Document de travail
Discussion paper

N°2025-10

https://lem.univ-lille.fr

When the Money Stops: Fiscal and Political Reactions to Changing Grant Eligibility

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When the Money Stops: Fiscal and Political Reactions to Changing Grant Eligibility

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December 15, 2025

Abstract

We study the incentive effects of vertical equalizing transfers in France. Using a reform in 2017 that tightened eligibility criteria for an urban equalization grant, we analyze how grant loss affects municipal budget choices. Drawing on a large panel of urban municipalities over the 2014–2024 period, we employ a staggered difference-in-differences framework to identify the causal impact of grant losses. First, on average, municipalities substitute grants for local tax revenues. Second, municipalities with populations close to the eligibility threshold engage in sorting, and those that sort to qualify for the grant primarily reduce their current expenditure. Moreover, political factors unveil heterogeneity in responses. Left-wing municipalities increase spending and simultaneously raise taxes and debt. In contrast, right-wing municipalities increase taxes but consolidate debt. Municipalities led by narrowly elected mayors pursue stronger fiscal discipline, reducing expenditures and debt. Political fragmentation within municipal councils generates patterns consistent with coalition bargaining.

Keywords: Inter-governmental transfers, Municipalities, Budget responses, Politics, Population-thresholds, Bunching.

JEL Classification: D78, H70, H72, C23

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1 Introduction

The structure of fiscally decentralized intergovernmental systems varies across countries, shaped by the objectives and constraints of central governments. However, the key upstream issues are broadly similar: how to devolve spending responsibilities and assign revenue-raising powers across levels of government; how to design fiscal transfers and borrowing rules; and how to build arrangements that ensure local accountability.

Intergovernmental transfers are widely used to support local governments. In France, the latter receive transfers from the central government, most notably through the global operating grant, which is intended to provide them with the financial autonomy[1] and stability necessary to fulfill their mandates. It also serves as an equalization mechanism, designed to assist municipalities with limited tax capacity and/or significant expenditure needs.

Evaluating the effectiveness of such grants is important, but understanding their incentive effects is equally essential—particularly when policymakers revise eligibility criteria. Our objective is to identify behavioral responses to changes in equalization grants, while documenting both their beneficial and potentially adverse consequences. A key question concerns how municipalities respond to transfer cuts[2]: through increased revenue mobilization, expenditure restraint, or more complex budgetary re-allocations.

The empirical study of intergovernmental transfers raises endogeneity concerns, as allocation rules often depend on municipal characteristics such as population size and wealth. To address this challenge, we exploit the 2017 reform that tightened eligibility for the urban solidarity grant, redistributing resources toward more vulnerable urban municipalities. The reform resulted in transfer cuts for some municipalities, while others retained eligibility, enabling us to identify the causal impact of grant losses on municipal budgetary decisions.

As population thresholds determine grant eligibility, the potential manipulation of municipal population figures is also an issue, as highlighted in the literature (Foremny et al. (2017) and Eggers et al. (2018)). Although French population figures are produced using nationally

[1]French municipalities have full budgetary autonomy. Before 2014, their only fiscal constraint was the golden rule that forbids borrowing to fund operating expenditures. A non-binding expenditure ceiling has been introduced since 2014.

[2]Between 2014 and 2018, the French government reduced the central grant to municipalities to promote fiscal discipline.

reliable census procedures, municipalities also contribute to the process at the local level. The census rules, which rely on annual rolling population surveys and municipalities' participation, leave room for sorting, that is, to be located on the good side of the threshold. We therefore exploit discontinuities around eligibility population thresholds and apply bunching estimation methods to detect potential manipulation in the grant allocation.

We contribute to the literature on the incentive effects of intergovernmental transfers focusing on the French equalizing system, which has not been previously examined. Using a large panel dataset of urban municipalities covering the 2014–2024 period, we adopt a staggered difference-in-differences framework and obtain the following main results.

First, the predominant response to the grant loss is an increase in tax revenues, consistent with a substitution mechanism whereby local governments offset declining transfers by mobilizing own-source revenues. By contrast, total spending does not display systematic adjustments. These findings point to asymmetric budget rigidity, where revenue tools adjust more readily than spending commitments.

Second, we provide evidence of manipulation and bunching around the eligibility population thresholds, i.e., a subset of municipalities with population close to the thresholds engage in sorting. Moreover, as these thresholds governing grant eligibility also trigger other public policies, municipalities face opposite incentives that lead them to weigh the benefits against costs of those policies. While some municipalities may have incentives to cluster above, and others below, this results in two-sided bunching patterns. We find that municipalities that sorted to gain the grant behave differently from the rest of the sample. When they lose it, they tend to adjust primarily through expenditure restraint, especially in current and personnel spending, indicating stronger fiscal discipline.

Third, our main substitution result, an aggregate effect obtained on the whole sample, does not reflect the heterogeneity of budgetary responses which are shaped by political factors, such as partisanship, electoral competition and fragmentation within the municipal council. Left-wing municipalities react to the grant loss by increasing personnel and investment spending while simultaneously raising taxes and expanding debt, indicating an expansive fiscal policy. Right-wing municipalities, in contrast, increase taxes but consolidate debt without major spending shifts. Municipalities led by narrowly elected mayors pursue stronger fiscal discipline—reducing expenditures and debt—while those with wide electoral margins rely more on tax increases. Political fragmentation within municipal councils generates patterns consistent with coalition bargaining and encouraging compromise allocations across budget items, such as higher personnel expenditures and lower equipment spending in highly fragmented councils.

These heterogeneous reactions highlight the importance of institutional and political context in interpreting the effects of intergovernmental grants. They also reveal strategic behavior: some municipalities seem to engage in signaling toward voters or the central state, adopting targeted expenditure patterns. Such opportunistic responses underscore the complex incentives created by temporary or uncertain transfers.

There is a large literature on the effects of intergovernmental grants on local governments, extensively reviewed in Lago et al. (2024). Crowding-out effects occur frequently and are well documented. Increased transfers led Polish municipalities to reduce their tax revenues (Banaszewska (2023)). Italian municipalities faced with grant cuts increased taxes rather than reduce spending (Marattin et al. (2022)). Empirical studies show that spending tends to increase when grants rise (Ashworth et al. (2013); Jaaidane and Larribeau (2023)) and reveal asymmetric responses: spending cuts following grant reductions tend to be smaller than spending increases following grant expansions. The contrasted response with spending that adjust more slowly than tax revenues is consistent with public choice arguments (Olson (1965)) that organized beneficiaries force adjustment to occur mainly through broader-based taxes borne by taxpayers. Recent empirical studies highlight this spending stickiness. Shani et al. (2023) show that grant cuts led Israeli local governments to raise local tax rates and increase annual deficits, but did not affect spending levels; in contrast, grant increases led to higher spending and lower tax rates and deficits. Similarly, Helm and Stuhler (2024) show that German municipalities adjust budgets sluggishly, with spending reacting more slowly than revenues. When it comes to the incentives to limit debt, evidence often supports the soft-budget constraint hypothesis (Kornai et al. (2003)), where local governments expect future bailouts by central government and thus overspend or under-tax and accumulate debt (Pettersson-Lidbom (2010); Berset and Schelker (2020)). However, Dahan (2022) finds that equalizing grants in Israel reduced municipal debt.

Politics further shape these effects. Partisanship naturally does matter (see Jimenez et al. (2025) finding that US cities switching from a Democratic to a Republican mayor improve budgetary solvency). More specifically related to transfers, Danish municipalities led by left-wing mayors are more inclined to raise spending when grants increase and to raise taxes when grants are reduced (Baekgaard and Kjaergaard (2016)). Spanish left-wing municipalities tend to maintain expenditure and increase taxes when grants are cut (Lago-Penas (2008)). The increase in tax revenues (rather than cuts in spending) is more likely in Italian municipalities with low levels of electoral competition (Marattin et al. (2022)). Spanish municipalities where incumbents have less control over budget allocations tend to respond more strongly to increases in grants (Rios et al. (2022)). The importance of political fragmentation is also documented theoretically–Buchanan and Tullock (1965) and Weingast et al. (1981) show that fragmented councils exhibit high expenditure volatility and overspending generated by logrolling–and empirically (see Le Maux and Zhang (2013) for an application to the French local governance).

We also relate to the literature on bunching around cutoffs that trigger specific policies (Kleven and Waseem (2013); Kleven (2016); Foremny et al. (2017)). While the issue received particular attention in the fields of income taxation (Saez (2010); Chetty et al. (2011)) and consumer credit (e.g. Mikhed et al. (2024)), few papers study its application at the municipal level with a political, budgetary, and governance focus. Among them[3], Fontana and d'Agostino (2025) uncover the role of organized crime in manipulating municipal procurement at thresholds, resulting in corruption and inefficiency.

The rest of the paper is structured as follows. Section 2 presents institutional background of the French municipal equalization system. Section 3 describes the data and sampling methodology, while Section 4 details the empirical strategy. Sections 5 and 6 report the main results and concluding remarks respectively. Section 7, an online Appendix, provides robustness checks and additional information.

[3]De Witte et al. (2018) study whether Belgian municipalities engage in manipulation of population figures. Although they do not evidence bunching, they show that approaching important population thresholds leads to lower local tax rates and the granting of additional building permits.

2 Institutional context

Many factors contribute to inequality among France's 35,000 municipalities. To mitigate these disparities, municipalities receive a global operating grant[4] from central State. This grant comprises a lump-sum component received by all municipalities and an equalizing component allocated to those that meet specific eligibility criteria. The solidarity component itself includes three sub-components: the urban grant, the rural grant, and the national solidarity grant.

This study focuses[5] on the urban solidarity grant as we want to take advantage of a reform to assess its causal impact on municipalities' behavior.

The global operating grant calculation follows a formula that combines both resources and needs criteria, reflecting multiple dimensions of inequality. Tax capacity—i.e., the fiscal revenue a municipality would collect if it applied the national average tax rates to its own tax bases—captures disparities in resources that do not result from local fiscal choices. Other criteria, such as population size, land area, and population density, help estimate the needs and costs of local public service provision. Additional indicators account for specific local characteristics.

2.1 Urban solidarity grant (USG)

The urban solidarity grant (USG) targets urban municipalities, though not all are eligible. In 2024, approximately €2.6 billion was distributed among 860 urban municipalities. Eligibility depends first on population size: two demographic groups are targeted—municipalities with 5,000–10,000 inhabitants, and those with over 10,000 inhabitants—with priority given to those most vulnerable in terms of resources and needs.

Eligibility is next determined through a ranking system that combines both resources and needs indicators. These include municipal fiscal wealth, average household income, number of social housing units, and the share of residents receiving income support or housing

[4] *Dotation Globale de Fonctionnement.*

[5] We ignore the two other sub-components, and in particular the third, which relevance is under debate. As is attributed to nearly all municipalities and has a limited redistributive power, its amount has been frozen for 2023 and 2024 (see Jaaidane and Larribeau (2024)).

allowances. Once a municipality's synthetic index (SI) is computed, it is ranked within its demographic group in descending order (with rank 1 corresponding to the highest SI). Changes in a municipality's position—due, for instance, to variations in local wealth or residents' income—can therefore lead to gains or losses in USG eligibility.

The amount received by eligible municipalities depends on the same set of criteria, as well as the tax effort[6] and other contextual indicators such as the share of residents living in underprivileged neighborhoods, hereafter poor district areas (PDA).

The administrative body responsible for calculating and announcing municipal grant amounts uses a specific population measure known as population-grant. This is derived from the legal municipal population, which includes all residents usually living in the municipality as well as specific groups such as prisoners, students, and residents of nursing homes. The legal population is then "augmented" by adding one inhabitant per secondary residence, one inhabitant per caravan space in traveler reception areas, and an additional adjustment for small municipalities with a high proportion of secondary residences or significant tourism activity[7].

2.2 The 2017 reform of the urban solidarity grant

The 2017 reform introduced several key changes to the USG. Mainly, it reduced the number of eligible municipalities while increasing the total granted amount. The grant covers 10% of municipalities with 5,000–10,000 inhabitants (as before 2017), and two-thirds (down from three-quarters before 2017) of those with more than 10,000 inhabitants. The computation of the SI, in particular the weighting of each criterion, was modified to better target the most financially vulnerable municipalities. A new criterion was also introduced and taken into account: the financial capacity[8]. To be eligible, a municipality's average financial capacity must be below a threshold equal to 2.5 times the average financial capacity of its demographic group.

Smoothing rules govern the evolution of USG amounts in case a municipality ceased to be eligible for the USG in 2017. It received 90% of the amount received in 2016 in 2017, 75% in

[6]The tax effort is measured as the ratio of local tax revenues to tax capacity.

[7]For more details on eligibility and amount determination, see the online Appendix subsection A.1.

[8]This indicator–obtained by adding the tax capacity to the lump-sum part of the global operating grant–represents a measure of a municipality's fiscal and public resource capacities.

2018, 50% in 2019, the amount being null in 2020. In some particular cases, the guarantee applies over four or five years. Moreover, for a municipality falling below the threshold of 5,000 inhabitants, a 10-year guarantee applies ensuring a smoother transition out of the system.

2.3 Are the criteria subject to manipulation?

We examine which criteria municipalities could plausibly influence in determining eligibility and grant.

Tax capacity, obtained by applying national average tax rates to local taxable bases, ensures an impartial and standardized measure of fiscal wealth and prevents municipalities from inflating or deflating their capacity through tax rate decisions. Moreover, local tax bases are assessed using national cadastral data administered by central authorities. These data rely on objective valuation criteria—such as property type, size, and location—and are updated annually[9], leaving minimal room for local manipulation.

Tax effort may be subject to local discretion. However, it influences only the grant amount, not eligibility.

Household income data used for eligibility are provided by the National Institute of Public Statistics (INSEE) and based on tax declarations from the General Directorate of Public Finances, combined with social benefit data from multiple national organizations. These are thus considered reliable and resistant to manipulation.

Population data, however, warrant closer scrutiny. INSEE defines the legal municipal population as all individuals residing in France for at least 12 months, regardless of nationality or legal status, excluding short-term visitors, commuters, and seasonal workers. Since 2004, France has replaced traditional decennial censuses with annual rolling population surveys[10]. For municipalities with fewer than 10,000 inhabitants, the population is surveyed exhaustively every five years, with one-fifth of municipalities surveyed each year. For those over 10,000 inhabitants, sample surveys cover about 8% of dwellings annually, leading to 40% cov-

[9] Cadastral rental values approximate the annual income a municipality would earn if properties were rented. However, these reference values have not been comprehensively reassessed since 1970 and have only been updated through uniform revaluation coefficients.

[10] See the full description of the process in Cour-Comptes (2024).

erage over five years. Population estimates are then derived using cumulative five-year data. Although highly accurate at the national level, sampling uncertainty introduces variability for individual municipalities, with margins of error ranging from 0.25% to 3%, depending on size. Small year-to-year population shifts may thus reflect statistical noise rather than real demographic change. Municipalities are also directly involved in the census process—they recruit field agents, verify address lists, and encourage resident participation—which is essential for data accuracy but introduces potential incentives near key thresholds (e.g., 5,000 and 10,000 inhabitants). Although no explicit population manipulation may occur, municipalities just below thresholds may face implicit incentives to ensure that counts favor eligibility outcomes.

Furthermore, the share of residents living in PDA influences the grant amount. Municipalities may pursue targeted hiring or social programs in these poor neighborhoods to demonstrate stronger social investment, potentially reinforcing their case for higher USG allocations.

2.4 Other population-based policies

Population thresholds not only define eligibility for the USG but also triggers other regulations municipalities might fall under, as shown in table 1.

Table 1: Population-based policies at different cutoffs

cutoff	USG	TDRET	Council Size	Executive Allowance	Transparency	Electoral Rule
1,000	No	No	No	Yes (+)	No	Yes (-)
3,500	No	No	Yes (+)	Yes (+)	Yes (-)	No
5,000	Yes (+)	Yes (-)	Yes (+)	No	No	No
10,000	Yes (+)	No	Yes (+)	Yes (+)	Yes (-)	No

Notes. The table specifies the population cutoffs at which different policies are triggered. We retain cutoffs relevant to the period analysis. The USG is defined using population-grant cutoffs while the other regulations are based on and legal population. "TDRET" denotes the taxes due on real estate transactions. (+) (resp. (-)) means that the policy benefits (resp. harms) the municipality if its population is above (resp. below) the cutoff.

First, crossing the 5,000 or 10,000-inhabitant thresholds benefits the municipality since it increases the likelihood of being eligible for the USG. Indeed, according to the institutional rules of the USG, having at least 5,000 inhabitants is the first criteria for eligibility and the likelihood of being eligible increases from 10% below 10,000 to 66.67% above 10,000 (75%

before 2017).

Second, crossing the 5,000-inhabitant threshold has broader implications beyond the USG. It also affects horizontal equalization mechanisms, notably through the Transfer Duties on Real Estate Transactions (TDRET). These taxes, collected during property sales, are shared between municipalities, counties and the State. Municipalities below 5,000 inhabitants do not collect TDRET directly; instead, revenues are pooled at the county level, creating an equalization fund that stabilizes revenue. Municipalities above 5,000 inhabitants collect TDRET directly, gaining fiscal autonomy but losing the stabilizing benefits of pooling[11]. Crossing this population threshold therefore alters both revenue predictability and exposure to local real estate market fluctuations.

Moreover, municipal council size are tied to population cutoffs at 3,500, 5,000 and 10,000 inhabitants, while executive allowances are tied to the 1,000, 3,500 and 10,000 cutoffs. Larger populations imply larger councils and higher allowances for mayors and deputies. The electoral system changes at 1,000 inhabitants—from a majoritarian open-list to a proportional list system[12].

Drafting and voting of budgets are subject to regulations that vary with population thresholds (see Table A.1 in the online Appendix for additional details). Transparency requirements get more demanding as population increases, particularly at the 3,500 and 10,000 cutoffs, in accordance with the 2015 NOTRe Law[13].

3 Data and sample selection

This section describes our data sources, the construction of the analytical sample and the main descriptive features. Data on intergovernmental transfers are available online[14] for the 2014–2024 period. We observe both the annual amount of the USG received by each municipality and the corresponding municipal population measures—the legal population and population-grant—which determine eligibility and allocation. Additional information on el-

[11] Article 1595 bis of the French General Tax Code (CGI) (in force since June 23, 2018) establishes the system for collecting transfer duties (TDRET) based on the 5,000 cutoff.

[12] See the online Appendix section A.5 for further details.

[13] The *Nouvelle Organisation Territoriale de la République* Law, in force since August 7, 2015.

[14] Open data on local governments from the French government: data.gouv.fr

igibility criteria is available from 2018 onward. We complement these data with municipal budgetary records, also publicly accessible through official databases, covering the same period. The study period therefore spans eleven years (2014–2024).

Political variables are publicly available only for municipalities with more than 1,000 inhabitants. Specifically, we collect information on mayoral party affiliations[15], electoral competition (margin of victory, number of competing lists) and other election outcomes (allocation of seats in the municipal council) for municipalities governed under the proportional closed-list electoral system during the 2014 and 2020 municipal elections[16].

We build our sample as follows. We first select all urban municipalities that received a strictly positive USG amount in both 2016 and 2017—the year immediately preceding and the year of the reform—yielding a set of 875 municipalities. Municipalities are defined as treated if their USG amount decreased in 2017 relative to 2016 by at least 10%, and if the declining rate was applied during the following three years (the guarantee phase introduced in the 2017 reform): -17 % in 2018, -33 % in 2019, and -100 % in 2020, except for cases where the phase-out was smoothed over four years or more. Beginning in 2020, these treated municipalities receive no USG payments. A small subset regained eligibility in 2019 or later; for for these cases, the corresponding observations are marked as missing to avoid confounding effects. Consequently, treated municipalities, once treated, remain treated throughout the observation period[17].

All other municipalities serve as controls. They are identified by a non-decreasing USG amount between 2017 and 2018, implying no adverse effect from the 2017 reform. Municipalities that lost eligibility after 2018 for reasons unrelated to the reform are excluded for the relevant years to prevent noise. Consequently, control municipalities maintain consistent status during the observation period. Following this procedure, our final sample comprises 875 municipalities, including 97 treated cases (11% of the total sample).

Figure 1 compares treated and control group municipalities in terms of the evolution the USG amount over the period and the population-grant distribution. Figure 1a shows that,

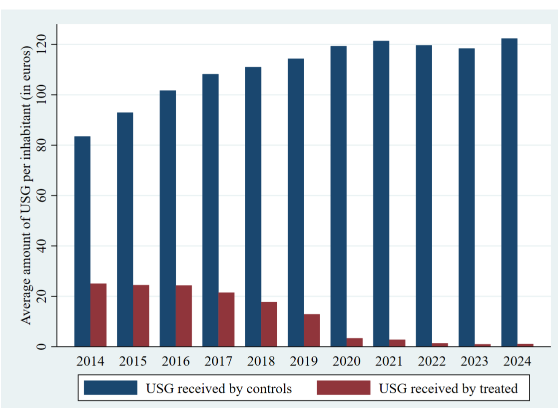
[15] Party affiliation classifications follow the French Ministry of the Interior. We focus on left-wing and right-wing mayors, excluding centrist, far-left, far-right, and minor parties due to limited representation.

[16] These political data are available on data.gouv.fr

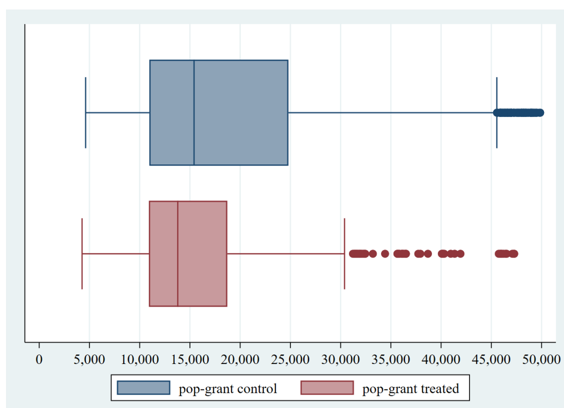
[17] This allows us to consider the treatment as staggered in the empirical strategy.

before the 2017 reform, those that kept the grant received on average 93€ per inhabitant, almost four times more than those that lost it (25€ per inhabitant). For the latter treated municipalities, starting in 2017—the reform year—the average grant drops sharply until 2020 and vanishes later on, indicating that they completely lost grant support.
Figure 1b shows that, for both the control and treated groups, the bottom 25% least populated municipalities range between 5,000 and 11,000 inhabitants. The treated group has a lower median population (13,800 inhabitants) and a significantly lower third quartile (18,700 inhabitants), showing a more homogeneous distribution compared to the control group. Thus, the treated group tends to consist of smaller municipalities than the control group.

Figure 2 presents descriptive statistics on budgetary items in our sample of 875 municipalities over the 2014-2024 period. Panels (a) and (b) indicate that the median for current and investment expenditures amounts to 1,360€ and 473€ per inhabitant respectively. On average, the personnel expenditure represents 54% of current expenditure while equipments count for 68% of investments. Panel (c) shows that the median for tax capacity (1,070€ per inhabitant) is almost twice higher than for tax revenues (631€ per inhabitant), suggesting most cities collect much less than their capacity. Finally, panel (d) shows that the outstanding debt has a median value (1,002€ per inhabitant) close to the median for tax capacity and a more dispersed distribution.

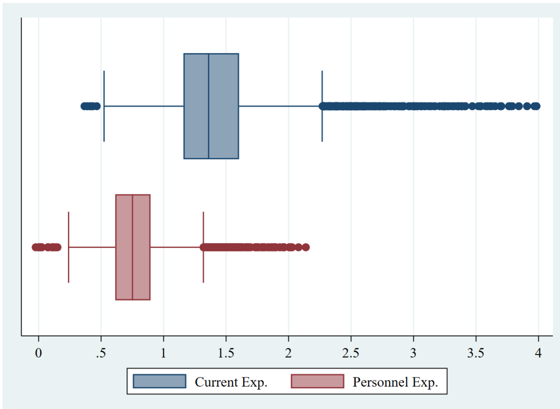


(a) Evolution of USG in 2024 € p.c.

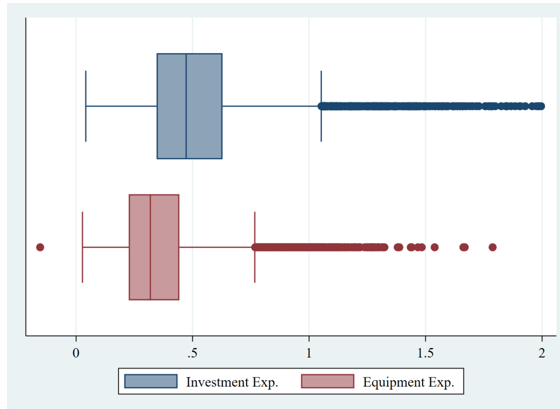


(b) Population-grant distribution

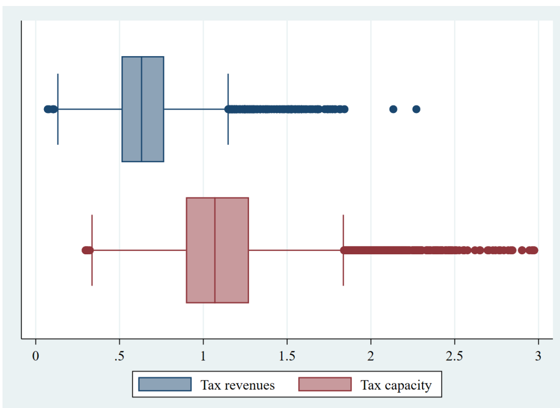
Figure 1: Comparison between treated and control municipalities



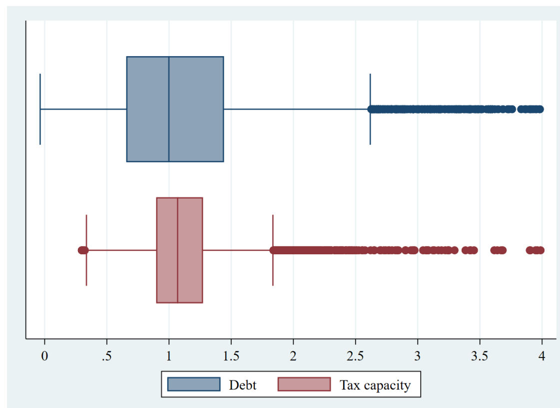
(a) Current–personnel spending



(b) Investment–equipment spending



(c) Tax revenues and tax capacity



(d) Debt and tax capacity

Figure 2: Distribution of budgetary items in thousand 2024 € per inhabitant. Whole sample over the period 2014-2024.

We now provide descriptive statistics on the political characteristics of our sample to refine the analysis by exploring the potential influence of political factors on budgetary decisions. First, we focus on the mayor's party affiliation. In the 2014 municipal election, among the 875 municipalities, 380 were led by left-wing (LW) mayors and 437 by right-wing (RW) mayors. In the 2020 municipal election, 351 were led by LW and 321 by RW mayors. The remaining municipalities were led by other parties, including the centrist party. Over the period, 41.9% of the municipalities are LW, while 43.9% are RW.

Figure B.1 in the online Appendix subsection B.1 displays boxplots that describe the distribution of budgetary items by political sub-sample. They suggest some insights into budget-related decisions of the urban municipalities of our sample that may reflect partisan preferences. RW municipalities tend to be more fiscally conservative, with lower current and personnel expenditures and may prioritize capital investment relatively more, possibly reflecting policy preferences that emphasize infrastructure, urban renewal, or long-term projects. Debt per inhabitant is higher in RW municipalities, consistent with higher investment levels. In comparison, LW municipalities consistently show larger current and personnel budgets. The median tax capacity in RW and LW municipalities is roughly the same, while RW municipalities seem to collect more than their LW counterparts. It is worth noting that LW and RW municipalities were impacted differently by the 2017 USG reform: 6% of LW municipalities lost the grant, compared to 16% of RW municipalities. Consequently, on average, LW municipalities receive much larger solidarity grants.

Additional information on electoral margins and fragmentation of the municipal council for both 2014 and 2020 municipal elections are given in the online Appendix subsections B.2 and B.3, as well as boxplots that describe the distribution of budgetary items in sub-samples by electoral margin and council fragmentation.

4 Empirical strategy

4.1 Manipulation Tests and Bunching

Before estimating the causal effects of the 2017 USG reform, we first investigate whether population-based eligibility criteria may have been manipulated by municipalities. As the allocation of the USG depends on the population-grant, assessing potential discontinuities around key cutoffs is essential.

From the 35,000 French municipalities, we select urban municipalities—those classified by INSEE as having the highest or intermediate population density (levels 1 and 2). This selection yields a panel of 3,588 municipalities over the 2014–2024 period for which we collect both population-grant and legal population.

We first perform density manipulation tests (McCrary (2008), Cattaneo et al. (2018), Cattaneo et al. (2020), Cattaneo et al. (2024)) around the 5,000 and 10,000 thresholds of population-grant which determine eligibility for the USG. Strong evidence of manipulation emerges at both cutoffs. At the 5,000 cutoff, we detect a drop in the population-grant distribution and find a jump at the 10,000 cutoff, as shown in Table 2 and the corresponding Figures 3 and 4. We represent the manipulation testing plots choosing a 3-order (resp. 2-order) polynomial for 5,000 (resp. 10,000), as suggested in Table 2. These contrasting patterns suggest that some municipalities just above 5,000 inhabitants may under-report their population, while those just below 10,000 may over-report to gain or maintain eligibility for the USG.

Table 2: Population manipulation test on the 5,000 and 10,000-inhabitant cutoffs.
Using population-grant over the period 2014-2024.

cutoff	5,000	5,000	5,000	10,000	10,000	10,000
pol. order	1	2	3	1	2	3
T	-0.996	0.068	-2.37**	1.401	3.772***	0.994
p-value	0.319	0.945	0.018	0.161	0.0002	0.320
Bandwidth	254.8; 286.4	854.9; 858.1	1758.9; 1759.0	546.2; 581.5	3292.3; 3478.3	2835.4; 2835.6
Effective obs.	698+675	2,471+2,136	5,767+4,243	731+774	5,488+3,074	4,530+2,743

Notes. RD Manipulation test using local polynomial density estimation. Population-grant of all urban municipalities (3,588) considered over the period 2014-2024. Triangular Kernel and Jackknife VCE method are used. Bandwidth c;d means that c (resp. d) is the bandwidth used to the left (resp. right) to the cutoff. Effective observations a+b means a (resp. b) observations below (resp. above) the cutoff. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

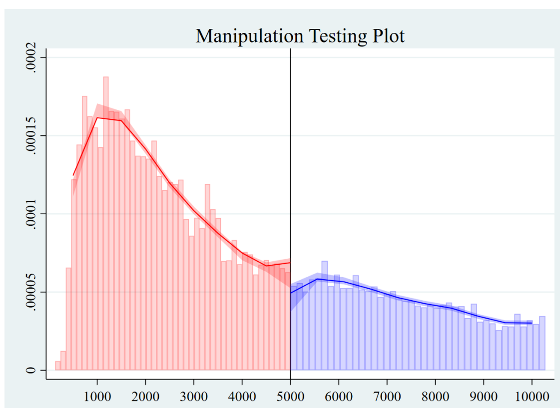


Figure 3: RD manipulation testing plot for the 5,000-inhabitant cutoff.

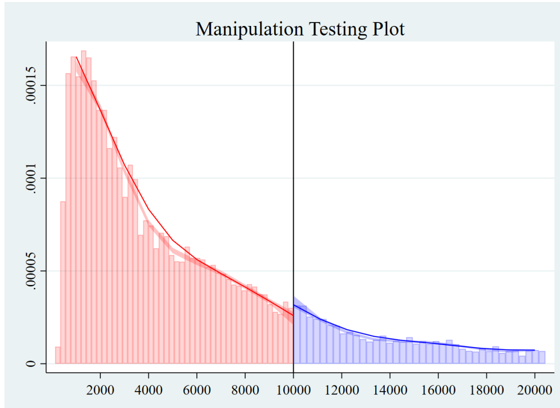


Figure 4: RD manipulation testing plot for the 10,000-inhabitant cutoff.

As explained in the institutional context, population cutoffs are also linked to other regulatory policies—such as municipal council size, executive compensation and budget transparency requirements (see Table 1)—which could confound these results. To account for this, we replicate the manipulation tests using legal population at the 1,000, 3,500, 5,000, and 10,000 cutoffs. Results presented in Table 3 indicate several facts.

Table 3: Additional population manipulation tests. Using legal population over 2014-2024.

cutoff	1,000	3,500	5,000	10,000
pol. order	2	2	3	2
T	-2.986**	-4.256***	-2.392*	3.403***
p-value	0.0028	0.0000	0.0167	0.0007
Bandwidth	400.5; 400.7	684.4; 685.9	1756.8; 1756.9	2952.1; 3092.9
Effective obs.	2,845+2,902	3,039+2,247	5,791+4,391	4,706+2,735

Notes. RD Manipulation test using local polynomial density estimation. Legal population of all urban municipalities (3,588) considered over the period 2014-2024. Triangular Kernel and Jackknife VCE method are used. Bandwidth c;d means that c (resp. d) is the bandwidth used to the left (resp. right) to the cutoff. Effective observations a+b means a (resp. b) observations below (resp. above) the cutoff. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

We observe a negative discontinuity at the 1,000 cutoff. When this threshold is crossed, local officials receive higher allowances and voting rules shift to proportional representation. The consequences of this voting rule change likely outweigh the benefit of larger allowances. At the 3,500-inhabitant threshold, a corresponding drop indicates a net negative effect, as transparency obligations impose costs despite potential positives from higher allowances and larger council sizes.

Using legal population figures confirms the drop at 5,000 and the jump at 10,000 inhabitants, indicating stronger incentives to cross the USG threshold near 10,000 than at 5,000. At the

5,000 threshold, the loss of TDRET pooling benefits likely outweighs gains from potential USG eligibility and larger council size. At both the 3,500 and 10,000 thresholds, the same three regulations apply, but only 10,000 is a USG eligibility cutoff; the observed drop at 3,500 versus the jump at 10,000 thus suggests that USG access drives the upward jump.

Using the official schedule of executive allowances, we calculate the monetary incentives associated with crossing the thresholds of 1,000, 3,500, and 10,000 inhabitants. These gains remained unchanged before and after 2017 (see Table A.2 in the online Appendix, subsection A.4) for the 10,000 threshold. Consequently, the positive discontinuity around 10,000 is unlikely to result from higher compensation but rather from strategic behavior aimed at obtaining the USG.

In order to clarify these results, we also run, year-by-year, population manipulation tests on the 5,000 and 10,000 cutoffs. The corresponding findings are presented in Table C.1 in the online Appendix. At the 5,000 cutoff, the test shows a negative and significant coefficient from 2017 up to 2024, with the lowest empirical p-values in 2019 and 2020. This reveals that the incentive to benefit from the TDRET becomes effective starting in 2017. At the 10,000 cutoff, an expected negative and highly significant coefficient appears in 2016, following the 2015 NOTRe Law that reinforced budgetary transparency requirements. In 2017 and 2018, the coefficient becomes positive and highly significant as a consequence of incentives to obtain or retain the USG. In the following years, it remains positive but is sometimes insignificant, probably because negative and positive effects offset each other.

We next apply bunching estimation methods (Kleven and Waseem (2013), Kleven (2016), Foremny et al. (2017)) that confirm our previous results on population manipulation. Bunching is quantified as the excess mass of municipalities around a cutoff relative to an estimated counterfactual density. We define the manipulation zone—where manipulation may occur—as the interval $[r_L; r_U]$. Municipalities are grouped into population bins (indexed by j) of 1% of the cutoff (i.e., 50 inhabitants per bin at 5,000 and 100 at 10,000). The counterfactual distribution is estimated by fitting a polynomial of order q to the empirical distribution of population bin counts (c_j), where dummies are included for the bins in the manipulation zone:

$$c_j = \sum_{h=0}^{q} \beta_h (r_j)^h + \sum_{i=r_L}^{r_U} \gamma_i \mathbf{1}[r_j = i] + \epsilon_j \tag{1}$$

The (estimated) counterfactual distribution is built with the polynomial coefficient estimates obtained in Equation 1, namely,

$$\hat{c}_j = \sum_{h=0}^{q} \hat{\beta}_h (r_j)^h \tag{2}$$

The difference between actual and counterfactual densities yields the estimated bunching mass. In theory, when there are no conflicting incentives to cross the threshold, r_L may be chosen either according to institutional aspects or visually and r_U must be calibrated such that excess mass equals missing mass on opposite sides of the threshold. In practice, it might not always be obvious how to choose the range limits.

Figure 5 shows a clear and expected under-reporting at the 5,000 threshold with a manipulation zone[18] identified as $[4,350; 5,400]$. Our calculation gives approximately 100 urban municipalities falling within the excess mass below 5,000, thus under-reporting their population, during the 2014–2024 period—around 9 municipalities per year. This result means that municipalities below the 5,000-inhabitant threshold do not cluster above 5,000 to gain the grant. As a consequence, it seems irrelevant to go further in our analysis since only municipalities with a population above 5,000 may be eligible for the grant.

[18]We choose both range limits visually. On opposite sides of the threshold, we choose r_L and r_U as the first time that the empirical density crosses markedly the counterfactual density. See the online Appendix section D for further details.

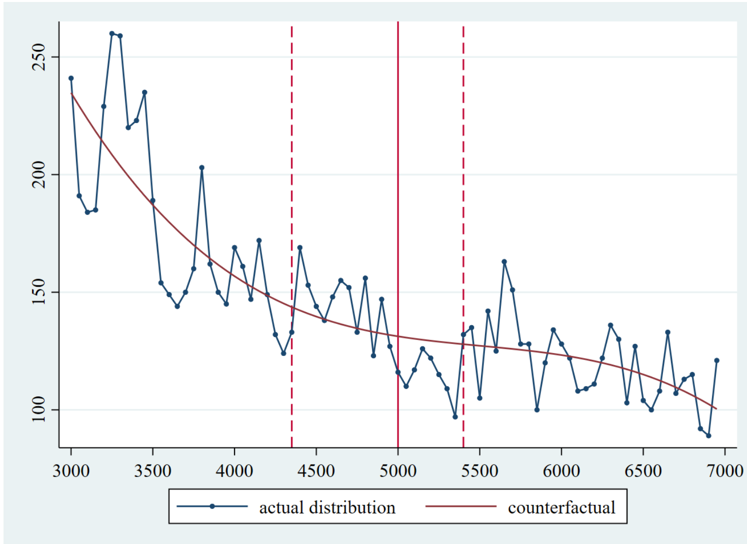


Figure 5: Bunching at the 5,000-inhabitant cutoff. Period 2014-2024.
Notes. The bin-size corresponds to 50 inhabitants-groups per bin at the 5,000 threshold. The counterfactual is estimated using 80 bins and a 3-order polynomial.

At the 10,000 threshold, Figure 6 reveals a more complex pattern, i.e. bunching on both sides. Below 10,000, one can observe a missing mass in the range $[8,850; 9,550]$ followed by an excess mass in $]9,550; 10,000[$. Above 10,000, a clear excess mass appears in $[10,000; 10,700]$.

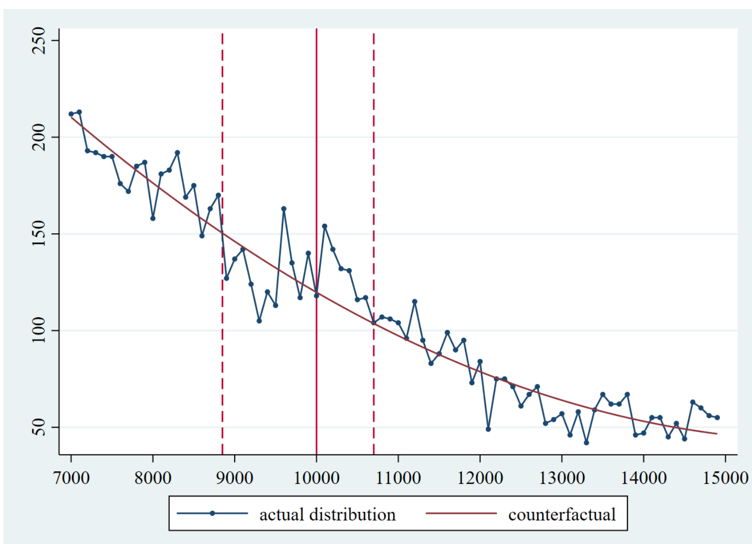


Figure 6: Bunching at the 10,000-inhabitant cutoff. Period 2014-2024.
Notes. The bin-size corresponds to 100 inhabitants-groups per bin at the 10,000 threshold. The counterfactual is estimated using 80 bins and a 2-order polynomial.

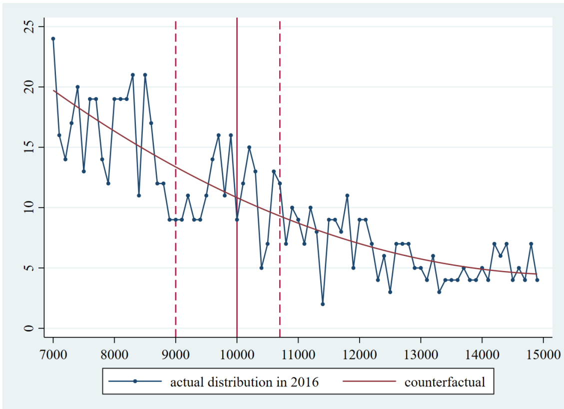
As explained earlier, this dual pattern might reflect heterogeneous incentives: crossing 10,000 inhabitants increases eligibility for the USG but also imposes heavier transparency and reporting obligations. These include detailed budget justifications and often public access to certain financial and administrative documents, not to mention increased political accountability before opponents within the municipal council. Municipalities weigh the benefits of USG eligibility against these accountability costs. For some, these administrative costs may outweigh financial gain. Thus, while some municipalities may have incentives to over-report, others may prefer to under-report to avoid additional scrutiny.

To further clarify our bunching results at the 10,000 cutoff, we conduct the same analysis year-by-year. We focus on 2016, 2017, and 2018—the years surrounding the reform—since the RD manipulation tests for these three years yield particularly striking results. For each year, we determine the manipulation zone as the range where the excess mass (above the counterfactual) equals the missing mass (below the counterfactual). Results are presented in Table 4 and Figure 7. They show that the bunching below the cutoff, clearly visible in 2016 (right after the 2015 NOTRe Law), almost disappears in 2018, when the bunching above the cutoff strongly dominates. We also identify the manipulation zone as the range [8,850; 10,700] and find that around 25 municipalities manipulate their population each year.

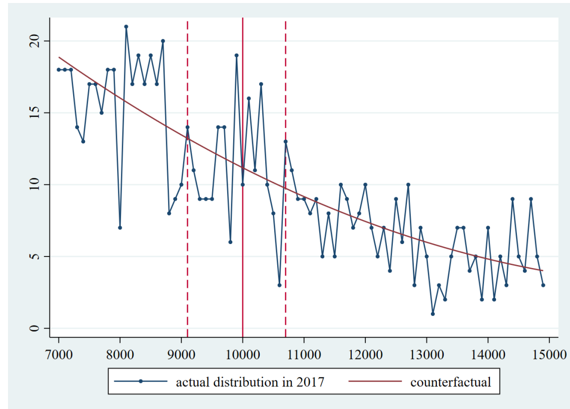
Table 4: Results on bunching at the 10,000 cutoff. Year-by-year.
Using population-grant.

Year	2016	2017	2018
Range $[r_L; r_U]$	$[9,000; 10,700]$	$[9,100; 10,700]$	$[8,800; 10,600]$
Excess Mass	26.66	27.50	23.37
Missing Mass	-27.99	-28.73	-23.58
R^2	0.825	0.801	0.702

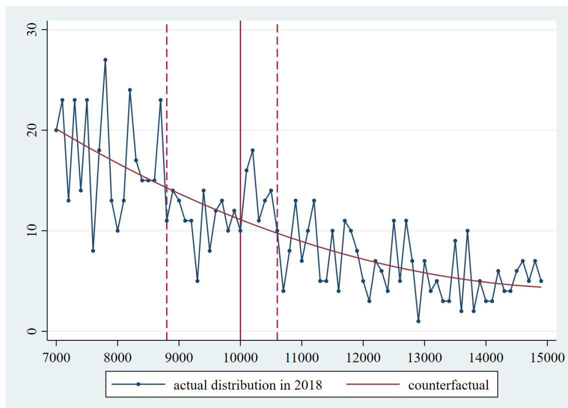
Notes. Population-grant of all urban municipalities (3,588) considered. We compute the counterfactual by using a 2-order polynomial and a bin-size=100. The manipulation zone is determined as the range such that the excess mass (above the counterfactual) is equal to the missing mass (below the counterfactual). The excess mass (resp. the missing mass) is computed as the sum of the difference between the actual distribution and the counterfactual when the difference is positive (resp. negative), over the corresponding range $[r_L; r_U]$.



(a) In 2016



(b) In 2017



(c) In 2018

Figure 7: Bunching at the 10,000-inhabitant cutoff. Year-by-year.

Moreover, to close the loop, we complete the analysis by quantifying the monetary incentive to cross the 10,000 threshold to increase the likelihood of being eligible for the grant. We consider municipalities just below and just above the 10,000-cutoff and compute the average expected grant amount on both sides of the cutoff, before and after the 2017 reform. Let $\bar{G}_{below}$ and $\bar{G}_{above}$ denote the average per capita grant for control municipalities located respectively in the ranges $[8,850; 10,000[$ and $[10,000; 10,700]$. These amounts, on each side of the cutoff, are weighted by the probability of being eligible for the grant: 10% of municipalities below 10,000 are eligible for the grant and 75% above 10,000 before 2017 (66.67% after 2017). Crossing the cutoff represents thus an expected gain equal to $E(gain) = \bar{G}_{above} * 0.75 - \bar{G}_{below} * 0.1$ before 2017, and $E(gain) = \bar{G}_{above} * 0.6667 - \bar{G}_{below} * 0.1$ after 2017.

The results presented in Table 5 show first that the monetary incentive to cross the 10,000-cutoff is substantial since the expected grant above the cutoff is around five times larger than the expected grant below. Second, it increases significantly both in absolute and relative terms after the 2017 reform.

Table 5: Quantifying the monetary incentive of crossing the 10,000-cutoff

	Before 2017	After 2017
$\bar{G}_{below}$	137.06	145.62
$\bar{G}_{above}$	83.41	124.30
$E(gain)$ in €	48.85	68.31
$E(gain)$ in %	356.4	469.1

Notes. Amounts expressed in 2024 € per inhabitant. $\bar{G}_{below}$ is the average per capita grant for control municipalities located in the range $[8,850; 10,000[$ and $\bar{G}_{above}$ the average per capita grant for control municipalities located in the range $[10,000; 10,700]$.

Finally, we provide robustness checks at both cutoffs, 5,000 and 10,000. First, we complement our analysis of bunching by choosing alternative bin sizes and polynomial orders. Results are presented in Tables D.1 and D.2 as well as Figures D.1 and D.2 in the online Appendix section D. We keep the same manipulation zones as those identified previously. We notice that, unlike the analysis year-by-year, pooling over 11 years doesn't always lead to get the excess and missing mass close to each other. Moreover, quantifying the excess (and missing) mass strongly depends on the choice of both the bin size and the polynomial order. This confirms that competing incentives around the thresholds lead to complex bunching patterns. Second, we run density manipulation tests on placebo cutoffs around 5,000 and 10,000, but outside their respective manipulation zone (see the online Appendix subsection E). They confirm that the manipulation zone around the 10,000 cutoff is very well identified.

4.2 Staggered Difference-in-Differences

We now present the identification strategy to estimate the causal effects of the 2017 USG reform on various budgetary outcomes. Since the treatment–loss of the grant–is likely to be endogenous, we adopt a Two-Way Fixed Effects (TWFE) model. This method controls for both time-invariant municipal characteristics and common time shocks, enabling causal inference under standard parallel trend assumptions.

We first consider the static TWFE linear regression model. It is specifically adapted to take count for the bunching zone observed above the 10,000 threshold. Since the bunching zone at the 5,000 threshold lies to the left, it is not relevant to consider as only municipalities with populations exceeding 5,000 are eligible for the grant. The corresponding model is specified as follows.

$$Y_{it} = \alpha_i + \nu_t + \beta D_{it} + \gamma D_{it} * BZ_{it} + \epsilon_{it} \tag{3}$$

where Y_{it} is the outcome of interest in municipality i at time t and D_{it} is a binary variable, the treatment of municipality i at time t. The binary variable equals to 1 if municipality i is treated at time t and 0 if not treated. BZ_{it} is a binary variable equal to 1 if municipality i belongs to the bunching zone to the right of the 10,000-inhabitant threshold at time t, and 0 otherwise. α_i and ν_t are respectively municipality and time fixed effects and ϵ_{it} denotes the error term. The coefficient of interest β captures the average difference in the outcome after the treatment between municipalities that are treated and those that are not. The coefficient of the interaction term γ measures the specific treatment effect for municipalities located within the bunching zone; the total effect for these municipalities is therefore $\beta + \gamma$ if the municipality lies in the bunching zone.

While the static model identifies the average post-treatment difference, it does not capture the dynamic evolution of effects. To assess this, we conduct a dynamic TWFE (or event-study) analysis that, in addition, allows to test the parallel trend assumption. Focusing on municipalities outside the manipulation zone around the 10,000 cutoff, the dynamic TWFE model can be written as follows:

$$Y_{it} = \alpha_i + \nu_t + \sum_{e=-K}^{-2} \gamma_e D_{it}^e + \sum_{e=0}^{L} \beta_e D_{it}^e + \upsilon_{it} \tag{4}$$

where D_{it}^e is an an indicator for municipality i being e periods away from initial treatment at time t, K and L are positive constants and υ_{it} denotes the error term. The last not treated period $e = -1$ is used as "base-period" and γ_{-1} is normalized at 0. In this dynamic specification, we focus on the post-treatment coefficients β_e that are typically interpreted as measuring the effect of participating in the treatment at different lengths of exposure to the treatment. The pre-treatment coefficients γ_e are used to check that the parallel trends assumption between control and treated groups holds.

To estimate Equation 4, we implement the estimator proposed by Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021) which accommodates multiple time periods and staggered treatment adoption. This framework yields robust inference and consistent estimation of average treatment effects in settings where once-treated municipalities remain treated thereafter.

In the following section, the event-study results are reported in tables that include aggregate summary measures of the pre-treatment and post-treatment coefficients. We highlight statistically significant effects, discuss their implications, and complement the interpretation with dynamic plots showing the evolution of treatment effects before and after the 2017 reform.

5 Municipalities' responses to the grant loss

5.1 Hypotheses on the grant reform effects

This section examines how municipalities adjust their budgetary decisions when they lose eligibility for the urban grant. We develop three sets of hypotheses.

First, the literature (see the introduction in section 1) on intergovernmental transfers suggests that municipalities may compensate for reduced grants by raising their own revenues. We therefore test the hypothesis that there is a crowding-out effect. We also test the hypothesis of a heightened fiscal discipline leading to reductions in spending or debt. Moreover, we investigate the existence of spending composition effects as some expenditure categories may be more sensitive to the reform than others.

Second, because eligibility depends sharply on the 10,000-resident cutoff, and since some municipalities appear to have manipulated their population figures to remain or become eligible, we investigate whether municipalities in the bunching zone immediately above the cutoff react differently from those outside. We do not impose ex ante predictions in this dimension.

Third, we consider how political characteristics shape responses. Partisanship may influence the type of adjustment undertaken by local executives. Electoral competitiveness, proxied by the margin of victory, may enhance accountability pressure and discipline fiscal choices. Finally, fragmentation in the municipal council may affect budgetary decisions by increasing coalition-building costs and encouraging compromise allocations across budget items.

5.2 A crowding-out effect for all except in the bunching zone

We first study the static responses by estimating Equation 3. It is adapted to identify a treatment effect common to all municipalities and an effect specific to those in the bunching zone (above the 10,000 cutoff). Consistent with our previous analysis evidencing bunching, we investigate whether municipalities within this zone have a specific behavior.

Table 6 reports descriptive statistics for 2017, the year of the reform, comparing municipalities located in the manipulation and bunching zones respectively to other municipalities. This indicates that, in 2017, municipalities in both the manipulation and bunching zones were twice more likely to be treated than others. Likewise, municipalities located just above the 10,000-resident threshold are disproportionately represented among treated municipalities, confirming significant over-reporting to qualify for the grant.

Table 6: Municipalities in manipulation and bunching zones in 2017

	Manipulation Zone $[8,850; 10,700]$	Bunching Zone $[10,000; 10,700]$
share in total sample	7.43% (65/875)	5.49% (48/875)
share of treated in the corresponding zone	21.54% (14/65)	22.92% (11/48)
share in the overall treated	14.43% (14/97)	11.34% (11/97)

Notes. In the total sample, 97 municipalities out of 875 are treated in 2017 which represents a share of 11%.

Results are presented in Table 7. We find a strong and statistically significant increase in tax revenues following the loss of the grant. This indicates that municipalities substitute lost transfers with higher own-source revenues. By contrast, total expenditures do not significantly decline. The dominant adjustment therefore occurs on the revenue side rather than the spending side.

Municipalities in the bunching zone exhibit a distinct pattern. They do not increase their tax revenues after losing eligibility. Instead, they reduce their current and personnel expenditures, consistent with stronger fiscal discipline. These findings suggest that municipalities suspected of misreporting population figures adopt a different adjustment strategy.

Table 7: Effects of the 2017 reform: losing eligibility for USG.

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
LOSS	0.016	0.006	0.043	0.032^{+}	0.024*	-0.024
	(0.021)	(0.006)	(0.028)	(0.019)	(0.010)	(0.042)
LOSS*BZ	-0.040^{+}	-0.016*	0.045	0.039	-0.023^{+}	0.040
	(0.021)	(0.007)	(0.049)	(0.051)	(0.012)	(0.034)
N	9,289	9,289	9,289	9,289	9,289	9,289
Within-R2	0.087	0.132	0.011	0.040	0.058	0.193

Notes. Panel of 875 urban municipalities over the period 2014-2024. Dependent variables are budgetary items expressed in thousand 2024 € per inhabitant. LOSS is the treatment variable. It is equal to 0 if the municipality is in the control group. It is equal to 1 if the municipality is in the treated group for years from 2017 to 2024, and 0 for years 2014 to 2016. BZ is a dummy variable equal to 1 if the municipality is in the bunching zone, i.e. its population-grant lies in the interval [10,000; 10,700], and 0 otherwise. Static TWFE model estimated by Within method. Municipal fixed effects and annual dummies (2017 is the reference year) are used. Standard errors in parentheses are clustered at the municipal level. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Additional descriptive statistics highlight distinct features of municipalities within the bunching zone. Figure 8 shows that the frequency gap between municipalities just above and just below the 10,000 threshold (within the manipulation zone) peaks in 2016, immediately before the 2017 reform, and declines after 2020, when the reform became fully effective. This pattern suggests that over-reporting was more common before the reform and that incentives to manipulate diminished over time thereafter.

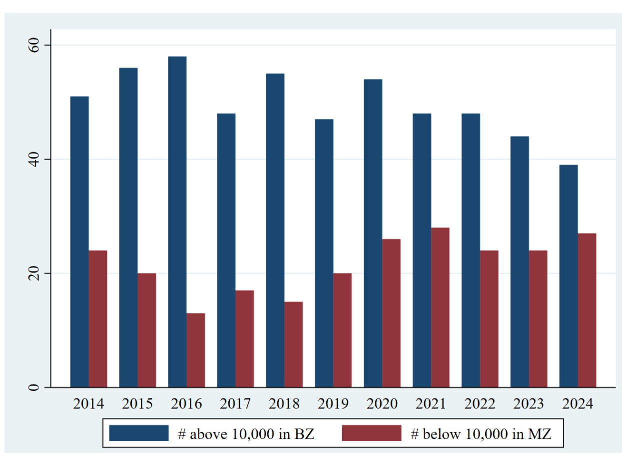


Figure 8: Evolution of the number of municipalities just below and just above 10,000.

Table 8: Descriptive statistics. Municipalities just above and just below 10,000 over the period 2014-2024.

	Mun. above 10,000 in $[10,000; 10,700]$	Mun. below 10,000 in $[8,850; 10,000[$	Post-treatment coeff. in the BZ
Current exp.	1.286	1.413	0.016-0.040^{+}
Personnel exp.	0.687	0.768	0.006-0.016*
Investment exp.	0.499	0.476	0.043+0.045
Equipment exp.	0.339	0.307	0.032^{+}+0.039
Tax Revenues	0.584	0.548	0.024*-0.023^{+}
Debt	1.003	1.210	-0.024+0.040
Tax capacity	1.042	0.972	
USG amount	96.37	135.16	
Treated (in %)	16.7	7.73	
Left-wing (in %)	45.49	59.67	
Right-wing (in %)	37.24	25.41	
Narrow margin (in %)	27.06	28.73	
Wide margin (in %)	31.09	29.28	
Small ENP (in %)	24.95	26.52	
Large ENP (in %)	21.30	30.94	
PDA-population	2432.6	2562.3	
N	518	181	

Notes. The budgetary items are average amounts in thousand 2024 € per inhabitant (except the USG amount expressed in 2024 € per inhabitant) over the period 2014-2024. Post-treatment coefficients in the BZ come from Table 7. Narrow vs wide margin refers to the electoral margin of victory in the municipal elections. Small (resp. large) ENP corresponds to a weakly (resp. highly) fragmented municipal council.

Table 8 first indicates significant differences in current and personnel expenditures between municipalities just above and just below the 10,000 threshold, while differences in other budget items remain minor. It also shows that municipalities within the bunching zone are more likely to be treated and, on average, receive smaller USG transfers. Moreover, LW municipalities are overrepresented outside the bunching zone, whereas RW municipalities are underrepresented. Similarly, large ENP municipalities (those with more fragmented councils) are overrepresented outside and underrepresented inside the bunching zone. These patterns suggest that the excess mass to the right of the 10,000 cutoff may partly reflect the behavior of RW municipalities and those with less fragmented councils.

We now investigate dynamic effects and adopt an event-study approach that allows to test for the absence of pre-trends. We estimate Equation 4 using the procedure proposed by Callaway and Sant'Anna (2021). We exclude 65 municipalities with populations within the manipulation zone $[8,850; 10,700]$ to avoid blurring the results[19].

Results presented in Table 9 show that post-treatment effects are consistent with those obtained without dynamics, confirming the strong and statistically significant increase in tax revenues following the loss of the grant. Moreover, the null hypothesis of parallel trends is not rejected for each budgetary item, except for personnel expenditure. The dynamic treatment effects, represented in Figure 9, reveal that tax revenues rise modestly in the years immediately following the reform and more sharply from 2021 onward, when the grant disappears[20] completely for treated municipalities. On average, the complete removal of the USG (a 100% decrease over four years) leads to an annual increase in tax revenues by 35€ per inhabitant equivalent to 140€ over four years representing 19% of pre-treatment levels, thus yielding an elasticity of -0.19. These results reinforce the existence of a substitution mechanism consistent with the crowding-out hypothesis, while providing little evidence of systematic expenditure reductions, contrary to what the fiscal discipline hypothesis might predict.

Table 9: Dynamic effects of the 2017 reform: losing eligibility for USG. Whole sample.

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
Pre-avg	-0.009	-0.009^{+}	-0.024	-0.019	0.010	-0.011
	(0.038)	(0.005)	(0.059)	(0.021)	(0.012)	(0.026)
Post-avg	0.010	-9.98e-07	0.025	0.020	0.035***	-0.014
	(0.042)	(0.007)	(0.064)	(0.024)	(0.007)	(0.043)
N	8,399	8,399	8,399	8,399	8,399	8,399

Notes. Panel of 810 municipalities outside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Dependent variables are budgetary items expressed in thousand 2024 € per inhabitant. Event study using the Callaway and Sant'Anna (2021) procedure. Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

[19] We present event-study results on the sub-sample of 65 municipalities that belong to the manipulation zone in the online Appendix section F. The presence of pre-trends–for personnel expenditure and tax revenues–raises concerns about a causal interpretation.

[20] This is consistent with Figure 1a, which shows that the USG amount for treated municipalities gradually decreases from 2017 to 2020 and disappears from 2021 to 2024.

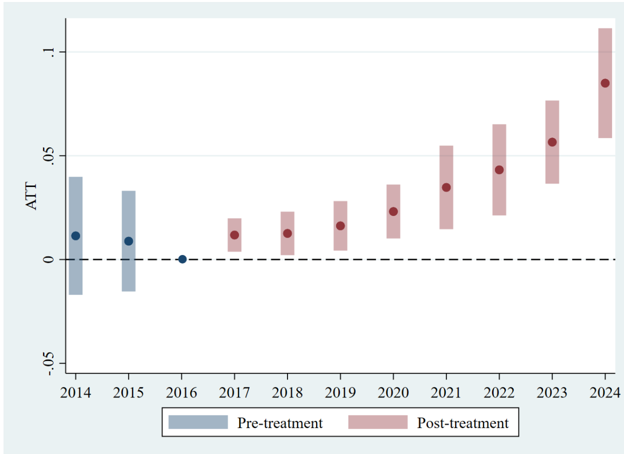


Figure 9: Dynamic effects of losing eligibility for USG on Tax Revenues.
Whole sample excluding the manipulation zone around 10,000 inhabitants.

5.3 Responses are shaped by political factors

Partisanship significantly shapes how municipalities respond to the loss of the grant as shown in Table 10. Left-wing (LW) municipalities increase their personnel and investment expenditures while also raising their tax revenues and expanding debt. These adjustments indicate an active and expansive fiscal response. Right-wing (RW) municipalities follow a different trajectory: they increase tax revenues but simultaneously reduce their debt, without changing expenditure levels. These contrasts confirm that ideological orientations are associated with distinct fiscal strategies.

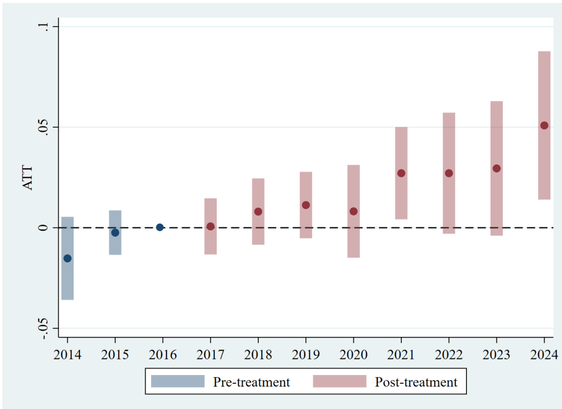
Table 10: Dynamic effects of the 2017 reform. Losing eligibility for USG.
Sub-samples by mayor's party.

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
Left-Wing						
Pre-avg	0.088**	-0.009	0.055	-0.025	0.019	0.041
	(0.028)	(0.007)	(0.059)	(0.052)	(0.024)	(0.039)
Post-avg	0.067+	0.020*	0.113+	0.020	0.030+	0.127+
	(0.038)	(0.009)	(0.062)	(0.060)	(0.016)	(0.072)
N	3,302	3,302	3,302	3,302	3,302	3,302
Right-Wing						
Pre-avg	-0.060	-0.016*	-0.057	-0.013	0.007	-0.033
	(0.049)	(0.006)	(0.076)	(0.023)	(0.015)	(0.033)
Post-avg	-0.033	-0.005	-0.018	0.020	0.040***	-0.119+
	(0.065)	(0.010)	(0.096)	(0.029)	(0.011)	(0.068)
N	3,604	3,604	3,604	3,604	3,604	3,604

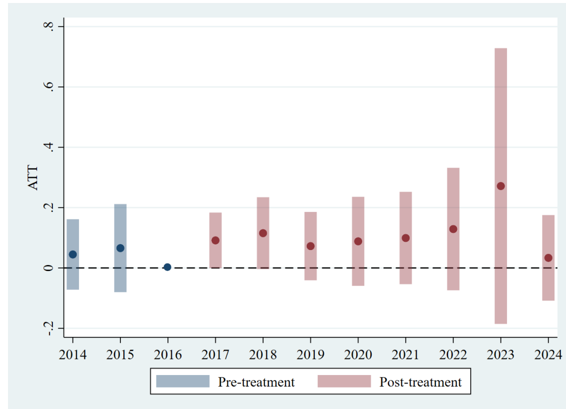
Notes. Panel of 810 municipalities excluding the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Dependent variables are budgetary items expressed in thousand 2024 € per inhabitant. Event study using the Callaway and Sant'Anna (2021) procedure. Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure 10 shows that LW municipalities increase their personnel expenditures starting from 2021 and increase slightly their tax revenues in 2018, then again more sharply in 2023 and 2024[21], as well as their debt starting from 2022. On average, the 100% decrease in USG (over four years) leads to an annual increase of 20€ in personnel expenditure (which represents 2.36% of personnel expenditure per inhabitant before the treatment), an annual increase of 113€ (23.41%) in investment expenditure, an increase of 30€ (4.25%) in tax revenues and an increase in debt of 127€ (11.89%). For LW municipalities, the corresponding elasticities to USG are respectively -0.0944 (personnel expenditure), -0.9364 (investment expenditure), -0.17 (tax revenues) and -0.4756 (debt).

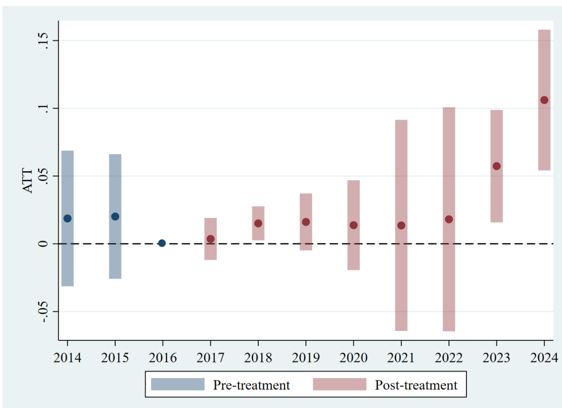
[21]This could be related to the end of the COVID safety net measures adopted by the State to help municipalities facing the sanitary crisis.



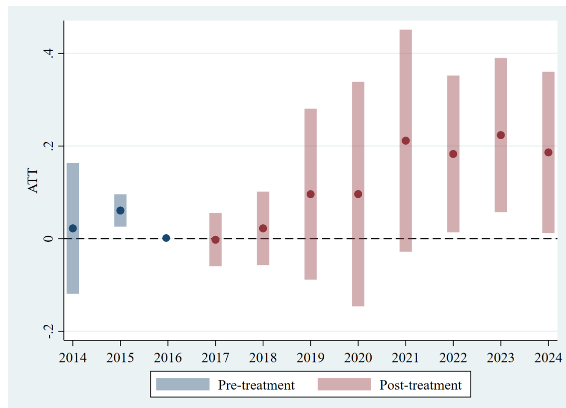
(a) Personnel expenditure



(b) Investment expenditure



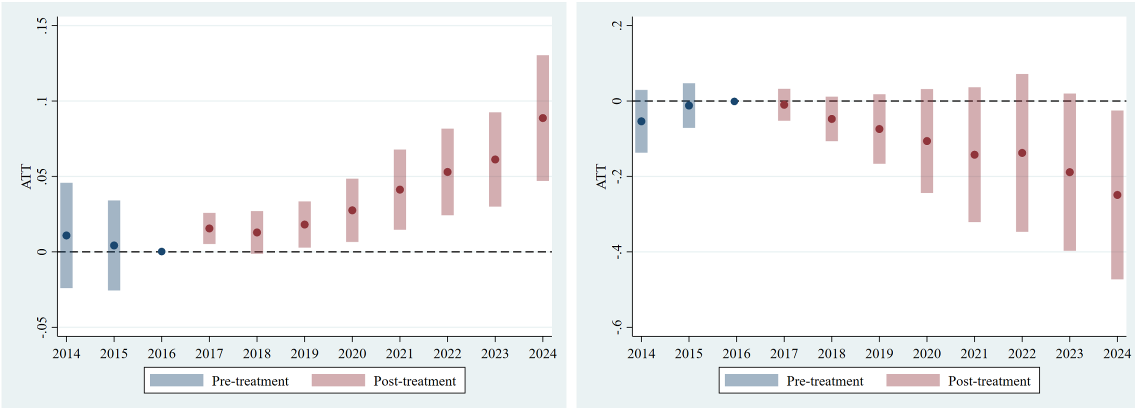
(c) Tax Revenues



(d) Debt

Figure 10: Dynamic effects of losing eligibility for USG. Left-Wing municipalities.

Figure 11 shows that RW municipalities exhibit an increase in tax revenues and a negative impact on debt as early as 2017. They display a tax revenues pattern similar to that of the whole sample. On average, the 100% decrease in USG leads to an annual increase of 40€ (5.25%) in tax revenues and an annual decrease of 119€ (8.55%) in debt. The corresponding elasticities to USG are respectively equal to -0.21 (tax revenues) and +0.342 (debt).



(a) Tax Revenues

(b) Debt

Figure 11: Dynamic effects of losing eligibility for USG. Right-Wing municipalities.

Electoral competitiveness[22] also affects municipal reactions. Results are displayed in Table 11 and illustrated in the following Figures. Figure 12 shows that when the mayor is elected with a narrow margin (less than 10 percentage points), municipalities drastically reduce their current and investment expenditures beginning in the fourth year after the reform and lower their debt starting in the third year. These adjustments indicate strong fiscal discipline under heightened accountability pressure. Tax revenues do not rise in these municipalities, suggesting that the substitution mechanism is muted when politicians face greater electoral scrutiny. Personnel expenditures exhibit a temporary decline during the reform year followed by a marked increase later on.

Conversely, municipalities where the mayor won by a wide margin (more than 30 percentage points) show an immediate and substantial rise in tax revenues, in line with the crowding-out hypothesis, but no reduction in expenditures, as shown in Figure 13. Together, these results provide strong support for electoral accountability[23].

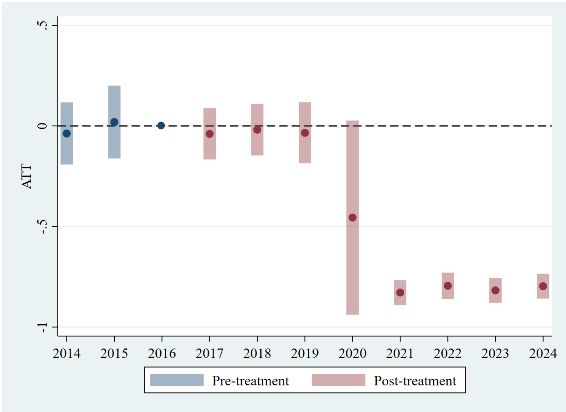
[22]The distribution of the electoral margin in our sample is given in Figure B.2 in the online Appendix subsection B.2.

[23]The elasticities to USG when the electoral margin is narrow are respectively +1.332 (current expenditure); -0.168 (personnel); +5.416 (investment); +0.84 (equipment) and +0.672 (debt). When the mayor is elected by a wide margin, the corresponding elasticity of tax revenues to USG is -0.304.

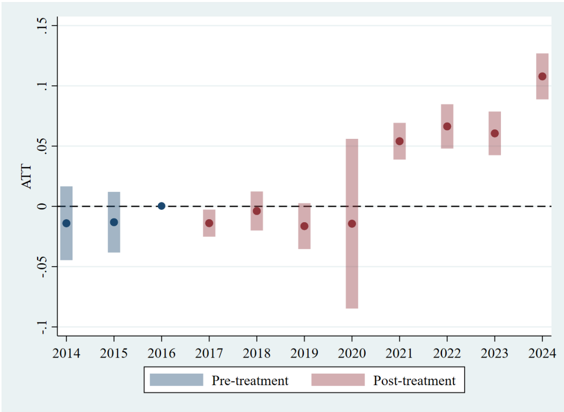
Table 11: Dynamic effects of the 2017 reform: losing eligibility for USG.
Sub-samples by electoral margin.

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
Narrow margin						
Pre-avg	-0.009	-0.013	0.028	0.001	0.026	-0.061
	(0.081)	(0.013)	(0.118)	(0.056)	(0.029)	(0.055)
Post-avg	-0.473***	0.030***	-0.830***	-0.073**	0.004	-0.220***
	(0.054)	(0.008)	(0.092)	(0.023)	(0.006)	(0.038)
N	1,746	1,746	1,746	1,746	1,746	1,746
Wide margin						
Pre-avg	-0.018	-0.006	-0.099	-0.058*	-0.002	-0.0005
	(0.082)	(0.007)	(0.126)	(0.029)	(0.019)	(0.049)
Post-avg	0.033	0.001	0.096	0.028	0.056***	-0.047
	(0.044)	(0.011)	(0.069)	(0.050)	(0.013)	(0.049)
N	2,050	2,050	2,050	2,050	2,050	2,050

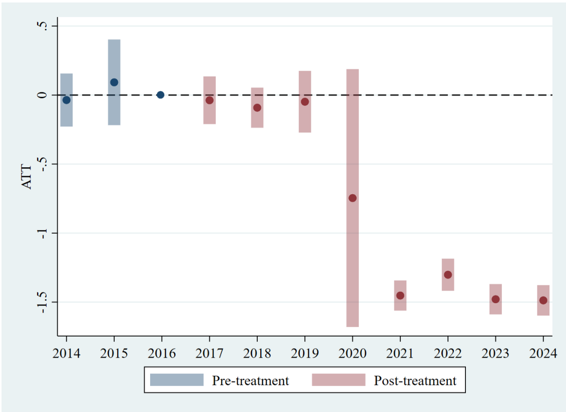
Notes. Panel of 810 municipalities outside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Dependent variables are budgetary items expressed in thousand 2024 € per inhabitant. Event study using the Callaway and Sant'Anna (2021) procedure. A narrow (resp. wide) electoral margin corresponds to a margin of less than 10 percentage points (resp. more than 30 points). Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.



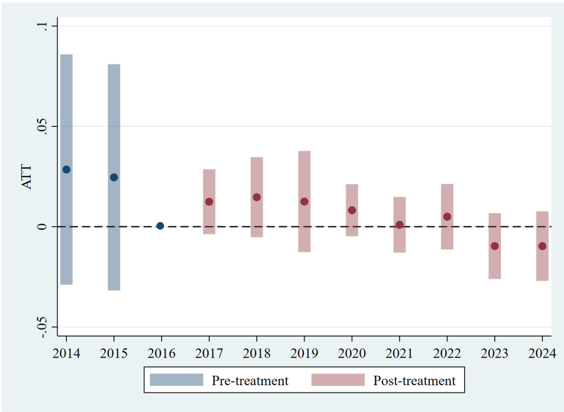
(a) Current expenditure



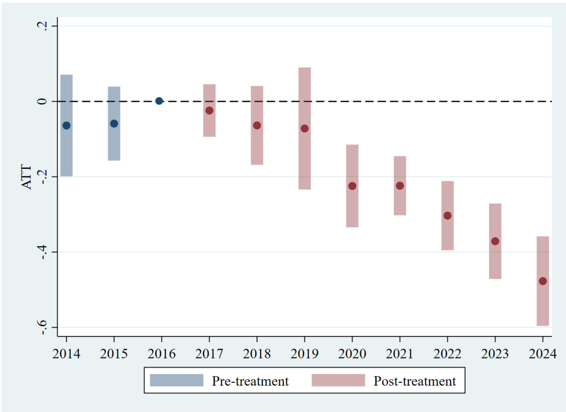
(b) Personnel expenditure



(c) Investment expenditure

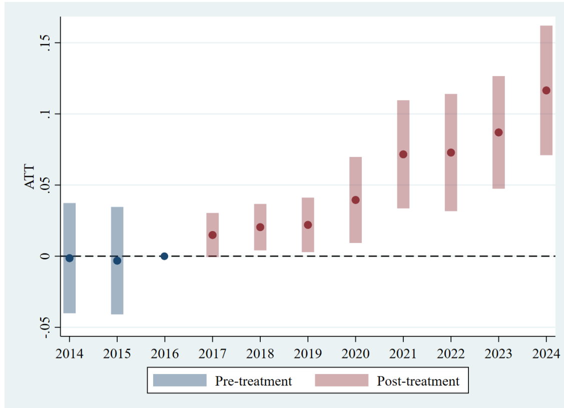


(d) Tax revenues



(e) Debt

Figure 12: Dynamic effects of losing eligibility for USG. Narrow electoral margin.



(a) Tax Revenues

Figure 13: Dynamic effects of losing eligibility for USG. Wide electoral margin.

Finally, fragmentation in the municipal council also affects the outcomes. Following the literature on the impact of fragmentation (Laakso and Taagepera (1979)), we consider the number of effective parties (ENP). The ENP is calculated from seat shares as a weighted measure[24] that adjusts for party size, thereby reflecting the influence and strength of parties in the municipal council. The higher the ENP, the more fragmented is the council[25]. It is a more nuanced and widely-used proxy for fragmentation than raw list count[26]. Moreover, following the electoral rules that determine the allocation of seats between the competing lists[27], if the election is won with a narrow margin, the council has a higher chance of being fragmented than with a wide margin.

We select both municipalities with a small ENP (less than the first quartile) and those with a large ENP (larger than the third quartile) and present our results in Table 12. In highly fragmented councils, personnel expenditures rise while equipment expenditures fall, and tax revenues increase only modestly, as shown in Figure 15. This reflects the complexity of assembling governing coalitions and the need to accommodate diverse preferences within the council. In less fragmented councils, Figure 14 shows that the response resembles that of

[24]The ENP is calculated as the inverse of the sum of squared seat shares.

[25]The distribution of the ENP in our sample in given in Figure B.4 in the online Appendix subsection B.3.

[26]We also considered the impact of electoral competition, using the number of competing lists (according to the round at which the mayor has been elected). Our sample includes municipalities with a single list (3.5%), 2 lists (29%), 3 lists (41%) and 4 and more (21.5%). The event study conducted on two sub-samples, one with 2 lists and the other with 4 lists or more, do not show any contrasted effects between them. See Table G.1 in the online Appendix subsection G.

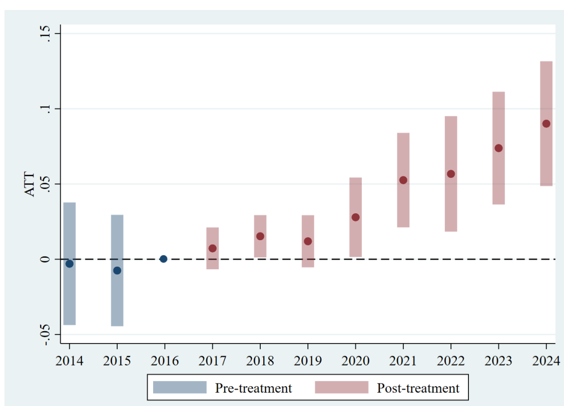
[27]See the online Appendix, subsection A for details.

the overall sample, with stronger increases in tax revenues and more limited expenditure adjustments. These patterns support the fragmentation hypothesis[28].

Table 12: Dynamic effects of the 2017 reform: losing eligibility for USG. Sub-samples by ENP.

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
Small ENP						
Pre-avg	0.067	-0.010	0.018	-0.058	-0.005	0.050
	(0.045)	(0.009)	(0.061)	(0.039)	(0.019)	(0.037)
Post-avg	0.063	0.002	0.128^{+}	0.042	0.042***	-0.076
	(0.043)	(0.009)	(0.076)	(0.071)	(0.010)	(0.055)
N	1,373	1,373	1,373	1,373	1,373	1,373
Large ENP						
Pre-avg	-0.026	-0.009	-0.044	-0.014	-0.006	-0.030
	(0.064)	(0.012)	(0.095)	(0.047)	(0.024)	(0.052)
Post-avg	-0.125	0.039*	-0.328	-0.063*	0.026*	-0.030
	(0.169)	(0.017)	(0.244)	(0.031)	(0.013)	(0.081)
N	1,641	1,641	1,641	1,641	1,641	1,641

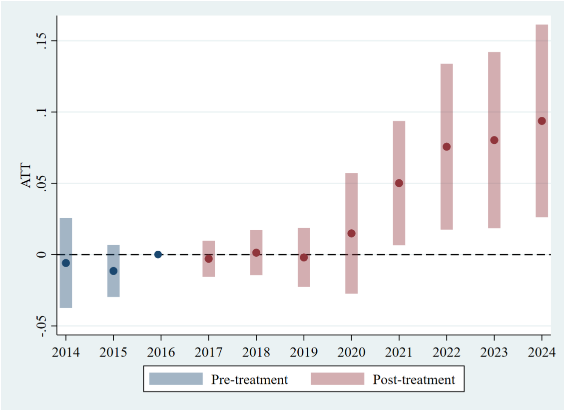
Notes. Panel of 810 municipalities outside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Dependent variables are budgetary items expressed in thousand 2024 € per inhabitant. Event study using the Callaway and Sant'Anna (2021) procedure. A small (resp. large) ENP corresponds to an ENP of less than 1.42 (resp. more than 1.62). Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.



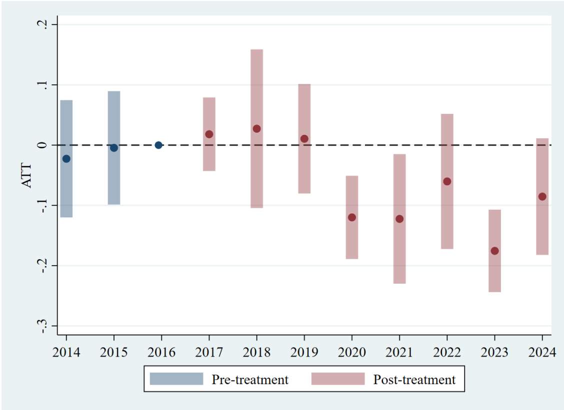
(a) Tax revenues

Figure 14: Dynamic effects of losing eligibility for USG. Small ENP.

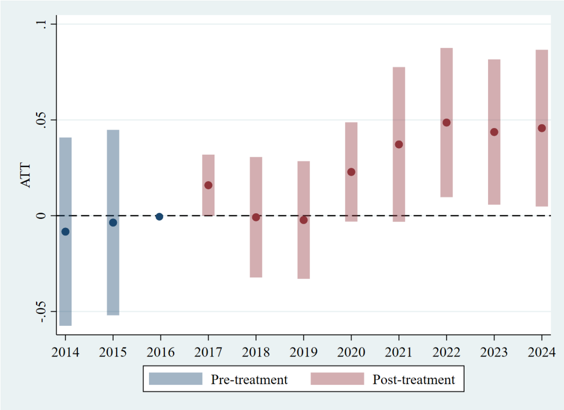
[28]In the online Appendix subsection H, we run an event-study analysis by combining a narrow electoral margin and a very fragmented council. The corresponding results are even more accurate.



(a) Personnel expenditure



(b) Equipment expenditure



(c) Tax Revenues

Figure 15: Dynamic effects of losing eligibility for USG. Large ENP.

5.4 Mechanisms

Our empirical findings can be interpreted through several complementary mechanisms. First, the systematic increase in tax revenues among most municipalities reveals a clear substitution mechanism. When external funding contracts, municipalities compensate by mobilizing their own fiscal resources. This pattern aligns with well-documented crowding-out effects in the literature and provides a direct explanation for the strong, positive revenue response among treated municipalities.

Second, political incentives shape the direction and intensity of adjustments. Left-wing executives prioritize personnel and investment expenditures and are willing to increase debt to sustain these commitments, whereas right-wing executives tend to consolidate debt and refrain from expanding expenditures. Moreover, electoral competitiveness generates an additional channel: mayors narrowly elected exhibit fiscal discipline, particularly by reducing expenditures and debt, as a result of electoral accountability.

Finally, political fragmentation introduces coalition-driven bargaining mechanisms. Highly fragmented councils exhibit higher transaction costs and reaching an agreement may require forming large coalitions. This might generate budget re-allocations that increase personnel expenditures while compressing equipment expenditures. These adjustments reflect compromise-oriented budgeting rather than programmatic fiscal strategy.

To go further, we propose first to investigate to what extent combining the two forces (electoral accountability and distribution of relative power within the council) may influence budgetary decisions. In Table 13, we show that personnel expenditures and debt are sensitive to both the margin of victory and the ENP. For personnel expenditures, these two indicators are at play in the same direction: when the margin is tighter and the council more fragmented, they tend to increase. In the case of debt, a tighter margin tends to discipline (decrease) while the fragmentation imposes more pragmatism and puts a pressure towards more debt.

This combined mechanism may explain the increase in personnel expenditures we observe in competitive municipalities (with a narrow margin and/or a large ENP) after losing the USG, in contrast with the disciplining effect of electoral competition. It may reflect a signaling strategy toward voters, showing administrative capacity or social commitment even under constrained resources.

Table 13: To what extent the electoral margin and the ENP influence the budgetary decisions?

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
Margin	-0.019	-0.018*	0.054	0.056*	0.008	0.093**
	(0.024)	(0.008)	(0.038)	(0.026)	(0.010)	(0.034)
Margin*SmallENP	0.034+	0.004	0.010	-0.006	0.0003	0.004
	(0.020)	(0.006)	(0.031)	(0.022)	(0.008)	(0.028)
Margin*LargeENP	-0.064	-0.028*	-0.155*	-0.070	-0.031+	-0.167**
	(0.039)	(0.013)	(0.063)	(0.044)	(0.016)	(0.057)
N	8,584	8,584	8,584	8,584	8,584	8,584
Within-R2	0.091	0.139	0.014	0.041	0.058	0.199

Notes. Dependent variables: budgetary items expressed in thousand 2024 € per inhabitant. Panel of 810 municipalities outside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Electoral margin is between 0 and 100%. A small (resp. large) ENP corresponds to an ENP of less than 1.42 (resp. more than 1.62). Linear regression model estimated by Within method. Municipal fixed effects and annual dummies (2017 is the reference year) are used. Standard errors in parentheses are clustered at the municipal level. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

A second investigation in an attempt to explain the significant increase in personnel expenditure after losing the grant in LW municipalities, involves the role of poor district areas (PDA). We leverage information on municipalities hosting PDA to assess whether they had incentives to expand staff as a means to influence future grant allocations, since the equalization formula accounts for the presence of these districts. This could be a channel to send a signaling to central government: municipalities may attempt to demonstrate their social needs and capacity constraints to justify regaining the grant.

Population data for these neighborhoods, available from 2018 onward, serve to document their distribution across treated and control municipalities (Figure A.1a in the online Appendix subsection A.2) as well as by political affiliation (Figure A.1b). We conduct an event-study on personnel expenditure distinguishing municipalities with and without PDA. Table 14 shows that, both in the whole sample and the subsample of RW municipalities, with and without PDA, the grant loss has no significant effect on personnel expenditure. In contrast, LW municipalities with PDA increase their personnel expenditures in response to the grant loss.

Additionally, to test whether this mechanism extends to other spending categories, we also examine investment and equipment expenditures that could serve similar signaling purposes. Table I.1 in the online Appendix section I show no significant effect on the whole sample, neither on LW and RW municipalities, suggesting that the response is specific to personnel expenditures.

Table 14: Investigating the role of poor district areas (PDA).

	Personnel expenditure with PDA	Personnel expenditure without PDA
Whole sample		
Pre-avg	-0.001	-0.007
	(0.009)	(0.006)
Post-avg	-0.005	-0.002
	(0.010)	(0.009)
N	6,112	2,287
Left-Wing		
Pre-avg	-0.019	-0.003
	(0.019)	(0.007)
Post-avg	0.027^{**}	0.020
	(0.010)	(0.013)
N	2,521	781
Right-Wing		
Pre-avg	-0.004	-0.009
	(0.009)	(0.009)
Post-avg	-0.008	-0.007
	(0.012)	(0.014)
N	2,544	1,060

Notes. Dependent variables: personnel expenditure expressed in thousand 2024 € per inhabitant. Panel of 810 municipalities outside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Event study using the Callaway and Sant'Anna (2021) procedure. Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

6 Conclusion

While the French equalization system does provide some redistribution between municipalities, we have studied its incentive effects, specifically examining how municipalities respond to changes in the urban solidarity grant resulting from shifts in their eligibility status.

First, we find evidence of municipalities bunching just above the 10,000-inhabitant threshold that governs eligibility for the grant, involving around 25 urban municipalities per year. This sorting behavior is consistent with our computation of the monetary incentive to cross the 10,000 cutoff: the expected grant above is roughly five times larger than the expected grant below.

Second, our results highlight several distinct mechanisms that drive municipal responses to grant cuts. Most municipalities compensate for grant losses by increasing their own tax revenues, which points to a clear substitution effect in line with established crowding-out patterns in the literature. Political incentives also play a central role: LW municipalities prioritize personnel and investment spending and are willing to incur additional debt to sustain these priorities, while RW municipalities tend to consolidate debt and restrain expenditures. Tighter electoral margins systematically promote fiscal discipline, though increases in personnel expenditures suggest signaling motives aimed at demonstrating commitment to voters. Moreover, political fragmentation amplifies bargaining dynamics within councils. Greater fragmentation raises transaction costs and necessitates broader coalitions, often leading to higher personnel expenditures and reduced equipment budgets. Together, these findings demonstrate that municipal fiscal adjustments to grant shocks are complex, shaped jointly by institutional rules, political strategies, and local socio-economic context. Finally, while these findings are drawn from the French context, we believe they are relevant to other countries that employ similar systems. In many nations, intergovernmental grants are based on formulas and eligibility criteria. The structure of the equalization system, which plays a critical role in France, may have similarly significant effects elsewhere.

Funding. No funds, grants, or other support was received.

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7 Online Appendix for When the Money Stops: Fiscal and Political Reactions to Changing Grant Eligibility

Contents

A Institutional details

A.1 USG: Eligibility and amount determination

To determine eligibility for USG, municipalities are assigned a rank according to a synthetic index (SI) based on resources and need indicators. Before the 2017-reform, the SI was defined as

$$SI_i = 0,45\frac{FCAP}{FCap_i} + 0,15\frac{SocHouse_i}{SOCHOUS} + 0,3\frac{HousA_i}{HOUSA} + 0,10\frac{INCOME}{Income_i}$$

where $Fcap_i$ denotes the financial capacity per inhabitant of municipality i and $FCAP$ is the corresponding average of the demographic group it belongs. $SocHouse_i$ and $SOCHOUS$ indicate the proportions of social housing within the municipality and its demographic group, respectively. Likewise, $HousA_i$ and $HOUSA$ represent the shares of residents receiving housing allowances in each case. Finally, $Income_i$ and $INCOME$ correspond to the per capita income of the municipality and its demographic group.
After 2017, it became:

$$SI_i = 0,3\frac{FCAP}{FCap_i} + 0,15\frac{SocHouse_i}{SOCHOUS} + 0,3\frac{HousA_i}{HOUSA} + 0,25\frac{INCOME}{Income_i}$$

The amount granted to municipality i, if eligible, is computed using its rank SI_i according to the formula:

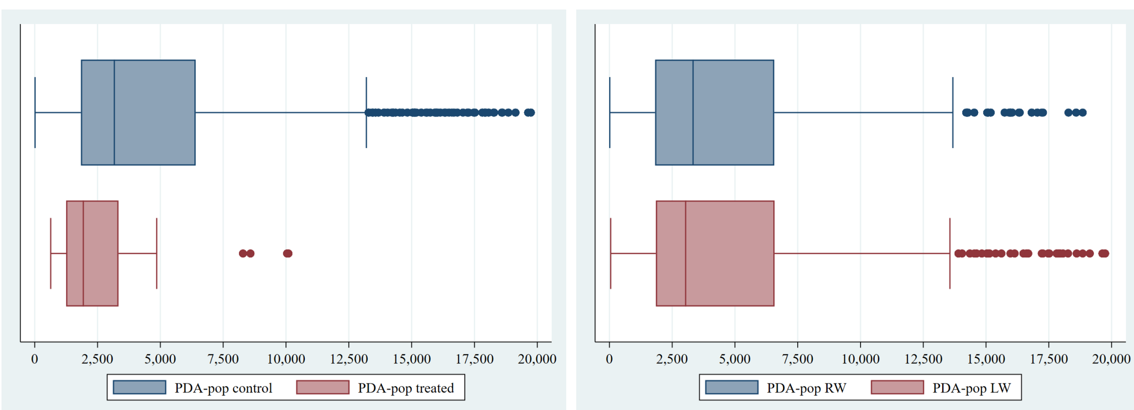
$$USG_i = SI_i * Population_i * TaxEffort_i * Coef_{PDA_i} * Coef_{UFZ_i} * Coef_{maj_i} * PV$$

that takes into account its population-grant and its tax effort. Adjustment coefficients are applied to increase the amount to compensate specific challenges faced by populations located in Poor District Areas (PDA) and Urban Free Zones[29]. The $Coef_{maj}$, ranging from 1 to 8, is applied to offset additional constraints. The tougher the municipality's socio-economic conditions–such as large poverty or unemployment rates–the higher the adjustment coefficient. The point value (PV) is fixed by the authorities in charge of the allocation.

[29] An Urban Free Zone (*Zone Franche Urbaine*) is a disadvantaged area offering tax and social charge exemptions to businesses to boost economic development and employment.

A.2 Poor district areas (PDA)

Our sample include municipalities with Poor District Areas (PDA). These were established by Decree No. 2014-1750 of December 30, 2014, and came into effect on January 1, 2015. Their boundaries were defined until December 31, 2023. The map of the PDAs is set by central authorities according to socio-economic information provided by INSEE and the State representatives at the county level (préfets) and a national agency in charge of the territories (ANCT). This map remained largely unchanged for nearly nine years, from 2015 to 2024. Being classified as a municipality with PDA is important as it signals its vulnerability. The share of its population residing in those PDA is taken into account in the formula computing the USG it receives if eligible. Below, figure A.1a compares, among the municipalities having PDA, the distribution of PDA population for control and treated municipalities. Moreover, figure A.1b compares the distribution of PDA population between RW and LW municipalities.



(a) Control and treated municipalities. (b) RW and LW municipalities.

Figure A.1: PDA population distribution. Sample of municipalities having PDA.

A.3 Transparency requirements of budget orientation debate

As detailed in Table A.1, in accordance with the 2015 NOTRe Law, municipalities above 3,500 inhabitants must conduct a budget orientation debate and prepare multi-annual investment plans. Those above 10,000 inhabitants must provide detailed personnel expenditure reports, justifying payroll structure and projections. Personnel expenditure is particularly scrutinized.

Table A.1: Transparency requirements of budget orientation debate

Municipality size	$\geq 3,500$ inhab.	$> 10,000$ inhab.	$> 20,000$ inhab.	$> 50,000$ inhab.
Art. CGCT	L.2312-1	L.2312-1	L.2311-1-2, L.2312-1	L.2311-1-1, L.2312-1
Situation regarding sustainable development				Yes
Situation regarding gender equality		Yes	Yes	Yes
Budget orientations	Yes	Yes	Yes	Yes
Planned multi-year commitments	Yes	Yes	Yes	Yes
Debt structure and management	Yes	Yes	Yes	Yes
Workforce structure		Yes	Yes	Yes
Personnel expenditure		Yes	Yes	Yes
Actual working hours		Yes	Yes	Yes

Notes. The table specifies the articles of the Code Général des Collectivités Territoriales (CGCT) regarding municipal budget, version in force since the 2015 NOTRe Law.

A.4 Change in representatives' allowances

Using the official schedule of executive allowances, we calculate the monetary incentive associated with crossing the cutoffs 1,000, 3,500 and 10,000. For the sake of simplicity, we consider only the Mayors' allowance. Deputy mayors' follow a similar pattern. As shown in Table A.2, these gains, rather limited, remained unchanged before and after 2017 for the 10,000 threshold.

Table A.2: Relative changes in representatives' allowances (mayor) when crossing population cutoffs

Year	Gain from crossing 1,000 cutoff	Gain from crossing 3,500 cutoff	Gain from crossing 10,000 cutoff
2014	38,71%	27,91%	18,18%
2016	38,71%	27,91%	18,18%
2017	38,71%	27,91%	18,18%
2019	38,71%	27,91%	18,18%
2024	28,04%	6,59%	18,18%

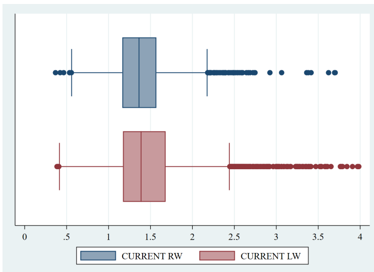
A.5 Voting system

The voting rules follow the electoral Code (Article L262). Below 1,000 inhabitants, voters elect candidates or groups of candidates and may cross off or add names from the lists. The candidates with the most votes win. In municipalities above 1,000 inhabitants, a two-round proportional closed-list system is used with a majority bonus. Each competing list encompasses a number of candidates at least equivalent to the number of seats in the municipal council. In the first round, if a list obtains the absolute majority of votes, it obtains half of the council seats. The remaining seats are distributed between the lists that have reached 5% of the valid votes (including the winning list) according to a proportional rule based on the highest average. When a list gathers more than 50% of the votes there is no second round of voting. Otherwise, a second round is organized with all lists obtaining more than 10 % of the votes, and the procedure for allocating seats is similar to that of the first round: the leading list secures half of the seats while the remaining seats are filled proportionally among the lists that obtained at least 5% of the valid votes. Between the first and second rounds, lists that received more than 5% of the valid votes can merge with other lists as long as those lists received more than 10% of the valid votes in the first round.

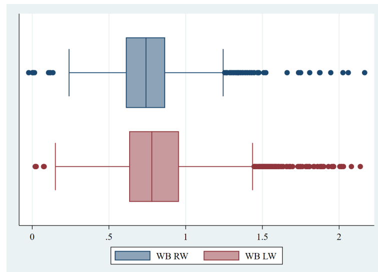
As a consequence, this electoral system ensures that one list (and party) gets a strong majority of the seats regardless of the election outcome. In other words, the fragmentation of the municipal council is reduced by the transformation system of the votes into council seats.

B Descriptive statistics

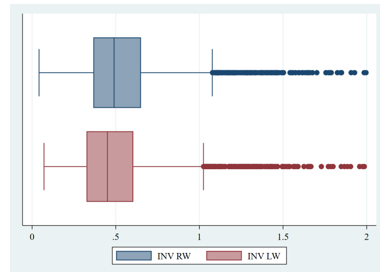
B.1 Descriptive statistics: sub-samples by party



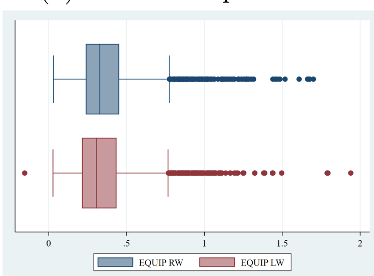
(a) Current expenditure



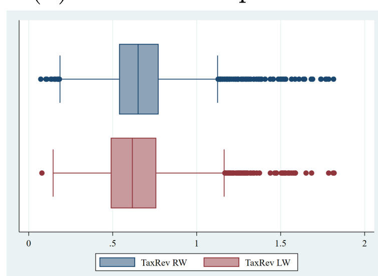
(b) Personnel expenditure



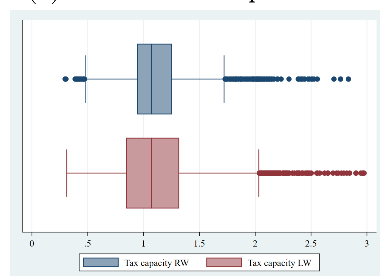
(c) Investment expenditure



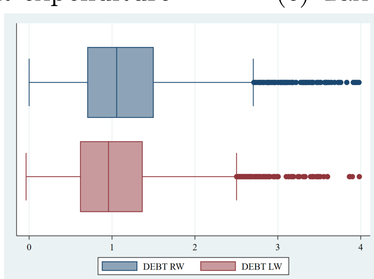
(d) Equipment expenditure



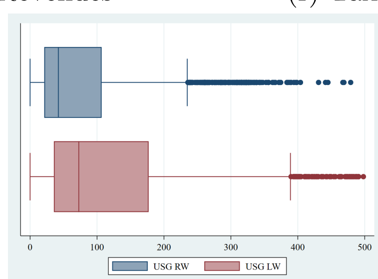
(e) Tax Revenues



(f) Tax capacity



(g) Debt



(h) USG amount in €

Figure B.1: Distribution of budgetary items in thousand 2024 € per inhabitant. Comparison between RW and LW municipalities. Sub-samples over the period 2014-2024.

B.2 Descriptive statistics: sub-samples by electoral margin

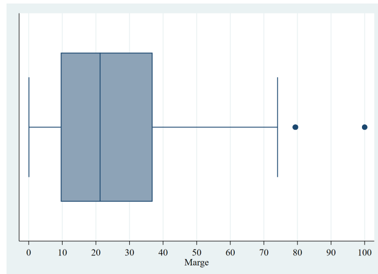
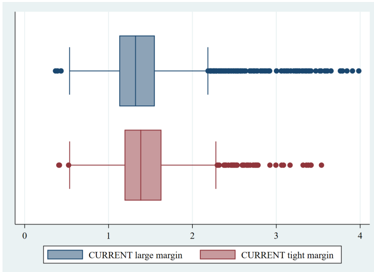
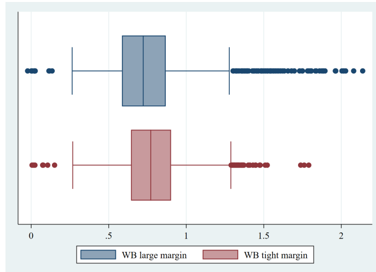


Figure B.2: Distribution of the electoral margin (in %). Sample over the period 2014-2024.

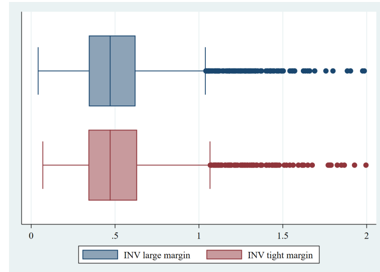
Figure B.2 describes the distribution of the electoral margin in our sample, covering the two municipal elections. In 2014 (respectively 2020), 259 (196) mayors were elected with a narrow margin (less than 10 percentage points). In 2014 (respectively 2020), 257 (366) mayors were elected with a wide margin (more than 30 percentage points). Figure B.3 displays boxplots that describe the distribution of budgetary items and give a comparison between the narrow margin and the wide margin sub-samples.



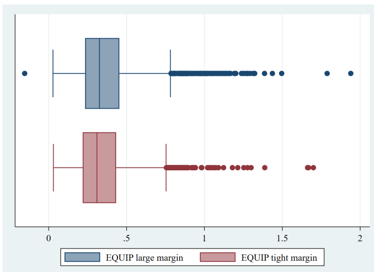
(a) Current expenditure



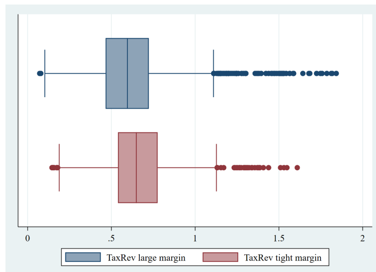
(b) Personnel expenditure



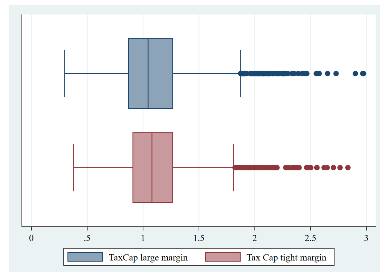
(c) Investment expenditure



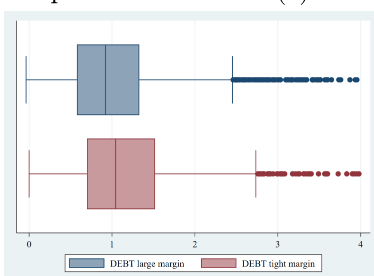
(d) Equipment expenditure



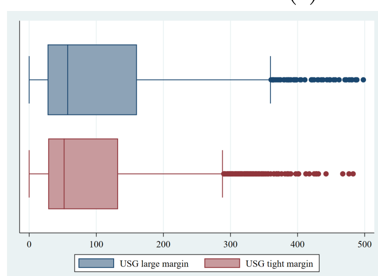
(e) Tax Revenues



(f) Tax capacity



(g) Debt



(h) USG amount in €

Figure B.3: Distribution of budgetary items in thousand 2024 € per inhabitant. Comparison between wide and narrow electoral margin. Sub-samples over the period 2014-2024.

B.3 Descriptive statistics: sub-samples by ENP

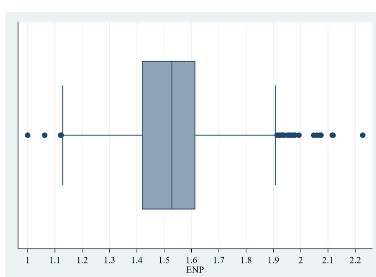
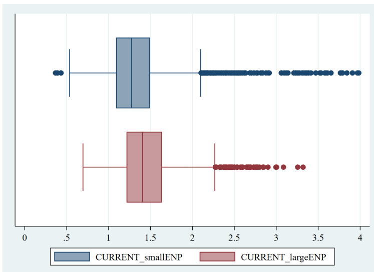
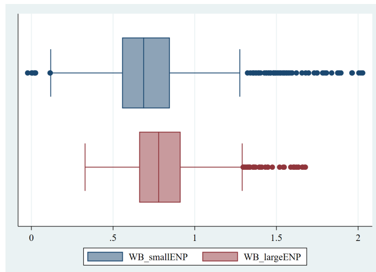


Figure B.4: Distribution of the ENP. Sample over the period 2014-2024.

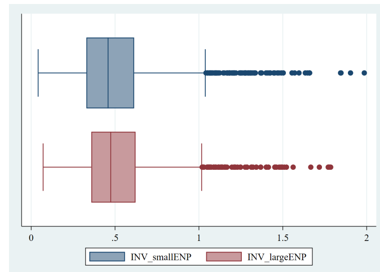
Figure B.4 describes the distribution of the ENP, calculated as the inverse of the sum of squared seat shares. The higher the ENP, the more fragmentation in the council. Figure B.5 displays boxplots that describe the distribution of budgetary items and give a comparison between the small ENP (less than 1.42 the first quartile) and the large ENP (more than 1.62 the third quartile) sub-samples.



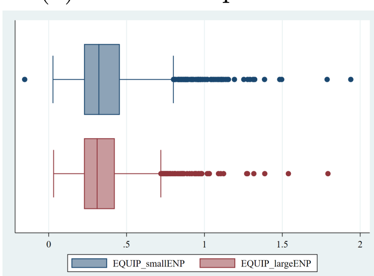
(a) Current expenditure



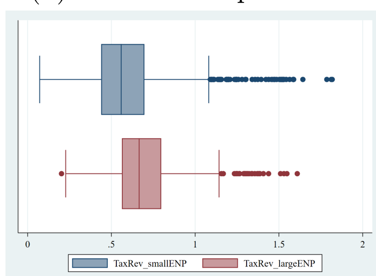
(b) Personnel expenditure



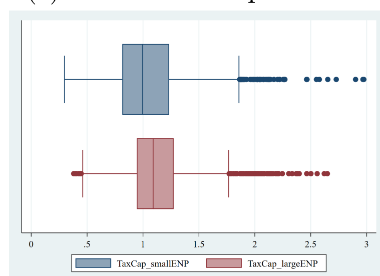
(c) Investment expenditure



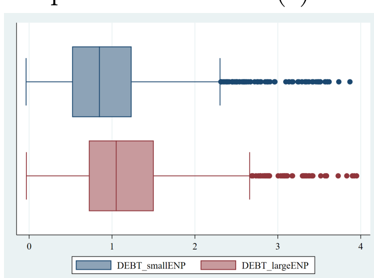
(d) Equipment expenditure



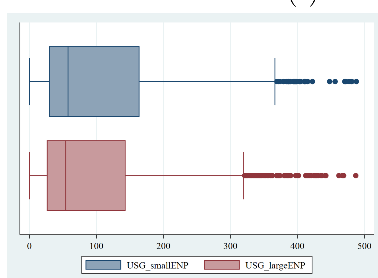
(e) Tax Revenues



(f) Tax capacity



(g) Debt



(h) USG amount in €

Figure B.5: Distribution of budgetary items in thousand 2024 € per inhabitant. Comparison between small and large ENP. Sub-samples over the period 2014-2024.

C Additional population manipulation tests on the 5,000 and 10,000-inhabitant cutoffs. Year-by-year.

Table C.1: Manipulation test on the 5,000 and 10,000-inhabitant cutoffs.
Using population-grant. Year-by-year.

	cutoffs	5,000	5,000	5,000	10,000	10,000	10,000
	pol. order	1	2	3	1	2	3
2014	T	0.5096	0.4247	0.6016	-0.0598	0.6491	-0.4734
	p-value	(0.6103)	(0.6711)	(0.5474)	(0.9523)	(0.5162)	(0.6359)
2015	T	-0.2715	0.0244	0.3071	0.6332	1.0408	-0.0400
	p-value	(0.7860)	(0.9806)	(0.7587)	(0.5266)	(0.2980)	(0.9681)
2016	T	-1.5337	-0.9769	-0.7400	1.1776	-3.2538^{**}	0.7507
	p-value	(0.1251)	(0.3286)	(0.4593)	(0.2389)	(0.0011)	(0.4528)
2017	T	-1.8812^{+}	-1.0552	-0.7670	1.7131^{+}	-1.2465	1.8498^{+}
	p-value	(0.0599)	(0.2913)	(0.4431)	(0.0867)	(0.2126)	(0.0643)
2018	T	-1.1009	-1.5462	-2.0053^{*}	2.1356^{*}	1.7720^{+}	2.3757^{*}
	p-value	(0.2709)	(0.1221)	(0.0449)	(0.0327)	(0.0764)	(0.0175)
2019	T	-1.0572	-2.1680^{*}	-2.7770^{**}	1.1654	1.3361	1.3615
	p-value	(0.2904)	(0.0302)	(0.0055)	(0.2438)	(0.1815)	(0.1734)
2020	T	-0.3693	-1.7456^{+}	-2.6899^{**}	1.9459^{+}	1.3791	1.8247^{+}
	p-value	(0.7119)	(0.0809)	(0.0071)	(0.0517)	(0.1679)	(0.0680)
2021	T	-0.1458	-1.2515	-2.1064^{*}	1.3644	1.4693	0.8622
	p-value	(0.8841)	(0.2107)	(0.0352)	(0.1724)	(0.1418)	(0.3886)
2022	T	-1.0085	-1.5017	-1.8287^{+}	1.7961^{+}	1.6491^{+}	1.1719
	p-value	(0.3132)	(0.1332)	(0.0674)	(0.0725)	(0.0991)	(0.2412)
2023	T	-0.5862	-1.3793	-1.7149^{+}	0.1535	1.5685	0.1013
	p-value	(0.5577)	(0.1678)	(0.0864)	(0.8780)	(0.1168)	(0.9193)
2024	T	-1.2423	-1.4839	-1.7282^{+}	0.3331	1.3029	0.3120
	p-value	(0.2141)	(0.1378)	(0.0840)	(0.7390)	(0.1926)	(0.7550)

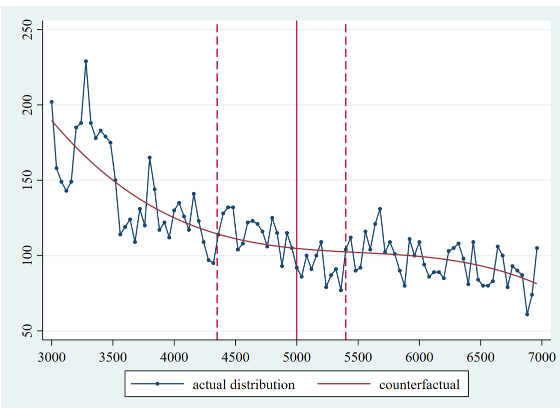
Notes. RD Manipulation test using local polynomial density estimation. Population-grant of all urban municipalities (3,588) considered. Triangular Kernel and Jackknife VCE method are used. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

D Robustness check: Additional results on bunching

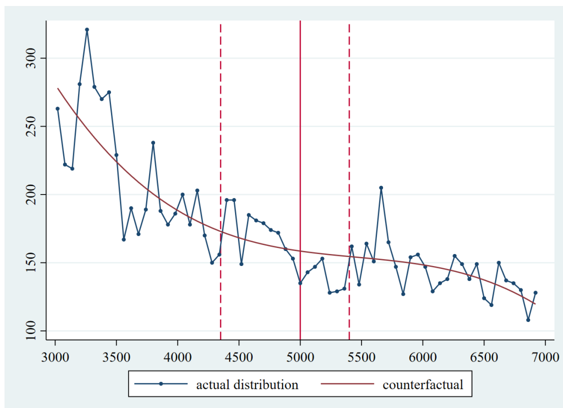
Table D.1: Additional results on bunching at the 5,000 cutoff.
Using population-grant.

pol. order	2	3	3	3	4
bin size	50	40	50	60	50
range $[r_L; r_U]$	$[4,350; 5,400]$	$[4,350; 5,400]$	$[4,350; 5,400]$	$[4,350; 5,400]$	$[4,350; 5,400]$
Excess Mass	14.08	103.28	93.19	75.54	162.69
Missing Mass	-99.58	-109.74	-105.87	-106.76	-51.92
R^2	0.741	0.750	0.772	0.777	0.772

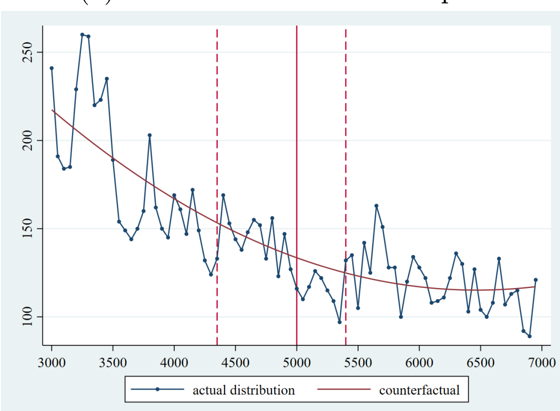
Notes. Population-grant of all urban municipalities (3,588) is considered over the period 2014-2024. The excess mass (missing mass) is computed as the sum of the difference between the actual distribution and the counterfactual over the interval $[4,350; 5,000[$ (resp. $[5,000; 5,400]$).



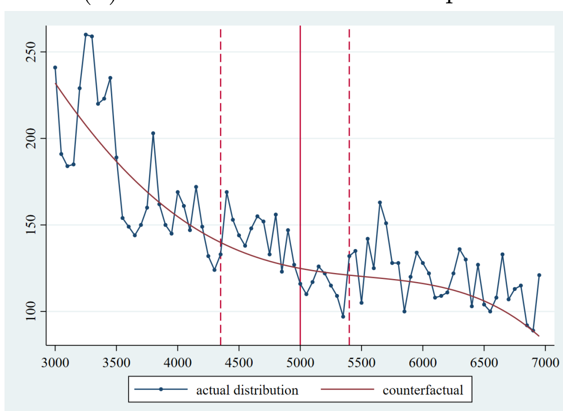
(a) Bin-size=40 and 3-order pol.



(b) Bin-size=60 and 3-order pol.



(c) Bin-size=50 and 2-order pol.



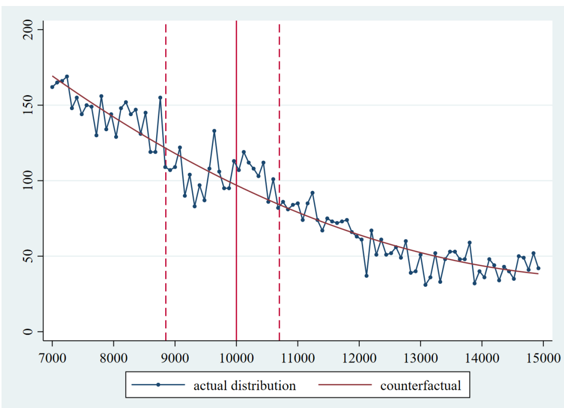
(d) Bin-size=50 and 4-order pol.

Figure D.1: Bunching at the 5,000-inhabitant cutoff

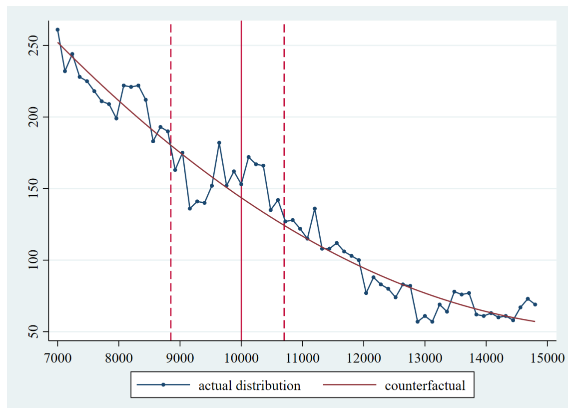
Table D.2: Additional results on bunching at the 10,000 cutoff.
Using population-grant.

pol. order bin size	2 80	2 100	2 120	3 100	4 100
range $[r_L; r_U]$	$[8,850; 10,700]$	$[8,850; 10,700]$	$[8,850; 10,700]$	$[8,850; 10,500]$	$[8,850; 10,500]$
Excess Mass	176.72	181.70	169.07	79.31	85.91
Missing Mass	-129.97	-125.82	-101.55	-253.73	-231.76
R^2	0.957	0.969	0.977	0.974	0.975

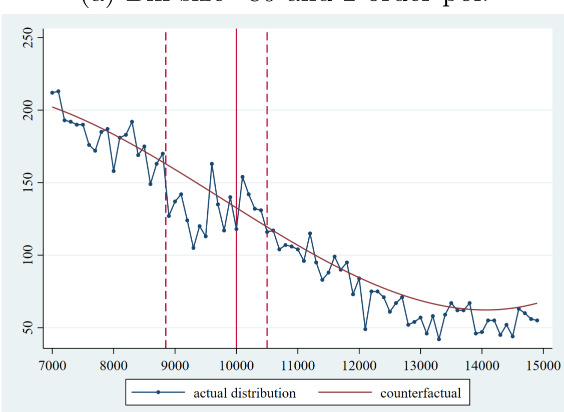
Notes. Population-grant of all urban municipalities (3,588) is considered over the period 2014-2024. The excess mass (resp. the missing mass) is computed as the sum of the difference between the actual distribution and the counterfactual when the difference is positive (resp. negative), over the corresponding range $[r_L; r_U]$.



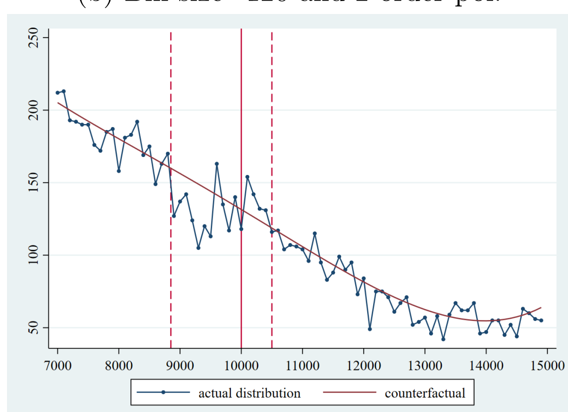
(a) Bin-size=80 and 2-order pol.



(b) Bin-size=120 and 2-order pol.



(c) Bin-size=100 and 3-order pol.



(d) Bin-size=100 and 4-order pol.

Figure D.2: Bunching at the 10,000-inhabitant cutoff

E Robustness check: Placebo thresholds outside the manipulation zones

As another robustness check, we run density manipulation tests on placebo cutoffs. We choose them around the 5,000 and 10,000 cutoffs, outside the manipulation zones identified in section 4.1. Results around the 5,000 cutoff, presented in Table E.1, show that the test may be statistically significant if the cutoff is too close to the bounds of the manipulation zone, but it turns insignificant as it moves away from the bounds. Note that the test is significant at the 4,000 cutoff probably because it belongs to the manipulation zone around the 3,500 threshold.

Results around the 10,000 cutoff (Table E.2) show clearer results: the test is insignificant except at the 8,800 and 10,800 cutoffs, that are very close to the bounds of the manipulation zone. This confirms that the manipulation zone around the 10,000 cutoff is well identified.

Table E.1: Placebo cutoffs around the 5,000-inhabitant manipulation zone.
Using population-grant.

cutoffs		1-order pol.	2-order pol.	3-order pol.
4,000	T	1.4836	-0.6414	2.8415**
	p-value	(0.1379)	(0.5212)	(0.0045)
4,100	T	-0.2999	-1.3354	-1.2793
	p-value	(0.7642)	(0.1817)	(0.2008)
4,200	T	-1.1243	-2.2625*	-4.3561***
	p-value	(0.2609)	(0.0237)	(0.0000)
4,300	T	-0.5352	-0.2434	-3.5277***
	p-value	(0.5925)	(0.8077)	(0.0004)
5,500	T	-0.6862	2.2397*	3.6826***
	p-value	(0.4926)	(0.0251)	(0.0002)
5,600	T	2.3307*	1.9742*	2.0879*
	p-value	(0.0198)	(0.0484)	(0.0368)
5,700	T	-1.0800	-1.3230	-1.2798
	p-value	(0.2802)	(0.1858)	(0.2006)
5,800	T	-2.8059**	-3.3724***	-3.5013***
	p-value	(0.0050)	(0.0007)	(0.0005)
5,900	T	1.5266	-1.3962	-3.3139***
	p-value	(0.1269)	(0.1626)	(0.0009)
6,000	T	0.2159	0.0741	-2.4705*
	p-value	(0.8291)	(0.9409)	(0.0135)
6,100	T	-0.5485	1.0168	-1.0495
	p-value	(0.5834)	(0.3093)	(0.2939)

Notes. RD Manipulation test using local polynomial density estimation. Placebo thresholds are chosen outside the manipulation zone $[4,350; 5,400]$. Population-grant of all urban municipalities (3,588) considered over the period 2014-2024. Triangular Kernel and Jackknife VCE method are used. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table E.2: Placebo cutoffs around the 10,000-inhabitant manipulation zone. Using population-grant.

cutoffs		1-order pol.	2-order pol.	3-order pol.
8,500	T	-0.6095	-0.1681	0.2141
	p-value	(0.5422)	(0.8665)	(0.8305)
8,600	T	0.2193	-0.4821	0.1669
	p-value	(0.8264)	(0.6298)	(0.8674)
8,700	T	1.3308	-0.8869	0.0570
	p-value	(0.1833)	(0.3751)	(0.9545)
8,800	T	0.0591	-1.9055^{+}	-0.7047
	p-value	(0.9529)	(0.0567)	(0.4810)
10,800	T	0.1004	-1.5011	-2.5608*
	p-value	(0.9200)	(0.1333)	(0.0104)
10,900	T	0.7616	0.0189	-1.4277
	p-value	(0.4463)	(0.9849)	(0.1534)
11,000	T	1.2939	1.3410	-0.3380
	p-value	(0.1957)	(0.1799)	(0.7353)

Notes. RD Manipulation test using local polynomial density estimation. Placebo thresholds are chosen outside the manipulation zone $[8,850; 10,700]$. Population-grant of all urban municipalities (3,588) considered over the period 2014-2024. Triangular Kernel and Jackknife VCE method are used. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

F Results on the sub-sample of municipalities belonging to the manipulation zone.

Table F.1: Dynamic effects of the 2017 reform: losing eligibility for USG. Municipalities inside the manipulation zone.

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
Pre-avg	-0.101	0.016^{+}	-0.410	-0.064	0.050^{+}	0.094
	(0.099)	(0.009)	(0.397)	(0.127)	(0.028)	(0.078)
Post-avg	-0.024	-0.010	0.152	0.123*	0.021^{+}	0.095
	(0.080)	(0.012)	(0.173)	(0.058)	(0.012)	(0.062)
N	560	560	560	560	560	560

Notes. Panel of 65 municipalities inside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Dependent variables are budgetary items expressed in thousand 2024 € per inhabitant. Event study using the Callaway and Sant'Anna (2021) procedure. Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

G Results by electoral competition. Using the number of competing lists.

Table G.1: Dynamic effects of the 2017 reform: losing eligibility for USG.
Sub-samples by number of competing lists.

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
Two lists						
Pre-avg	0.034	-0.010	0.027	-0.044	0.018	0.035
	(0.044)	(0.009)	(0.078)	(0.043)	(0.018)	(0.031)
Post-avg	0.086	0.014	0.095	0.064	0.035^{**}	0.026
	(0.079)	(0.012)	(0.175)	(0.109)	(0.013)	(0.076)
N	1,666	1,666	1,666	1,666	1,666	1,666
Four lists or more						
Pre-avg	-0.102	-0.015^{+}	-0.187	0.002	0.022	-0.044
	(0.132)	(0.008)	(0.202)	(0.031)	(0.037)	(0.077)
Post-avg	0.012	-0.020	0.038	0.018	0.086^{*}	-0.064^{+}
	(0.061)	(0.016)	(0.083)	(0.027)	(0.036)	(0.038)
N	1,764	1,764	1,764	1,764	1,764	1,764

Notes. Dependent variables: budgetary items expressed in thousand 2024 € per inhabitant. Panel of 810 municipalities outside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Event study using the Callaway and Sant'Anna (2021) procedure. Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

H Results combining electoral margin and fragmentation.

We investigate whether a narrow electoral margin combined with a very fragmented council produces even more accurate results. Symmetrically, we also consider municipalities combining a wide electoral margin and a small fragmented council[30]. The results are displayed in Table H.1. In the case of municipalities with a large ENP and narrow margin, results are very close to those obtained with narrow margin even if the sample size is more than twice smaller (807 vs 1,746 observations). For municipalities with a small ENP and wide margin, again, results are very close to those obtained with wide margin even if the sample size is less than twice smaller (1,234 vs 2,050 observations). Overall, this reveals that the magnitude of the electoral margin is the main political channel. When fragmentation and electoral competitiveness are considered jointly, the results indicate that the margin of victory is the primary determinant of behavior, while fragmentation amplifies particular expenditure responses.

[30]The two other cases can not be considered due to too few observations: we count zero observation with a narrow margin and a small ENP and only 76 observations with a wide margin and a large ENP.

Table H.1: Dynamic effects of the 2017 reform: losing eligibility for USG. Sub-samples by ENP and electoral margin.

	Current Expenditure	Personnel Expenditure	Investment Expenditure	Equipment Expenditure	Tax Revenues	Debt
Large ENP and narrow margin						
Pre-avg	-0.052	-0.008	-0.086	0.033	0.004	-0.095
	(0.115)	(0.013)	(0.167)	(0.071)	(0.044)	(0.085)
Post-avg	-0.487***	0.039***	-0.831***	-0.029	-0.004	-0.237***
	(0.069)	(0.011)	(0.112)	(0.029)	(0.009)	(0.051)
N	807	807	807	807	807	807
Small ENP and wide margin						
Pre-avg	0.082^{+}	-0.006	0.022	-0.069^{+}	-0.009	0.050
	(0.047)	(0.009)	(0.063)	(0.039)	(0.020)	(0.039)
Post-avg	0.069	0.001	0.127^{+}	0.038	0.042***	-0.051
	(0.045)	(0.010)	(0.076)	(0.071)	(0.010)	(0.056)
N	1,234	1,234	1,234	1,234	1,234	1,234

Notes. Panel of 810 municipalities outside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Dependent variables are budgetary items expressed in thousand 2024 € per inhabitant. Event study using the Callaway and Sant'Anna (2021) procedure. A narrow (resp. wide) electoral margin corresponds to a margin of less than 10 percentage points (resp. more than 30 points). A small (resp. large) ENP corresponds to an ENP of less than 1.42 (resp. more than 1.62). Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I Additional results on the role of PDA.

Table I.1: Investigating the role of poor district areas (PDA).

	Investment exp. with PDA	Investment exp. without PDA	Equipment exp. with PDA	Equipment exp. without PDA
Whole sample				
Pre-avg	-0.084	0.026	0.006	-0.016
	(0.241)	(0.056)	(0.045)	(0.026)
Post-avg	-0.184	0.079	0.014	0.016
	(0.274)	(0.060)	(0.046)	(0.028)
N	6,112	2,287	6,112	2,287
Left-Wing				
Pre-avg	0.095	0.101	0.051	-0.039
	(0.131)	(0.102)	(0.135)	(0.053)
Post-avg	0.111	0.180	0.022	0.029
	(0.098)	(0.114)	(0.117)	(0.064)
N	2,521	781	2,521	781
Right-Wing				
Pre-avg	-0.173	-0.047	0.003	-0.016
	(0.377)	(0.053)	(0.028)	(0.032)
Post-avg	-0.311	0.026	0.047	0.010
	(0.403)	(0.053)	(0.040)	(0.040)
N	2,544	1,060	2,544	1,060

Notes. Dependent variables: investment and equipment expenditures expressed in thousand 2024 € per inhabitant. Panel of 810 municipalities outside the manipulation zone (around the 10,000-inhabitant cutoff) over the period 2014-2024. Event study using the Callaway and Sant'Anna (2021) procedure. Pre-avg (resp. Post-avg) stands for the aggregate summary measure of the pre-treatment (resp. post-treatment) coefficients. Robust and asymptotic standard errors in parentheses. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.