



The impact of fare reductions on public transportation use[☆]

María Cervini-Plá^{a,b}, Mariona Tomàs^c, Javier Vázquez-Grenno^{c,d,*}

^a Universitat Autònoma de Barcelona, Spain

^b EQUALITAS, Spain

^c Universitat de Barcelona, Spain

^d Institut d'Economia de Barcelona, Spain

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ABSTRACT

This paper empirically examines how consumers respond to reductions in public transportation fares by analyzing changes in their use of public transport. We exploit a policy that lowered fares across several municipalities in the metropolitan area of Barcelona. Our findings indicate a 39 percent increase in public transportation use during the year following the policy's implementation. To assess whether the visibility of the policy shaped this response, we also examine the effects of the subsequent announcement of its financing scheme, nearly a year later. The announcement clarified that the fare reduction would be funded through a tax on homeowners. Once taxpayers became fully aware of both the fare reduction and its financing mechanism, their use of public transportation increased further, supporting the hypothesis that the visibility of public policies can shape individuals' behavior regarding their use of public services. Finally, we estimate partial welfare effects, identifying both winners and losers.

1. Introduction

Public authorities worldwide have recently implemented a range of policies aimed at reducing private transportation use and increasing public transport ridership. In this regard, municipal authorities in the *Àrea Metropolitana de Barcelona* (the Barcelona Metropolitan Area, hereafter AMB) are no exception.¹ Starting from January 1st, 2019, several municipalities previously categorized as public transport “zone 2” concerning the city of Barcelona, were reclassified as “zone 1”.² This reclassification led to changes in transport fares within the metropolitan area, resulting in a halving of fares between the affected municipalities and the city of Barcelona. The distinction between “zone

1” and “zone 2” relates to their distance from Barcelona. Previously, “zone 1” included municipalities bordering Barcelona, while “zone 2” encompassed a ring of municipalities located further away.

However, this reduction in fares was not a free gift for residents in these municipalities. Their respective city councils introduced a new tax (*Tribut Metropolità*) applicable to all property owners in the municipality. The tax is based on the cadastral value of the property, irrespective of whether or not owners use public transport. Despite this, and primarily for electoral reasons (in this case, the municipal elections held at the end of May 2019), local politicians highlighted the fare reduction but said little about the new tax. In fact, they had not even got round to explicitly defining the tax (i.e., tax rates, tax base, etc.) to

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* Corresponding author at: Universitat de Barcelona, Spain.

E-mail address: jvazquezgrenno@ub.edu (J. Vázquez-Grenno).

¹ The AMB is the urban core of a large urban area and accounts for 40% of the population of Catalonia and 52% of its GDP. The AMB is home to 3.3 million inhabitants and 36 municipalities, most of which are immediately contiguous and some cities are physically adjacent to Barcelona and connected by subway.

² The municipalities affected are *Badia del Vallès, Barberà del Vallès, Begues, Castellbisbal, Cerdanyola del Vallès, Cervelló, Corbera de Llobregat, Molins de Rei, Paltejà, la Palma de Cervelló, el Papiol, Ripoll, Sant Andreu de la Barca, Sant Climent de Llobregat, Sant Cugat del Vallès, Sant Vicent dels Horts, Santa Coloma de Cervelló, Torrelles de Llobregat*.

³ “Salience can be strategically manipulated by firms or politicians. Salient ads or messages increase the prominence of some attributes while distracting consumers or voters from others”, Bordalo et al. (2022).

⁴ See the announcement from 21 October 2019 by the municipality of *Barberà del Vallès*, introducing the new tax and linking it to the fare reduction of 1st January. (Accessed September 2025.)

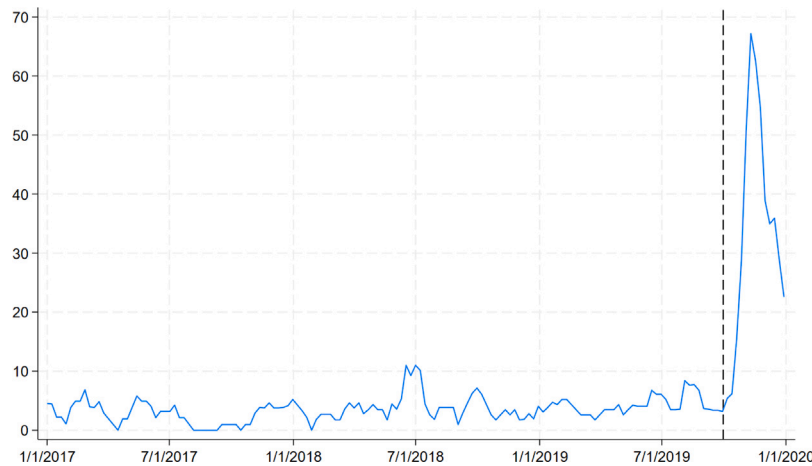


Fig. 1. Google trends.

NOTE: Google trends search on “tribut metropolità” & “tributo metropolitano” & “impuesto metropolitano” & “impuesto transporte” as a search topic. The search was restricted to the province of Barcelona. The vertical red line indicates the October 1st.

Source: Google Trends.

finance the fare reduction when it was approved at the end of 2018.³ However, all property owners in the affected municipalities received a tax receipt from the AMB at the beginning of the last quarter of 2019, almost five months after the municipal elections. Most property owners were unaware of this tax and knew even less about its connection to public transportation fare reduction.⁴ Indeed, we found evidence that most property owners became aware of it when they received their payment receipt. A simple Google search for “Tribut Metropolità” provides us with a good idea of the sudden interest generated by the metropolitan tax. Thus, we detect a spike in queries just after the 1st October 2019, coinciding with the date taxpayers received notification of payment (see Fig. 1).

The municipal elections partially explain the delay between the immediate effects of the policy (i.e. the fare reduction) and the belated awareness of the tax among property owners (almost one year later). This delay provides us with an interesting setting to examine the impact of the policy’s salience -specifically, the fare reduction and the tax levied to finance it- on the consumption of public services, such as public transportation.⁵ The underlying assumption is that the more prominent the policy is, the more it will lead to an increase in consumption. There are two mechanisms behind this idea that, a priori, operate in the same direction. First, infrequent public transportation users may not have been aware of the fare reduction when it was initially implemented, only noticing it upon receiving their tax receipts (prominence). Second, the visibility of the tax (highlighted by the receipt) made public transportation users aware that they were essentially funding the fare reduction through this newly introduced tax.

The primary objective of this paper is to analyze the impact of the fare reduction on the consumption of public transportation. In addition, we examine how awareness of the policy, particularly its financing mechanism, influenced homeowners’ behavior regarding their use of public transportation. We also investigate whether the fare reduction led to changes in the use of other modes of transportation, potentially reflecting substitution effects across transport options. Finally, we assess the welfare implications of the policy change.

To carry out our analysis, we leverage a policy change that, starting on January 1st, 2019, reduced public transportation fares in 18 of the 36 municipalities that make up the Barcelona Metropolitan Area

(AMB). We utilize a confidential and unique dataset containing hourly ticket validations on public transportation across these municipalities. Exploiting the timing of both the fare reduction and the subsequent salience of its financing mechanism — a new tax — we employ a difference-in-differences research design to estimate their respective effects. Specifically, we compare the *consumption of public transportation* (measured via ticket validations) in treated and untreated municipalities before and after the fare reduction. Furthermore, we examine additional sources of variation by analyzing periods before and after the municipal elections, as well as before and after homeowners received receipts for the new tax, interacting these events with the policy implementation. To explore potential substitution effects across transportation modes, we also use data from the *Working Day Mobility Survey* (*Enquesta de Mobilitat en Dia Feiner*) and apply a similar empirical strategy to assess whether the fare reduction influenced the use of alternative transport options. Lastly, we approximate the welfare impacts of the policy by identifying both winners and losers.

First, our results show a significant impact of the fare reduction. Comparing treated and non-treated municipalities, we find that public transportation use in treated areas increased by approximately 39% in 2019 relative to the two previous years. This 39% increase, driven by a 50% fare reduction, corresponds to a fare elasticity of -0.79 . Second, we find that after individuals became fully aware of the fare reduction and its financing through a new tax (after October 1st), public transportation usage in the affected municipalities increased by an additional 9 percentage points. We validate these findings through several robustness checks and a heterogeneity analysis, both of which are presented in [Appendix A.1](#) for brevity. Using survey data collected in the last quarter of each year, we document a modal shift in the treated municipalities following the policy change—away from private transportation (cars, motorcycles, and small trucks), as well as from biking and walking, and toward public transportation, particularly trains. Finally, we quantify the direct gains and losses resulting from the policy, distinguishing among different types of users, renters and homeowners, and the public sector.

Related literature

Our paper contributes to two distinct strands of the literature. First, it extends previous empirical evidence on the effects of changes in transport fares on public transportation demand (e.g., [Guajardo Ortega and Link \(2025\)](#), [Andor et al. \(2025\)](#), [Wallimann et al. \(2023\)](#), [Gravert and Collentine \(2021\)](#), [Bull et al. \(2021\)](#), [Kholodov et al. \(2021\)](#), [Liu](#)

⁵ We use “salience” to refer to the prominence, visibility or awareness of the fare reduction and the tax levied on the part of potential public transport users. For example, in our case study, if the tax had been given the same publicity as the fare reduction we would consider this a salient tax.

et al. (2019), Cats et al. (2017), Sharaby and Shiftan (2012), and Holmgren (2007)). These studies evaluate various fare policy changes, showing that fare reductions and/or fare integrations, which often lower fares for many passengers, increase public transport use. In most cases, this increase is driven by travelers switching from private transportation.

Additionally, we estimate that fare reductions increase demand for public transportation and lead to a modal shift from private to public transport, particularly reducing bike usage and walking trips. This substitution between environmentally friendly modes of transport following public fare reductions has been previously highlighted in the transport literature (e.g., Guajardo Ortega and Link, 2025; Bigazzi and Wong, 2020; Fearnley et al., 2018).

A recent overview of the economics of transportation, specifically focusing on the relationship between pricing and financing mechanisms and their impact on public transport demand, can be found in Hörcher and Tirachini (2021). This study highlights numerous cases in which short- and medium-term elasticities align closely with our findings.⁶

Complementing this demand-side perspective, a recent related study by Albalade et al. (2024) examines the environmental implications of fare reductions by studying the 2022 nationwide policy in Spain and its impact on air quality. They find no improvement, even in areas where fares were lowered or eliminated. While their policy was implemented in September 2022, our analysis focuses on a fare reduction introduced in January 2019 in the metropolitan area of Barcelona. The studies differ in several important respects. We exploit within-AMB variation to define treated and control municipalities, whereas they employ a regression discontinuity in time based on a national policy. Gasoline prices remained stable during our study period but spiked in 2022, potentially affecting their results. Moreover, their focus is on air quality, while we examine changes in transport use. Their null pollution effect may reflect a modal shift from non-polluting modes (e.g., walking, cycling) to public transport, which could increase ridership without improving air quality. Thus, despite addressing similar policies, our study provides causal evidence on how fare reductions influence transport mode choice.

Second, our paper also relates to the literature estimating policy salience and individual behavior (see Bordalo et al., 2022, for a comprehensive review). When the price change of a good or service is not salient, it can affect consumer behavior. For instance, Finkelstein (2009) examines the adoption of an electronic toll collection system in the US and finds that, following its implementation, toll rates were higher than they would have been under a manual toll collection system.⁷

Therefore, our study contributes to this growing body of literature by providing empirical evidence on how the salience of a public policy influences the consumption of public services. We show that the increase in public transportation ridership is even greater when considering the period after property owners became fully aware of the fare reduction and its funding through a new tax.

The rest of the paper is organized as follows. Section 2 describes the policy change and the chronology of events. Section 3 presents the data, describes the main variables, and outlines the methodology. The results are shown in Section 4, and Section 5 concludes by summarizing the main findings.

⁶ Our estimates can be expressed as elasticities ranging from -0.6 during the first few months to -0.79 over the course of the entire year following the policy change.

⁷ Relatedly, several papers explore the concept of tax salience, analyzing how the visibility of taxes can affect taxpayer behavior (e.g., Chetty et al. (2009), Goldin and Homonoff (2013), Taubinsky and Rees-Jones (2018), and Bradley and Feldman (2020)). Using natural and field experiments, these studies document that, on average, consumers of private goods under-react when sales taxes are not fully salient.

2. Background and policy reform

This section explains the institutional context of the fare reform implemented in the AMB on January 1, 2019. Barcelona's metropolitan area is the most institutionally developed in Spain. In 2010, the Catalan Parliament passed the AMB Act to end longstanding institutional fragmentation. The AMB replaced three prior entities after the 2011 municipal elections.⁸

When the AMB was established, 18 municipalities became part of fare zone 1 and were subject to a dedicated tax (*Tribut Metropolità*) to fund public transport. The remaining 18 municipalities were in zone 2, resulting in fare disparities for travel into Barcelona. To harmonize fares, the AMB approved a reform in October 2018 to reclassify all municipalities into zone 1 starting January 1, 2019. This effectively halved fares for zone 2 residents.⁹

To offset lost fare revenue, the AMB introduced the metropolitan tax (*Tribut Metropolità*) on properties in the formerly zone 2 municipalities. It was scheduled for collection in late 2019 and linked to each property's cadastral value. However, the exact amount was left unspecified in 2018, likely due to the upcoming May 2019 municipal elections. As AMB council members include local mayors and councilors, they were reluctant to reveal a new tax before the vote. Thus, no public explanation linked the fare cut to a new tax.¹⁰

The new fare system was implemented on January 1, 2019, making fares uniform across all 36 AMB municipalities. Local elections followed in May, but political messaging celebrated the fare reduction and omitted mention of the tax.¹¹ A review of AMB press releases from 2018–2021 confirms that the metropolitan tax was not mentioned at all in 2019.

On October 1, 2019, property owners in the 18 newly added municipalities began receiving tax bills. The tax rate was 0.16% of the cadastral value, with reductions and rebates based on income, property type, and municipality.¹² Most households incurred a tax liability starting at 3 €, with an average yearly amount of 60 €.

The public reaction was swift. In mid-October 2019, several municipalities publicly acknowledged the tax and its link to the January fare reform. Google searches for “*Tribut Metropolità*” spiked, suggesting surprise and low prior awareness. Civic platforms against the tax emerged rapidly — 15 in total — coordinating protests across the metropolitan area under the name “*Plataforma Unificada NO al Tribut AMB*”.¹³

3. Data and methodology

3.1. Data and identification

The empirical analysis conducted here exploits a unique dataset containing public transport ticket validations. Specifically, we have ticket validation data for every hour and every day at each train and metro station in the AMB for the period 2017–2019. This information is derived from the tickets that permanent residents of the AMB typically use, as the day and time of travel are only included on tickets of this type.¹⁴ By limiting our study to these tickets, we are able to clean our data (e.g., eliminate the use of public transport by the city's tourists,

⁸ These were the Metropolitan Transport Entity, the Metropolitan Entity of Water and Waste Treatment, and the Association of Municipalities. See Vallbé et al. (2017).

⁹ The annual savings for a daily commuter were almost 400 € per year, see Section 4.3.

¹⁰ See *Diari Ara*, accessed September 2025.

¹¹ See *Diari Ara*, accessed September 2025.

¹² Exemptions applied for main residences below a certain cadastral value, typically 46,210.11 € or 72,033.78 € in areas updated since 2012.

¹³ See *La Vanguardia* and *El Mundo*, accessed September 2025.

¹⁴ We considered validations of the following tickets: “T-10”, “T-Mes”, “T-Trimestre”, “T-50/30” and “T-70/30”.

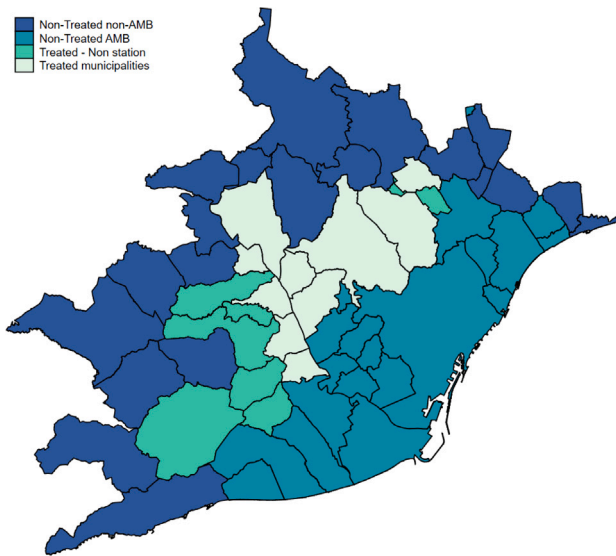


Fig. 2. Treated and non-treated municipalities in the AMB.

infrequent users, and other groups benefiting from reduced and/or free fares) and ensure the analysis is primarily focused on the effects on the regular users of public transport.

As explained in Section 2, our analysis includes treated and non-treated (control) municipalities. The former correspond to the municipalities where the fare reduction was introduced on January 1st, 2019, whereas the latter include some that have never experienced a fare reduction and others where the reduced fare was introduced around 2005.¹⁵ Fig. 2 distinguishes between the treated and the two groups of non-treated municipalities in the AMB. It is important to note that among the 18 treated municipalities, we only have data for 10, as they are the ones with a train and/or metro station within the municipality.¹⁶

The differences between affected and non-affected municipalities, in terms of our main outcome variable, can be visualized by plotting the mean daily validations for each group of municipalities, as presented in Fig. 3.

Fig. 3 specifically shows the aggregate validations in both treated and non-treated municipalities. Validations are significantly higher in the control municipalities, largely because Barcelona is included in this group. However, both groups display parallel trends prior to the fare reduction, diverging only after the policy change. This pattern is particularly evident in Panel B, where validations for both groups are normalized to the last period before the fare reduction. Specifically, Panel B shows that while the trend in non-affected municipalities remains stable, the trend in affected municipalities shifts dramatically, confirming that the effect stems from changes occurring in the treated municipalities.

3.2. Methodology

Following the recent methodological innovations reviewed in Roth et al. (2023), we employ a difference-in-differences strategy to assess the impact of the fare reduction policy on public transportation

use among residents in the affected municipalities. Notably, in our regression analysis we include several control variables that may be correlated with the treated municipalities and so confound our estimates. Among them, we include municipal-level figures on income, the number of firms, unemployed individuals, the property tax base, and the number of cars, with all variables expressed in logarithms (Institut Nacional de Estadística and Diputació de Barcelona).¹⁷

The main identification assumption in a difference-in-differences analysis is that, conditional on the controls, we have parallel trends between affected and non-affected municipalities before the treatment, and that those trends would have continued parallel in the absence of the policy change. However, even controlling for observable characteristics, sometimes systematic differences in trends across municipalities appear, despite there is a clear change in the main outcomes after the policy change. Following Dustmann et al. (2022) and Elias et al. (2025) we allow for linear municipalities specific time trends prior to the policy change to account for potentially different evolutions in transport use across municipalities unrelated to the fare reduction. This strategy is used in previous work, among others, Greenstone et al. (2010), Goodman-Bacon (2021), Miller et al. (2021), Dustmann et al. (2022) and Elias et al. (2025).¹⁸

First, after removing municipality-specific linear time trends using the pre-treatment period, we estimate whether the difference in public transportation use between the affected and non-affected municipalities changed following the fare reduction.¹⁹ To examine this, we estimate the following equation:

$$\ln(\text{validations}_{jt}) = \beta_0 + \beta_1 \text{Post}_t + \beta_2 \text{Treat}_j + \beta_3 \text{Post}_t \times \text{Treat}_j + \rho' X_{jt} + d_t + w_t + \epsilon_{jt} \quad (1)$$

where validations_{jt} is the number of ticket validations made in each municipality j at time t . Our data are daily and, therefore, t represents each day throughout the study period. Post_t is a categorical variable that is equal to 1 in 2019 and 0 for other years. Treat_j is also a dummy variable that takes a value of 1 for municipalities that experienced the fare reduction on January 1st, 2019 and 0 otherwise. Our parameter of interest is β_3 and given that our main dependent variable is in logs, it can be interpreted as the $100 \times \beta_3\%$ variation in the dependent variable experienced when our interaction takes the value 1. X_{jt} are controls (income, number of firms, unemployment, property tax base and # of cars) at the municipal level introduced to capture differences among municipalities.²⁰ We also included days of the week, d_t (Mondays as the reference category) and week of the year fixed effects, w_t . With these two fixed effects we intend to capture the seasonality in the use of public transportation. Finally, ϵ is the error term and in all our estimates we cluster standard errors at the municipal level.

Second, to analyze the timing of the effect, we apply a dynamic difference-in-differences specification at weekly frequency. For this exercise we estimate the following equation:

$$\ln(\text{validations}_{jt}) = \sum_{j=1, j \neq 104}^{156} \beta_j 1[j = t] \times \text{Treat}_j + \rho' X_{jt} + w_t + \epsilon_{jt} \quad (2)$$

¹⁷ As a robustness check of our main estimates, we present a doubly robust difference-in-differences estimator as proposed by Sant'Anna and Zhao (2020). This Difference-in-Differences estimate represents the Average Treatment Effect on the Treated, under the assumption that the parallel trends condition holds conditional on a set of pre-treatment controls.

¹⁸ See Dustmann et al. (2022) and Elias et al. (2025) for a discussion on this empirical approach.

¹⁹ We conducted the same regressions at the station level, and the results remain consistent.

²⁰ Controls are defined as the average of year, monthly or quarterly frequency, depending on data availability. Results remain practically unchanged when we include the same controls but time-varying.

¹⁵ We exploit these varying situations among the municipalities in the control group to conduct some of the robustness checks (see Appendix A.1).

¹⁶ The treated municipalities with train and/or metro stations are: Barberà del Vallès, Castellbisbal, Cerdanyola del Vallès, Molins de Rei, Pallegà, el Papiol, Sant Andreu de la Barca, Sant Cugat del Vallès, Sant Vicent dels Horts, Santa Coloma de Cervelló.

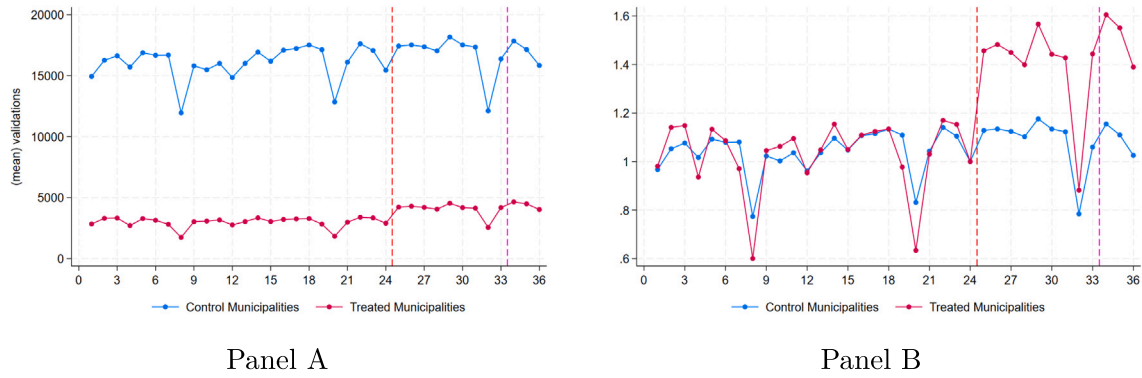


Fig. 3. Raw validations.

NOTE: Panel A presents the monthly average of daily validations (in thousands) in treated and non-treated municipalities. The Panel B normalizes the figure on the left using the last observation before the fare reduction. Vertical lines are used to mark significant events related with the policy: the introduction of new fares on January 1st, 2019 (red) and the distribution of tax receipts starting on October 1st (purple).

Source: Own elaboration based on AMB data.

With this specification, we estimate the interaction of week dummies, denoted by $1[j = t]$, and the treatment variable ($Treat_j$) using the last week before the policy change as a reference period. In this case, the coefficients of interest are the β_j , which we can plot in event-type graphs.

Third, in order to capture the effect after different moments of the year, we extend the previous difference-in-differences specification considering different definitions of the variable $Post_t$. Then, we estimate the following regression for different definitions of the post policy indicator ($Post_t$):

$$\begin{aligned} \ln(\text{validations}_{jt}) = & \beta_0 + \beta_1 Y_{rt} + \beta_2 Post_t + \beta_3 Treat_j + \gamma_1 Y_{rt} \times Post_t \\ & + \gamma_2 Y_{rt} \times Treat_j \\ & + \gamma_3 Post_t \times Treat_j + \delta_1 Y_{rt} \times Post_t \times Treat_j + \rho' X_{jt} + d_t + w_t + \epsilon_{jt} \end{aligned} \quad (3)$$

In Eq. (3) all the variables are same as in Eq. (1) with the addition of a categorical variable Y_{rt} representing each year included in our sample, where the reference year is 2017. Additionally, in this specification, $Post_t$ is dummy that is equal to 1 if the observations are after October 1st, 2019, when property owners in the affected municipalities realized they had to pay a new tax to finance the fare reduction. Likewise, we analyze another period of time, after May 26, 2019, to see if there was any effect after the municipal elections. In this specification, we capture the main effect for different post definitions by interacting three variables. Therefore, our parameter of interest in these cases is represented by δ_1 . $Treat_j$ is again a dummy variable that takes a value of 1 for municipalities that experienced the fare reduction on January 1st, 2019 and 0 otherwise. Nevertheless, as outlined in the robustness checks presented in Appendix A.1, we validate these estimates by employing various treated and control group configurations. Finally, as in previous specifications, we also include controls at municipal level and fixed effects for the day of the week and the week of the year.

4. Results

4.1. Main results

The difference-in-differences estimates obtained using the case of Eq. (1) capture the effect of the reduction in public transport fares on the use of the public transportation (estimated parameter of the interaction of variables “Post” -i.e., January 1st, 2019- and “Treat”). As can be seen in the first column of Panel A in Table 1 treated municipalities experienced an increase in the number of ticket validations compared to the corresponding number in the control municipalities of 39.4%. This increase in the number of ticket validations (column one) can be considered a weighted average of the effect in two different periods:

first, the response in public transport use due to the fare reduction in the first nine months of the year when users were still non fully aware that they would have to pay the new tax (column two) and, second, the response of users after October 1st when they started to receive receipts for the new tax (column three), that is, when many property owners in the treated municipalities realized they would have to pay for the reduction in transportation fares. The difference between columns two and three of Panel A in Eq. (1) shows an increase in utilization in almost 10 p.p. once the year has elapsed. The increased visibility of the policy, driven by the municipal election campaign and the distribution of new tax receipts in the last quarter of the year, may have played a key role in influencing individuals who previously did not use this mode of transportation but are now aware of its reduced price. We present the same specification in Panel B of Table 1, using a doubly robust difference-in-differences estimator as proposed by Sant’Anna and Zhao (2020). The results remain largely unchanged.²¹

To visualize any differences in response in the course of 2019, in Fig. 4 we present our difference-in-differences estimates graphically (event study using Eq. (2)). To analyze the effect more thoroughly over the year, we divided our post-period analysis into three distinct intervals: from January 1st to May 26th (referred to as the “pure fare effect”), from May 26th to October 1st (the “municipal elections effect”), and from October 1st to the end of the year (focused solely on the “tax salience effect”). Fig. 4 shows a clear increase in public transportation use in 2019 compared to the same weeks in the previous year. During the first period, we observe an approximately 30 percent increase, although with high volatility in the estimates. From May 26th until October 1st effect becomes more stable, with slight break in the trend after October 1st. This figure also suggests that the various events occurred throughout 2019 influenced public transport consumption. Specifically, the fare reduction implemented on January 1st gained more visibility over time, leading to increased public transport use in the latter part of the year, particularly after October 1st. This aligns

²¹ Although not the primary focus of this article, it is important to briefly address what happened to public transport supply following the substantial increase in demand induced by the policy. Our analysis suggests that service frequency remained largely unchanged around the time of the fare reduction. According to data from the *Autoritat del Transport Metropolitana* (ATM), key indicators of supply, such as the number of lines, network length, and total train-kilometers, remained relatively stable in 2019 compared to the previous year (see *TransMet Xifres 2019*, accessed September 2025.). This suggests that trains were operating below full capacity prior to the policy, which enabled the system to absorb the additional demand without requiring immediate adjustments to supply.

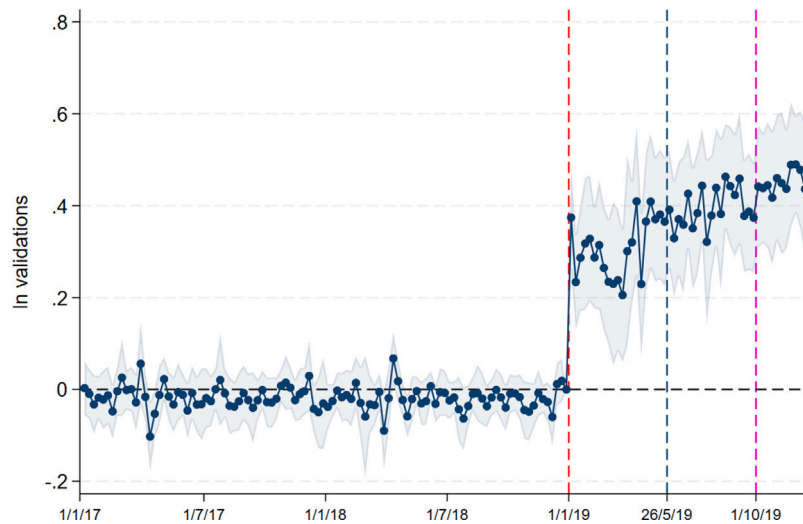


Fig. 4. Estimates of the effect of the fare reduction on public transportation use.

Table 1

Estimates of the effect of the fare reduction on public transportation use.

	(1) All 2019	(2) Before October 1st	(3) After October 1st
Panel A: Difference-in-differences			
Post $\times$ treatment	0.394*** (0.066)	0.370*** (0.065)	0.468*** (0.070)
R^2	0.528	0.536	0.511
Observations	87,430	65,900	21,530
Panel B: Doubly robust difference-in-differences (ATET)			
ATET	0.411*** (0.065)	0.391*** (0.063)	0.475*** (0.079)

NOTE: This table shows the estimates of the log of ticket validations on public transportation in the metropolitan area of Barcelona. Estimates in Panel A are based on a difference-in-differences strategy. Estimates include day of the week and week of the year fixed effects and controls at municipal level: income, number of firms, unemployment individuals, property tax base and number of cars. Column 1 considers as the post-policy period the entire year of 2019, column 2 excludes observations after 1st October each year, and column 3 considers only the weeks after 1st October each year. Panel B, as robustness, we present same specifications using Doubly Robust Difference-in-Differences estimates conditional on pre-treatment characteristics. We show estimates based on stabilized inverse probability weighting and ordinary least squares. Standard errors cluster at municipality level in parentheses. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

with the period following the municipal elections and, more notably, the time when property owners began receiving receipts for the new tax. Importantly, and crucial for our identification strategy, we do not observe any substantial differences between the treated and control municipalities during the pre-policy years (2017 and 2018).

The visual evidence shown in Fig. 4 corroborates the estimates provided in columns two and three of Table 1. Since the break in the trend after October 1st is slight (Fig. 4), we interpret this differential effect (before and after property owners in affected municipalities received their tax receipt) as being driven by increased awareness of the fare reduction among public transport users, coupled with the realization that they were required to pay a new tax as a result of the fare policy. This increase can be attributed either to the heightened prominence of the tax, the greater visibility of the fare reduction, or possibly both. More specifically, public transportation users in the affected municipalities respond to the salience of the tax and/or the publicity about the fare reduction by increasing their use of public transportation.

Another approach to quantify the impact of the policy change, and specifically to disentangle the effects of the reduction in transport fare,

Table 2

Estimates of the effect of fare reduction, tax receipt and elections on public transportation use.

	(1) All 2019	(2) After October 1st	(3) After May 26th
Post $\times$ treatment	0.394*** (0.066)	0.085*** (0.019)	0.127*** (0.026)
R^2	0.528	0.531	0.532
Observations		87,430	

NOTE: This table shows the estimates of the log of ticket validations on public transportation in the metropolitan area of Barcelona. Estimates are based on a difference-in-differences strategy. Estimates include day of the week and week of the year fixed effects and controls at municipal level: unemployment, population, number of cars, foreign employment, property tax base, number of firms and average income. Column 1 considers as the post-policy period the entire year of 2019 (same as column one in Table 1), column 2 considers treated are only observations after October 1st each year, and column 3 considers treated only the weeks after May 26th each year. Standard errors cluster at municipality level in parentheses. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

the salience of the new tax, and/or the municipal elections, is to reset the date used to define the pre- and post- policy periods. In column 1 in Table 2 we reproduce the estimates reported in the same column of Table 1; however, here, in column 2 we define our *Post* indicator as a dummy that takes a value of 1 for observations after October 1st and 0 otherwise. When we interact this *Post* indicator with the variables *Year* and *Treated*, we capture the differential effect for the last three months of the year, between the treated and control municipalities, in 2019 compared to the two previous years. As can be seen in column 2 of Table 2, the estimated effect on the use of public transportation as a consequence of receiving the tax receipts was 8.5%, a magnitude that is close to the difference between the estimates in columns three and four of Table 1.

As explained, municipal elections were held in Spain on May 26th, 2019. Thus, we define one additional *Post* indicators to explore whether there is any differential effect in the use of public transportation attributable to discussions held during the campaign for the municipal elections. In column 3, we show the effect from May 26th to the end of the year. The effect is greater than that observed after October 1st since it reflects the cumulative impact of the last seven months of the year, whereas the October estimate captures only the increase from the final three months. Then, an increase in utilization is observed throughout the year, likely influenced by the elections and the distribution of receipts regarding the new tax, which helped to publicize the price reduction and consequently increased usage. This suggests that new

consumers are beginning to adopt this mode of transportation, likely substituting it for their previous methods. In addition to existing users increasing their usage, new users are now considering this option as a viable alternative due to the price reduction.

4.2. Estimates of the effect of fare reduction using the working day mobility survey

This section provides additional findings based on an alternative database derived from a survey. Specifically, we utilize the Working Day Mobility Survey (*Enquesta de Mobilitat en Dia Feiner*) conducted by the Metropolitan Transport Authority (*Autoritat del Transport Metropolità, ATM*). This survey provides insights into the mobility patterns of residents in the metropolitan area, encompassing the evolution of mobility, distinctive features, residents' opinions and perceptions of transportation, and their daily journeys.

For our analysis, we employed the travel mode module of the survey, covering the waves conducted in 2017, 2018, and 2019. One notable difference from our primary database is that this survey queries respondents about various modes of transportation. This distinction allows us to categorize responses into public and private transport. Specifically, our definition of public transport includes buses, subways, trams, and trains, while our definition of private transport encompasses cars, motorcycles, and vans or small trucks. We also have walking or cycling as transportation options, which represent the most environmentally friendly choice.

Another distinctive feature of this database is that, for all these waves, it was conducted during the last quarter of the year. As a result, all observations are post October 1st, allowing us to evaluate the combined effects of the fare reduction (initiated in early 2019), the campaign of municipal elections and the distribution of tax receipts (post-October 1st). In essence, this analysis offers valuable insights into the cumulative impact of fare reductions, municipal elections and the effects of the new tax used for financing.

The estimated regression follows Eq. (1), where the dependent variable alternates between the logarithm of public transport trips (both total and only trains), private trips, and bicycle/walk trips within a given municipality for a specific year. The variable *Treat* is a binary indicator, taking a value of one for treated municipalities and zero for control municipalities, while *Post* is a dichotomous variable set to one for the year 2019 and zero for the other two years. Our regressions also include the same control variables as in our main analysis, such as municipality income, unemployment rate, number of firms, property tax base, and number of cars, all expressed in logarithms.

The results of this analysis are presented in Table 3. On one hand, we observe positive coefficients for public transportation, which are slightly larger when we focus solely on train usage; however, these effects do not reach statistical significance. It is important to note that the magnitude of the effect is smaller compared to our previous analysis that used ticket validation data.

On the other side, the most intriguing finding of this analysis emerges in the case of private transportation, where we find reduction in the usage of this mode of transport despite not statistically significant at standard levels. However, we also observe a decrease in the use of cleaner modes of transportation, such as cycling and walking. Then, the increase on public transportation usage in affected municipalities was accompanied with a decrease in the use of private and bicycle/walk modes of mobility, highlighting a significant substitution effect.

Finally, consistent with Albalade et al. (2024), our results do not provide definitive insights into the potential effects on pollution. On the one hand, private car usage and other private, polluting modes of transportation have been partially replaced by public transportation, which is generally more environmentally friendly. On the other hand, the reduced cost of public transportation has also led to an increase in its use among people who previously cycled or walked, meaning this substitution may have contributed to higher pollution levels.

Table 3

Estimates from the Working Day Mobility Survey.

	(1) Public transport	(2) Private transport	(3) Other (bicycle & walk)	(4) Public transport by train
<i>Main Results</i>	0.190* (0.106)	−0.137 (0.084)	−0.118* (0.068)	0.240 (0.201)
<i>R</i> ²	0.794	0.789	0.812	0.722
Observations	186			

NOTE: This table presents the estimated effects of the policy on the logarithmic values of public and private transportation usage, as well as on other environmentally friendly modes of transportation (column four), in the metropolitan area of Barcelona. Column one includes all types of public transportation and column two considers private vehicles (i.e., cars and motorbikes). The third column, other, includes only cycling and walking. The column fourth column only consider riderships using train or metro. Estimates are based on a difference-in-differences strategy (Eq. (1)). We match the same treated municipalities as in our main estimates (Table 1). Estimates include controls at municipal level: unemployment, population, number of cars, foreign employment, property tax base, number of firms and average income. Standard errors cluster at municipality level in parentheses. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table 4

Theoretical gains and losses of the policy.

	Frequent users	Less-frequent users	Non users
Renters	Reduction of 50% of the PT fare	Reduction of 50% of the PT fare	No effect
Owners	Reduction of 50% of the PT fare and payment of the new tax	Reduction of 50% of the PT fare and payment of the new tax	Payment of the new tax
Public sector	Receives less revenue per tickets, but sells more tickets and collects revenue from the new tax		

4.3. Welfare analysis

To evaluate the success of the policy, we conduct a welfare analysis that quantifies its direct effects. Using both available data and our causal estimates, we assess the gains and losses generated by the policy throughout 2019, focusing on the different types of agents affected by the change. We then relate these effects to the policy's costs and the new financing mechanisms. Table 4 outlines the theoretical distribution of gains and losses within the treated areas, while Table 5 provides their empirical quantification.

Assumptions and data used in our analysis

1. We consider six different types of private agents, based on a combination of frequent users, less-frequent users, and non users of public transportation, further differentiated by housing tenure (renters vs. owners), as summarized in the previous table. Additionally, we include the public sector and society as a whole in our analysis.
2. Frequent users are assumed to take 35 trips per month over 11 months of the year, while less-frequent users take 15 trips per month over the same period. These figures are based on usage patterns of the T-10 ticket, which is by far the most commonly used ticket type.
3. Based on data from the *Enquesta de Mobilitat en Dia Feiner* (EMDF), we estimate that 34% of adults in the affected municipalities use public transportation. We further assume that half of these are frequent users.
4. Using the 2011 Spanish Household and Population Census, we estimate that 13.4% of residents in the affected municipalities are renters. We have verified that this proportion remains approximately the same in the 2021 census.
5. Public transportation fares are sourced from the *Autoritat del Transport Metropolità* (ATM) website.

Table 5

Gains and losses of the policy: Individual private agents.

	Frequent users	Less-frequent users	Non users
Renters	387 €	166 €	No effect
Owners	347 €	126 €	-40 €

Note: Based on own calculations, with all figures expressed in €..

Table 6

Gains and losses of the policy: Private agents.

	Frequent users	Less-frequent users	Non users
Renters	0.72	0.31	...
Owners	4.28	1.55	-6.56

Note: Based on own calculations, with all figures expressed in millions of €.

Table 7

Gains and losses of the policy: Public sector.

	Change in revenues from ticket sales (trains and metro)	Change in tax collection
Public sector	-4.81	6.63

Note: All figures expressed in millions of €.

6. According to data from the *Àrea Metropolitana de Barcelona* (AMB), the average metropolitan tax payment per household was €60 per year. We assume an average of 1.5 adults per household.
7. Total tax revenue collected in 2019 in the affected municipalities was also obtained from AMB.
8. Finally, using our data on ticket validations, we estimate the change in ticket revenue between 2018 and 2019.

We then calculate the benefits and costs for each type of private agent. The following table summarizes these results in €. For example, frequent users who are renters save approximately €387 per year when comparing 2018 and 2019. This figure is calculated as follows:

$$3.5 \text{ (\# of tickets/month)} \times 20.10 \text{ (price ticket/2018)} \\ \times 0.5 \text{ (50\% fare reduction)} \times 11 \text{ (months/year)}$$

The difference for frequent users who are owners lies in the additional cost of the metropolitan tax. Specifically, owners must pay the tax, which we estimate at €40 per adult per household per year, reducing their net benefit compared to renters.

We extrapolate these individual estimates to the total population in the affected municipalities. To do this, we multiply the working-age population by the estimated individual gains or losses, weighted by the proportion of public transportation users and the share of renters or owners in the population.

For example, to estimate the total benefit for frequent users who are renters in affected municipalities (amounting to approximately €0.72 million in Table 6), we multiply:

$$387 \text{ (indiv. benefit)} \times 0.131 \text{ (\% of renters)} \\ \times 0.106 \text{ (\% of frequent users)} \times 133788 \text{ (working-age pop.)}$$

To analyze the impact on the public sector, we consider two components: (1) the change in revenue from ticket sales, and (2) the amount collected through the metropolitan tax (as reported by the AMB). In both cases, we compare data from the affected municipalities before and after the policy change, specifically between 2018 and 2019.

For ticket revenues, we calculate the difference by subtracting the total revenue in 2019 from that of 2018. Revenues are computed as the number of tickets sold (by ticket type) multiplied by their respective prices. This results in a revenue loss of approximately €4.81 million in 2019 compared to 2018 (Table 7).

Regarding ticket sales revenue, the observed reduction is smaller than the total gains estimated for public transportation users. We argue

that this difference can be attributed to the increase in demand for public transport following the fare reduction.

If we adjust the €4.81 million revenue loss by the estimated increase in demand from our main specification (0.394), we obtain an adjusted figure of approximately €7.94 million. This value aligns more closely with our aggregate user benefit estimate from Table 6, which totals €6.86 million (comprising €0.72 million + €0.31 million + €4.28 million + €1.55 million).

Furthermore, a comprehensive welfare analysis should also consider potential externalities of the policy change, such as modal substitution effects and environmental impacts (e.g., changes in pollution levels). In our analysis (Section 4.2), we document a substitution away from walking and cycling toward increased use of public transportation. We also observe a reduction in private vehicle usage, although our estimates for this effect are not statistically significant.

Regarding environmental externalities, a closely related study by Albalade et al. (2024), which examines another fare reduction policy in Spain, finds no significant change in pollution levels following the implementation of the policy.

5. Conclusion

This paper examines whether changes in public transportation fares influence consumer use of public transport. We find that the fare reduction, together with the publicity surrounding local elections and the introduction of a new tax to finance the policy, significantly increased demand for public transportation. Specifically, we document an overall increase in ticket validations of approximately 39% during the year following the policy change. However, this annual figure masks important temporal variation. In particular, once taxpayers became aware that the fare reduction was being funded through a new tax—after October 1st, public transportation use in treated municipalities rose by an additional 10 percentage points. This pattern underscores the role of policy salience in amplifying behavioral responses. Our results are robust to a variety of model specifications and alternative treated-control definitions. We also find that both the fare reduction effect and the salience effect are more pronounced in smaller municipalities.

Overall, our paper contributes new causal evidence to the well-established finding in the transportation literature that fare reductions significantly increase public transportation demand.

Using survey data, we also observe a modal shift away from private transportation modes — such as cars, motorcycles, and small trucks — as well as from walking and cycling. This suggests that the fare reduction induced a shift not only from more polluting modes, but also from environmentally friendly alternatives. As a result, the net environmental impact of the policy remains ambiguous, an important consideration for policymakers designing mobility-related interventions.

In addition, we provide a welfare analysis that quantifies the direct gains and losses of the fare reduction policy across different groups of private agents and the public sector. We find that frequent public transportation users, especially renters, experienced sizable monetary benefits, while non-users who are property owners incurred net losses due to the metropolitan tax. The public sector, in turn, experienced a reduction in fare revenue, partially offset by increased ticket sales, along with higher revenue from the new tax. Overall, our calculations suggest that the policy effectively redistributed resources from non-user property owners to public transportation users.

Finally, our findings highlight the importance of public awareness in shaping policy effectiveness, particularly in metropolitan areas with high administrative and institutional fragmentation. A key contribution of our study lies in identifying a novel link between the visibility of taxes and the consumption of public services.

CRedit authorship contribution statement

María Cervini-Plá: Writing – review & editing, Writing – original draft, Investigation, Formal analysis, Data curation. **Mariona Tomàs:** Writing – review & editing, Writing – original draft, Formal analysis, Conceptualization. **Javier Vázquez-Grenno:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Appendix. Additional evidence

A.1. Robustness checks

In [Table A.1](#) we perform some additional exercises to highlight the reliability of our main results. In all the exercises, we estimate both Eqs. (1) and (3) for different samples of municipalities or different types

Table A.1
Robustness checks.

	(1) All 2019	(2) After October 1st	(3) After May 26th
Baseline	0.394*** (0.066)	0.085*** (0.019)	0.127*** (0.026)
R ²	0,528	0,531	0,532
Observations		87,430	
Robustness 1 No municip. who had a reduced fare	0.391*** (0.066)	0.076*** (0.020)	0.121*** (0.027)
R ²	0,510	0,513	0,514
Observations		72,279	
Robustness 2 No municip. who never change the fare	0.402*** (0.067)	0.116*** (0.030)	0.147*** (0.029)
R ²	0,680	0,684	0,684
Observations		27,020	
Robustness 3 Eliminate neighbor municip.	0.392*** (0.066)	0.088*** (0.019)	0.131*** (0.026)
R ²	0,528	0,531	0,532
Observations		84,197	
Robustness 4 Eliminate control municip. further away	0.406*** (0.067)	0.098*** (0.021)	0.126*** (0.026)
R ²	0,674	0,677	0,678
Observations		38,917	
Robustness 5 Eliminate treated municip.	−0.014 (0.021)	−0.041 (0.033)	−0.026 (0.024)
R ²	0,495	0,499	0,499
Observations		76,647	
Robustness 6 Only T-10 Tickets	0.424*** (0.072)	0.117*** (0.033)	0.155*** (0.033)
R ²	0,692	0,692	0,692
Observations		87,393	
Robustness 7 Only reduced or free Tickets	0.115** (0.050)	0.025 (0.037)	0.029 (0.031)
R ²	0,717	0,718	0,717
Observations		86,244	

NOTE: This table shows the estimates of the log of ticket validations on public transportation in the metropolitan area of Barcelona. The estimates in the first row reproduce the results presented in [Table 2](#) and the columns are the same as those in [Table 2](#). Robustness 1: We eliminate those municipalities from the control group that introduced the reduced fare in the 2000s. Robustness 2: We eliminate those municipalities that never operated a reduced fare. Robustness 3: We eliminate neighboring municipalities who never reduced the public transportation fare. Robustness 4: We eliminate distant lying controls. Robustness 5: We eliminate the treated municipalities. Robustness 6: We consider only T-10 tickets. Robustness 7: We consider only free or reduced price tickets. All estimates include day of the week and week of the year fixed effects and controls at municipal level: unemployment, population, number of cars, foreign employment, property tax base, number of firms and average income. Standard errors cluster at municipality level in parentheses. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

Table A.2

Heterogeneous results by income and population.

	(1) All 2019	(2) After October 1st
Panel A: Baseline		
<i>Main Results</i>	0.394*** (0.066)	0.085*** (0.019)
R ²	0.528	0.531
Observations		87,430
Panel B: Income		
<i>Below the mean</i>	0.371*** (0.059)	0.072*** (0.025)
R ²	0.514	0.516
Observations		82,076
<i>Above the mean</i>	0.417*** (0.115)	0.098*** (0.018)
R ²	0.512	0.516
Difference test (χ^2)	0.13	1.27
Observations		82,001
Panel C: Population		
<i>Below the mean</i>	0.503*** (0.095)	0.114*** (0.016)
R ²	0,514	0,517
Observations		82,001
<i>Above the mean</i>	0.286*** (0.057)	0.055** (0.020)
R ²	0.514	0.517
Difference test (χ^2)	3.94	15.10
Observations		82,076

NOTE: This table shows the estimates of the log validations in the public transportation in the metropolitan area of Barcelona. Estimates in Panel A, reproduce the results presented in columns 1 and 2 of [Table 2](#). Panel B and Panel C present same estimates dividing the sample above and below the median of the income per capita and population of the municipality in 2017 respectively. Estimations include day of the week and week of the year fixed effects and controls at municipal level: unemployment, population, number of cars, foreign employment, property tax base, number of firms and average income. Standard errors cluster at municipality level in parentheses. ***, **, * indicate significance at the 1, 5 and 10% level, respectively.

of train tickets. The first row reproduces our baseline results already presented in [Table 2](#).

Robustness check one presents the results of our main specification after excluding municipalities that had already implemented a reduced transport fare prior to our analysis period. Notably, our results remain virtually unchanged. This exercise holds particular relevance as it involves the exclusion of major cities within the AMB (namely, Barcelona, L'Hospitalet de Llobregat, and Badalona) and allows for a comparison between our treated municipalities and those that have never had a reduced fare.

Our second robustness check involves comparing the treated and control municipalities within the metropolitan area of Barcelona. In other words, we exclude from our control group those municipalities that have never changed their status to Zone 1 and, consequently, have never been part of a reduced fare zone. Once more, our results remain virtually unchanged.

To account for the potential influence of behavioral changes among residents in municipalities neighboring the treated ones on our main outcome, our third robustness exercise excludes those municipalities that never experienced a change in fare status. The aim of this exercise is to eliminate any potential behavioral responses from public transportation users residing in neighboring areas. This includes commuters who may travel to the treated municipalities via private transport and then switch to public transportation. If such behavior were influencing our estimates, it could lead to an overestimation of the true effect. However, the results of this exercise closely align with our main results presented in the previous section.

To ensure that the distances between the municipalities in our exercise and the city of Barcelona are more comparable, our fourth

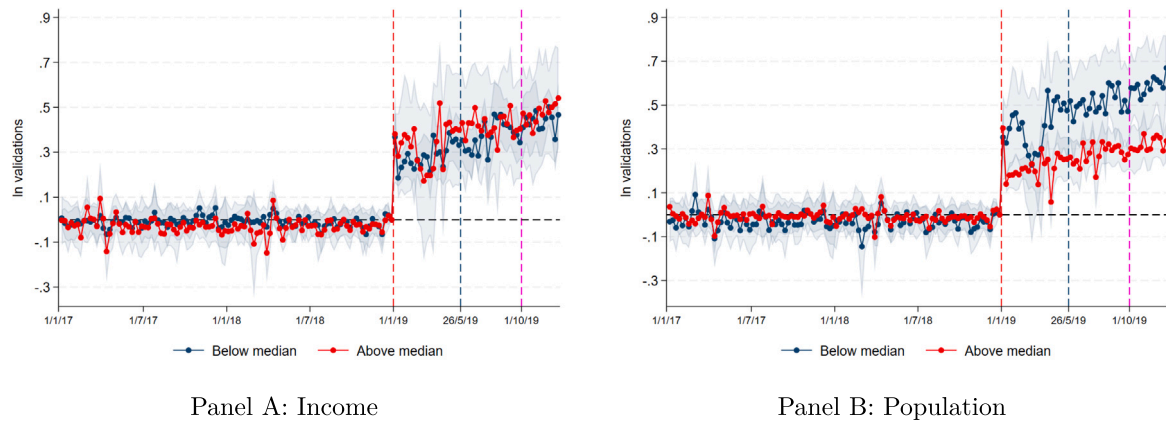


Fig. A.1. Estimates of the effect of the fare reduction on public transportation by income and population.

NOTE: Difference-in-differences estimates from Eq. (1) using weekly data. The dependent variable is the log of validations. To mitigate the impact of seasonality and the variations in public transportation usage at different times of the year, we aggregate the data on a weekly frequency. Vertical lines are used to mark significant events related with the policy: the introduction of new fares on January 1st, 2019 (red), the period following the municipal elections held on May 26th (blue), and the distribution of tax receipts starting on October 1st (purple).

robustness check focuses exclusively on the 36 municipalities within the AMB (comprising 18 treated and 18 control municipalities), along with their neighboring counterparts. This means that we exclude municipalities located outside of the AMB. Once again, the estimates align with the findings of our baseline results.

Fifth, another approach to ensure that our estimates accurately capture the effect of the policy change is to conduct a placebo test, in which we redefine the treatment and control groups. Specifically, this exercise excludes our treated municipalities. In other words, we compare the municipalities in our original control group, -those that had implemented a reduced fare since the mid-2000s (now considered as “new treated”)- with municipalities that have never been in a reduced fare zone (now considered as “new control”). As shown in the fifth robustness exercise in Table A.1, the results are notably small and, as expected, lack statistical significance.

Our two last robustness exercises involve data restrictions based on two specific types of train tickets. First, robustness check six, we only consider the most frequently used ticket in the AMB, the T-10 and second, we restrict the sample to train tickets with reduced or entirely free fares.

As observed in Table A.1, the results of robustness 6 align with our baseline findings, with slightly larger point estimates. In our final robustness exercise, we restrict the sample to train tickets designed for specific groups, such as young people, pensioners, large families, and single parents, among others. It is important to note that these ticket categories were not included in our baseline estimates. The results show a positive effect for the entire year of 2019 (column one). However, this effect is significantly smaller, measuring only about one-quarter of our baseline point estimate. In contrast, for the other time periods analyzed (columns two and three), the effects are negligible and statistically indistinguishable from zero. The positive estimates found in column one may be driven by individuals who benefit from further reductions in already discounted transport fares, or by individuals accompanying regular passengers.

A.2. Heterogeneous analysis

In this section, we consider the possibility of heterogeneous effects of the policy on consumption of public transportation. We aim to explore whether our results might differ based on two distinct dimensions of the municipalities: income and population.

In Table A.2, we present the results from estimating Eqs. (1) and (3) to analyze the effects of fare reduction (column one) and the distribution of tax receipts (column two) on public transportation consumption.

Specifically, we perform separate estimations of both equations for two categories of treated municipalities, based on income per capita (Panel B) and population (Panel C), distinguishing between those above and below the median. For comparison, we also report our baseline results in Panel A.

The Panel B of Table A.2 presents the results for treated municipalities above and below median per capita income.²² The impact of the fare reduction appears to be more pronounced in relatively affluent municipalities (column 1), although the point estimates are not statistically different. Similarly, the effect attributed to the distribution of tax payment receipts (e.g., that could be interpreted as tax salience and/or awareness of the fare reduction) is also more substantial in wealthier municipalities (column two). This heterogeneity may be attributed to different factors. Firstly, wealthier municipalities may have greater flexibility to shift from private transportation to public transportation, and the fare reduction clearly influences this margin. Secondly, in high-income municipalities, the salience of the tax could be more significant. Because of the high property values, fewer property owners were eligible for tax exemptions, and the average amount of tax liabilities in these municipalities would also be larger.

In Panel C of Table A.2, we present the estimates for treated municipalities below and above the median population. In this exercise, we find a significantly greater effect in relatively small municipalities for the entire 2019. Similarly, for the period after October 1st, although the differences in this case are not statistically significant between relatively small and big municipalities. One possible explanation for this result is that in smaller municipalities, which are typically less well-connected to Barcelona by public transportation, the reduced fare compensates for this poorer connectivity.

Finally, Fig. A.1 graphically presents the heterogeneous effects by income and population. The left panel shows the dynamic effects for treated municipalities that are wealthier or poorer than the median, with both groups displaying similar patterns. In contrast, the right panel displays the dynamic estimated effects for treated municipalities with populations above and below the median. It is evident that the fare reduction had a stronger impact on public transportation use in less populated municipalities. As in our main estimates, in these two cases, we do not observe differences between the treated and control municipalities during the pre-policy periods.

²² See Goldin and Homonoff (2013) for evidence about heterogeneous responses to tax salience along individuals' income distribution.

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