.239	0.964
MSE bandwidth	253	365	406	485	306	385	174	349
Mean transits w/in bw	18.27	17.19	0.21	0.20	88.95	84.09	18.45	17.33
Observations	21294	30618	34146	40698	25746	32298	14658	29358
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is the natural logarithm of the number of hourly private vehicle accesses at each CZ gateway. The unit of observation is a gateway-hour. Estimation method: local-linear regression with triangular kernel weights and MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). All specifications include controls for temperature, rainfall hours, gateway fixed effects, hour fixed effects, day of week fixed effects. Inference is based on standard errors robust to clustering at the gateway level and to the uncertainty in the selection of the optimal bandwidth. *, **, and *** denote the 10%, 5%, and 1% significance levels, respectively.

A Effect on vehicles accessing the CZ

Table 4 reports the estimated impact of the CZ re-instalment on the number of private vehicles accessing the area. It shows that the policy has a negative impact on vehicles required to pay the fee, with citizens living outside of the area being penalised the most. Columns 1-4 show the results for residents living within the CZ, and columns 5-8 for others. As for the political analysis, we propose the estimates with uniform and triangular kernel weights for each analysed outcome. Columns 1-2 show that the policy reduced access between by 17% ($= e^{-0.183} - 1$) and 20% ($= e^{-0.219} - 1$) for vehicles subject to fee and belonging to residents living within the area. Both estimates are statistically significant at 1 percent level. As expected, residents' vehicles exempted from the fee (e.g., low emission vehicles) are not affected by the policy re-instalment, as reported in columns 3-4. Columns 5-6 demonstrate that the policy has the greatest impact on those citizens residing outside of the charging zone and whose vehicles are required to pay the fee. The negative impact on the transits of these vehicles is estimated to be approximately by 28% ($= e^{-0.33} - 1$) ans statistically significant at 1 percent level. Lastly, we report that the policy does not affect free of charge vehicles belonging to citizens living outside of the CZ, in columns 7-8.

As a test of the robustness of the results, we propose two further analyses. First, we compute the RD estimates on the vehicle counts (not log transformed) presented in B.4 in the Appendix. The results are very similar in terms of magnitude and statistical significance. Second, we estimate the differential effect of vehicles subject to fee (with respect to those free of charge) from separate equations for CZ residents and others.[26] The results reported in Table B.5 in the Appendix confirm that only the vehicles subject to the fee have been affected by the reintroduction of the road pricing scheme, and that citizens residing outside the CZ have been more adversely impacted.

Figure 8 decomposes the effects estimated in Table 4 by transit time. Since the road pricing is effective on working days between 7.30 a.m. and 7.30 p.m., we expect its re-instalment to decrease traffic congestion especially in this time window, while we do not expect large effects in other time slots.[27] Specifically, we estimate that the re-instalment reduced the entries of vehicles subject to fee by 40% for those owned by CZ residents (black circles) and up to 55% for those owned by individuals not living in the CZ (black triangles) during the rush hours. Consistently with the results estimated in Table 4, the impact of the re-instalment on the number of accesses by vehicles subject to the fee is larger for citizens living outside of the CZ across the road pricing regime enforcement time window. As expected, vehicles exempt from the fee payment are instead unaffected by the policy change.

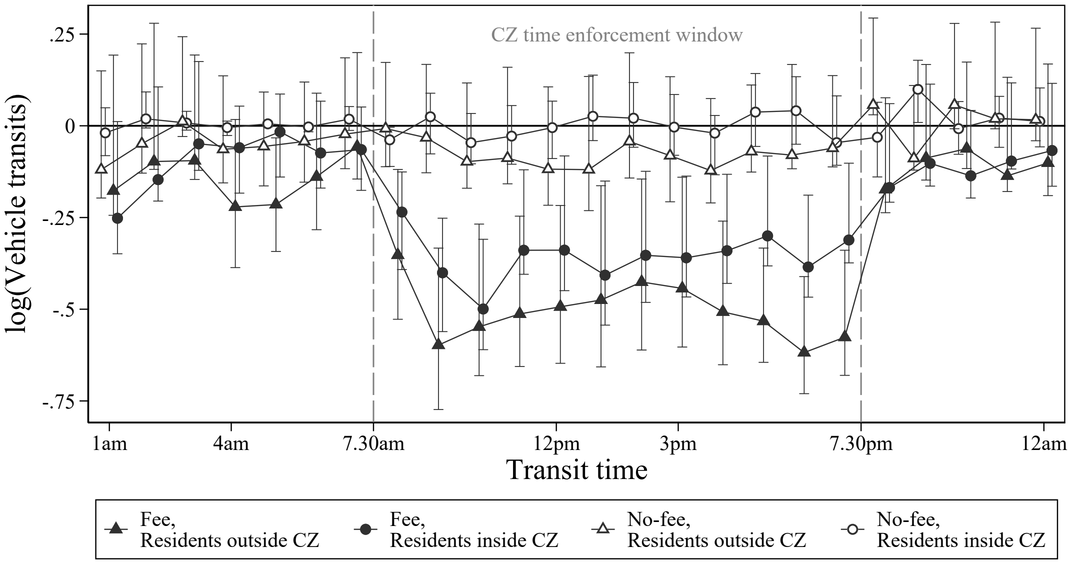
Overall, the effects of CZ on vehicle traffic patterns appear to be consistent with expectations. Firstly, it can be observed that vehicles that are not subject to the fee are not experiencing any

[26]We estimates the differential effect of vehicles subject to fee from the following equation $y_{it} = \delta_0 + \delta_1 x_{i,t} + Reinstall_t(\gamma_0 + \gamma_1 x_{i,t}) + Fee_i \left[\alpha_0 + \alpha_1 x_{i,t} + Reinstall_t(\beta_0 + \beta_1 x_{i,t})\right] + \epsilon_{it}$, where $x_{i,t}$ represents the time distance to the CZ re-instalment and β_0 captures the differential effect for vehicles subject to fee.

[27]Estimating heterogeneous effects by transit time also allows to establish whether a sizeable number of vehicles tend to enter the restricted area just before 7.30 a.m. or just after 7.30 p.m. to avoid the fee. If this behavior were salient, we would expect the re-instalment to increase the number of accesses into the CZ before 7.30 a.m. and after 7.30 p.m. as well as the opposite effects during the 7.30 a.m. to 7.30 p.m. frame.

FIGURE 8

HETEROGENEOUS TRANSIT EFFECTS BY TRANSIT TIME



Notes: The figure illustrates the RD effects and 95 percent robust bias-corrected confidence intervals of the re-instalment on vehicles accessing the area by transit time. The dependent variable is the natural logarithm of the number of hourly private vehicle accesses at each CZ gateway. The unit of observation is a gateway-hour. Estimation method: local-linear regression with triangular kernel weights and MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). All specifications include controls for temperature, rainfall hours, gateway fixed effects, and day of week fixed effects. Each regression restricts the sample to transits recorded in a time window equal to a specific hour. Inference is based on standard errors robust to clustering at the gateway level and to the uncertainty in the selection of the optimal bandwidth.

impact from the road pricing re-instalment. This reassures us that the model is not capturing potential confounding effects, such as the reopening of schools. Secondly, individuals residing outside of the CZ are more severely penalised. In fact, the "Area C" scheme requires a €5 fee for polluting vehicles in general, but only a €2 fee after 40 free daily entrances for residents living within the CZ. In addition to the the discounted fee, residents living within the charging zone can take advantage of free mobility with their vehicle within the area, since fees are required only to access the zone.

B EFFECT ON POLLUTION

We complement the heterogeneity analysis by studying the effect of the CZ re-instalment on pollution. This analysis allows us to estimate the potential impact of the charging zone on pollution within the area and at a larger scale within the municipality. Furthermore, it may suggest potential traffic patterns across the city. It is important to note that the reduction in vehicles accessing the city centre does not necessarily imply a reduction in the use of private vehicles in general. Individuals who have to cross the city may have decided to circumvent the city centre instead of driving through it. This could even result in an increase in traffic just outside the charging zone, which would further exacerbate the situation for those who live in those areas.

TABLE 5

EFFECT OF CZ RE-INSTALMENT ON POLLUTION

Dep. var.	CO (mg/m^3)				NO2 (μg/m^3)			
	Inside CZ		Outside CZ		Inside CZ		Outside CZ	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Kernel	Uniform	Triang	Uniform	Triang	Uniform	Triang	Uniform	Triang
RD Effect	-0.17***	-0.20***	-0.14***	-0.15***	-5.06***	-10.01***	1.22	-0.35
	(0.017)	(0.017)	(0.039)	(0.053)	(1.806)	(0.685)	(2.646)	(3.146)
Robust p-value	0.000	0.000	0.280	0.183	0.141	0.000	0.201	0.383
MSE bandwidth	368	391	368	391	221	267	221	267
Mean transits w/in bw	0.82	0.81	0.82	0.81	55.91	53.50	55.91	53.50
Observations	732	778	2175	2313	796	955	2571	3105
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The dependent variables are the CO and NO2 levels registered in each air quality station. The unit of observation is a station-hour. Estimation method: local-linear regression with triangular kernel weights and MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). All specifications include controls for temperature, rainfall hours, rain, humidity, wind speed, wind direction fixed effects, station fixed effects, hour fixed effects, day of week fixed effects,. Inference is based on standard errors robust to nearest neighbours and clustering at the station level. *, **, and *** denote the 10%, 5%, and 1% significance levels, respectively.

However, the impact of the policy on pollution within the specified model may be challenging to measure for several reasons. Pollution is only partially generated by road transport and even less by private vehicles. For instance, only half of particulate matter (PM) registered in cities is produced by factors located within the city, and their values can be relatively stable within a short time.[28] In light of the these considerations, our analysis is primarily focused on local pollutants, such as carbon monoxide (CO) and nitrogen dioxide (NO2), which are primarily emitted by fossil fuel-powered engines. These pollutants are registered on a hourly basis by eight air quality stations situated within the administrative boundaries of Milan (only four for CO), with two of these stations located within the charging zone (only one for CO).

Table 5 reports the estimates of the CZ re-instalment on pollution. The results indicate that the policy may have resulted in a reduction in the concentration of local pollutants only inside the charging zone. Columns 1-4 present the results for carbon monoxide (CO), while columns 5-8 display the findings for nitrogen dioxide (NO2). Separate estimates are computed for both pollutants at air quality stations located inside and outside of the CZ. The results are presented for uniform and triangular kernel weights. Columns 1-2 show that the policy reduced CO concentrations by 0.17 to 0.20 mg/m^3 inside the CZ, indicating a -21% and -24% impact with respect to their pre-treatment mean values, respectively. Columns 3-4 report that the effect is slightly less pronounced outside of the CZ. However, the bias-robust p-values remain well above the 0.1 level, which renders these estimates not statistically significant. Columns 5-6 demonstrate that the policy has also an impact on CO2 within the CZ. The estimated negative impact on nitro-

[28]The annual report on Air Quality in European Cities by the JRC - European Commission indicates that 57% of 2015 annual PM 2.5 in Milan is produced within the city (only 30% in 2022), and approximately 16% depends on the road transport within the city (less than 10% in 2022).

gen dioxide is between 5 and 10 $\mu g/m^3$, resulting in a -9% and -18% reduction respectively. In this case, the bias-corrected robust p-values are below the 1% level when the triangular kernel weights are used (column 6). Column 7-8 indicate that the policy has no discernible impact on NO2 levels outside of the charging zone.

Our estimates should be interpreted with caution due to both the persistence and dispersion of these pollutants in the atmosphere.[29] Nevertheless, our RD results on pollution levels are in line with the observed effect on traffic and the voter's behaviour. The air quality of the regulated area is benefiting the most from the road pricing re-instalment, while other areas are less impacted by the policy. These results could indicate that the policy may have only partially reduced traffic in the city, while it has displaced part of the traffic outside the charging zone. In fact, the decreased levels of pollutants in the near charging zone could be partially compensated by an increased traffic, which circulates around the CZ, leaving the levels of these pollutants in the areas outside of the CZ almost unaltered. It is possible that citizens residing outside the road-pricing zone may have experienced no improvement in the air quality of their neighbourhood, and may even have observed an increase in congestion. Moreover, these estimates further support the conjecture that those individuals residing just outside the CZ are the actual losers of the policy.

VII Concluding remarks

This paper employs a quasi-experimental design based on geographic discontinuities to investigate the political implications of the introduction of a charging zone (i.e., a pollution-based road pricing) in the city centre of Milan. We compare electoral precincts located in close proximity to boundaries of the charging zone before and after the implementation of the policy under a spatial regression discontinuity design. We also develop a novel methodology, that combines the RD design with the synthetic control method, to distinguish the source of the effect which originates from electoral precincts situated inside and outside of the area.

Our findings indicate that electoral precincts situated outside but in close proximity to the boundary of the charging zone have exhibited a decline in turnout in municipal elections. Individuals residing just outside of the area have a high probability of needing to reach locations inside of it, yet they lack access to the fee discounts reserved for residents of the charging zone. Families with children, who rely heavily upon their cars to move, and homeowners, whose cost to relocate is high, are the most negatively affected by the traffic regulation change.

Overall, we show that individuals adversely impacted by the policy may not punish the incumbent or a specific party in the event of high political alignment between opponent parties towards the environmental policy. Dissatisfied citizens, who perceive a lack of representation in the local political system, have demonstrated a decreased interest in participating in municipal elections. Notably, this disinterest appears to persist over time, with no evidence of a reversal in the three municipal elections following the initial implementation of the policy. The observed persistence might be in line with the daily costs that the policy imposes.

[29] According to NASA, the lifetime of CO in the atmosphere is about a month, persisting long enough to be transported long distances by winds. Thus, confounding factors could impact our estimates.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2010. "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program." *Journal of the American Statistical Association* 105 (490):493–505.

Abadie, Alberto and Javier Gardeazabal. 2003. "The economic costs of conflict: A case study of the Basque Country." *American Economic Review* 93 (1):113–132.

Bernharda, Rachel and Justin de Benedictis-Kessnerb. 2021. "Men and women candidates are similarly persistent after losing elections." *Proceeding of the National Academy of Science* 118 (26):375–396.

Besley, Timothy and Torsten Persson. 2023. "The Political Economics of Green Transitions*." *The Quarterly Journal of Economics* 138 (3):1863–1906.

Calonico, Sebastian, Matias D Cattaneo, and Rocio Titiunik. 2014. "Robust nonparametric confidence intervals for regression-discontinuity designs." *Econometrica* 82 (6):2295–2326.

Cattaneo, Matias D, Michael Jansson, and Xinwei Ma. 2020. "Simple local polynomial density estimators." *Journal of the American Statistical Association* 115 (531):1449–1455.

Colantone, Italo, Livio Di Lonardo, Yotam Margalit, and Marco Percoco. 2024. "The political consequences of green policies: Evidence from Italy." *American Political Science Review* 118 (1):108–126.

Conte, Bruno, Klaus Desmet, and Esteban Rossi-Hansberg. 2022. "On the Geographic Implications of Carbon Taxes." Working Paper 30678, National Bureau of Economic Research.

Cruz, José-Luis and Esteban Rossi-Hansberg. 2022. "Local Carbon Policy." (30027).

Davis, L. W. 2008. "The Effect of Driving Restrictions on Air Quality in Mexico City." *Journal of Political Economy* 116 (1):38–81.

Dechezleprêtre, Antoine, Adrien Fabre, Tobias Kruse, Bluebery Planterose, Ana Sanchez Chico, and Stefanie Stantcheva. 2022. "Fighting climate change: International attitudes toward climate policies." Tech. rep., National Bureau of Economic Research.

Douenne, T. and A. Fabre. 2022. "Yellow Vests, Pessimistic Beliefs, and Carbon Tax Aversion." *American Economic Journal: Economic Policy* 14 (1):81–110.

EEA. 2018. "Europe's urban air quality: re-assessing implementation challenges in cities." *European Environment Agency Report* (24).

———. 2020. "Air quality in Europe — 2020 report." *European Environment Agency Report* (09).

Gehrsitz, M. 2017. "The effect of low emission zones on air pollution and infant health." *Journal of Environmental Economics and Management* 83 (1):121–144.

Gibson, M. and M. Carnovale. 2015. "The effects of road pricing on driver behavior and air pollution." *Journal of Urban Economics* 89 (1):62–73.

Green, C. P., J. S. Heywood, and M. Navarro. 2016. "Traffic accidents and the London congestion charge." *Journal of Public Economics* 113 (1):11–22.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano. 2016. "Do fiscal rules matter?" *American Economic Journal: Applied Economics* :1–30.

Hahn, J., P. Todd, and W. Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69 (1):201–209.

Imbens, G. W. and T. Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics* 142 (2):615–635.

Lalive, Rafael. 2008. "How do extended benefits affect unemployment duration? A regression discontinuity approach." *Journal of Econometrics* 142 (2):785–806.

Leape, J. 2006. "The London Congestion Charge." *Journal of Economic Perspectives* 20 (4):157–176.

Martino, Angelo. 2012. "Milano, from pollution charge to congestion charge."

McCrary, J. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142 (2):698–714.

Percoco, Marco. 2013. "Is road pricing effective in abating pollution? Evidence from Milan." *Transportation Research Part D: Transport and Environment* 25:112–118.

Stiglitz, J. E., N. Stern, and Others. 2017. "Report of the High-Level Commission on Carbon Prices." .

Stokes, Leah C. 2016. "Electoral backlash against climate policy: A natural experiment on retrospective voting and local resistance to public policy." *American Journal of Political Science* 60 (4):958–974.

Viard, V Brian and Shihe Fu. 2015. "The effect of Beijing's driving restrictions on pollution and economic activity." *Journal of Public Economics* 125:98–115.

Wang, L., J. Xu, and P. Qin. 2014. "TWill a driving restriction policy reduce car trips? The case study of Beijing, China." *Transportation Research Part A* 67 (1):279–290.

Wasserman, Melanie. 2023. "Gender Differences in Politician Persistence." *The Review of Economics and Statistics* 105 (2):275—-291.

Wiltshire, Justin. 2022. "ALLSYNTH: Stata module to automate estimation of (i) bias-corrected synthetic control gaps (" treatment effects")." .

Wolff, H. 2015. "Keep Your Clunker in the Suburb: Low Emission Zones and Adoption of Green Vehicles." *The Economic Journal* 124 (578):481–512.

FIGURE A.1
ECOPASS AND AREA C SCHEMES

ECOPASS (2008 – 2011)			AREA C (2012 -)		
Vehicle	**Access**	RESIDENT SUBSCRIPTION	**Vehicle**	**Access**	RESIDENT TICKETS
Class I Class II	FREE		Zero/Low Emissions	FREE	
Class III Class IV Class V	€ 2 € 5 € 10	€ 50 € 125 € 250	Less Polluting	€ 5	40 free € 2
			More Polluting	BANNED	

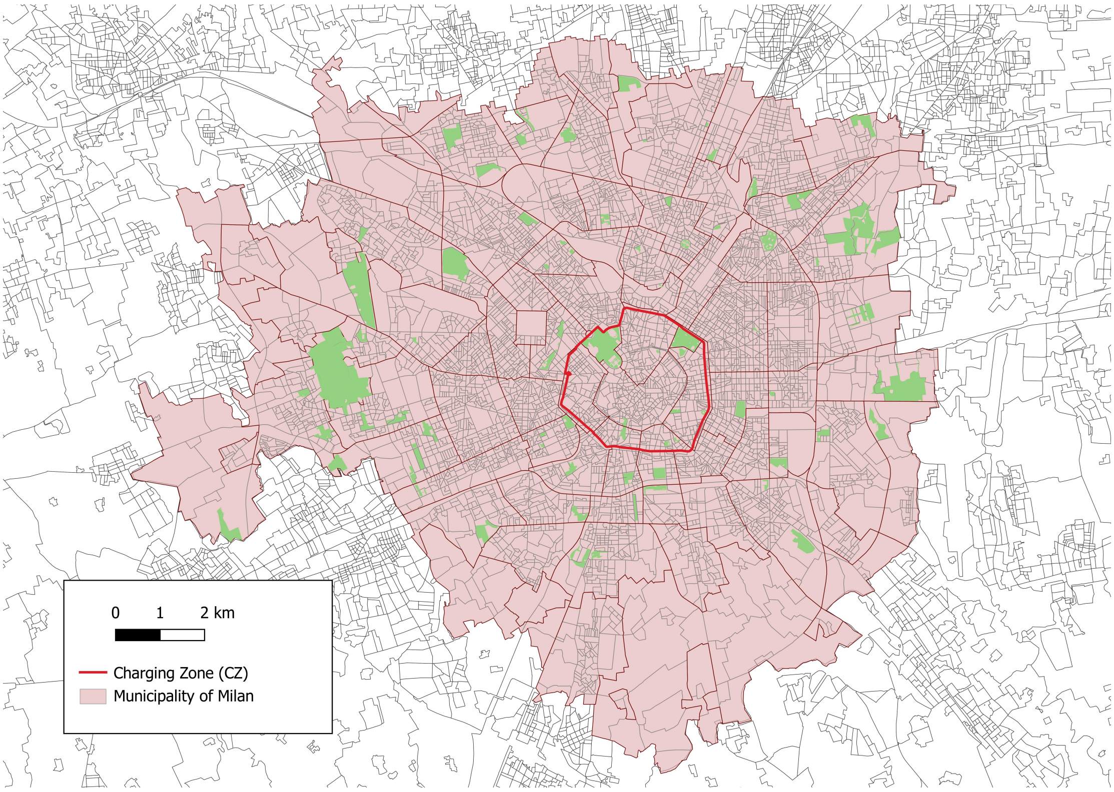
Notes: Class I includes Bi-fuel, Hybrid and Full electric vehicles; Class II includes Petrol Euro 3 and later vehicles, Diesel with DPF Euro 3 diesel and later vehicles, and Diesel without DPF Euro 5; Class III includes Petrol Euro 1 and 2; Class IV includes Petrol Euro 0 and Diesel Euro 1, 2, 3 and Diesel without DPF Euro 4; Class V includes Diesel Euro 0.

FIGURE A.2
RIGHT-WING POLITICAL SIGN AGAINST THE CHARGING ZONE IN 2011

Notes: This picture represents a political sign of the right-wing candidate posted in the city of Milan just before the second ballot of the municipal elections in 2011. The sign states: *"The Ecopass scheme* (i.e., first scheme of the charging zone) *will cost 10 euro for everyone if the left-wing wins. Letizia* (i.e. the right-wing incumbent who has actually introduced the CZ) *has abolished it"*.

FIGURE A.3

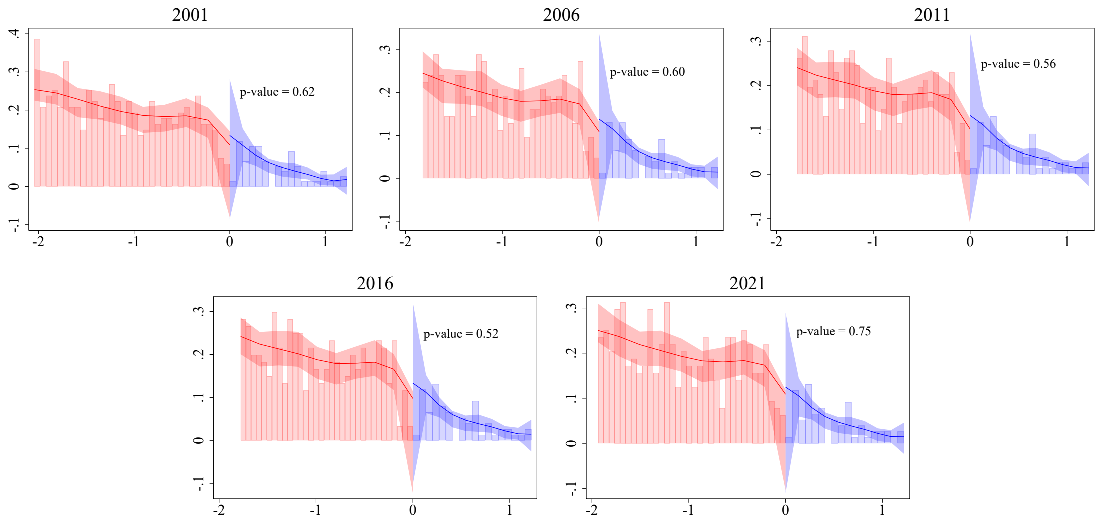
MAP OF THE CITY OF MILAN AND THE CHARGING ZONE



Notes: This picture shows the map of Milan and the charging zone based on an authors' elaboration on data elaboration from the municipality of Milan and the Italian Institute for Statistics (ISTAT).

FIGURE A.4

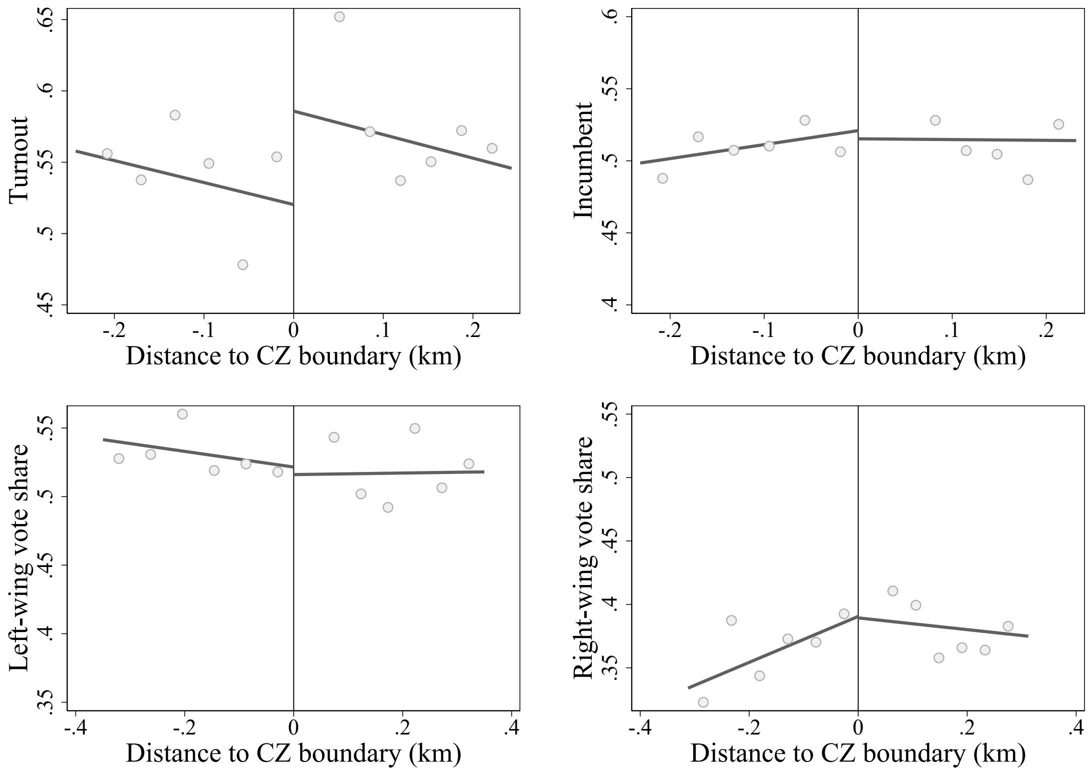
CATTANEO, JANSSON, AND MA (2020) TEST FOR MANIPULATION OF THE RUNNING VARIABLE



Notes: Log-density discontinuity and standard errors are computed performing a formal McCrary (2008) with optimal bandwidth using the method proposed by Cattaneo, Jansson, and Ma (2020). 95% confidence intervals are based on standard errors robust to clustering at the precinct level and to the uncertainty in the selection of the optimal bandwidth.

FIGURE A.5

RD EFFECTS IN MUNICIPAL ELECTIONS



Notes: The figure illustrates the RD effect on turnout (top-left), incumbent vote share (top-right), left-wing vote share (bottom-left) and right-wing vote share (bottom-right) for pooled municipal elections following the implementation of the road pricing policy (i.e., 2011, 2016 and 2021). The running variable is the distance (in kilometres) to the closest point along the CZ boundary. Estimation method: local-linear regression within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014).

B TABLES

TABLE B.1
PRE-TREATMENT ELECTORAL OUTCOMES AT CZ BOUNDARY

Dep. var.	Turnout		Incumbent		Left-wing		Right-wing	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Kernel	Uniform	Triang	Uniform	Triang	Uniform	Triang	Uniform	Triang
Panel A. Municipal elections.								
RD Effect	0.007	0.021	0.008	0.021	0.024	0.026	-0.022	-0.016
	(0.023)	(0.029)	(0.042)	(0.038)	(0.038)	(0.031)	(0.040)	(0.034)
Robust p-value	0.802	0.623	0.857	0.394	0.476	0.589	0.600	0.929
MSE bandwidth	0.264	0.243	0.245	0.232	0.286	0.350	0.286	0.312
Mean w/in bw	0.63	0.63	0.54	0.54	0.46	0.46	0.45	0.45
Observations	117	104	106	102	137	174	137	150
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Panel B. National elections.								
RD Effect	-0.002	0.015	-0.005	-0.013	0.012	0.004	-0.031	-0.032
	(0.026)	(0.025)	(0.013)	(0.011)	(0.045)	(0.029)	(0.046)	(0.030)
Robust p-value	0.988	0.560	0.391	0.159	0.858	0.718	0.595	0.490
MSE bandwidth	0.211	0.241	0.285	0.378	0.251	0.422	0.255	0.345
Mean w/in bw	0.81	0.81	0.41	0.41	0.40	0.41	0.45	0.44
Observations	84	104	137	188	110	216	111	171
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The unit of observation is a electoral precinct-election year. *Panel A* collects pooled data for 2001 and 2006 municipal elections; *panel B* collects pooled data for 2001 and 2006 national elections. *Incumbent* represents the vote share received by the coalition that won the most seats in the municipal council (or Parliament respectively) in the previous election. Estimation method: local-linear regression within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014) with uniform or triangular kernel weights. All specifications include controls for distance to the polling station, distance to the closest metropolitan line station and election year fixed effects. Inference is based on standard errors robust to clustering at the electoral precinct level. Conventional standard errors are shown in parentheses, also robust bias-corrected p-value and 95% confidence intervals are reported. *, **, and *** denote the 10%, 5%, and 1% significance levels, respectively.

TABLE B.3

EFFECT OF CZ ON (2011 CENSUS) DEMOGRAPHIC CHARACTERISTICS

Variable	Conventional RD effect	Robust p-value	Robust 95% c.i.	MSE Bandwidth	Mean w/in bw	Effective Obs.	Total Obs.
Population	–0.091	0.750	[–0.385; 0.278]	0.391	4.979	590	5718
Female	–0.009	0.373	[–0.035; 0.013]	0.377	0.539	568	5718
Married	0.007	0.658	[–0.028; 0.045]	0.318	0.399	490	5718
Divorced	0.001	0.561	[–0.007; 0.014]	0.353	0.043	537	5718
Children	–0.009	0.186	[–0.028; 0.006]	0.360	0.089	544	5718
Under 30	–0.000	0.857	[–0.031; 0.026]	0.378	0.263	568	5718
Over 60	0.017	0.503	[–0.029; 0.059]	0.373	0.297	563	5718
Graduated	0.025	0.168	[–0.014; 0.081]	0.251	0.397	377	5718
Italians	0.005	0.539	[–0.020; 0.038]	0.313	0.904	485	5718
Non-europeans	–0.003	0.670	[–0.031; 0.020]	0.349	0.069	530	5718
Students	0.001	0.976	[–0.015; 0.015]	0.438	0.071	664	5718
Commuters	–0.012	0.648	[–0.037; 0.023]	0.409	0.479	621	5718
Employed	0.007	0.645	[–0.017; 0.027]	0.345	0.952	526	5681
Household size	0.007	0.978	[–0.053; 0.055]	0.449	0.670	673	5707

Notes: The unit of observation is the census block of the 2011 Italian Population Census. *Population* and *household size* varibles are log tranformed. Some census block have no information on the commuting and household surveys. Results estimated with local linear regressions, triangular kernel weights and MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). P-values are based on robust bias-corrected inference.

TABLE B.2

EFFECT OF CZ ON MUNICIPAL TURNOUT ACROSS DIFFERENT MODEL SPECIFICATIONS

Dep.var.: Municipal Turnout						
Model	RD		Weighted RD		Diff-in-disc	
	(1)	(2)	(3)	(4)	(5)	(6)
Controls		✓		✓		✓
CZ	0.101**	0.104***	0.099**	0.103***		
	(0.042)	(0.038)	(0.045)	(0.039)		
CZxPost					0.082**	0.093**
					(0.038)	(0.042)
MSE bandwidth	0.243	0.243	0.243	0.243	0.243	0.243
Mean w/in bw	0.63	0.63	0.63	0.63	0.771	0.876
Observations	150	150	150	150	254	252

Notes: The unit of observation is a electoral precinct-election year. Columns 1-2 report estimates from a standard RD design; columns 3-4 report results from a RD weighted for the eligible voters in each precincts; columns 5-6 report estimates from a different-in-discontinuities model (see Grembi, Nannicini, and Troiano, 2016) estimating the following equation $y_{it} = \delta_0 + \delta_1 x_{i,t} + CZ_i(\gamma_0 + \gamma_1 x_{i,t}) + Post_t \left[\alpha_0 + \alpha_1 x_{i,t} + CZ_i(\beta_0 + \beta_1 x_{i,t})\right] + \epsilon_{it}$, where $x_{i,t}$ represents the distance of each electoral precinct to the CZ boundary and β_0 captures the *diff-in-disc* estimator. Estimation method: local-linear regression within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014) with triangular kernel weights. Specifications with controls include distance to the polling station, distance to the closest metropolitan line station and election year fixed effects. Inference is based on standard errors robust to clustering at the electoral precinct level. *, **, and *** denote the 10%, 5%, and 1% significance levels, respectively.

Table B.4

Effect of CZ re-instalment on vehicle transits

Dep. var.: Vehicle transits	Residents inside CZ				Residents outside CZ			
	Fee		No-fee		Fee		No-fee	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Kernel	Uniform	Triang	Uniform	Triang	Uniform	Triang	Uniform	Triang
RD Effect	-4.144***	-4.860***	0.008	0.008	-35.900***	-35.806***	0.401	-0.930
	(0.675)	(0.855)	(0.015)	(0.012)	(6.712)	(5.383)	(0.514)	(0.814)
Robust p-value	0.000	0.000	0.488	0.996	0.000	0.000	0.013	0.345
MSE bandwidth	210	732	486	509	479	506	222	550
Mean transits w/in bw	18.52	14.71	0.20	0.20	79.30	77.92	18.34	15.51
Observations	17682	61446	40866	42714	40278	42462	18606	46242
Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The dependent variable is the natural logarithm of the number of hourly private vehicle accesses at each CZ gateway. The unit of observation is a gateway-hour. Estimation method: local-linear regression with triangular kernel weights and MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). All specifications include controls for temperature, rainfall hours, gateway fixed effects, hour fixed effects, day of week fixed effects. Inference is based on standard errors robust to clustering at the gateway level and to the uncertainty in the selection of the optimal bandwidth. *, **, and *** denote the 10%, 5%, and 1% significance levels, respectively.

Table B.5

Differential effect of CZ re-instalment on vehicle transits

Dep. var.: Vehicle transits	Residents inside CZ		Residents outside CZ	
	(1)	(2)	(3)	(4)
Kernel	Uniform	Triang	Uniform	Triang
ReinstallxFee	-0.196***	-0.224***	-0.268***	-0.264***
	(0.060)	(0.060)	(0.047)	(0.041)
Reinstall	0.009	0.008	0.000	-0.063
	(0.011)	(0.011)	(0.077)	(0.069)
MSE bandwidth	260	363	243	386
Mean w/in bw	18.32	17.26	90.31	83.93
Observations	43764	60900	40740	64764

Notes: The dependent variable is the natural logarithm of the number of hourly private vehicle accesses at each CZ gateway. The unit of observation is a gateway-hour. The table reports results from the following equation $y_{it} = \delta_0 + \delta_1 x_{i,t} + Reinstall_t(\gamma_0 + \gamma_1 x_{i,t}) + Fee_i \left[\alpha_0 + \alpha_1 x_{i,t} + Reinstall_t(\beta_0 + \beta_1 x_{i,t})\right] + \epsilon_{it}$, where $x_{i,t}$ represents the time distance to the CZ re-instalment and β_0 captures the differential effect for vehicles subject to fee. Estimation method: local-linear regression with uniform and triangular kernel weights and MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). All specifications include controls for temperature, rainfall hours, gateway fixed effects, hour-fee fixed effects, dow-fee fixed effects. Inference is based on standard errors robust to clustering at the gateway level and to the uncertainty in the selection of the optimal bandwidth. *, **, and *** denote the 10%, 5%, and 1% significance levels, respectively.

C INSTITUTIONAL DETAILS OF THE CHARGING ZONE

In this section, we offer the reader the necessary details on the traffic reduction policy studied in this paper with a specific focus on the path to the enforcement of a charging zone (CZ), the political debates over the policy, its first attempt and later reforms – which we evaluate in this paper – and a period of unexpected suspension followed by a re-introduction of the policy. The reported figure summarises the key facts in a timeline of the events.

THE PATH TO THE ENFORCEMENT OF A CHARGING ZONE. The local government of Milan started to address pollution and congestion caused by road vehicles during the 1990s when the municipality implemented a policy known as "Sundays with alternate number plates" (*Domeniche con il blocco delle auto di targa pari/dispari*). This policy aimed to reduce traffic by restricting vehicle access based on license plate numbers on Sundays.[30] The restriction had a limited impact because it did not affect road vehicles during weekdays.

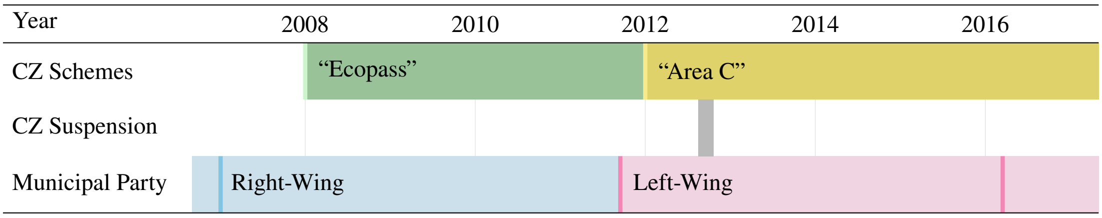
In turn, the national government assigned in 2001 the incumbent mayor of Milan special powers to tackle vehicle traffic congestion affecting the city centre via the implementation of innovative and more stringent policies. Despite these special powers, the ruling center-right coalition was unable to take action during the 2001–2006 term. In the campaign approaching the 2006 municipality election, traffic reduction policies became a key issue at stake and parties debated over alternative policies to limit traffic congestion and to fight pollution especially in the central area of the city. Specifically, the centre-right coalition advocated in favor of the introduction of a road pricing regime on the entry of polluting vehicles while the centre-left coalition proposed the creation of large pedestrian areas. Overall, both coalitions agreed on the necessity of implementing traffic restrictions in the city centre.

"ECOPASS" SCHEME: 2008–2011. After the 2006 election, won again by the centre-right block, the newly appointed city administration announced the introduction of a charging zone to regulate the access of road vehicles to the city centre of Milan. In January 2nd 2008, the "Ecopass" scheme has been put in place, forcing vehicles to pay a fee for having access to the city center depending on their polluting levels. The road pricing scheme was operating from Monday to Friday between 7.30 a.m. and 7.30 p.m. and the charging zone covered an area of 8.2 km^2 (i.e., 4.5 percent of the total municipality's surface). The enforcement was based on the disposition of traffic cameras with automatic number plate recognition placed at each of the forty-three access gateways along the perimeter of the city centre. Figure A.3 plots the CZ boundaries in comparison to the municipality territory.

Vehicles entering one of the access gateways to the city centre were charged four different fees, between 0 euros and 10 euros, based on their pollution levels.[31] Moreover, individuals

[30] According to the policy, vehicles with license plates ending in an odd number were prohibited from circulating on certain Sundays, while those with even-numbered plates were restricted on alternate Sundays. By rotating the restriction between odd and even numbers, the policy aimed to evenly distribute the reduction in traffic volume and emissions across different vehicles.

[31] Electric and hybrid vehicles, bi-fuel vehicles, and recent petrol (Euro 3, 4, and 5) and diesel (Euro 4 and 5) vehicles were granted free access. Euro 1 and 2 petrol (diesel) cars were subject to 2 euros (5 euros) fee, while older



living inside the gateways of the city centre were offered the option to buy yearly subscriptions for 50/125/250 euros, depending on the pollution class of their vehicle. The initial design of the policy and the failure to update the definitions of polluting vehicles resulted to be quite permissive to owners of private vehicles. Martino (2012) documents that 38.7 percent of vehicles entering the CZ just before the introduction of the payment scheme were indeed subject to the fee: 14 percent of vehicles were subject to the 2-euros ticket while only 24.7 percent of vehicles were subject to the 5-euros ticket. [32] Approaching the 2011 municipal elections, both coalitions criticised the charging zone and blamed the opponent party for the its introduction.[33]

"AREA C" SCHEME: SINCE 2012. Just after the 2011 municipal election, a referendum including a question regarding the charging zone took place, showing 80 percent of the voters supported the traffic restrictions. The newly elected centre-left administration restricted the opportunities for free entrance into the CZ and reduced the number of fees to two. Starting from January 2012, the "Area C" scheme replaced the former "Ecopass" operating with the same space-time restrictions. The key novelties concerned the limitation of the classes of vehicles entitled to access the city centre free of charge, the introduction of a unique fee for all other vehicles (5 euros in its standard form), and a ban on the most polluting classes of vehicles.[34] Moreover, the policy embedded a rolling agenda of further limitations of the number of vehicles granted free access and further expansions of the number of vehicles banned from the city centre to be introduced over time.[35] In addition, residents inside the CZ were granted 40 free daily entrances and charged a reduced fee of 2 euros on subsequent accesses.

The new system was significantly more effective than its predecessor in charging owners of private vehicles. According to Martino (2012), only 9 percent of private vehicles entering the CZ just before the revision of the payment scheme were exempt from the fee (compared to 85.6

petrol (diesel) vehicles were charged 5 euros (10 euros) per entry. Euro categories are the EU emission standards for vehicles' pollution, ranging from Euro 1 (vehicles matriculated not before 1992) to Euro 6 (vehicles matriculated not before 2014). See Figure A.1 in the Appendix for a summary of "Ecopass" fee scheme.

[32] Martino (2012) also documents that 73.5 percent of commercial vehicles were subject to the fee: 2.7 percent faced the 2-euros fee; 50.5 percent faced the 5-euros fee and 20.3 percent faced the 10-euros fee.

[33] Interestingly, the incumbent right party – which introduced the "Ecopass" scheme – blamed the left coalition for its introduction and proposed to abolish it. Figure A.2 provides evidence of electoral signs posted in Milan just before the second ballot of the 2011 municipal elections pointing the left party as the real policy supporter.

[34] Electric and hybrid vehicles and bi-fuel vehicles were granted free access. All other vehicles admitted into the CZ were subject to the payment of a 5-euros fee. Older than euros 1 petrol vehicles as well as euros 1 and 2 diesel vehicles were banned since the introduction of "Area C". See Figure A.1 in the Appendix for a summary of the fee and bans regime. Martino (2012) also documents that the ban on most polluting vehicles did not affect private vehicles since virtually no accesses of those vehicles in the CZ had registered even before the implementation of the policy.

[35] Since 2019, bi-fuel vehicles are subject to the 5-euros fee payment. Diesel Euro 3 vehicles belonging to non-residents individuals were banned starting from January 2016 while diesel Euro 3 vehicles belonging to residents were banned starting from October 2016. Petrol Euro 1 vehicles were banned starting from October 2019 and October 2020 for residents and non-residents, respectively.

percent of vehicles that were exempt from the previous policy at the same time).[36]

JUDICIAL APPEAL AGAINST THE CZ AND TEMPORARY SUSPENSION. During 2012, the owner of a private parking space (called Parcheggio Mediolanum) located inside the CZ appealed against the road pricing regime to the judiciary court responsible to rule on decisions undertaken by the public administration (*Tribunale Amministrativo Regionale*). After a first instance judgment in favour of the municipality of Milan, the second instance judges (*Consiglio di Stato*) forced the municipality to suspend the road pricing during the period necessary to cast a final decision. The suspension became immediately effective upon the court's decision on 26th July 2012. The CZ was restored two months later when the court overruled its previous decision and traffic cameras at gateways were restored on September 17th.

[36]The municipality of Milan introduced a ban on the most polluting vehicles (i.e. Low Emission Zone) in approximately 70% of the territory of the city of Milan in February 2019.

Chapter 2

Do pollution charges matter? Evidence from small businesses in Milan *

ABSTRACT

Road transport is a major source of air pollution and greenhouse gas emissions. For this reason, policy makers are increasingly pursuing measures to reduce the use of private vehicles generating potential unintended effects on businesses and their economic performance. In this paper, I examine the consequences of a road pollution charge on small businesses using geo-referenced firm-level data on activities located in Milan, which has introduced a charging zone in its city centre with fees that vary according to the pollution level of the vehicle. The overlap of the charging zone with the city centre poses a challenge for causal estimates due to pre-existing differences in business types inside and outside of the area. To address this issue, I use a difference-in-discontinuities approach based on the the distance from firms' locations to the boundary of the charging zone. The impact of road pricing on businesses is contingent upon their industry and revenue generation model. When focusing on existing small retail businesses, I find that the charging zone has a negative and sizeable effect on retail sales. While no effect on firm mobility or local competition is found, the negative impact on retail sales is consistent with the hypothesis that the policy diminishes customer flow by increasing the cost of reaching the location.

*I am thankful to Davide Cipullo, Tommaso Colussi, Ludovica Gazzè and Olmo Silva for helpful comments and discussions. Thanks to Eugenio Songia for the support in the data collection. The project also benefited from comments by participants at Università Cattolica del Sacro Cuore PhD-seminar and the "Minestrone" reading group.

I Introduction

Road transport contributes significantly to both air pollution and greenhouse gas emissions. In terms of air pollution, it has an estimated contribution to almost 40 percent of nitrogen dioxide (NO_2) and 10 percent of particulate matter (PM) concentration in Europe (EEA, 2020). Both these pollutants represent prominent environmental risks to public health, especially in densely populated areas.[1] Furthermore, passenger cars and heavy goods vehicles are responsible for almost 20 percent of green gas emissions in Europe, having a significant impact on the climate (EEA, 2022). As a result, policymakers at EU, national, and local levels have introduced legislative changes aimed at reducing the use of private vehicles. Measuring the potential impact of such traffic reduction policies on customer flow of businesses is crucial to evaluate their potential socio-economic consequences and to understand their sustainability.

Among the various types of traffic regulations, some target congestion as the primary problem to solve (Buchanan, 1963; Arnott and Small, 1994; Parry, Walls, and Harrington, 2007), while others focus more on the social cost of air pollution (Dockery et al., 1993; Pope et al., 1995; Chay and Greenstone, 2003). On the one hand, policies aimed at reducing congestion typically adopt price-based measures, such as urban tolls and road pricing, implemented in the most congested areas (e.g., Leape, 2006). On the other hand, policies aimed at reducing air pollution favour the implementation of bans on polluting vehicles in specific areas, such as low emission zones, or in specific days (e.g., Davis, 2008). Economists have traditionally favoured the use of price mechanisms, such as charges, to mitigate the negative externalities of road transport (Rouwendal and Verhoef, 2006), but these are rarely employed by policy makers for pollution reduction.

This paper examines the unintended effects of charging polluting vehicles on businesses. In 2008, the city of Milan introduced a charging zone (CZ) in its city centre that combines features of both strategies aimed at reducing pollution and congestion: a pricing mechanism which operates during working hours with different charges based on the polluting level of the vehicle. The peculiar design of the policy allows to provide quasi-experimental evidence of the impact of emission-related road pricing on small businesses. This study links balance sheet data of private companies to the urban area of Milan to document the CZ effect on the revenues and survival rates of SMEs that are located within the area. To identify the causal effect of this traffic reduction policy, I use a difference-in-discontinuities approach which allows to control for pre-existing differences between businesses located inside the historic centre and those outside of it.

Although several traffic reduction policies have been introduced especially in Europe, the causal impact of these legislative changes on businesses remains uncertain and rarely documented.[2] While previous studies have demonstrated the effectiveness of the charging zone in

[1] According to the European Environmental Agency, estimates of the health impact of long-term exposure to air pollution in Europe indicate that nitrogen dioxide (NO_2) was responsible for 55,000 premature deaths and particulate matter (PM2.5) for approximately 417,000 in 2018.

[2] Some scholars studied the relationship between road pricing (e.g., congestion charges in London and Stockholm) and businesses' performance (Leape, 2006; Quddus et al., 2007; Daunfeldt, Rudholm, and Rämme, 2009; Eliasson et al., 2009).

reducing vehicle access to the regulated area in Milan (Gibson and Carnovale, 2015; Cipullo, Colussi, and Nicolini, 2024), this study examines the unintended consequences on economic activities. The most closely related studies are those conducted by Viard and Fu (2015) and Galdon-Sanchez et al. (2023). The former scholars examine the consequences of Beijing's driving restrictions on television viewership, with the aim of exploring potential reductions in labour supply. The latter provide evidence of the effect of the low emission zone in Madrid on the reduction in credit card usage in stores within the regulated area. With respect to both studies, which examine the impact of vehicle bans, this presents the direct effect of charging polluting vehicles on the total sales of retail businesses and explores potential effects on firm mobility and local competition.

The charging zone implemented in Milan's city centre have some specific features. First, the CZ is similar to a traditional congestion charge (e.g., London, Stockholm, Singapore, etc.) but it incorporates fundamental environmental features since access charges are determined by the polluting levels of the vehicle.[3] Second, the policy creates a sharp geographical discontinuity in traffic regulation, which impacts asymmetrically very close firms competing in the same local market. This impact depends solely on whether they are located barely inside or outside of the charging zone. Third, the emission-related costs are directly borne by customers rather than firms, which is normal for congestion charges but not for other environmental regulations. For instance, if customers who do not reside in the charging zone choose to purchase goods from a retail business within the area, they must factor in an upfront cost when using their vehicle.

I first show that the charging zone has no average effect on the survival rates and revenues of pre-existing small businesses. It is essential to notice, however, that the revenues of numerous industries are unlikely to be affected by traffic regulation due to the nature of their business model. Thus, I restrict my attention to retail activities, whose customer flow is more likely to be influenced by the policy, e.g. businesses specialising in the sale of clothing, books, newspapers, toys, electronics, and furniture. I estimate a significant and statistically reduction in retail sales, ranging from -26 to -31 percent. Furthermore, I exclude an impact of the policy on mobility of retail activities.

I then examine the possible mechanisms underlying the decline in retail sales. The observed result could be due to both demand and supply factors. Indeed, the policy could have reduced the flow of customers into the regulated area due to the higher cost of reaching the location, but it could also have led to an increase in retail competition due to the higher quality of the renovated area. First, I show that the negative impact of the pollution charge on retail sales is concentrated in those stores located away from the metropolitan line. This result suggests that the reduction in customer flow could be particularly pronounced when the availability of other alternative means of transport is limited. Second, I provide empirical evidence that the decline in revenues of existing retail businesses is unlikely to be caused by increased local competition within the observational window. To test this hypothesis, I compute the Herfindahl-Hirschman

[3]The first scheme, "Ecopass", has four differentiated fees depending on polluting levels of the vehicles (no fee for less polluting vehicles and 2, 5 or 10 euros fee for more polluting vehicles). The second scheme, "Areac C", reduced the number of charges to two levels (no fee for less polluting vehicles and 5 euros fee for the others) and banned most polluting vehicles from the area. See Section II for details.

index of retail activities for each census block.

This analysis contributes to the debate surrounding the economic sustainability of environmental policies (Albrizio, Kozluk, and Zipperer, 2017; Dechezleprêtre, Nachtigall, and Stadler, 2020; Dussaux, 2020). In particular, it is linked to the strand about the effect of environmental regulation on firm's competitiveness. This relationship is normally studied by international and trade economists to determine whether asymmetric regulations in certain industries or countries may cause production to shift towards under-regulated firms or regions, by leaving unresolved the problem of emissions at a global level (e.g., Levinson and Taylor, 2008), or whether stringent environmental regulations could promote innovation among affected firms (Porter and Linde, 1995; Popp, 2002; Aghion et al., 2016).[4] My work is based on a substantially different policy design; still, it allows to quantify the effects of asymmetric environmental regulations on firms' performance at the micro level.

My analysis is also closely related to the literature on the effect of congestion charges on real estate prices (Percoco, 2014; Agarwal, Koo, and Sing, 2015; D'Arcangelo and Percoco, 2015; Tang, 2021) and the economic impact of air quality (Greenstone, 2002; Chay and Greenstone, 2005; Zivin and Neidell, 2012; Isen, Rossin-Slater, and Walker, 2017; Currie and Walker, 2019; Ito and Zhang, 2020).

The remainder of the paper proceeds as follows. Section II provides the necessary institutional details about the functioning of the charging zone in Milan. Section III describes the data collection and the linkage of firm's location with the urban area of Milan. Section IV introduces the empirical framework and tests the underlying assumptions. Section V presents the results of the analysis. Section VI concludes.

II Background

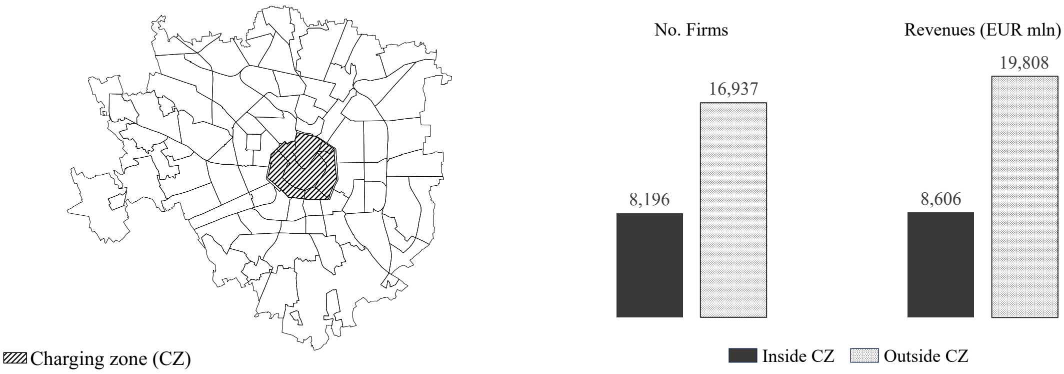
Milan is the second most populated city in Italy, with a population of about 1.4 million in the municipality and 3.2 million in the metropolitan city. It is considered the economic capital of the country with strengths in the fields of services, finance, design and fashion. For many decades, the city has been suffering from severe traffic congestions causing serious problems for mobility and air quality, especially in its city centre. In 2007, a study reported Milan as one of the cities with the highest car ownership rate in Europe, with more than half of its citizens using private cars and motorcycles regularly. The city was ranked as with the third-highest concentration of particle matter (PM10) among large European cities, both in terms of average annual level and number of days above the 50 μg/m^3 limit set by the European Union.[5] These conditions added up the public concern about the problems of traffic congestion in the city centre with the potential consequences on health, and pushed the proposal of possible solutions on the top of the local political agenda.[6]

As a response, a pollution charge, called "Ecopass", was introduced in the city centre of Milan in 2008. The scheme required a payment for vehicles accessing the city centre depending

[4] For reviews of empirical studies, see Jaffe and Palmer (1997) and Dechezleprêtre and Sato (2017).
[5] The study was conducted by the consulting firm *Ambiente Italia*.
[6] For a more detailed analysis of the political debate, see Cipullo, Colussi, and Nicolini (2024).

FIGURE 1

(A) MAP OF THE CITY OF MILAN AND CZ (B) SMALL BUSINESSES INSIDE CZ



Notes: Panel A shows the map of the municipality of Milan and the charging zone; Panel B reports the number of small businesses and their revenues in 2004 for those located inside and outside the charging zone.

on their level of pollutant emissions. According to the Municipal Agency of Mobility and Environment, 13 percent fewer vehicles entered the charging zone and traffic decreased by 4 percent outside the area during the first year of policy enforcement. These effects on traffic led to a fall in the number of days exceeding the permitted level of particulate matter – arriving to 83 days when the average for the past five years was 125 days – and to a significant reduction in traffic related emissions, PM10 decreased by 23 percent and CO2 emissions by 14 percent. Despite the positive effects on traffic congestion and pollution, the introduction of a charging zone in the city centre created a political debate and public concern about its implications on the businesses located inside the area.

After the initial success of the program, the unchanged definition of polluting vehicles decreased its effectiveness and put pressure on its financial sustainability during the following years.[7] In 2012, a new scheme, called "Area C", was introduced as an evolution of the previous one.[8] While maintaining the same geographic scope and enforcement periods, the revised scheme introduced more stringent measures through the redefinition of vehicle pollution, the prohibition of the most polluting vehicles from the area, and the imposition of a single fee for polluting vehicles entering the area.[9]

The charging zone covers an area of 8.2 km^2 (i.e., 4.5 percent of the total municipal area) located in the historic city centre, 43 cameras with automatic number plate recognition are installed along the border monitoring over all roads leading to the zone, and the road pricing – operating between 7.30 a.m. and 7.30 p.m. from Monday to Friday – varied the levels of charges between the two schemes. The "Ecopass" scheme (active from 2008 to 2011) required a payment of between 0 to 10 euros depending on the vehicle's pollution level. Free access was granted to less polluting vehicles – electric and hybrid vehicles, bi-fuel vehicles, and petrol (Euro 3, 4, and

[7] As time passed, more vehicles could access the area for free since newer vehicles have lower emissions by law. To avoid this problem, a rolling agenda changing the definitions of polluting vehicles was required.

[8] In addition, a political debate on the benefits and costs of the pollution charge led to a popular consultation through a political referendum in 2011. In 2011 referendum, out of a turnout rate of 49 percent of the eligible population, 80 percent of voters supported the tightening of restrictions on the polluting vehicles.

[9] The policy introduced a rolling updating agenda for further polluting levels definitions and limitations.

5) and diesel (Euro 4 and 5) vehicles –, a 2 euros (5 euros) fee was needed for Euro 1 and 2 petrol (diesel) vehicles, while older petrol (diesel) vehicles were charged 5 euros (10 euros) per entry.[10] The “Area C” scheme (active since 2012) has limited the free access to electric and bi-fuel vehicles, imposed a flat 5 euros fee on polluting vehicles with a rolling agenda for their definitions, and banned more polluting cars from the area (i.e, becoming also a low emission zone). Both schemes have fundamental environmental features since charges depends on polluting levels of the vehicle and less polluting vehicles can always access the area for free.[11]

Figure A.1a illustrates the area covered by the charging zone with respect to the territory of the city of Milan. Figure A.1b shows the crucial importance of the area from the business perspective. In fact, one third of the small businesses was located inside the CZ in 2004, despite the fact the area covers only 4.5 percent of the surface of the city.

III Data

The empirical analysis relies on firm-level data from Analisi Informatizzata Delle Aziende (i.e, the Italian extraction of Orbis database). The database collects balance sheet data of all Italian companies that are required to file annual financial statements with the Companies House (i.e., "Registro delle imprese"), excluding banks, insurance companies, and public bodies. I use data on small firms located in Milan in 2004, reporting their annual balance sheet values and locations from 2005 to 2013.[12] Additionally, I include spatial information on the city of Milan (e.g., the charging zone), as provided by the Municipality of Milan.

Sample restriction. This paper examines the impact of charging polluting vehicles on the revenues of small businesses as a function of their location. Two principal concerns arise in the context of this spatial analysis. First, the dataset only provides information about the legal and operational address, without indicating whether businesses operate in multiple locations or through several branches. In turn, revenue generation could be unrelated or only partially related to the location used. Second, businesses have quite precise control over where to locate. Distinguishing the impact of the policy can be difficult as businesses may strategically position themselves inside or outside of the charging zone.

I focus on micro, small and medium enterprises (SMEs), whose reported location is more likely to be relevant for their revenue generation. The SMEs are defined according to the European Commission’s definition, which includes enterprises with fewer than 250 employees and an annual balance sheet total of no more than EUR 43 million.[13] Holding firms and limited by

[10] Euro categories are the EU emission standards for vehicles’ pollution, ranging from Euro 1 (vehicles matriculated not before 1992) to Euro 6 (vehicles matriculated not before 2014).

[11] Moreover, they both provide discounts for residents living inside the CZ and several payment methodology (e.g., by debit and credit card, by a dedicated phone number, online or through an automatic bank payment). The “Ecopass” scheme provided annual subscriptions to residents inside CZ for 50/125/250 euros depending on the pollution class of their vehicle. The “Area C” scheme provides 40 free daily entrances and a reduced 2 euros fee on subsequent accesses to the CZ residents.

[12] The period covers the maximum time span available. Before 2004 only information on a restricted number of firms were available, while 2013 was the latest year provided from the same source.

[13] To define the firm size, I use the values for employees and balance sheet total reported in 2004.

shares companies (i.e., "S.p.A." and "S.p.A. a socio unico") are excluded from the analysis.

In order to address concerns regarding endogeneity, the sample is limited to small businesses that were already active in the first reported year of the dataset (i.e., 2004, four years prior to policy implementation). Companies established after that year are excluded.[14] Furthermore, I keep the location of each firm fixed over time to avoid reverse causality.[15] This time-based restriction avoids the possibility for companies to manipulate their treatment status, and implies the empirical analysis will estimate an intention-to-treat effect (see Lee and Lemieux, 2010).

Moreover, small businesses in the top and bottom 1 percent of revenue distribution are excluded to avoid potential misreporting or outliers close to the CZ boundary influencing the results.

GEOGRAPHICAL LINKAGE. In the spatial analysis, I compute the geographical position of each firm using its first reported operational address.[16] I then calculate the distances between the location of each business and the CZ boundary, and link each firm with its corresponding census block.[17]

DESCRIPTIVE STATISTICS. The final dataset collects the revenues, location (i.e., latitude and longitude), type of company and economic activity for 25,133 small businesses existing in Milan in 2004, of which 8,196 are located inside the CZ. It should be noted that retail activities represent only 4 percent of all SMEs, yet they are central to this analysis. Table B.1 shows descriptive statistics for small businesses inside and outside the charging zone on the main variable of interest and other covariates.

IV EMPIRICAL STRATEGY

This section describes the approach used to analyse the effect of the CZ on the existing small businesses and the necessary assumptions for the identification strategy.

To assess the impact of the policy on small businesses, I exploit the variation over time of the traffic regulation at the boundary of the charging zone. However, the overlap of the charging zone with the city centre presents a challenge for estimating the causal effect on businesses. In fact, the structural differences of buildings within the historic center may have influenced the type, dimension and operations of activities inside the area well before the introduction of the pol-

[14] I exclude also firms never reporting financial data after 2008 (i.e., the year of the CZ instalment). In fact, many firms failed due to the economic crisis generating quite different estimates between models with and without firm fixed effects. I show this exclusion is not related to the treatment assignment in the Appendix.

[15] The year of the first reported address of each firm varies from 2004 to 2006. If a firm was still operational in 2006, it reported the address of that year. Otherwise, it used the last reported address, which was from the year of closure. However, all used address are at least two years before the implementation of the policy.

[16] The legal address is employed solely in instances where the operational address is unavailable. Smaller businesses are more likely to fail to report any operational address. I assume their legal and operational location being the same.

[17] To obtain the longitude and latitude of the headquarters, the following steps are taken: (i) reported coordinates of the legal address are used when it coincides with the operational address in the AIDA database; (ii) missing coordinates are geocoded from OpenstreetMap. Since legal and operational addresses may not match for small differences (e.g., a space, a missing word, words with inverted order, etc.), I assigned the same coordinates when the probability of being the same address is higher than 0.55. The spatial analysis is computed through a GIS software.

FIGURE 2

Notes: This map shows small businesses located within 400 metres of the charging zone (CZ) boundary. White dots represent the location of small business, orange lines show the minimum distance from each business to the CZ boundary, brown area indicates the CZ.

icy.[18] Table B.1 indicates significant similarities between firms inside and outside the charging zone at large during the 2005-2013 period. However, when focusing on companies close to the boundary of the area, pre-existing differences could be observed. To test this hypothesis, I employ a regression discontinuity (RD) design (Hahn, Todd, and Van der Klaauw, 2001) in which the running variable is the distance in kilometres of each business to the CZ boundary.[19] Table 1 presents the RD estimates for various business characteristics computed on 2004 values and shows evidence of pre-existing discontinuities between activities barely inside and outside the charging zone. These differences are not solely in terms of the density of limited-by-guarantees firms, but also in the size and type of economic activity.

The pre-existing discontinuities in business types prevents the use of a standard RD design and force to look at the differences of the discontinuities before and after the implementation of the policy. The difference-in-discontinuities (*diff-in-disc*) approach (Lalive, 2008; Grembi, Nannicini, and Troiano, 2016) combines the regression discontinuity design with a before-after estimation to study how already existing discontinuities at the threshold evolve once the treatment

[18]The charging zone is delimited by the internal ring road, which consists of two parallel avenues following the route of the Spanish walls of Milan, also known as the "Bastions". These walls were constructed by the Spaniards in 1560 and were demolished between the late 19th and early 20th centuries.

[19]The model is computed within the Calonico, Cattaneo, and Titiunik (2014) optimal bandwidth. Distances and bandwidths are computed in kilometres.

TABLE 1

COVARIATE BALACING ON SMALL BUSINESSES ACROSS THE CZ BOUNDARY

Variable	Conventional RD effect	p-value	Mean w/in bw	Robust 95% c.i.	MSE Bandwidth	Eff. Obs.
Type of company						
Limited by guarantee (S.r.l.)	–0.018	0.066	0.970	[–0.043; -0.001]	0.303	2809
Type of economic activity						
Wholesale	–0.025	0.242	0.092	[–0.082; 0.013]	0.223	2150
Retail	–0.009	0.517	0.042	[–0.035; 0.028]	0.218	2130
Hospitality	–0.049	0.002	0.080	[–0.089; -0.018]	0.400	3736
Business and other services	0.027	0.401	0.308	[–0.041; 0.106]	0.286	2637
Size						
Micro	–0.058	0.021	0.803	[–0.114; 0.002]	0.331	3108
Small	0.045	0.035	0.174	[0.001; 0.098]	0.449	4088
Medium	0.008	0.461	0.021	[–0.016; 0.035]	0.268	2516

Notes: RD results on 2004 small businesses' characteristics estimated with local linear regressions, uniform kernel weights and MSE-optimal bandwidths. The size categories follow the criteria of the European Commision: *micro* are businesses with less than 2 EUR millions as balance sheet total and 10 employees, *small* are businesses with less than 10 EUR millions as balance sheet total and 50 employees, and *medium* are businesses with less than 43 EUR millions as balance sheet total and 250 employees.

of interest is in place.[20] This methodology allows to study the impact at the threshold of the traffic reduction policy on firms controlling for already existing discontinuities caused by the overlap between the charging zone and the historic centre of the city. Specifically, I estimate the following equation:

$$\begin{aligned} Y_{it} = \delta_0 + \delta_1 X_i + CZ_i(\gamma_0 + \gamma_1 X_i) \\ + Post_t \left[\alpha_0 + \alpha_1 X_i + CZ_i(\beta_0 + \beta_1 X_i)\right] + \epsilon_{it} \quad s.t. \quad X_i \leq h \end{aligned} \tag{1}$$

where Y_{it} represents the outcomes of interest, X_i is the distance in kilometres to the CZ boundary with negative values for those units outside the area, CZ_i is a dummy variable for businesses inside the CZ (i.e, $X \geq 0$) and $Post_t$ is a dummy variable for post CZ periods (i.e., $year \geq 2008$). Basically, the model computes the *pre* and *post* discontinuities at the threshold where the X_i represents the running variable (or score) of a standard RD design and the coefficient β_0 captures the *diff-in-disc* estimator as the interaction of CZ_i and $Post_t$. I estimate the model within an h MSE-optimal bandwidth (Calonico, Cattaneo, and Titiunik, 2014) and provide robustness checks of my results to multiple bandwidths.

The *diff-in-disc* approach requires two main identifying assumptions (see Grembi, Nannicini, and Troiano, 2016). The first assumption states that the change in potential outcomes must be continuous across the threshold. This assumption cannot be directly tested and is typically eval-

[20] Lalive (2008) refers to this approach as *before–during-RDD (BD-RDD)*, but it is the same model formalized in Grembi, Nannicini, and Troiano (2016). For similar methodologies, see Lemieux and Milligan (2008); Cellini, Ferreira, and Rothstein (2010); Pettersson-Lidbom (2012). For other applications of this methodology, see Campa (2011); Leonardi and Pica (2013); Casas-Arce and Saiz (2015); Carta and Rizzica (2018); Spenkuch and Toniatti (2018); He, Wang, and Zhang (2020); Bennedsen et al. (2022); Gamalerio and Trombetta (2023).

uated by examining procedures designed to detect changes in the manipulative sorting of units. As stated in Section III, the sample is limited to firms that were already active in 2004 to prevent endogeneity issues. However, I provide further empirical support by analysing the density of businesses that survived. Differently from a standard RD design, the potential outcomes must be continuous depending on two sources of variation at the same threshold. In this setting, those units with $X_i \geq 0$ exhibit discontinuities for both being inside the historic centre and the CZ simultaneously. To indirectly test the continuity of potential outcomes based on two sources of variation, the standard density test based on McCrary (2008) cannot be relied upon. Instead, it is necessary to evaluate the differences in densities before and after the policy implementation, exploiting the adoption of the CZ. Figure A.1 provides evidence of the continuity of the differences in active firms' density across the CZ boundary before and after 2008. The figure represents the differences in average density every 50 metres of active companies between the pre-period (2005-2007) and the post-period (2008-2013). The second assumption needs the pre-existing discontinuities at the threshold (i.e., due to the historic city centre) not to vary over time in case of no CZ was installed. In essence, it requires businesses barely inside and outside the city centre continue to have a constant gap in the analysed outcome in the absence of the new road pricing. This assumption is empirically tested in Section V, by looking at the local parallel trends of cross-section RD estimates on the outcomes prior the implementation of the CZ.

The continuity and local parallel trend assumptions allow to identify the causal effect of the traffic reduction policy in a neighborhood of the threshold on those activities already located in the city centre (i.e., ATT at the threshold). In order to obtain a more general estimand identifying the local causal effect of a traffic reduction policy at the threshold not only on companies located inside the city centre (i.e., ATE at the threshold), the identification should rely on a third assumption stating no interaction between the treatment (i.e., the charging zone) and the pre-existing condition (i.e., the historic city centre) is in place. In this setting, the homogeneity assumption would be violated if companies barely inside and outside the CZ, which were already inside the city centre, reacted differently to the traffic reduction policy. As show in Table 1, pre-existing differences in the types of economic activities across the border may suggest that an interaction between the the pre-existing condition and policy is plausible and cannot be completely ruled out. Moreover, city centres in Europe represent the cultural and historic (often commercial) area of a city, normally located in its geographic heart.[21]

V Results

This section presents the diff-in-disc estimates of the impact of the charging zone on turnover of small businesses. In light of the fact that the turnover is only reported by surviving companies, I will also present the results on the survival rate. Consequently, after demonstrating that the policy has no effect on the probability of company closure, I will be able to study its impact on revenues. In particular, I show that the pollution charge had a negative impact on the revenues

[21] Despite the *diff-in-disc* estimator could identify only an ATT at the threshold, the estimand remains relevant. In fact, from the policy makers' perspective, it would be unusual to put such a traffic reduction policy in places with significantly different characteristics from the city centres.

of retail activities, provide several robustness checks, and present evidences on potential mechanisms.

THE AVERAGE EFFECT ON SMEs. I first present the effect of the instalment of the charging zone on the whole sample of small businesses. From the perspective of the policymaker, it may be of interest to ascertain the average impact of policy on SMEs, irrespective of their industry. Table B.2 in the Appendix reports the effect of the charging zone on closures (panel A) and revenues (panel B) of small businesses. The *diff-in-disc* estimates shows that the policy had an average null effect on SMEs on both their closure rate and revenues. Figure A.2a in the Appendix provides cross-sectional RD estimates computed in separate regressions for each year. It illustrates that both closures and revenues have null and constant discontinuities at the boundary before and after the policy introduction. Figure A.2b in the Appendix collapses the before-after change providing a graphical representation of the *diff-in-disc* estimates. Closures and revenues do not show any slope or discontinuous change at the CZ boundary. The null effects persist even when changing the bandwidth used in the estimation, as shown in Figure A.2c in the Appendix. These results pertain to activities across all industries, including those whose operations and sales are unlikely to be impacted by traffic reduction policies due to their business model. For instance, business, financial and IT services account for almost 30 percent of the sample. Additionally, the null effect may also obscure opposing effects that cancel each other out when aggregated.

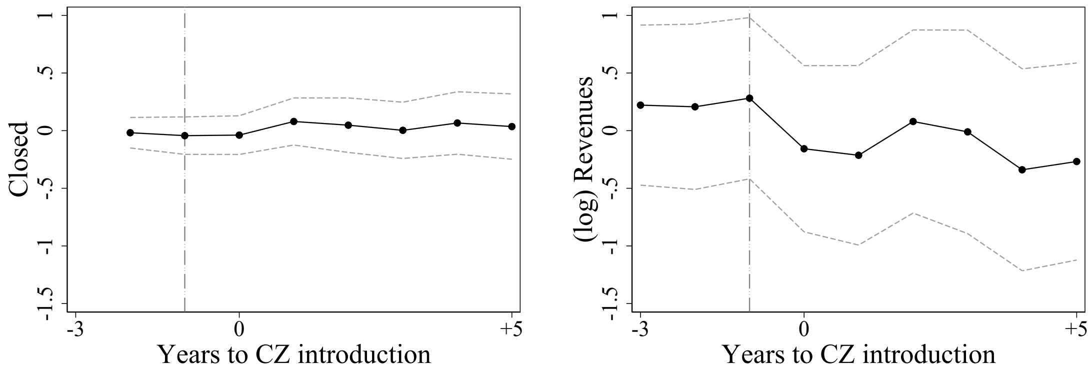
A THE EFFECT ON RETAIL BUSINESSES

The principal objective of this paper is to analyse those economic activities that are more susceptible to a potential change in customer flow due to traffic reduction policies. In particular, this study examines the impact of pollution-based road pricing on the retail sector of specialised businesses (i.e., Ateco2002 no. 52.4), which includes shops and stores selling clothes, books, newspapers, toys, electronics, and furniture, which represent approximately 4 percent of all SMEs.

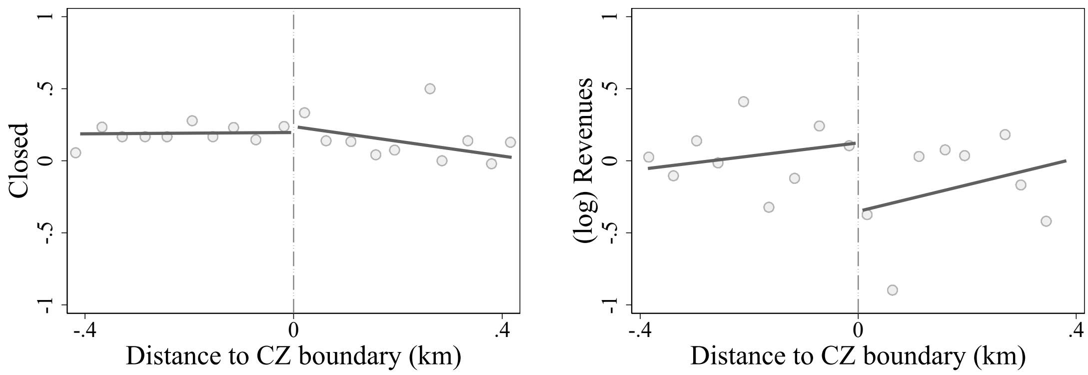
Following Grembi, Nannicini, and Troiano (2016) approach, I evaluate the validity of the local parallel trend assumption by presenting cross-sectional RD estimates in the observational window. Figure 3a reports the RD estimates at the CZ boundary by year on closures and sales of the retail businesses. On the one hand, RD estimates on closure rate demonstrate a parallel trend during the pre-treatment period that remained unaltered following the introduction of the charging zone. It should be noted that the RD estimate at $t = -3$ is missing due to the limited number of closures of retail businesses within the optimal bandwidth. On the other hand, RD estimates on log(revenues) show a local parallel trend in the pre-treatment period, which is violated and decreased once the policy is in place. Interestingly, the evolution of the discontinuities during the post-treatment period appears to align with the stringency of traffic regulation. Initially, the RD estimates exhibit a rapid decline, followed by an increase and subsequent stabilisation. Finally, they decline in the last two periods of the observational window. These waves are consistent with the effectiveness of the different schemes in reducing traffic inside the charging zone (i.e., "Ecopass" from 2008 to 2011 and “Area C” since 2012), as described in Section II.

FIGURE 3

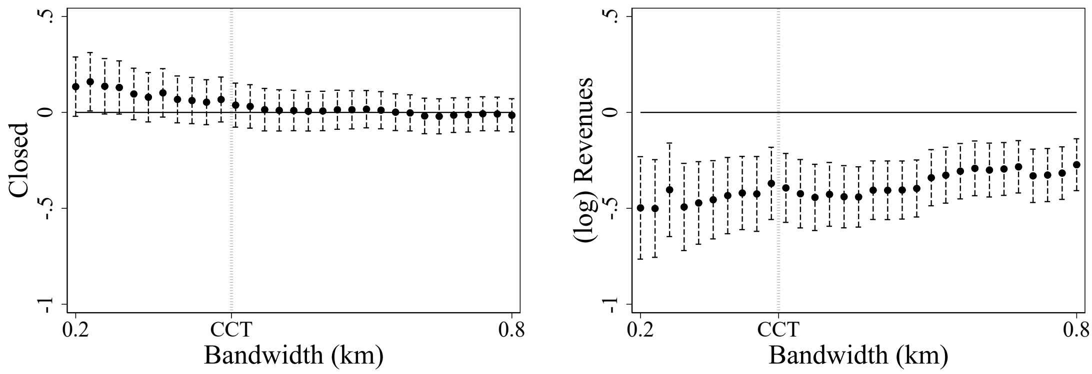
(A) CROSS-SECTIONAL RD EFFECTS



(B) DIFF-IN-DISC EFFECTS



(C) DIFF-IN-DISC ESTIMATES BY BANDWIDTH



Notes: This figure collects estimates of the effect of the CZ policy on small retail businesses. Panel A shows the cross-sectional RD estimates and 95 percent confidence intervals during the 2005-2013 period. RD estimates are computed separately for each year using the MSE-optimal bandwidth and a uniform kernel. RD estimate on the probability of closure at $t = -3$ is missing due to too few closures of small retail businesses within the optimal bandwidth. Panel B represents the *diff-in-disc* estimates capturing the before–during change at the boundary. Panel C plots the evolution of the *diff-in-disc* effects and 95 percent confidence intervals based on fixed effects specifications across different distances to the CZ boundary.

Table 2 reports the *diff-in-disc* estimates of the CZ impact on closure rate (panel A) and revenues (panel B) of retail businesses. Columns 1 and 2 represent estimates within the optimal bandwidth (Calonico, Cattaneo, and Titiunik, 2014), while columns 2 and 4 report the results

TABLE 2

DIFF-IN-DISC ESTIMATES OF THE CZ EFFECT ON RETAIL BUSINESSES

Distance to boundary	< CCT		< 2 x CCT	
	(1)	(2)	(3)	(4)
Fixed effects		✓		✓
Panel A. Dep. var.: Closed (Perc.)				
CZ x Post	0.053	0.053	-0.044	-0.044
	(0.085)	(0.085)	(0.062)	(0.062)
Constant	0.068	0.162	0.035	0.176
MSE bandwidth	0.419	0.419	0.838	0.838
Effective obs	1494	1494	3114	3114
Panel B. Dep. var.: log(Revenues)				
CZ x Post	-0.383**	-0.380***	-0.304**	-0.319***
	(0.186)	(0.143)	(0.152)	(0.108)
Constant	13.187	13.627	13.414	13.519
MSE bandwidth	0.388	0.388	0.776	0.776
Effective obs	1130	1130	2275	2275

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results of the *diff-in-disc* approach reported in equation 1 on small retail businesses. Panel A reports the estimates on the probability of being closed, while panel B shows the results on log(revenues). Columns (1)-(2) estimates the effects within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014); columns (2)-(3) report estimates using a doubled bandwidth. Columns (2) and (4) include year and unit fixed effects. Standard errors are shown in parentheses and are clustered at the census block level.

when doubling the optimal bandwidth. As shown in panel A, the policy has no statistically significant effect on closure rates in all the specifications. However, it has a relevant and statistically significant effect on revenues. As reported in panel B, the charging zone generated a negative impact on revenues for retail businesses ranging from -26 ($= e^{-0.304} - 1$) to -31 ($= e^{-0.383} - 1$) percent. Estimates are significant at 5 percent level when not including year and firm fixed effects, and at 1 percent level otherwise. Figure 3b shows the graphical representation of the *diff-in-disc* estimator reported in column 1 of Table 2. Estimates on closures reveal a flat slope outside of the charging zone and a slightly negative inclined trend inside the charging zone, but no discontinuity at the boundary. In contrast, estimates on revenues show a positive trend but a negative discontinuity at the threshold. This would imply increased revenues (before-during the policy) for businesses closer to the city centre, but a negative effect of the policy on those located inside the charging zone.

The choice of the bandwidth – and so the sample used in the estimation – can seriously impact the results of the CZ effect. Figure 3c plots *diff-in-disc* estimates within several bandwidth, raging from 200 to 800 metres, and provides evidence that the results are consistent when using businesses within different distances to the boundary. Estimates on revenues within smaller bandwidths generate both larger point estimates and confidence intervals accordingly with the fact that small bandwidths are usually considered to reduce precision but also selection bias.[22]

[22] As the RD estimates, the *diff-ind-disc* effect tends to have high internal validity (e.g., reducing selection bias) but limited external validity. In fact, the estimated treatment effect is generally not representative of the treatment that would occur for units with scores away from the cutoff.

Thus far, the analysis has focused on the impact of road pricing on the closures and revenues of businesses. However, it is also important to consider the possible implications of the policy on the displacement of firms. In fact, the policy could have increased the willingness of retail activities to change location. Despite standard models of spatial equilibrium suggest that mobile firms should relocate across the boundaries of the targeted area, my empirical evidence suggests otherwise. Unfortunately, the availability of data does not allow for the testing of the local parallel trend assumption on this outcome.[23] Nevertheless, when estimating the *diff-in-disc* effect on firm mobility, no result is statistically different from zero. In Table B.5 in the Appendix, panel A shows the results with different specifications and bandwidth selection.

There are several potential explanations for the absence of a statistically significant impact on retail activities relocating across the boundary. First, the relocation of a business is not a cost-free or risk-free process. Second, the evaluation by the activities of the CZ impact on their own revenues would not be so apparent even when knowing performances of all other businesses. Figure 3b illustrates an upward trend in the change of revenues, indicating that activities situated closer to the city centre have reported relatively enhanced revenues. Consequently, activities located between 300 and 400 metres within the CZ are subject to an equal change in profitability (i.e., when summing up the slope effect and the CZ discontinuity) to those situated within the same distance to the boundary but outside of the charging zone. This phenomenon may present a challenge for retail businesses in evaluating the impact of road pricing on their own profitability.

ROBUSTNESS TESTS. In addition to the various bandwidths employed in the estimation process, I propose further robustness tests whose results are reported in the Appendix.

Table B.3 in the Appendix reports the estimates of a difference-in-differences model within the optimal bandwidth (Calonico, Cattaneo, and Titiunik, 2014), which corresponds to a 0-order polynomial on the running variable X_i reported in equation 1. The estimates on retail closures and revenues are similar for relevance and magnitude to the *diff-in-disc* estimator (i.e., revenues show a decrease by 18 percent significant at 5 percent level). Figure A.3 in the Appendix plots the dynamic TWFE coefficients of the event study figures to test the parallel trend assumption during the pre-treatment period.

Table B.4 in the Appendix reports results for micro and small companies only (i.e., less than 10 EUR millions as balance sheet total and 50 employees) to ensure results are not driven by few relatively large firms (i.e., medium size) close to the CZ boundary. Results are consistent with previous estimates across all the specifications.

Finally, Figure A.4 in the Appendix shows that the results are valid also when using revenues in absolute terms with no log transformation. Table B.6 in the Appendix reports the results on the probability of closure when including those firms that have been excluded in the main analysis since they do not report any financial data after to the implementation of the policy. The null effects prove that the selection of the surviving businesses is not related to the treatment assignment.

[23]Note that in the estimation of the CZ impact on firm mobility (reported in Panel A of Table B.5) there is only one pre-treatment period (i.e., 2007) since almost all firms report their location in 2006 for the first time.

B Mechanisms

The introduction of the charging zone could have influenced the economic performance of existing retail businesses through both demand and supply channels. Indeed, the policy could have reduced customer flows as a consequence of the increased cost of reaching the location by car. However, it is also plausible that it could have increased local competition due to the pressure of new entrants in the renewed market, characterised by less congestion and pollution. Overall, my findings are mostly consistent with the channel of the reduced customer flows.

Cost of reaching the location. The pollution-based road pricing can have a significant impact on the propensity of individuals to reach destinations within the area, particularly in a city such as Milan at that time, which was identified as one of the European cities with the highest car ownership rate. Previous studies have demonstrated the negative impact of the policy on private vehicles accessing the CZ (e.g., Gibson and Carnovale, 2015). Such increased difficulty in reaching the businesses located within the charging zone could have resulted in a decline in customer flow and, consequently, in sales.

This subsection presents evidence indicating that retail businesses situated in closer proximity to a metropolitan line station, where the cost of reaching the location is reduced, were less adversely affected by the policy than those situated further away. This evidence is consistent with the hypothesis that the cost of reaching an activity has a direct impact on its economic performance.

Figure 4a illustrates the *diff-in-disc* estimates computed on two subsamples of the retail businesses. On the left, it plots the point estimates, 90 (bars) and 95 (caps) confidence intervals for those activities located in close proximity to the metropolitan line (i.e., at a distance to the closest station below the median). On the right, it reports the results for those situated at a greater distance (i.e., at a distance to the closest station above the median).[24] The median value of the closest metropolitan line station is computed within the optimal bandwidth and is equal to 409 metres. The average distance to the metropolitan line for the firms below the median is 235 metres, while it is 672 metres for the sample above the median.

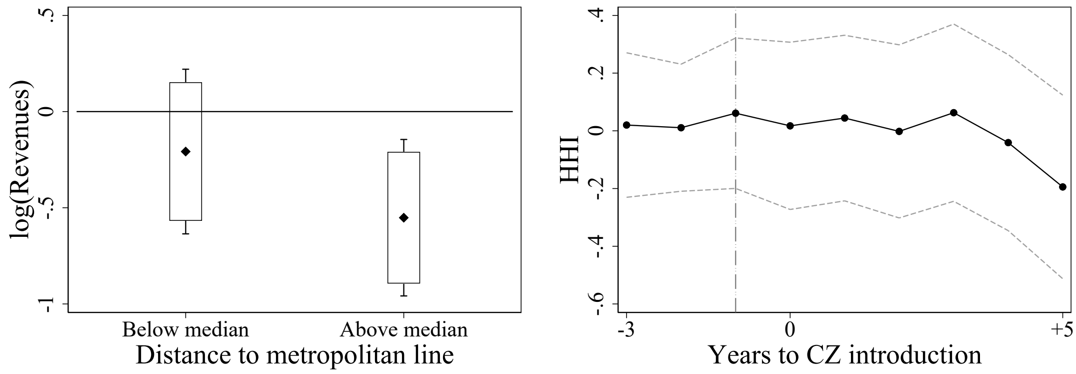
The CZ effect on those retail businesses located in close proximity to a metropolitan station is smaller and not statistically different from zero. In contrast, it is significantly greater for those not having a close access to the metropolitan line. The heterogeneity analysis indicates that the estimated CZ impact on retail sales may be driven by a reduction in customer flow as a direct consequence of the increased cost of reaching those activities by car, which is diminished when an alternative mean of transport is available.

Pressure of new entrants in the market. The decrease in the number of polluting vehicles has resulted in a reduction in congestion, noise and air pollution within the area. As demonstrated by Chay and Greenstone (2005), individuals are willing to value places with higher air quality. Thereby, the renovated area may have attracted new businesses, increasing local

[24] The metropolitan line stations employed in the heterogeneity analysis are only those opened before 2006. This ensures to condition on a pre-determined variable.

FIGURE 4

(A) DIFF-IN-DISC HETEROGENEITY (B) CROSS-SECTIONAL RD EFFECTS



Notes: This figure collects estimates of the effect of the CZ policy on small retail businesses. Panel A represents the *diff-in-disc* estimates, 90 (bars) and 95 (caps) confidence intervals computed separately on a sub-sample collecting those activities located with a distance to the closest metropolitan line station below the median (on the left) and above the median (on the right) within the optimal bandwidth (Calonico, Cattaneo, and Titiunik, 2014) computed on the whole sample. Panel B shows the cross-sectional RD estimates and 95 percent confidence intervals during the 2005-2013 period on the Herfindahl–Hirschman Index (HHI) computed at the census block level. RD estimates are computed separately for each year using the MSE-optimal bandwidth and a uniform kernel.

competition.

In order to analyse the potential effect of the policy on local competition, I calculate the Herfindahl-Hirschman Index (HHI) for the retail activities within each census block. The municipality of Milan is comprised of 5,899 census blocks, with an average area of 21,227 square metres each.[25] The use of census blocks as the unit of analysis allows for the consideration of not only pre-existing retail businesses but also new entrants (i.e., activities established after 2004). The estimation of the CZ effect on HHI is based on equation 1, but it employs the distance between the centroids of the census blocks and the CZ boundary as the running variable.

Figure 4b illustrates the cross-sectional RD estimates on HHI. The local parallel trend condition is met during the pre-treatment period and it seems not to be affected by the CZ policy initially. However, the RD estimates indicate a decline beginning at time $t = +4$, which coincides with the implementation of the new and more restrictive "Area C" scheme. This would suggest that the more stringent restrictions on polluting vehicles have led to an decrease in HHI, and so an increase in local competition for retail businesses where individuals can are more likely to enjoy the areas. However, when considering the overall effect of the two schemes of the CZ within this observational window, no statistically significant effect is estimated. In Table B.5 in the Appendix, panel B reports the results of the estimates obtained with different specifications and bandwidths.

Overall, the findings suggest that both the demand and supply channels are plausible. Nevertheless, the estimated negative impact of the CZ on retail businesses within our observational window is more likely to be caused by the reduced customer flow resulting from the increased

[25]Due to the low number of small retail activities, only those blocks with an HHI higher than 0.05 and lower than 0.95 are considered in order to avoid extreme changes in the index.

cost of reaching those shops and stores located within the regulated area. Indeed, the policy's impact on retail competition is only discernible from the fourth period (with the introduction of the "Area C" scheme), while its effect on revenues is immediately observable in the same year as the initial policy implementation.

VI Conclusions

This article examines the impact on small businesses of a pollution-based road pricing, by studying the Milan's charging zone implemented in 2008. I compare revenues of businesses located barely outside and inside of the regulated area by using a difference-in-discontinuity approach. More specifically, the policy has a significant impact on retail activities. The pollution charge have led to a prominent reduction in the revenues of small retail businesses situated barely inside of the regulated area. The negative effects can be attributed to the decline in customer flow due to the overlap of the scheme's enforcement time with the working hours of businesses in this industry. In contrast, no effect on firm mobility or local competition within the observational window has been identified. The findings of this study demonstrate that pollution charges in the form of road pricing can have a significant impact on small retail businesses, whose customer flow is susceptible to the cost of reaching the location.

The peculiar design of the policy enables the provision of new causal estimates of the impact of charging polluting vehicles on small businesses. This contributes to the debate about the economic sustainability of such policies and their effect on a firm's competitiveness. Specifically, the CZ has introduced a emission-related road pricing in a delimited area of one of the most polluted zones in Europe. The asymmetric legislative changes introduced by such a policy for firms competing in the same local market emphasise the need for ex-post evaluation of these legislative changes in order to consciously evaluate their costs and design the necessary environmental policies.

References

Agarwal, Sumit, Kang Mo Koo, and Tien Foo Sing. 2015. "Impact of electronic road pricing on real estate prices in Singapore." *Journal of Urban Economics* 90:50–59.

Aghion, Philippe, Antoine Dechezleprêtre, David Hemous, Ralf Martin, and John Van Reenen. 2016. "Carbon taxes, path dependency, and directed technical change: Evidence from the auto industry." *Journal of Political Economy* 124 (1):1–51.

Albrizio, Silvia, Tomasz Kozluk, and Vera Zipperer. 2017. "Environmental policies and productivity growth: Evidence across industries and firms." *Journal of Environmental Economics and Management* 81:209–226.

Arnott, Richard and Kenneth Small. 1994. "The economics of traffic congestion." *American Scientist* 82 (5):446–455.

Bennedsen, Morten, Elena Simintzi, Margarita Tsoutsoura, and Daniel Wolfenzon. 2022. "Do firms respond to gender pay gap transparency?" *The Journal of Finance* 77 (4):2051–2091.

Buchanan, Colin. 1963. *Traffic in Towns: A study of the long term problems of traffic in urban areas*. Routledge.

Calonico, Sebastian, Matias D Cattaneo, and Rocio Titiunik. 2014. "Robust nonparametric confidence intervals for regression-discontinuity designs." *Econometrica* 82 (6):2295–2326.

Campa, Pamela. 2011. "Gender quotas, female politicians and public expenditures: quasi-experimental evidence." *Working Paper* .

Carta, Francesca and Lucia Rizzica. 2018. "Early kindergarten, maternal labor supply and children's outcomes: evidence from Italy." *Journal of Public Economics* 158:79–102.

Casas-Arce, Pablo and Albert Saiz. 2015. "Women and power: unpopular, unwilling, or held back?" *Journal of Political Economy* 123 (3):641–669.

Cellini, Stephanie Riegg, Fernando Ferreira, and Jesse Rothstein. 2010. "The value of school facility investments: Evidence from a dynamic regression discontinuity design." *The Quarterly Journal of Economics* 125 (1):215–261.

Chay, Kenneth Y and Michael Greenstone. 2003. "The impact of air pollution on infant mortality: evidence from geographic variation in pollution shocks induced by a recession." *The Quarterly Journal of Economics* 118 (3):1121–1167.

———. 2005. "Does air quality matter? Evidence from the housing market." *Journal of Political Economy* 113 (2):376–424.

Cipullo, Davide, Tommaso Colussi, and Francesco Nicolini. 2024. "Pollution, traffic reduction and electoral outcomes." *Working Paper* .

Currie, Janet and Reed Walker. 2019. "What do economists have to say about the Clean Air Act 50 years after the establishment of the Environmental Protection Agency?" *Journal of Economic Perspectives* 33 (4):3–26.

Daunfeldt, Sven-Olov, Niklas Rudholm, and Ulf Rämme. 2009. "Congestion charges and retail revenues: Results from the Stockholm road pricing trial." *Transportation Research Part A: Policy and Practice* 43 (3):306–309.

Davis, L. W. 2008. "The Effect of Driving Restrictions on Air Quality in Mexico City." *Journal of Political Economy* 116 (1):38–81.

Dechezleprêtre, Antoine, Daniel Nachtigall, and Balazs Stadler. 2020. "The effect of energy prices and environmental policy stringency on manufacturing employment in OECD countries: Sector-and firm-level evidence." .

Dechezleprêtre, Antoine and Misato Sato. 2017. "The impacts of environmental regulations on competitiveness." *Review of Environmental Economics and Policy* .

Dockery, Douglas W, C Arden Pope, Xiping Xu, John D Spengler, James H Ware, Martha E Fay, Benjamin G Ferris Jr, and Frank E Speizer. 1993. "An association between air pollution and mortality in six US cities." *New England Journal of Medicine* 329 (24):1753–1759.

Dussaux, Damien. 2020. "The joint effects of energy prices and carbon taxes on environmental and economic performance: Evidence from the French manufacturing sector." .

D'Arcangelo, Filippo Maria and Marco Percoco. 2015. "Housing rent and road pricing in Milan: Evidence from a geographical discontinuity approach." *Transport Policy* 44:108–116.

EEA. 2020. "Air quality in Europe — 2020 report." *European Environment Agency Report* (09).

———. 2022. "Transport and environment report 2021." *European Environment Agency Report* (02).

Eliasson, J., L. Hultkrantz, L. Nerhagen, and S. Rosqvist. 2009. "The Stockholm congestion – charging trial 2006: Overview of effects." *Transportation Research Part A* 43 (1):240–250.

Galdon-Sanchez, Jose Enrique, Ricard Gil, Felix Holub, and Guillermo Uriz-Uharte. 2023. "Social benefits and private costs of driving restriction policies: The impact of Madrid Central on congestion, pollution, and consumer spending." *Journal of the European Economic Association* 21 (3):1227–1267.

Gamalerio, Matteo and Federico Trombetta. 2023. "Fiscal Rules and the selection of politicians: theory and evidence from Italy." *Working Paper SSRN 3035437* .

Gibson, M. and M. Carnovale. 2015. "The effects of road pricing on driver behavior and air pollution." *Journal of Urban Economics* 89 (1):62–73.

Greenstone, Michael. 2002. "The impacts of environmental regulations on industrial activity: Evidence from the 1970 and 1977 clean air act amendments and the census of manufactures." *Journal of Political Economy* 110 (6):1175–1219.

Grembi, Veronica, Tommaso Nannicini, and Ugo Troiano. 2016. "Do fiscal rules matter?" *American Economic Journal: Applied Economics* :1–30.

Hahn, J., P. Todd, and W. Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69 (1):201–209.

He, Guojun, Shaoda Wang, and Bing Zhang. 2020. "Watering down environmental regulation in China." *The Quarterly Journal of Economics* 135 (4):2135–2185.

Isen, Adam, Maya Rossin-Slater, and W Reed Walker. 2017. "Every breath you take—every dollar you'll make: The long-term consequences of the clean air act of 1970." *Journal of Political Economy* 125 (3):848–902.

Ito, Koichiro and Shuang Zhang. 2020. "Willingness to pay for clean air: Evidence from air purifier markets in China." *Journal of Political Economy* 128 (5):1627–1672.

Jaffe, Adam B and Karen Palmer. 1997. "Environmental regulation and innovation: a panel data study." *Review of Economics and Statistics* 79 (4):610–619.

Lalive, Rafael. 2008. "How do extended benefits affect unemployment duration? A regression discontinuity approach." *Journal of Econometrics* 142 (2):785–806.

Leape, J. 2006. "The London Congestion Charge." *Journal of Economic Perspectives* 20 (4):157–176.

Lee, D. and T. Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature* 48 (1):281–355.

Lemieux, Thomas and Kevin Milligan. 2008. "Incentive effects of social assistance: A regression discontinuity approach." *Journal of Econometrics* 142 (2):807–828.

Leonardi, Marco and Giovanni Pica. 2013. "Who pays for it? The heterogeneous wage effects of employment protection legislation." *The Economic Journal* 123 (573):1236–1278.

Levinson, Arik and M Scott Taylor. 2008. "Unmasking the pollution haven effect." *International Economic Review* 49 (1):223–254.

McCrary, J. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142 (2):698–714.

Parry, Ian W H, Margaret Walls, and Winston Harrington. 2007. "Automobile externalities and policies." *Journal of Economic Literature* 45 (2):373–399.

Percoco, Marco. 2014. "The impact of road pricing on housing prices: Preliminary evidence from Milan." *Transportation Research Part A: Policy and Practice* 67:188–194.

Pettersson-Lidbom, Per. 2012. “Does the size of the legislature affect the size of government? Evidence from two natural experiments.” *Journal of Public Economics* 96 (3-4):269–278.

Pope, C Arden, Michael J Thun, Mohan M Namboodiri, Douglas W Dockery, John S Evans, Frank E Speizer, Clark W Heath et al. 1995. “Particulate air pollution as a predictor of mortality in a prospective study of US adults.” *American Journal of Respiratory and Critical Care Medicine* 151 (3):669–674.

Popp, David. 2002. “Induced innovation and energy prices.” *American Economic Review* 92 (1):160–180.

Porter, Michael E and Claas van der Linde. 1995. “Toward a new conception of the environment-competitiveness relationship.” *Journal of Economic Perspectives* 9 (4):97–118.

Quddus, Mohammed A, Michael GH Bell, Jan-Dirk Schmöcker, and Achille Fonzone. 2007. “The impact of the congestion charge on the retail business in London: An econometric analysis.” *Transport Policy* 14 (5):433–444.

Rouwendal, Jan and Erik T Verhoef. 2006. “Basic economic principles of road pricing: From theory to applications.” *Transport Policy* 13 (2):106–114.

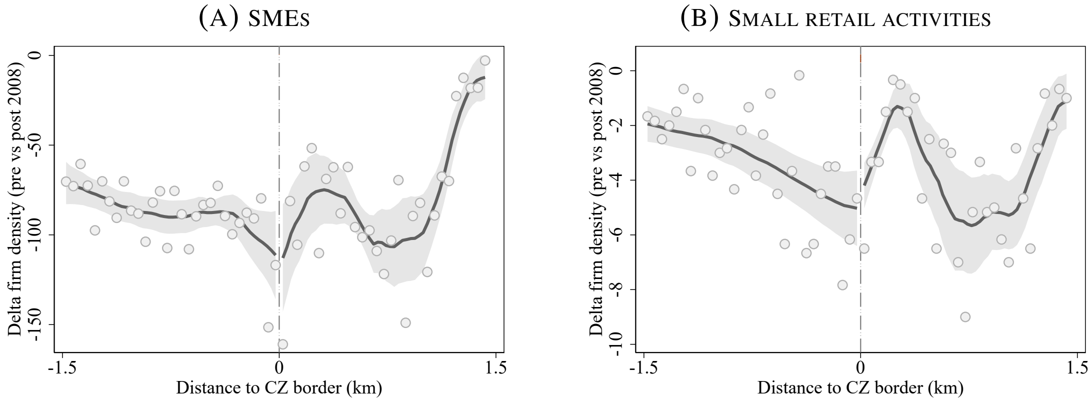
Spenkuch, Jörg L and David Toniatti. 2018. “Political advertising and election results.” *The Quarterly Journal of Economics* 133 (4):1981–2036.

Tang, Cheng Keat. 2021. “The cost of traffic: evidence from the London congestion charge.” *Journal of Urban Economics* 121:103302.

Viard, V Brian and Shihe Fu. 2015. “The effect of Beijing’s driving restrictions on pollution and economic activity.” *Journal of Public Economics* 125:98–115.

Zivin, Joshua Graff and Matthew Neidell. 2012. “The impact of pollution on worker productivity.” *American Economic Review* 102 (7):3652–3673.

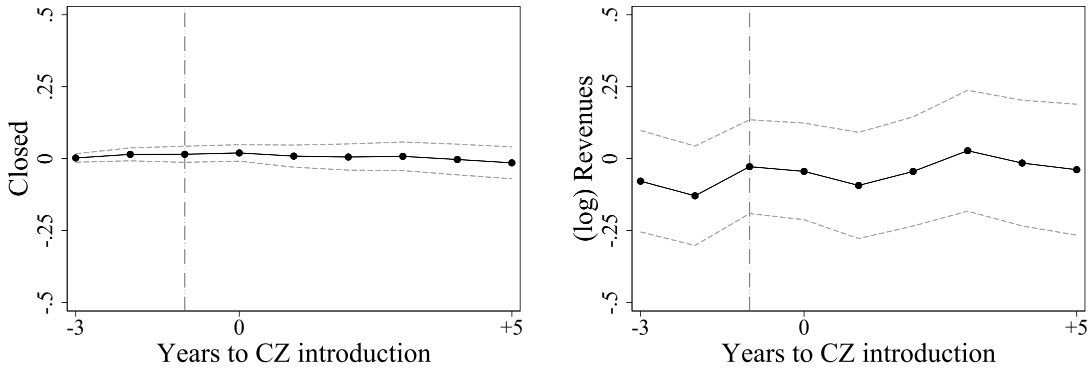
FIGURE A.1



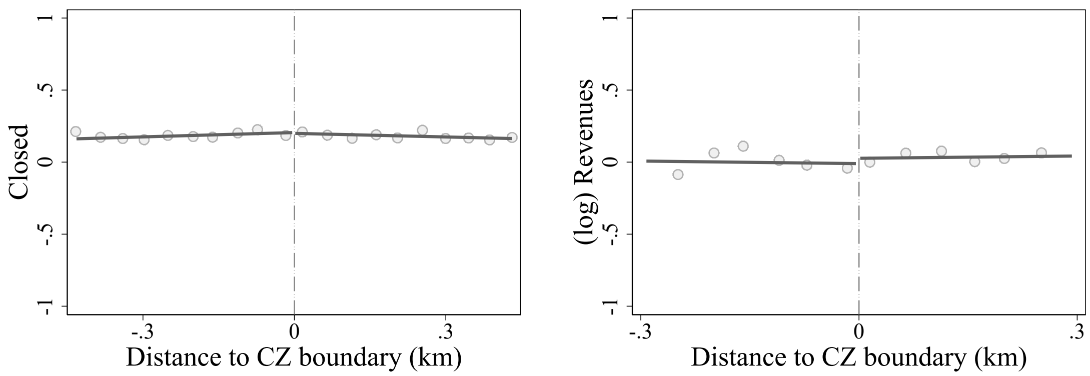
Notes: This figure tests the assumption of continuity of the delta density of active firms pre and post 2008 across the border of the CZ. It shows scatters and a kernel-weighted local polynomial plot with local mean smoothing of differences in densities of active firms between the annual average of the 2005-2007 pre-period and the 2008-2013 post-period. Averages of differences in densities are computed each 50 metres. Panel A shows results of the continuity test on the SME sample; panel B shows results of the continuity test on the small retail activities only.

FIGURE A.2

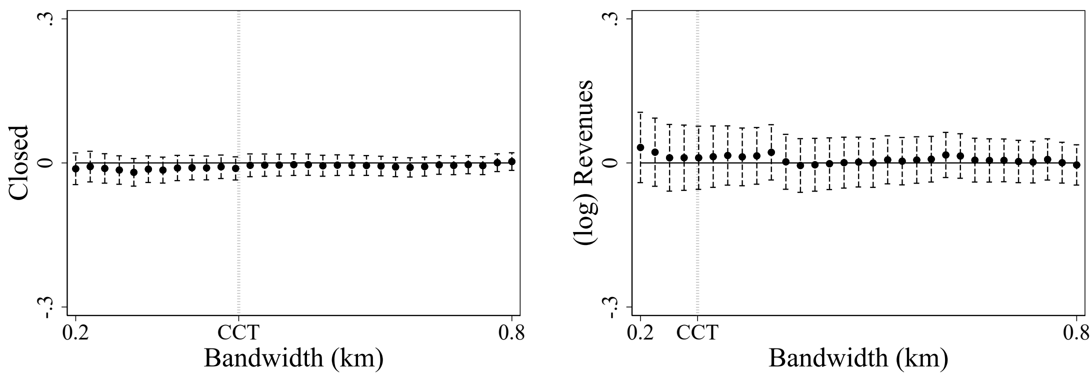
(A) CROSS-SECTIONAL RD EFFECTS



(B) DIFF-IN-DISC EFFECTS



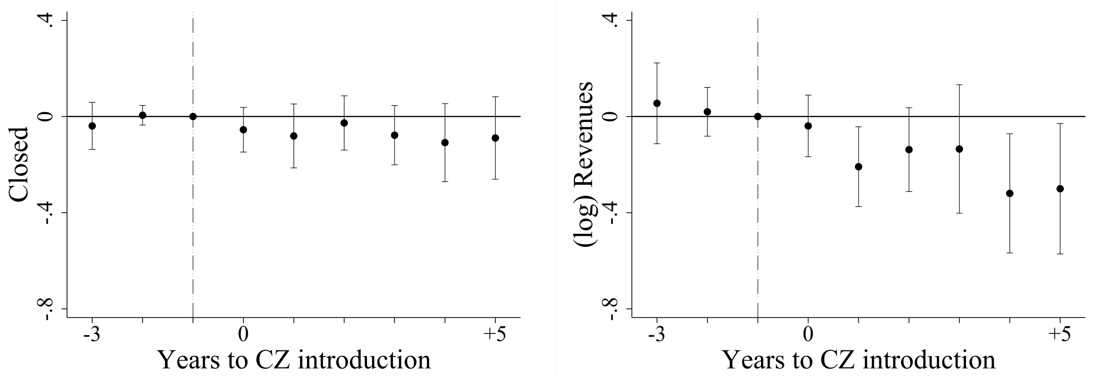
(C) DIFF-IN-DISC ESTIMATES BY BANDWIDTH



Notes: This figure collects estimates of the effect of the CZ policy on small businesses across all sectors. Panel A shows the cross-sectional RD estimates and 95 percent confidence intervals during the 2005-2013 period. RD estimates are computed separately for each year using the MSE-optimal bandwidth and a uniform kernel. Panel B represents the *diff-in-disc* estimates capturing the before–during change at the boundary. Panel C plots the evolution of the *diff-in-disc* effects and 95 percent confidence intervals based on fixed effects specifications across different distances to the CZ boundary.

FIGURE A.3

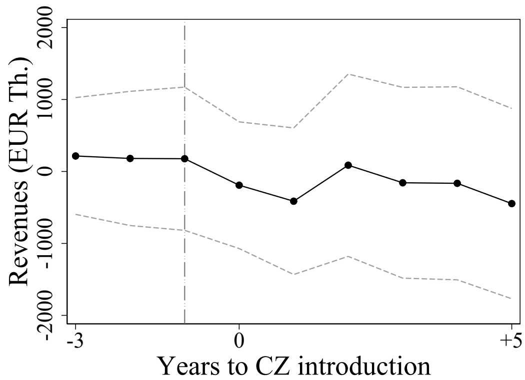
EVENT STUDY ESTIMATOR OF THE CZ EFFECT ON RETAIL BUSINESSES



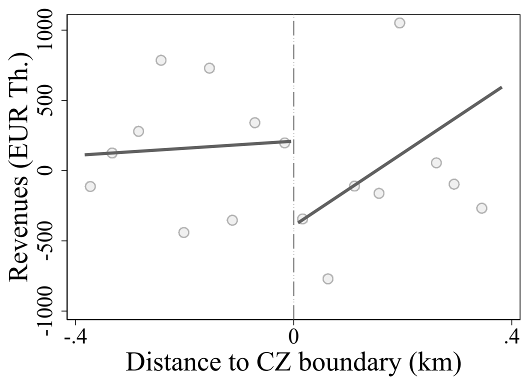
Notes: This figure shows the event-study plots constructed using the TWFE model estimates and 95 confidence intervals on small retail businesses within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014). Dependent variables are the probability of being closed (on the left) and the revenues in log (on the right).

FIGURE A.4

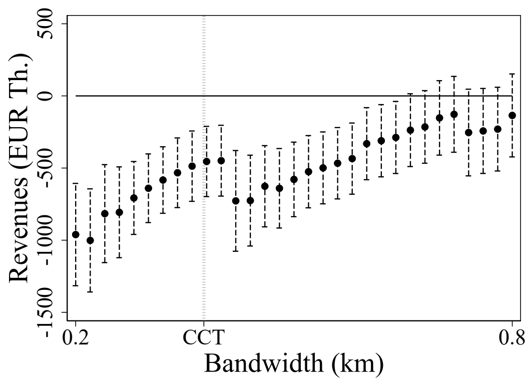
(A) CROSS-SECTIONAL RD EFFECTS



(B) DIFF-IN-DISC EFFECTS



(C) DIFF-IN-DISC ESTIMATES BY BANDWIDTH



Notes: This figure collects estimates of the effect of the CZ policy on small retail businesses. Panel A shows the cross-sectional RD estimates and 95 percent confidence intervals during the 2005-2013 period. RD estimates are computed separately for each year using the MSE-optimal bandwidth and a uniform kernel. RD estimates on the probability of closure at $t = -3$ is missing due to too few closures of small retail businesses within the optimal bandwidth. Panel B represents the *diff-in-disc* estimates capturing the before–during change at the boundary. Panel C plots the evolution of the *diff-in-disc* effects and 95 percent confidence intervals based on fixed effects specifications across different distances to the CZ boundary.

TABLE B.1
DESCRIPTIVE STATISTICS

	Inside CZ		Outside CZ		Delta
Variable	Mean	Obs	Mean	Obs	p-value
Outcome					
Closed	0.387	73764	0.372	152433	0.983
Revenues (EUR Th.)	1313.3	43900	1464.1	92982	0.972
Type of company					
Limited by guarantee (S.r.l.)	0.972	73764	0.944	152433	0.921
Other legal forms	0.028	73764	0.056	152433	0.921
Type of economic activity					
Wholesale	0.094	73764	0.148	152433	0.907
Retail	0.051	73764	0.033	152433	0.949
Hospitality	0.068	73764	0.067	152433	0.998
Business and other services	0.298	73764	0.279	152433	0.976
Size					
Micro	0.777	73764	0.830	152433	0.925
Small	0.188	73764	0.150	152433	0.943
Medium	0.035	73764	0.020	152433	0.948

Notes: All business activities reporting their headquarters in Milan in 2004. The sample collects data of these firms from 2005 to 2013. *Closed* variable reports 0 if the firm is operating and 1 if it is closed. *Revenues* variable reports the sales of operating firms only. The size categories follow the criteria of the European Commision: *micro* are businesses with less than 2 EUR millions as balance sheet total and 10 employees, *small* are businesses with less than 10 EUR millions as balance sheet total and 50 employees, and *medium* are businesses with less than 43 EUR millions as balance sheet total and 250 employees.

TABLE B.2
DIFF-IN-DISC ESTIMATES OF THE CZ EFFECT ON SMES

Distance to boundary	< CCT		< 2 x CCT	
	(1)	(2)	(3)	(4)
Fixed effects		✓		✓
Panel A. Dep. var.: Closed (Perc.)				
CZ x Post	-0.007	-0.007	-0.0009	-0.0009
	(0.02)	(0.02)	(0.01)	(0.01)
Constant	0.037	0.175	0.031	0.157
MSE bandwidth	0.433	0.433	0.867	0.867
Effective obs	35748	35748	68526	68526
Panel B. Dep. var.: log(Revenues)				
CZ x Post	0.042	0.018	0.050	0.0036
	(0.058)	(0.050)	(0.042)	(0.036)
Constant	13.230	13.199	13.160	13.227
MSE bandwidth	0.294	0.294	0.588	0.588
Effective obs	19432	19409	38533	38495

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results of the *diff-in-disc* approach reported in equation 1 on small businesses. Panel A reports the estimates on the probability of being closed, while panel B shows the results on log(revenues). Columns (1)-(2) estimates the effects within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014); columns (2)-(3) report estimates using a doubled bandwidth. Columns (2) and (4) include year and unit fixed effects. Standard errors are shown in parentheses and are clustered at the census block level.

Table B.3

Difference-in-differences estimates of the CZ effect on retail businesses

Dep. var.	Closed (perc.)		log(Revenues)	
	(1)	(2)	(3)	(4)
Fixed effects		✓		✓
CZ x Post	-0.062	-0.062	-0.21**	-0.20**
	(0.045)	(0.045)	(0.10)	(0.081)
Pre treated mean	0.232	0.232	13.485	13.485
MSE bandwidth	0.419	0.419	0.388	0.388
Effective obs	1494	1494	1130	1130

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results of a difference-in-difference model on small retail businesses within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014), which correspond to the *diff-in-disc* approach reported in equation 1 with a 0-order polynomial on the running variable X_i. Columns (1)-(2) report the estimates on the probability of being closed; columns (3)-(4) show the results on log(revenues). Columns (2) and (4) include year and unit fixed effects. Standard errors are shown in parentheses and are clustered at the census block level.

Table B.4

Diff-in-disc estimates of the CZ effect on micro-small retail businesses

Distance to boundary	< CCT		< 2 x CCT	
	(1)	(2)	(3)	(4)
Fixed effects		✓		✓
Panel A. Dep. var.: Closed (Perc.)				
CZ x Post	0.066	0.066	-0.030	-0.030
	(0.086)	(0.086)	(0.064)	(0.064)
Constant	0.069	0.163	0.051	0.163
MSE bandwidth	0.404	0.404	0.807	0.807
Effective obs	1422	1422	2880	2880
Panel B. Dep. var.: log(Revenues)				
CZ x Post	-0.372**	-0.375**	-0.250*	-0.305***
	(0.186)	(0.144)	(0.150)	(0.108)
Constant	13.147	13.608	13.391	13.479
MSE bandwidth	0.382	0.382	0.765	0.765
Effective obs	1107	1107	2233	2233

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results of the *diff-in-disc* approach reported in equation 1 on micro retail businesses. This subsample contains only micro-small businesses defined by the European Commission as those firms with less than 10 EUR millions as balance sheet total and 50 employees. Panel A reports the estimates on the probability of being closed, while panel B shows the results on log(revenues). Columns (1)-(2) estimates the effects within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014); columns (2)-(3) report estimates using a doubled bandwidth. Columns (2) and (4) include year and unit fixed effects. Standard errors are shown in parentheses and are clustered at the census block level.

TABLE B.5

DIFF-IN-DISC ESTIMATES OF THE CZ EFFECT ON MOBILITY AND COMPETITION OF RETAIL BUSINESSES

Distance to boundary	< CCT		< 2 x CCT	
	(1)	(2)	(3)	(4)
Fixed effects		✓		✓
Panel A. Dep. var.: Moved (Perc.)				
CZ x Post	0.033	0.080	0.067	0.078
	(0.080)	(0.082)	(0.056)	(0.061)
Constant	-0.022	0.050	0.031	0.054
MSE bandwidth	0.331	0.331	0.663	0.663
Effective obs	688	686	1448	1446
Panel B. Dep. var.: HHI by census block				
CZ x Post	-0.030	-0.049	-0.039	-0.060
	(0.064)	(0.056)	(0.047)	(0.040)
Constant	0.648	0.909	0.586	1.124
MSE bandwidth	0.348	0.348	0.696	0.696
Effective obs	299	290	650	631

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results of the *diff-in-disc* approach reported in equation 1 on small retail businesses. Panel A reports the estimates on firms' mobility, where the outcome is a dummy variable with 1 values for those businesses changing location. In this estimate, there is only one pre-treatment period (i.e., 2007) since the majority of firms report their first location in 2006. Panel B shows the results on the competition among businesses, where the outcome is the Herfindahl–Hirschman index (HHI) computed at the census block level. Columns (1)-(2) estimates the effects within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014); columns (2)-(3) report estimates using a doubled bandwidth. Columns (2) and (4) include year and unit fixed effects. Standard errors are shown in parentheses and are clustered at the census block level.

TABLE B.6

DIFF-IN-DISC ESTIMATES OF THE CZ EFFECT ON RETAIL BUSINESSES

Dep. var.: Closed (Perc.) Distance to boundary	< CCT		< 2 x CCT	
	(1)	(2)	(3)	(4)
Fixed effects		✓		✓
CZ x Post	0.007	0.007	-0.09	-0.09
	(0.09)	(0.09)	(0.07)	(0.07)
Constant	0.160	0.155	0.166	0.155
MSE bandwidth	0.376	0.376	0.752	0.752
Effective obs	1872	1872	3852	3852

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table reports results of the *diff-in-disc* approach reported in equation 1 on small retail businesses including those firms that remain operating only during the pre-treatment period. Columns (1)-(2) estimates the effects within the MSE-optimal bandwidths (Calonico, Cattaneo, and Titiunik, 2014); columns (2)-(3) report estimates using a doubled bandwidth. Columns (2) and (4) include year and unit fixed effects. Standard errors are shown in parentheses and are clustered at the census block level.