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Call of Duty: Bloc Ops

Effects of Bloc Majority Control in Swedish Municipalities, 1993–2022

Gabriel Tingvall (42863)

Abstract

This paper uses a fuzzy regression discontinuity design to estimate the effect of bloc control on municipal outcomes, comparing municipalities where the right bloc barely attains a council-seat majority to those where it barely falls short. The running variable is the seat-majority vote margin: the right bloc's vote share minus the minimum vote share required to win a seat majority under the modified Sainte-Laguë allocation rule. Because many close elections involve councils without a clear bloc majority, the estimates should mainly be interpreted as the effect of right-bloc majority status relative to non-right-majority outcomes, rather than as a pure left-versus-right ideological contrast. Across a range of fiscal and economic outcomes, I find no statistically significant discontinuities at the cutoff. The results are robust to alternative bandwidth choices, polynomial orders, and standard RDD diagnostics. Overall, the evidence suggests that close changes in right-bloc majority status do not translate into systematic differences in the municipal outcomes studied.

Keywords: partisan effects, proportional representation, bloc majority control

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Examiner:	Abhijeet Singh

AI disclosure

For this thesis, AI has been used as a support tool for coding in Stata and Python, primarily to identify syntax errors and assist with troubleshooting. AI has also been used for language editing and table formatting, with the aim of improving spelling and overall readability. All such assistance was provided using ChatGPT by OpenAI.

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1 Introduction

A longstanding question in political economy is whether political parties matter for policy. Do competing parties offer genuinely different policy alternatives, and, once in office, do they implement different policies? From a theoretical perspective, the answer is not obvious. Classical models of electoral competition suggest strong incentives for convergence. In Hotelling's spatial model, competitors move toward the center to attract marginal voters (Hotelling, 1929). Downs (1957) applies a similar logic to democratic elections, showing that in a bipartisan setting parties have incentives to converge toward the preferences of the median voter. Related probabilistic voting models likewise imply that parties moderate their positions in order to maximize electoral support (Lindbeck and Weibull, 1987). Taken together, these frameworks suggest that bloc competition may generate limited policy differences, regardless of which side wins office.

At the same time, other theories imply that partisan control should matter. In citizen-candidate models, politicians are policy-motivated actors, so electoral outcomes may translate into different policies because candidates themselves differ in their preferences (Osborne and Slivinski, 1996; Besley and Coate, 1997). Similarly, models of legislative bargaining emphasize that policy outcomes depend on the distribution of institutional power within elected assemblies, such as proposal power, agenda-setting authority, and coalition leverage (Baron and Ferejohn, 1989; Laver and Shepsle, 1990). Under this view, partisan control matters because different parties occupy different bargaining positions, allowing them to steer negotiations and shape policy in ways that reflect their own priorities.

Taken together, these competing theoretical perspectives leave the empirical question open. Whether partisan control translates into different policies and outcomes is ultimately a matter for evidence. This paper examines whether bloc majority control affects fiscal and economic outcomes in Swedish municipalities, a setting that is particularly well suited to studying this question because political competition has long been organised around a left and a right bloc, while proportional representation makes the translation from votes into political control more complex than in majoritarian systems. The main contribution of the paper is both substantive and methodological. Substantively, it provides new evidence on bloc majority effects in Swedish local politics in the post-1990s period and finds no statistically significant effects on the fiscal and economic outcomes considered. Methodologically, it develops an empirical strategy that measures bloc-majority closeness in a way that is consistent with proportional representation, by linking treatment assignment to the electoral rule rather than relying on a simple vote-share cutoff. This makes it possible to identify the causal effect of bloc majority control in a setting where the mapping from votes to political power is inherently non-linear and institutionally determined.

However, this identification task is not straightforward. Electoral outcomes are not randomly assigned, but instead reflect voter preferences, local political conditions, and deeper structural characteristics that may themselves shape subsequent policy choices and economic outcomes. This creates a central endogeneity problem: the same factors that determine which party or bloc wins office may also affect the outcomes of interest. For example, a municipality experiencing economic decline may be both more likely to elect a particular bloc and more likely to display weak fiscal or labour-market outcomes in subsequent years, even in the absence of any causal effect of bloc control.

A major advance in the empirical literature in overcoming this endogeneity problem came with the use of regression discontinuity designs (RDDs) in close elections. In an RDD, treatment is assigned according to whether an observed running variable crosses a known threshold. The key idea is that units located just above and just below that threshold are likely to be comparable in both observed and unobserved characteristics, so any discontinuous change in outcomes at the cutoff can be interpreted as causal under suitable continuity assumptions (Imbens and Lemieux, 2008). For example, a municipality experiencing economic decline may be both more likely to elect a particular bloc and more likely to display weak fiscal or labour-market outcomes even absent any causal effect of bloc control. But if we compare municipalities lying just above and just below the electoral threshold, that underlying decline should vary smoothly across the cutoff. In that case, a discontinuous jump in political control at the threshold allows the effect of bloc control to be isolated from the effect of the municipality's underlying economic conditions. Lee (2008) was among the first to establish this logic in electoral settings, showing that when elections are decided by very narrow margins, treatment assignment is locally as good as random near the cutoff.

Building on this insight, Pettersson-Lidbom (2008) was among the first to apply an RDD directly to the question of whether parties matter for policy outcomes. Using panel data from Swedish municipalities, he studies close elections in a bloc-based party system and uses a 50 percent vote-share cutoff to assign political control. He finds sizeable partisan effects: left-wing governments spend and tax about 2 to 3 percent more than right-wing governments, have around 7 percent lower unemployment, and employ about 4 percent more workers. However, subsequent research has shown that this approach is less well suited to proportional representation systems, where the relationship between votes and political control is governed not by a fixed 50 percent threshold, but by the electoral rule and the full distribution of votes across parties. Fiva et al. (2018) discuss these limitations extensively and show why identifying close elections in PR systems requires a more institutionally grounded measure of electoral closeness.[1]

Following these early contributions, a growing empirical literature has examined whether parties matter across different countries, levels of government, and electoral systems. Particularly important for proportional representation systems is Folke (2014), who develops what he describes as the first regression discontinuity design specifically tailored to proportional representation systems. The core difficulty in PR elections is that there is no single predetermined threshold at which treatment changes. Instead, seat thresholds are jointly determined by the full distribution of votes across parties, so a party can gain or lose representation even if its own vote share is unchanged. Folke's key contribution is to solve this problem by using the seat-allocation rule directly and defining closeness in terms of the minimum vote change required to alter seat allocation. Applying this design to Swedish municipalities, he studies the causal effect of individual party representation and finds substantial effects on immigration and environmental policy, but no statistically significant effect on the local tax rate.

Folke's paper also became an important methodological point of departure for later work. Fiva et al. (2018) explicitly build on this line of research in their study of Norwegian municipalities. They combine Folke's seat-threshold logic for party representation with a separate simulation-based procedure for bloc majority control, allowing them to study both changes in seat majorities and changes in council composition. Their results suggest that parties matter through both channels: a larger left-wing representation is associated with more property taxation, higher childcare spending, and lower elderly care spending, while effects on local public goods appear limited. Around the same time, Freier and Odendahl (2015) developed a related but more explicitly simulation-based approach for multi-party proportional representation systems in German municipalities. Rather than deriving closeness directly from the electoral rule, they repeatedly perturb observed vote vectors and examine whether these perturbations alter seat allocation and parties' voting power. Their focus is somewhat different from the majority-control literature, since they study changes in party voting power in coalition settings rather than shifts in bloc control. They find that greater power for the Social Democrats is associated with lower local taxes, while greater power for the Greens is associated with higher property taxes.

Building on these insights, I develop an empirical strategy tailored to bloc majority control

[1] In Pettersson-Lidbom's design, bloc vote share is used as the running variable, while political treatment is effectively determined by whether a bloc obtains a seat majority. The difficulty is that vote shares do not map cleanly into seat majorities in proportional representation systems, since the conversion of votes into seats depends on the electoral rule and on the full electoral context, including the distribution of votes across parties, the number of parties contesting the election, and the total number of seats in the council. Using seat shares directly as the running variable is also problematic, however, because seat outcomes are inherently discrete and therefore ill-suited to the continuity-based logic of regression discontinuity designs. Section 3 returns to this issue in greater detail.

in a proportional representation setting and introduce a novel running variable designed specifically for this purpose. The approach is closest in spirit to Folke (2014) in that it recovers the relevant electoral threshold from the seat-allocation rule itself, rather than imposing a fixed vote-share cutoff. However, the object of interest differs: whereas Folke studies individual party representation, I study bloc majority control. More specifically, I derive, for each municipality, the minimum bloc vote share required to obtain a seat majority under the modified Sainte-Laguë rule, and define the running variable as the distance between the observed bloc vote share and this municipality-specific threshold. The design also differs from the simulation-based approaches of Freier and Odendahl (2015) and Fiva et al. (2018). While my procedure also evaluates counterfactual vote configurations, it does so deterministically rather than through stochastic simulations. This produces a running variable that is closely tied to the institutional logic of proportional representation and allows for standard RDD implementation and graphical analysis.

Methodologically, the paper also departs from much of the earlier RDD literature on municipal elections. While prior studies such as Pettersson-Lidbom (2008), Folke (2014), Ferreira and Gyourko (2009), and Freier and Odendahl (2015) often relied more heavily on high-order polynomial specifications, broad bandwidth choices, and OLS-style control strategies, I follow more recent best practice in the regression discontinuity literature (Gelman and Imbens, 2019). In particular, I use local polynomial methods with low-order specifications, focusing on linear and quadratic estimators. Bandwidths are selected using the mean squared error optimal procedure proposed by Calonico et al. (2014), and all specifications employ a triangular kernel. Since municipalities are observed repeatedly across election periods, standard errors are clustered at the municipality level in all specifications. This allows for within-municipality dependence over time and avoids treating repeated observations from the same municipality as fully independent. This approach places the analysis more firmly within the modern RD framework, which emphasizes local identification and avoids the strong functional-form assumptions associated with high-order global polynomials.

The results of this paper provide limited support for the view that bloc majority control has large causal effects on the broad fiscal and economic outcomes considered here. Across specifications, the estimated effects are generally modest and statistically insignificant. However, for some outcomes the confidence intervals remain wide, so the results should be interpreted as evidence against large systematic discontinuities rather than as precise evidence of zero effects. This stands in contrast to Pettersson-Lidbom (2008), who finds sizeable bloc-majority effects in Swedish municipalities, but it is closer to Folke (2014), who also studies Swedish municipalities and finds that although individual party representation can have important effects in some policy areas, it does not generate a significant effect on the local tax rate. It is also consistent with Ferreira and Gyourko (2009), who find little evidence of large partisan effects on aggregate local policy outcomes in U.S. cities. However, the contrast with Pettersson-Lidbom should not be attributed to methodology alone. His study covers an earlier period of Swedish local politics, 1974 to 1994, whereas this paper examines the post-1990s period, during which Swedish local policymaking may have become more constrained by fiscal rules, EU-related institutional change, and broader political convergence. The weaker effects found here may therefore reflect not only differences in research design, but also differences in the historical and institutional setting, as well as the possibility that bloc majority control is too coarse a political margin to capture many of the policy mechanisms operating in proportional representation systems.

The remainder of the paper is structured as follows. Section 2 describes the data. Section 3 presents the empirical framework and robustness checks. Section 4 reports and discusses the results. And finally in Section 5, I conclude.

2 Data

This section provides brief institutional background on Swedish municipalities, describes the data used in the empirical analysis, and presents descriptive statistics for the main outcome variables.

2.1 Institutional background

As of 2026, Sweden has 290 municipalities. Municipalities are responsible for a wide range of local public services and welfare functions, including preschool and compulsory schooling, childcare, elderly care, social services, water and wastewater, waste management, local roads, spatial planning, housing-related functions, and rescue services. They also enjoy a high degree of self-government. Local self-government and the right to levy taxes are anchored in the Swedish constitutional framework, and municipalities are entitled to set their own local income tax rates. In both institutional and economic terms, local government plays a central role in Sweden. The broader municipal and regional sector accounts for about 20% of Sweden's GDP, around 70% of public consumption, and roughly one quarter of total employment (Government Offices of Sweden, 2018).

Municipalities are governed by elected municipal councils, which serve as the main decision-making bodies at the local level. These councils appoint the municipal executive board, which leads and coordinates municipal administration and policy implementation. Municipal council elections are held every fourth year (before 1994 they were held every third year). Council size varies with municipal population and ranges from 21 to 101 seats. Seats are allocated using the modified Sainte-Laguë method, which distributes representation proportionally on the basis of vote shares.

2.2 Election data

To examine whether party control affects economic outcomes, I use municipality-level election data from SCB on votes and seats by party, and convert these counts into vote shares of total votes and seat shares of the municipal council. The data covers all Swedish municipalities over the period 1973–2022, but I restrict the analysis to 1993–2022 due to limited availability of outcome variables. I define the political blocs as follows[2]:

Right bloc $M + C + L + KD$

Left bloc $S + V + MP$

Other SD, OVR

Here, OVR represents an aggregate category of local parties. I classify a municipality as having a right-wing government (left-wing government) if the right bloc (left bloc) holds an absolute majority of seats. I classify a municipality as having an undefined government if neither bloc holds a seat majority on its own.

The use of this data set offers some attractive features in the search for causal party effects on economic outcomes. First, it is a large panel data set consisting of 290 municipalities over a 29 year time span, allowing for the use of a RD design which typically is quite data-intensive due to requiring a large amount of observations close to the threshold of the treatment assignment variable. Second, Sweden can, at least as a first approximation, be treated as a two-bloc political system. While the electoral system is formally multiparty and proportional, political competition has long revolved around a relatively stable conflict between socialist and non-socialist blocs. Pettersson-Lidbom (2008) builds directly on this feature and treats Sweden as a bipartisan system, while the broader literature similarly characterizes Swedish politics as shaped by "two-bloc politics" (Aylott and Bolin, 2007; Hellström and Lindahl, 2021). For the purposes of identification, this is

[2]M = Moderate Party (*Moderaterna*); C = Centre Party (*Centerpartiet*); L = Liberals (*Liberalerna*); KD = Christian Democrats (*Kristdemokraterna*); S = Social Democratic Party (*Socialdemokraterna*); V = Left Party (*Vänsterpartiet*); MP = Green Party (*Miljöpartiet*); SD = Sweden Democrats (*Sverigedemokraterna*)

an important feature: when competition is structured around stable blocs, observations on either side of the cutoff are more likely to differ in a substantively meaningful way in terms of bloc control, rather than reflecting unstable shifts across fragmented party constellations.

There are also some limitations in the dataset. First, all small local parties are grouped into a single aggregate category, OVR, which complicates the construction of the running variable and leads to a small number of mismatches between predicted and actual seat majorities. This issue is discussed in Section 3.4. In short, the number of such mismatches is limited, and estimates from a restricted subsample that excludes them are very similar to the baseline estimates.

A further limitation of a more political-institutional nature, is that many Swedish municipalities do not produce a clear bloc majority, meaning that neither the left nor the right bloc holds an absolute majority of seats, and the prevalence of such councils has increased in recent elections, see Table 1. This has been reinforced by the rise of the Sweden Democrats (SD), which historically lacked a stable bloc alignment and often acted as a pivotal party, making the mapping from bloc seat shares to actual governing control less direct. In such cases, governing coalitions often depend on support from small local parties or other cross-bloc arrangements, but my dataset does not fully capture this because the bloc-based coding abstracts from coalition bargaining and all minor local parties are grouped into a single aggregate category (OVR).

Table 1: Counts by election period and government type

Election Period	Number of left-wing governments	Number of right-wing governments	Number of undefined governments
1974–1976	117	147	14
1977–1979	112	147	18
1980–1982	123	143	13
1983–1985	159	115	10
1986–1988	143	127	14
1989–1991	171	94	19
1992–1994	91	143	52
1994–1997	194	64	30
1998–2001	142	92	55
2002–2005	149	99	42
2006–2009	99	118	73
2010–2013	111	93	86
2014–2017	90	36	164
2018–2022	39	53	198

Notes: Election data are from Statistics Sweden (SCB, 2022). Right-wing governments are defined as coalitions consisting of C, M, L, and KD. Left-wing governments are defined as coalitions consisting of MP, V, and S. Undefined governments are defined as cases where neither the right nor the left bloc hold majority on its own

The implications of undefined majorities are analyzed in Section 3.5. In brief, undefined majorities are not necessarily problematic for identification, but they do alter the interpretation of the estimates. In particular, the estimated contrast is more appropriately interpreted as *right-bloc majority versus non-right-bloc majority.* As a result, the estimates may reflect not only ideological differences, but also differences in governing capacity, bargaining arrangements, and the ability to secure budget passage under majority control. Moreover, estimating the model on a restricted subsample that excludes all undefined majorities, thereby isolating the "ideological effect" more cleanly, produces results that remain very similar to the baseline (see Appendix F).

The coding of the Sweden Democrats (SD) as belonging to neither the right bloc nor the left bloc may appear surprising from today's perspective, but it is historically appropriate for most of the period studied here. More systematic cooperation between SD and the right bloc emerged only in the later part of the sample period. For most of the period analyzed here, by contrast, SD did not function as a stable member of either bloc and was instead treated as a politically isolated actor by the established parties (Leander, 2025). To assess whether the results are sensitive to this change over time in SD's effective bloc affiliation, I conduct a robustness check in which the model is re-estimated on a restricted pre-2018 subsample, when the cordon sanitaire against SD was more clearly intact and local cooperation with the party was less normalized, see Appendix E. The results from this subsample remain statistically insignificant and do not alter any of the substantive conclusions.

Still, SD may generate identification problems even during the cordon sanitaire period, since efforts by mainstream parties to contain its influence could induce cross-bloc co-operation that is not fully captured by the dataset's fixed bloc structure. I address this concern by estimating the model on the previously mentioned restricted subsample that excludes municipalities with undefined majorities (Appendix F). This removes the cases in which neither bloc holds an outright majority and in which SD is most likely to hold a pivotal kingmaker role. Reassuringly, the resulting estimates remain very similar to the baseline in terms of economic and statistical insignificance.

2.3 Outcome variables

All outcome variables used in this paper are listed in Table 2 together with their sources. The selection of outcomes is based mainly on data availability, especially the requirement that the variables cover a sufficiently long time period. In addition, the selected variables capture outcomes that municipal governments can plausibly affect, whether directly through policy and budgetary decisions or indirectly through administrative practices. Summary statistics for the main outcome variables are reported in Appendix J.

Table 2: Outcome variables used in the RD analysis

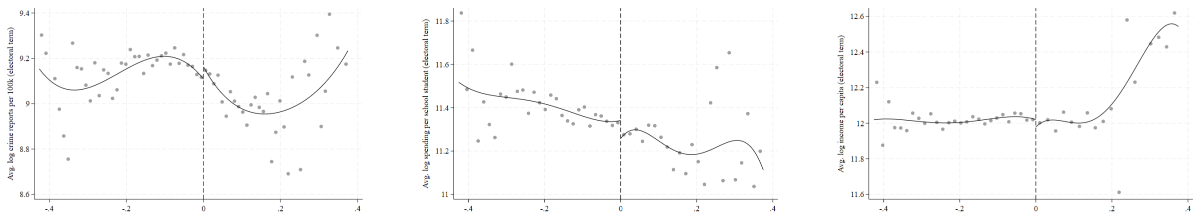
Variable	Description	Data
`Crime reports`	Number of crime reports per 100,000 inhabitants in logarithmic form.	Brå; 1996–2024
`Education expenditure`	Municipal expenditure per compulsory school student in logarithmic form.	Skolverket; 2002–2024
`Income per capita`	Income per capita. Constructed as the municipality's total taxable earned income divided by the number of tax-liable individuals in logarithmic form.	SCB; 2000–2024
`Operating costs`	Municipal government operating costs per capita in logarithmic form.	SCB 1998–2024
`Operating ratio`	Municipal operating ratio. Constructed as the log difference between municipal operating revenues per capita and operating costs per capita, i.e. the log revenue-to-cost ratio.	SCB 1998–2024
`Tax revenue`	Municipal tax revenue per capita in logarithmic form.	SCB 1998–2024
`Tax rate`	Municipal tax rate.	SCB; 2000–2025
`Employment ratio`	Share of individuals aged 25–64 classified as *gainfully employed*, calculated as gainfully employed/(gainfully employed + not gainfully employed).	SCB; 1993–2018
`Government grants`	General government grants and equalization transfers per capita. Not in logarithmic form, since the variable can take negative values for net-contributing municipalities.	SCB 1998–2024

Notes: All outcome variables are constructed as averages over the subsequent electoral term after the election. For instance, if an election was held in 2006, I take the average value of outcome variable Y_{mt} over the subsequent period 2006,2007,2008 and 2009.

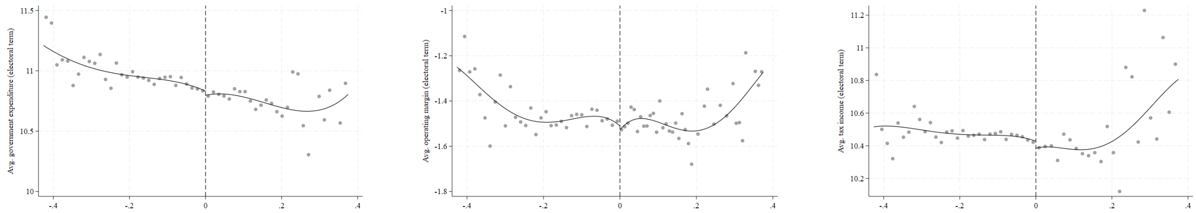
Outcomes are measured as averages over complete election cycles. This is done in part to account for policy lag, since the effect of a change in political control may emerge only gradually over the electoral term rather than immediately after the election. For each election, I therefore compute the mean value of the outcome over the corresponding cycle. To maintain consistency across observations, I exclude cycles that are only partially observed in the data. Thus, if a series is available only from 2000 to 2024 and an election was held in 1998, the 1998 to 2001 election cycle is excluded because it is not fully observed.

Additionally, the averaging window is restricted to one election cycle and is not extended further into subsequent terms. Extending the window beyond a single electoral period would risk contaminating the estimates by combining the effects of different governments, since bloc control may change at the following election. This is especially important in this case, as there is no observed statistically significant incumbency advantage among the municipalities around the cutoff, see Appendix G.

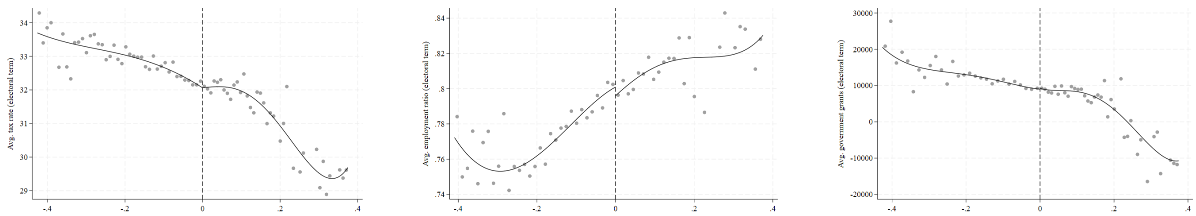
Before turning to the regression estimates, I first present graphical evidence on the relationship between the running variable and the main outcome variables. Figure 1 shows RD plots for the average level of each outcome over the subsequent electoral term. These figures are intended as descriptive illustrations of the conditional relationship between the running variable and the outcome variables.



(a) Crime reports (b) Education expenditure (c) Income per capita



(d) Operating costs (e) Operating ratio (f) Tax revenue



(g) Tax rate (h) Employment ratio (i) Government grants

Figure 1: RD plots of average outcomes over the subsequent electoral term

Notes: Each panel plots binned sample averages of the outcome against the running variable. The x-axis shows the running variable, defined as the distance to the right-bloc majority threshold, so values further to the right indicate municipalities that are more favourable to a right-bloc majority. The solid line shows a fourth-order polynomial fit estimated separately on each side of the cutoff. All outcomes are measured as averages over the subsequent electoral term following the election. These plots are descriptive; the main fuzzy RD estimates are reported in the regression tables.

Although the plots generally reveal little visual evidence of sharp discontinuities at the cutoff, the tails of the running-variable distribution display broader patterns consistent with conventional left-right differences. Municipalities located further on the right side tend to have lower tax rates and higher income per capita, while municipalities located further on the left side tend to have higher operating costs, lower employment rates, greater dependence on government grants, and lower income per capita.

3 Empirical framework

In this section, I describe the regression discontinuity design and explain how it is implemented in this paper.

In a typical “sharp“ RDD, treatment status is a deterministic function of an underlying continuous assignment variable:

$$T_i = T(x_i) = \mathbb{1}[x_i \geq c], \tag{1}$$

where $\mathbb{1}[\cdot]$ is an indicator function, x_i is the running variable, and c is the treatment threshold. The threshold separates units into two mutually exclusive groups: those receiving treatment ($T_i = 1$) and those who do not ($T_i = 0$). The identifying idea is to compare outcomes for units with x_i “just below” and “just above” c, since these units are expected to be similar on average in all respects except treatment status. Under this assumption, observations slightly below the cutoff provide a credible counterfactual for observations slightly above it.

Applied in the municipal government election context, one seemingly natural approach would be to use the bloc’s seat share as the running variable and define treatment as attaining a seat majority, that is, $\mathbb{1}\{\text{seat share} \geq 0.5\}$. However, this strategy is poorly suited for a RDD as seat shares are inherently discrete because the council size is finite. This implies that the running variable has mass points and, in practice, often moves in coarse steps (for example, by $1/S$ where S is the number of seats), which undermines the continuity-based intuition of RDD at the cutoff. Moreover, the same seat share can correspond to quite different underlying vote shares, since seat allocations depend not only on a bloc’s own votes but on the full distribution of votes across parties under the electoral rule.

A second seemingly natural approach is to use vote share rather than seat share as the running variable since vote shares are more finely distributed and closer to continuous. This is the strategy used by Pettersson-Lidbom (2008), who also studies Swedish municipalities under proportional representation. In that framework, bloc vote share is used as the running variable and a common 50 percent cutoff is imposed, while treatment is defined in terms of political control based on seat majorities. However, this is problematic from an RD perspective because the running variable does not directly determine treatment assignment. In a valid RD setup, the cutoff in the running variable should correspond to the threshold at which treatment changes. Under proportional representation, however, crossing 50 percent of the vote does not mechanically imply crossing the threshold for a seat majority. A common 50 percent vote-share cutoff is therefore difficult to interpret as the relevant treatment threshold for majority control.

As emphasized by Fiva et al. (2018), the central identification problem in a proportional representation system is that bloc majority control is determined by seats, while the most natural continuous running variable is based on votes. Under Sweden's modified Sainte-Laguë rule, the vote share required to obtain a seat majority is election-specific rather than fixed at 50 percent, since it depends on council size and on the distribution of votes across parties. Bloc vote share therefore does not map one-to-one into bloc majority status, while bloc seat share, although more closely tied to treatment, is inherently discrete and thus poorly suited as a running variable.

To address this problem, I implement a fuzzy regression discontinuity design using an instrumental-variables approach. The purpose is to align the continuous vote-based running variable with the seat-majority condition that determines bloc control. For each municipality-election pair (m, t), I compute a municipality-specific majority threshold vote share, s^*_{mt}, defined as the minimum right-bloc vote share required to win at least $\lfloor S_{mt}/2 \rfloor + 1$ seats under modified Sainte-Laguë method conditional on the observed party structure in that election.[3] This yields an election-specific mapping from vote share into the seat-majority condition. The running variable is then defined as

$$X^*_{mt} \;=\; s^R_{mt} - s^*_{mt}, \tag{2}$$

where s^R_{mt} denotes the right bloc's vote share in municipality m and election year t. Hence, X^*_{mt} measures distance to the relevant seat-majority threshold implied by the electoral rule in that municipality-year. For transparency and replicability, Appendix A documents the full algorithm used to compute s^*_{mt} from party-level vote vectors and to implement the modified Sainte-Laguë seat-allocation procedure.

The construction of the running variable can be understood as a rule-based recovery of the relevant majority threshold in vote space. For each municipality-election, I use the observed party configuration and the modified Sainte-Laguë seat-allocation rule to

[3]The first divisor in the modified Sainte-Laguë allocation rule changed during the sample period. Before the 2018 election, the first divisor was 1.4; from the 2018 election onward, it was lowered to 1.2. The running-variable algorithm accounts for this institutional change by applying the year-specific first divisor when computing the minimum right-bloc vote share required for a seat majority.

determine the smallest aggregate right-bloc vote share that would have been sufficient to secure a seat majority. This is done by constructing counterfactual vote vectors that preserve the within-right and within-non-right vote distributions while shifting aggregate vote mass across the two groups. The resulting threshold is therefore municipality-specific and institutionally grounded, rather than imposed through a universal 50 percent cutoff. In that respect, the approach is similar in spirit to Folke (2014), who also derives electoral closeness directly from the PR seat-allocation rule. The difference is that Folke studies whether an individual party barely gains or loses a seat, whereas I study whether a bloc barely attains or falls short of an absolute seat majority.

3.1 First stage results

In this subsection, I compare the first-stage results using the running variable X^*_{mt}, defined as the vote distance to the relevant seat-majority threshold implied by the electoral rule in that municipality-year, with those based on X^{50}_{mt}, defined as the vote margin to a majority under a universal 50 percent cutoff, such that $X^{50}_{mt} = s^R_{mt} - 0.5$.

Table 3 reports the predictive performance of both running variables, including their accuracy in classifying right-bloc seat-majority outcomes.

Table 3: Classification performance of running variables for right-bloc seat majority

Proxy	N	TP	FP	TN	FN	Mismatch	Mismatch (%)	Accuracy (%)
$(X^{50}_{mt} = s^R_{mt} - 0.5)$	2,317	550	11	1,704	52	63	2.72%	97.28%
$(X^{\star}_{mt} = s^R_{mt} - s^{R\star}_{mt})$	2,317	576	2	1,713	26	28	1.21%	98.79%

Notes: TP (true positives) counts municipality-election-year observations where the proxy predicts a right-bloc seat majority and the right bloc indeed holds a seat majority. TN (true negatives) counts correct predictions of no seat majority. FP (false positives) are predicted majorities that are not realized, and FN (false negatives) are realized majorities predicted as non-majorities. Mismatch count equals FP+FN and the mismatch rate equals $(\text{FP} + \text{FN})/N$. Accuracy equals $(\text{TP} + \text{TN})/N$.

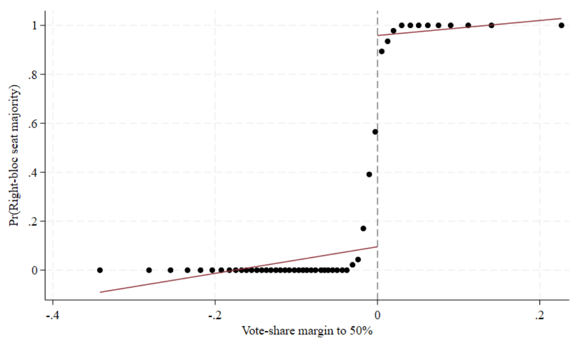
Both measures perform well in the full sample. However, the institutional *seat-majority vote margin* $X^*_{mt} = s^R_{mt} - s^*_{mt}$ delivers uniformly fewer classification errors than the conventional *vote-majority margin* $X^{50}_{mt} = s^R_{mt} - 0.5$, consistent with the fact that the relevant treatment threshold in proportional representation is determined by seats rather than votes.

Importantly, overall accuracy is not the key criterion for a regression discontinuity design. What matters is the strength and smoothness of the discontinuity in treatment take-up at the cutoff, i.e., the "first stage" relationship between crossing the threshold and the probability of right-bloc seat majority in a narrow neighborhood around $c = 0$. I therefore compare first-stage plots and local-polynomial estimates for X^{50}_{mt} and X^{*}_{mt} using MSE optimal bandwidth and triangular kernel in Table 4 and Figure 2.

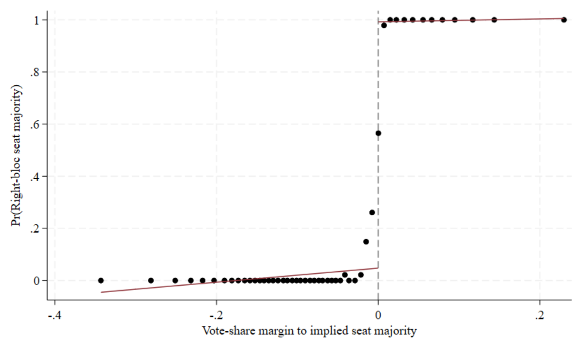
Table 4: First stage comparison: probability of right-bloc seat majority at the cutoff

	$X^{\star}_{mt}$	X^{50}_{mt}
RD effect (robust)	0.70937	0.15545
Robust z-stat	11.0727	1.2655
Robust p-value	0.000	0.206
95% CI	[0.563514, 0.80519]	[-0.063894, 0.29674]
N left / right	1741 / 576	1756 / 561
Effective N left / right	336 / 246	216 / 178
Bandwidth h left / right	0.048 / 0.048	0.032 / 0.032
Order p (est.)	1	1
Order q (bias)	2	2
Bandwidth selector	MSE-optimal	MSE-optimal
Kernel	Triangular	Triangular
VCE method	NN cluster	NN cluster

Notes: The table reports sharp RD estimates for the probability of right-bloc seat majority at the cutoff. $X^{\star}_{mt}$ denotes the seat-majority vote margin, while X^{50}_{mt} denotes the conventional vote-share margin to 50 percent. Standard errors are clustered at the municipality level. Both specifications use local linear estimation, triangular kernel, and MSE-optimal bandwidth selection.



(a) Running variable: X^{50}_{mt} (vote-share margin to 50%)



(b) Running variable: X^{*}_{mt} (seat-majority vote margin)

Figure 2: First stage: right-bloc seat majority under alternative running variables

The evidence indicates that X^{*}_{mt} delivers a substantially stronger first stage than X^{50}_{mt}. In Table 4, the discontinuity in the probability of a seat majority at the cutoff is markedly larger and estimated with much greater precision for X^*_{mt}, as reflected in the higher robust z-statistic and lower robust p-value. Figure 2 tells the same story visually: the jump at the cutoff is more pronounced and the relationship is cleaner around $c = 0$ when using X^*_{mt}, giving an appearance closer to a sharp change in treatment status. These results are robust to alternative bandwidth choices and kernel choices, see Appendix B.

Thus, I estimate the fuzzy RDD using a local instrumental-variables specification in which realized right-bloc seat-majority control, D_{mt}, is treated as endogenous and instrumented with the cutoff indicator $\mathbb{1}[X^*_{mt} \geq 0]$, where X^*_{mt} is the seat-majority-aligned running variable with cutoff at zero. The resulting two-stage model can be written as:

$$D_{mt} = \lambda + \kappa\, \mathbb{1}[X^*_{mt} \geq 0] + h(X^*_{mt}) + \xi_{mt}, \tag{3}$$

$$Y_{mt} = \gamma + \delta \widehat{D}_{mt} + g(X^*_{mt}) + \mu_{mt}. \tag{4}$$

Here, Y_{mt} denotes the outcome of interest, $\widehat{D}_{mt}$ is the fitted value from the first stage, $g(\cdot)$ and $h(\cdot)$ are local polynomial control functions in X^*_{mt}.

3.2 Manipulation testing

A discontinuity in the density of the running variable at the cutoff would indicate sorting, and would therefore cast doubt on the assumption that observations close to the threshold are as good as randomly assigned. While outright fraud is highly unlikely in Swedish municipal elections, strategic behaviour could still affect outcomes near the margin, for example via coordination or electoral alliances among smaller parties. Additionally, the running variable used in this paper is a constructed measure based on seat-allocation rules and a vote-rescaling procedure. As a result, it can potentially exhibit some discreteness and heaping, since seat allocations change only when the underlying quotient comparisons flip and because votes and seats are observed in integers. Such heaping can generate local spikes in the empirical distribution that are mechanical rather than evidence of strategic manipulation. To assess whether sorting is nevertheless present, I apply the manipulation test proposed by McCrary (2008). Figure 3 shows no visually apparent break in the density at the cutoff, suggesting no clear evidence of manipulation of the running variable.

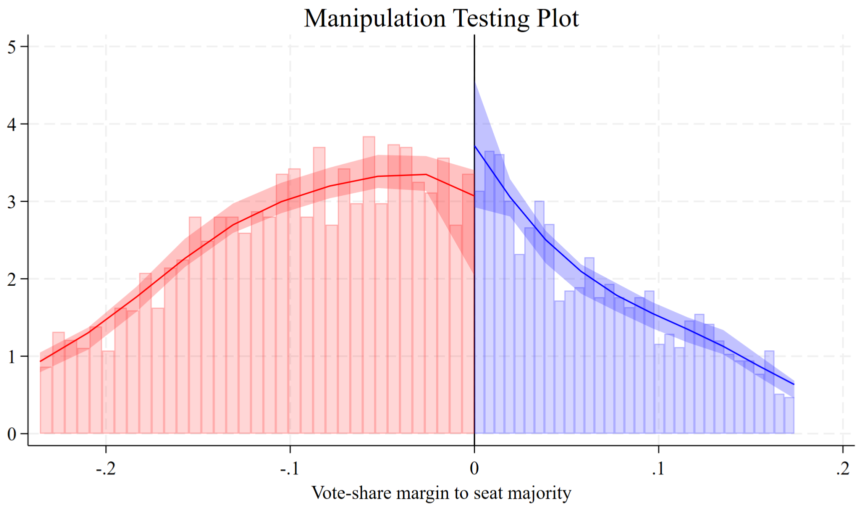


Figure 3: McCrary Test

Additionally, Appendix C reports a donut RDD sensitivity analysis in which I sequentially exclude observations in increasingly narrow windows around the cutoff. The estimates remain stable across donut sizes, offering no indication that local heaping or manipulation at the threshold is driving the results.

3.3 Covariate smoothness

A standard validity check in RD designs is that predetermined covariates are smooth at the cutoff (Lee and Lemieux, 2010; Imbens and Lemieux, 2008). I therefore test for discontinuities at $c = 0$ in a set of placebo covariates measured prior to the election at t. These include lagged political strength (right- and left-bloc seat shares from the previous election), lagged population, and several fiscal and socio-economic measures averaged over the previous electoral term (e.g., operating ratio, expenditures, intergovernmental grants, unemployment, employment, education expenditure, income per capita, crime, and the local tax rate). Each placebo is estimated using sharp RD specifications with the same running variable X^*_{mt}, triangular kernel, and MSE-optimal bandwidth selection. As reported in Appendix D, the estimated discontinuities are generally small and statistically insignificant, with no systematic pattern of pre-treatment breaks at the cutoff. This evidence supports the interpretation that municipalities near the threshold are comparable in predetermined characteristics.

3.4 Mismatches

As mentioned in Section 2, one limitation of the election data is the aggregation of local parties into a single “other” category (OVR). The running variable X^*_{mt} is constructed to approximate the vote-share margin to a right-bloc seat majority under the modified Sainte-Laguë rule. In a small number of close contests, this measure does not align perfectly with the realised seat-majority indicator, producing a small number of mismatches. These mismatches create imperfect compliance at the cutoff, since crossing the threshold in the running variable does not perfectly determine realised treatment status. This is why the baseline specification is estimated as a fuzzy RD rather than a sharp RD. A key source of these mismatches is the aggregation of local parties into a single “other” category, since party fragmentation matters for seat allocation under highest-quotient methods and collapsing several parties into one can shift simulated seat outcomes at the margin. Because mismatch status is generated by the running-variable construction, excluding mismatches is not an innocuous cleaning step, as it can disproportionately remove close elections central for identification. I therefore retain all observations in the baseline analysis and use the sample excluding mismatches only as a robustness check. In that restricted sample, the design becomes sharp by construction. The sharp RD results are reported alongside the main results in Section 4 and lead to the same substantive conclusion: the estimates remain statistically insignificant and provide no evidence of systematic discontinuities.

3.5 Decomposing political outcomes at the right-bloc majority cutoff

A key institutional feature of Swedish municipal politics is that, in many municipal councils, neither bloc secures an absolute majority. The prevalence of such councils has increased over time (see Table 1). In these cases, governing control may depend on support from parties outside the two main blocs, and the mapping from bloc seat shares to de facto policy control can be less direct.

This matters for the interpretation of the treatment indicator used in the main fuzzy RD design, where $T_i = 1$ and $T_i = 0$ otherwise. In this setup, the untreated category is not a pure “left-bloc majority” state. Instead, it combines two distinct political outcomes: (i) municipalities where the left bloc holds a majority and (ii) municipalities where neither bloc holds a majority. As a result, the first stage should be interpreted as a shift into right-bloc majority status from a mixed set of non-right-majority outcomes.

To better understand what the first stage represents in this institutional setting, I decompose the local effect of crossing the right-bloc majority cutoff into changes in the probabilities of three mutually exclusive and exhaustive political outcomes: a right-bloc majority, a left-bloc majority, and a council in which neither bloc holds a majority (undefined majority). Let

$$D^R_{mt} = \mathbb{1}\{\text{right-bloc majority}\}, \qquad D^L_{mt} = \mathbb{1}\{\text{left-bloc majority}\}, \qquad D^U_{mt} = \mathbb{1}\{\text{undefined majority}\},$$

so that, by construction,

$$D^R_{mt} + D^L_{mt} + D^U_{mt} = 1$$

for each municipality-election year observation.

I then estimate a separate sharp RD specification for each outcome indicator using the same running variable and the same manual RD specification:

$$D^j_{mt} = \alpha_j + \tau_j \, \mathbb{1}[X^*_{mt} \geq 0] + g_j(X^*_{mt}) + u_{j,mt}, \qquad j \in \{R, L, U\},$$

where X^*_{mt} is the right-bloc seat-majority running variable, $\mathbb{1}[X^*_{mt} \geq 0]$ indicates crossing the right-bloc majority cutoff, and $g_j(\cdot)$ is a local polynomial control function in the running variable. Table 5 reports the results for these political outcome indicators:

Table 5: RD decomposition of political outcomes at the right-bloc majority cutoff

Outcome indicator	RD effect (robust)	Robust z-stat	Robust p-value	95% CI
Right-bloc majority	0.70861	11.0727	0.000	[0.562930, 0.805079]
Undefined majority	-0.60210	-8.8009	0.000	[-0.719371, -0.457321]
Left-bloc majority	-0.10650	-2.0050	0.045	[-0.189167, -0.002150]
Sum of three RD effects	0.00001			

Notes: All three regressions are estimated with the same bandwidth, kernel, and polynomial orders, so that the estimated jumps τ_R, τ_L, and τ_U are directly comparable and reflect changes in the composition of political outcomes within the same local sample around the cutoff. The sample size is $N = 2317$ in all three models, with effective observations 342 (left of cutoff) and 249 (right of cutoff). Because the three political outcomes are mutually exclusive and exhaustive, the corresponding probability jumps should sum to zero, apart from rounding error.

The decomposition shows that crossing the right-bloc majority cutoff increases the probability of a right-bloc majority by about 70%. At the same time, the probability of an undefined majority outcome decreases by about 60%, while the probability of a left-bloc majority decreases by about 10%. Substantively, this implies that the first-stage shift into right-bloc majorities comes primarily from a reduction in municipalities where neither bloc holds a majority, and only to a smaller extent from municipalities with a left-bloc majority.

This result is useful for interpreting the main outcome regressions. In particular, it suggests that the estimated effects of crossing into a right-bloc majority should not be interpreted primarily as capturing ideological differences between the right and left blocs. Near the cutoff, the treatment mainly reflects a transition from fragmented or no-bloc-majority councils into a bloc-majority council. As a result, the estimated effects on fiscal and economic outcomes are likely to reflect, above all, changes in governing capacity, bargaining structure, and budget-passing conditions associated with majority status, rather than differences in ideology alone.

As a robustness check, I re-estimate the model on a restricted subsample that excludes all municipalities with undefined majorities. This isolates the more purely “ideological effect” by comparing only left-wing and right-wing governments, while also reducing concerns that the estimates are influenced by SD’s potential kingmaker role. These results are reported in Appendix F and remain similar to the baseline specification in terms of statistical insignificance.

4 Results and discussion

Table 6 reports the main regression results. Panel A presents estimates from the local linear specification, Panel B presents estimates from the local quadratic specification, and Panel C presents estimates from a local linear specification in which mismatches are dropped, thereby producing a sharp RD design.

Table 6: RDD results across outcomes and specifications

Panel A: Fuzzy RD ($p = 1$)

Outcome	First stage			Second stage			Effective obs.	Bandwidth h
	Estimate	SE	95% CI	Estimate	SE	95% CI		
Income per capita	0.69889***	0.08245	[0.560562, 0.837226]	-0.05284	0.05769	[-0.183681, 0.066686]	365	0.044
Crime reports	0.72963***	0.06652	[0.616418, 0.842847]	0.04795	0.10599	[-0.147343, 0.235764]	437	0.044
Education expenditure	0.77053***	0.05760	[0.672325, 0.868732]	-0.05124	0.05513	[-0.172883, 0.061658]	625	0.078
Operating costs	0.74878***	0.05784	[0.649565, 0.847998]	-0.03035	0.09038	[-0.172336, 0.121924]	514	0.053
Operating ratio	0.75132***	0.05671	[0.653680, 0.848970]	-0.05049	0.04513	[-0.153034, 0.041813]	525	0.054
Tax rate	0.64456***	0.07883	[0.507581, 0.781536]	-0.18781	0.46754	[-1.274320, 0.840833]	342	0.038
Tax revenue	0.73165***	0.06559	[0.620054, 0.843243]	-0.05694	0.07250	[-0.185273, 0.079262]	442	0.045
Government grants	0.73209***	0.06771	[0.620835, 0.843345]	855.23	2050.91	[-3400.49, 5238.49]	444	0.045
Employment ratio	0.74244***	0.05280	[0.650771, 0.834117]	-0.00766	0.00923	[-0.028516, 0.011548]	649	0.068

Panel B: Fuzzy RD ($p = 2$)

Outcome	First stage			Second stage			Effective obs.	Bandwidth h
	Estimate	SE	95% CI	Estimate	SE	95% CI		
Income per capita	0.73248***	0.06695	[0.618357, 0.846610]	-0.01992	0.06755	[-0.121869, 0.089993]	920	0.120
Crime reports	0.76670***	0.05077	[0.678664, 0.854739]	0.08709	0.07505	[-0.065140, 0.254258]	1171	0.131
Education expenditure	0.72608***	0.07067	[0.606252, 0.845917]	-0.05981	0.07947	[-0.210115, 0.093527]	900	0.117
Operating costs	0.73495***	0.06149	[0.630270, 0.839630]	-0.01499	0.12348	[-0.173404, 0.153171]	931	0.099
Operating ratio	0.74985***	0.05565	[0.653666, 0.846034]	-0.02687	0.04127	[-0.135447, 0.067898]	1037	0.113
Tax rate	0.67614***	0.06738	[0.560084, 0.792191]	-0.19727	0.38794	[-1.126510, 0.662362]	867	0.095
Tax revenue	0.73529***	0.06130	[0.630827, 0.839757]	-0.01905	0.09563	[-0.148267, 0.120904]	932	0.099
Government grants	0.75573***	0.05566	[0.662518, 0.848947]	755.31	1493.59	[-2748.38, 4659.79]	1089	0.119
Employment ratio	0.69683***	0.06530	[0.584733, 0.808931]	-0.00764	0.01432	[-0.032646, 0.018673]	924	0.101

Panel C: Sharp RD ($p = 1$)

Outcome	First stage			Second stage			Effective obs.	Bandwidth h
	Estimate	SE	95% CI	Estimate	SE	95% CI		
Income per capita	1.00000	0	[1.00000, 1.00000]	-0.02450	0.04468	[-0.105678, 0.059477]	558	0.071
Crime reports	1.00000	0	[1.00000, 1.00000]	0.06638	0.04381	[-0.025313, 0.197835]	790	0.085
Education expenditure	1.00000	0	[1.00000, 1.00000]	-0.06686	0.04631	[-0.176710, 0.026808]	605	0.078
Operating costs	1.00000	0	[1.00000, 1.00000]	-0.03612	0.04851	[-0.118674, 0.053332]	992	0.110
Operating ratio	1.00000	0	[1.00000, 1.00000]	-0.02178	0.03054	[-0.100086, 0.046680]	712	0.074
Tax rate	1.00000	0	[1.00000, 1.00000]	-0.14415	0.18739	[-0.676501, 0.295175]	705	0.079
Tax revenue	1.00000	0	[1.00000, 1.00000]	-0.03630	0.05382	[-0.113353, 0.055312]	741	0.080
Government grants	1.00000	0	[1.00000, 1.00000]	252.76	921.47	[-2353.35, 3119.10]	728	0.077
Employment ratio	1.00000	0	[1.00000, 1.00000]	-0.00676	0.00812	[-0.023099, 0.009329]	713	0.077

Notes: The table reports RD estimates for three specifications: fuzzy RD with $p = 1$, fuzzy RD with $p = 2$, and sharp RD after dropping mismatches. All specifications use the running variable X^*_{mt}, cutoff $c = 0$, triangular kernel, and MSE-optimal bandwidth selection. Standard errors are clustered at the municipality level. Robust 95% confidence intervals are shown in brackets. Effective observations refer to observations within the selected bandwidth. In Panel C, the first stage equals one by construction. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Across all specifications, the estimated effects are generally modest and statistically insignificant, as shown by the second stage estimate. The overwhelmingly null findings in this paper should not be interpreted as evidence that political parties never matter in local politics. Rather, they suggest that in Swedish municipalities during more recent periods, close changes in bloc majority control do not generate large or systematic discontinuities in the broad fiscal and economic outcomes studied here. This conclusion is robust to alternative bandwidth choices (Appendix H) and to further robustness checks. Specifically, the results remain similar when the sample is restricted to the pre-2018 period, when the cordon sanitaire against the Sweden Democrats (SD) was more clearly intact (Appendix E), and when municipalities with undefined majorities are excluded to sharpen the ideological contrast between left-wing and right-wing governments (Appendix F). This suggests that the baseline findings are not driven by cross-bloc contamination, SD-related political ambiguity, or noise stemming from cases in which bloc control is less clearly defined.

To the best of my knowledge, Pettersson-Lidbom (2008) is the only previous study of majority bloc control in Swedish municipalities, and he reports significant effects on outcomes such as employment, tax rates, and size of government. However, there are important differences between his study and the present one, both in terms of methodology and historical setting, that may help explain the contrasting results. Notably, Pettersson-Lidbom studies the period 1974 to 1994, whereas this paper examines 1993 to 2022. One possible interpretation is that partisan effects may have weakened over time as Swedish local politics became more constrained and potentially more convergent, for example in the context of EU membership, stricter budgetary frameworks, and broader institutional changes in the modern period. To assess whether partisan effects were stronger in the earlier part of the sample, I split the full sample in half and re-estimate the model for the first subperiod, 1993 to 2006; see Appendix I. The estimated effects remain statistically insignificant also in this earlier subsample. This suggests that the null findings are not driven simply by the inclusion of more recent years, although two possibilities remain: first, that the period 1993 to 2006 may already belong to a more constrained and politically convergent era than the one studied by Pettersson-Lidbom, and second, that the divergence in results is due to the substantial methodological differences between the present design and that of Pettersson-Lidbom (2008).

The contrast with Folke (2014), who examines the partisan effects of individual party representation in Swedish municipalities, is also informative. Folke finds strong effects of individual party representation on immigration and environmental policy, but not on the local tax rate, which is consistent with my findings, and argues that secondary policy areas may be shaped more by coalition bargaining than by simple bloc majorities. This suggests that bloc majority control may be too coarse a political treatment to capture many of the relevant policy mechanisms in proportional representation systems. In such settings, policy may depend less on whether the left or right bloc narrowly secures a majority and more on the relative strength of specific parties, coalition bargaining, and issue-specific alliances. On this interpretation, the null results reported here may indicate that bloc majorities are not the central political margin for the outcomes considered.

Another possible explanation for why earlier studies report stronger partisan effects is that several influential contributions relied more heavily on broader identifying windows, high-order polynomial controls, and more global functional-form assumptions than the modern local RD approach used here. In my data, the descriptive plots in Figure 1 indicate that, although broader left-right differences are visible in the tails of the running-variable distribution, there is little evidence of a sharp local discontinuity at the cutoff itself. This in turn suggests that more global specifications may be more likely to generate statistically significant results by capturing systematic differences between municipalities away from the cutoff, whereas the present design is deliberately focused on the local causal comparison at the threshold.

An important qualification is that the increased probability of a right-bloc majority at the cutoff is driven largely by a reduction in undefined majorities rather than by a direct shift from left-bloc to right-bloc control. The estimates should therefore not be interpreted as a pure ideological left-versus-right comparison. Instead, they reflect the effect of right-bloc majority control relative to a broader set of political outcomes, including cases in which no bloc holds a clearly defined majority. This may help explain the muted results, since the relevant contrast at the threshold is often between right-bloc majority government and more fragmented or less clearly defined governing situations rather than between clearly left-wing and clearly right-wing governments. However, this is unlikely to be the full explanation, as the results remain similarly small and statistically insignificant when municipalities with undefined majorities are excluded, thereby yielding a stricter left-versus-right comparison.

At the same time, the null results are not inconsistent with theory. Classical models of electoral competition, including Hotelling-style spatial competition, Downsian convergence, and probabilistic voting, imply that parties seeking broad electoral support have incentives to moderate their positions and move toward the preferences of pivotal voters. From this perspective, close changes in bloc majority control need not generate large differences in broad municipal outcomes such as taxes, expenditures, employment, and income. This interpretation is reinforced by empirical research on Swedish politics. Wennström (2019) finds evidence of policy convergence between the Social Democrats and the Moderate Party in the Swedish context, while Rydgren and van der Meiden

(2019) argue that Swedish politics has become less structured by the traditional socioeconomic left-right dimension and more shaped by newer sociocultural conflicts, alongside greater convergence among mainstream parties. In that setting, close changes in bloc control may have limited effects on broad municipal fiscal and economic outcomes even if political conflict remains intense on other dimensions.

A final possible explanation is more pragmatic in nature. The time horizon examined here may simply be too short for changes in bloc control to translate into measurable differences in broad economic outcomes. Many municipal policies operate with long implementation lags, and their effects on variables such as income, employment, taxation, or expenditure may emerge only gradually. This may be especially relevant given the absence of a statistically significant incumbency advantage in the data (Appendix G), which points to relatively frequent turnover in political control. If governments switch often, parties may lack both the time and the continuity required to leave a clear imprint on aggregate outcomes. On this interpretation, the null findings may reflect not only ideological convergence, but also the practical difficulty of generating measurable economic differences within short and politically unstable governing periods.

5 Conclusion

This paper shows that, once bloc-majority closeness is measured in a way that is consistent with proportional representation, large partisan effects are not evident in Swedish municipalities in the post-1990s period. Across the fiscal and economic outcomes considered, the estimates provide little evidence that right-bloc majority status generates systematic discontinuities, and the results remain stable across alternative specifications and sample restrictions. These findings do not imply that politics never matters at the local level, but rather that close shifts in bloc majority control do not appear to be the central margin through which broad municipal economic outcomes are determined in modern Sweden. Taken together, the results point instead to a setting in which policy convergence, coalition bargaining, institutional constraints, and short governing horizons may limit the extent to which narrow changes in political control translate into measurable economic differences. More broadly, the paper highlights the importance of aligning empirical design with the logic of proportional representation when studying partisan effects in multi-party systems.

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A Computation of running variable

This appendix documents the construction of the running variable used in the fuzzy RDD when parties are grouped into three blocs: a right bloc, a left bloc, and an "undefined" bloc. The purpose is to define bloc vote shares and seat-majority outcomes in a way that is consistent with the electoral system, and then to compute, for each municipality-election year (m, t), the minimum right-bloc vote share required to obtain a seat majority under the modified Sainte-Laguë rule. This municipality-specific threshold is then used to construct the main running variable in the empirical analysis. I also define an indicator for "undefined majorities," where neither the right nor the left bloc holds a seat majority.

A.1 Inputs and notation

For each municipality-year (m, t), the inputs are: (i) the set of parties P_{mt} observed in the election data, (ii) party-level vote totals $\{v_{pmt}\}_{p \in P_{mt}}$, (iii) party-level seat totals $\{s^{obs}_{pmt}\}_{p \in P_{mt}}$ from the data, and (iv) total council seats S_{mt}.

I define three mutually exclusive bloc sets:

$$R = \{M, C, L, KD\}, \qquad L = \{S, MP, V\}, \qquad U = \{SD, OVR\}.$$

Let

$$V_{mt} = \sum_{p \in P_{mt}} v_{pmt}$$

denote total votes. Define observed bloc vote shares as

$$s^R_{mt} = \frac{\sum_{p \in R \cap P_{mt}} v_{pmt}}{V_{mt}}, \qquad s^L_{mt} = \frac{\sum_{p \in L \cap P_{mt}} v_{pmt}}{V_{mt}}, \qquad s^U_{mt} = \frac{\sum_{p \in U \cap P_{mt}} v_{pmt}}{V_{mt}}.$$

Let the seat-majority threshold be

$$S^{maj}_{mt} = \left\lfloor \frac{S_{mt}}{2} \right\rfloor + 1.$$

In the empirical data, realized bloc seat totals are computed as

$$S^{R,obs}_{mt} = \sum_{p \in R \cap P_{mt}} s^{obs}_{pmt}, \qquad S^{L,obs}_{mt} = \sum_{p \in L \cap P_{mt}} s^{obs}_{pmt}, \qquad S^{U,obs}_{mt} = \sum_{p \in U \cap P_{mt}} s^{obs}_{pmt}.$$

Define seat-majority indicators

$$D_{mt}^{R} = 1\{S_{mt}^{R,obs} \geq S_{mt}^{maj}\}, \qquad D_{mt}^{L} = 1\{S_{mt}^{L,obs} \geq S_{mt}^{maj}\}.$$

Finally, define an “undefined majority” indicator as

$$D_{mt}^{U} = 1\{D_{mt}^{R} = 0 \text{ and } D_{mt}^{L} = 0\},$$

that is, neither the right nor the left bloc holds a seat majority.

A.2 Modified Sainte-Laguë seat allocation

Given any party vote vector $v = (v_p)_{p \in P}$ and total seats S, seats are allocated sequentially by the modified Sainte-Laguë method. For each party p, define divisors

$$d_p(0) = d_{1t}, \qquad d_p(k) = 2k + 1 \quad \text{for } k \geq 1,$$

where the first divisor is year-specific:

$$d_{1t} = \begin{cases} 1.4, & t < 2018, \\ 1.2, & t \geq 2018. \end{cases}$$

Thus, the algorithm applies the divisor in force at the time of each municipal election when computing the seat allocation and the corresponding right-bloc majority threshold. Initialize seat counts $s_p = 0$ for all parties. For $\ell = 1, \ldots, S$, compute quotients

$$q_p(\ell) = \frac{v_p}{d_p(s_p)},$$

award seat ℓ to the party with the largest quotient, and increment that party's seat count by one. Ties are broken deterministically using the alphabetical ordering of party codes. Since party codes are sorted before seat allocation, a tied quotient is assigned to the first party in this fixed ordering. This ensures replicability.

This procedure maps any party-level vote vector into a corresponding seat allocation under the electoral rule.

A.3 Counterfactual vote scaling

To determine the vote share required for a right-bloc seat majority, the algorithm constructs counterfactual vote vectors that preserve the set of parties and the within-group vote distribution while shifting aggregate vote mass between the right bloc and all non-right parties.

Let

$$V_{mt}^{R} = \sum_{p \in R \cap P_{mt}} v_{pmt}$$

denote observed right-bloc votes, and let

$$V_{mt}^{-R} = V_{mt} - V_{mt}^{R}$$

denote votes among all non-right parties, including both the left bloc and the undefined bloc.

For a candidate right-bloc vote share $s \in [0, 1]$, I define scaling factors

$$a_R(s) = \frac{sV_{mt}}{V_{mt}^{R}}, \qquad b_R(s) = \frac{(1-s)V_{mt}}{V_{mt}^{-R}}.$$

The counterfactual votes are then given by

$$\tilde{v}_{pmt}^{R}(s) = \begin{cases} a_R(s) v_{pmt}, & p \in R, \\ b_R(s) v_{pmt}, & p \notin R. \end{cases}$$

This transformation keeps total votes fixed at V_{mt}, sets the right bloc's aggregate vote share equal to s, and preserves the within-right and within-non-right vote composition proportional to the observed composition.

A.4 Threshold computation by bisection

Define $g_{mt}^{R}(s)$ as the number of right-bloc seats obtained when the modified Sainte-Laguë allocation is applied to $\tilde{v}_{mt}^{R}(s)$ with S_{mt} total seats. The algorithm computes the minimal right-bloc vote share

$$s_{mt}^{R*} = \inf\{s \in [0, 1] : g_{mt}^{R}(s) \geq S_{mt}^{maj}\}.$$

Thus, s_{mt}^{R*} is the smallest right-bloc vote share that would be sufficient to obtain a seat majority in municipality m at election t, given the observed party configuration and council size.

In practice, s^{R*}_{mt} is obtained numerically by bisection. Starting from the observed right-bloc vote share s^{R}_{mt}, the algorithm repeatedly checks whether a candidate value of s yields a right-bloc seat majority under the modified Sainte-Laguë rule and narrows the interval until the threshold is approximated with high precision.

A.5 Running variable

Using the threshold s^{R*}_{mt}, I define the running variable as

$$X^{*}_{mt} = s^{R}_{mt} - s^{R*}_{mt}.$$

This variable measures the distance between the observed right-bloc vote share and the minimum right-bloc vote share required for a seat majority under the electoral rule in that municipality-year.

The corresponding predicted treatment indicator is

$$Z_{mt} = 1\{X^{*}_{mt} \geq 0\}.$$

In the empirical analysis, this is the main assignment variable used in the fuzzy RD design.

A.6 Interpretation and assumptions

The construction of X^{*}_{mt} is designed to express the seat-majority condition in vote-share space rather than relying on a universal 50 percent cutoff. The threshold therefore varies across municipality-elections depending on the observed party structure and the number of council seats.

This approach rests on some assumptions. First, when aggregate right-bloc support is varied, the relative vote distribution within the right bloc is held fixed. Second, all non-right parties are scaled proportionally as a single residual group, so the relative distribution between left-bloc and undefined-bloc parties is also held fixed. Third, total votes are kept constant, so the exercise should be interpreted as reallocating vote mass across blocs rather than changing turnout.

A further limitation is that the category OVR aggregates multiple local parties into a single residual bloc component. Under a highest-quotient rule such as Sainte-Laguë, party fragmentation matters for seat allocation. As a result, the computed threshold should be interpreted as an approximation to the institutional seat-majority threshold rather than a perfect reconstruction in every case. This is also why a small number of municipality-election observations may display mismatches between predicted majority status and realized majority status. Such cases motivate the fuzzy, rather than sharp, RD specification.

B Bandwidth sensitivity analysis and kernel choice

Table 7: First-stage RD estimates across bandwidth choices

Bandwidth multiple	Point estimate	Eff. obs	Robust S.E.	95% CI	p-value	z
$0.5h^*$	0.67991	303	0.09253	[0.545871, 0.943002]	0.000	7.3480
$0.75h^*$	0.67322	428	0.08514	[0.519877, 0.862585]	0.000	7.9064
$1.25h^*$	0.72572	737	0.07564	[0.522906, 0.791372]	0.000	9.5950
$1.5h^*$	0.75079	889	0.06979	[0.543633, 0.785843]	0.000	10.7581
$2h^*$	0.79236	1154	0.05972	[0.589405, 0.793727]	0.000	13.2677

Notes: Outcome is realized right-bloc seat majority. Running variable is X^*_{mt}. Cutoff $c = 0$. Local polynomial order $p = 1$ with bias order $q = 2$. Triangular kernel. Standard errors are clustered at the municipality level using `vce(nncluster kommun_id)` in `rdrobust`. Bandwidth multiples are based on the MSE-optimal bandwidth $h^* = 0.048$ from the baseline specification.

Table 8: Kernel sensitivity of RD estimates for right-bloc seat majority at the cutoff

Kernel	Point estimate	z-stat	p-value	Eff. obs	Eff. obs (L/R)
Triangular	0.69779	11.8464	0.000	582	336 / 246
Epanechnikov	0.69944	12.3616	0.000	563	320 / 243
Uniform	0.71681	13.8091	0.000	512	286 / 226

Notes: Outcome is realized right-bloc seat majority. The running variable is X^*_{mt}, the seat-majority vote margin. The cutoff is $c = 0$. Bandwidths are selected using `bwselect(mserd)`. All specifications use local linear estimation with polynomial order $p = 1$ and bias order $q = 2$. Standard errors are clustered at the municipality level using `vce(nncluster kommun_id)` in `rdrobust`. Effective observations refer to the number of observations within the selected bandwidth. The estimates show that the first stage remains strong and statistically significant across kernel choices.

C Donut RDD sensitivity analysis

To assess whether the results are sensitive to observations located arbitrarily close to the cutoff, I implement a donut RDD. This approach excludes a small neighbourhood around the threshold, which can mitigate concerns about local heaping, rounding, or strategic sorting exactly at the cutoff. Specifically, letting X denote the running variable and $c = 0$ the cutoff, I drop all observations with $|X| \leq \delta$ for a grid of donut radii $\delta > 0$, and then re-estimate the RD effect on the remaining sample.

Table 9: Donut RD sensitivity: excluding observations close to the cutoff

Donut δ	Deleted	N	h	Robust coef.	z	p
0.001	17	2300	0.048	0.65593	9.4224	0.000
0.002	29	2288	0.049	0.65021	8.9398	0.000
0.005	63	2254	0.049	0.67394	9.1619	0.000
0.010	122	2195	0.062	0.81679	14.4867	0.000

Notes: Each row drops observations with $|X| \leq \delta$, where X is X^*_{mt} and the cutoff is $c = 0$. Outcome is realized right bloc seat majority. Bandwidths h are selected by `bwselect(mserd)` after applying the donut restriction. Kernel is triangular; local polynomial order $p = 1$ with bias order $q = 2$; NN variance estimator. Reported estimates are the robust bias-corrected RD effects from `rdrobust`.

D Covariate smoothness

Table 10: Covariate smoothness tests at the right-bloc cutoff (sharp RD)

(1)	(2)	(3)	(4)
Operating ratio	**Operating costs**	**Government grants**	**Unemployment ratio**
0.01689	−0.02716	−371.23	0.00773
[−0.054928, 0.112732]	[−0.128781, 0.075167]	[−2678.66, 2015.44]	[−0.009037, 0.023244]
$p = 0.499$	$p = 0.606$	$p = 0.782$	$p = 0.388$
Employment ratio	**Right bloc seat share**	**Left bloc seat share**	**Population**
−0.00773	−0.00456	0.00670	−0.05465
[−0.023244, 0.009037]	[−0.021121, 0.013054]	[−0.022777, 0.032095]	[−0.380818, 0.265738]
$p = 0.388$	$p = 0.644$	$p = 0.739$	$p = 0.727$
Education expenditure	**Income per capita**	**Crime reports**	**Tax rate**
−0.01697	−0.01024	0.07827	−0.07763
[−0.094270, 0.075771]	[−0.090129, 0.079849]	[−0.023035, 0.217973]	[−0.678691, 0.476189]
$p = 0.831$	$p = 0.906$	$p = 0.113$	$p = 0.731$

Notes: Each cell reports a sharp RD estimate from `rdrobust` for a predetermined covariate measured prior to the election at t; lagged seat shares and lagged population are measured before treatment, while the remaining variables are averaged over the previous electoral term. The running variable is X^*_{mt} with cutoff $c = 0$. All specifications use local linear estimation with polynomial order $p = 1$, bias order $q = 2$, triangular kernel, and MSE-optimal bandwidth selection. Standard errors are clustered at the municipality level using `vce(nncluster kommun_id)` in `rdrobust`. Robust 95% confidence intervals are reported in brackets and robust p-values are reported below each estimate.

The covariate smoothness tests provide no evidence of systematic discontinuities in predetermined characteristics at the cutoff. None of the reported placebo covariates is statistically significant at conventional levels when standard errors are clustered at the municipality level.

E Swedish Democrats robustness check

Table 11: Fuzzy RD robustness check excluding years 2018 and later (pre-2018 sample)

Outcome	First stage	Second stage	p-value	95% CI	N	Eff. obs	h
Crime reports	0.72105*** (10.1610)	0.06754 (0.8126)	0.416	[-0.112091, 0.270861]	1447	412	0.051
Education expenditure	0.67780*** (7.7681)	-0.12931** (-2.1607)	0.031	[-0.282124, -0.013744]	1154	330	0.050
Income per capita	0.68321*** (8.2058)	-0.02830 (-0.5529)	0.580	[-0.147037, 0.082332]	1160	332	0.050
Operating costs	0.73633*** (11.2529)	-0.08960 (-1.4674)	0.142	[-0.234591, 0.033717]	1449	469	0.056
Operating ratio	0.77922*** (14.9318)	-0.03249 (-0.8379)	0.402	[-0.135750, 0.054445]	1449	619	0.076
Tax revenue	0.76245*** (13.3746)	-0.03530 (-0.7081)	0.479	[-0.147576, 0.069240]	1449	553	0.068
Tax rate	0.71357*** (9.6794)	-0.53892* (-1.6926)	0.091	[-1.354830, 0.099165]	1160	398	0.060
Employment ratio	0.74244*** (14.0624)	-0.00766 (-0.8301)	0.406	[-0.028516, 0.011548]	1734	649	0.068
Government grants	0.74070*** (11.1182)	-1753.20 (-1.2105)	0.226	[-5431.55, 1283.91]	1449	484	0.058

Notes: Entries report fuzzy RD estimates from `rdrobust` using the running variable X^*_{mt}, cutoff $c = 0$, triangular kernel, local linear estimation, and MSE-optimal bandwidth selection. The sample excludes years 2018 and later. Standard errors are clustered at the municipality level using `vce(nncluster kommun_id)`. The first-stage and second-stage columns report point estimates with robust z-statistics in parentheses. Effective observations refer to observations within the selected bandwidth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The results are broadly consistent with the main specification. Most estimated treatment effects remain statistically insignificant in the pre-2018 sample, suggesting that the baseline findings are not driven by the post-2018 period, when cooperation patterns involving the Sweden Democrats became more politically relevant. One exception is education expenditure, where the estimate is negative and statistically significant at the 5 percent level. Since this outcome is measured in logarithmic form, the point estimate of -0.129 implies approximately 12 percent lower education expenditure per student under right-bloc majority control in this restricted sample. This result should be interpreted cautiously, however, since it appears in one outcome among several robustness checks and is not statistically significant in the main full-sample specification. I therefore treat it as suggestive evidence of possible pre-2018 heterogeneity in education spending, rather than as evidence that changes the overall conclusion.

F Excluding undefined majoritites

Table 12: Fuzzy RD robustness check excluding municipalities with undefined majorities (defined-majority sample)

Outcome	First stage	Second stage	p-value	95% CI	N	Eff. obs	h
Crime reports	0.47289*** (3.8983)	0.05043 (0.7720)	0.440	[-0.156110, 0.359002]	1120	485	0.087
Education expenditure	0.37594*** (2.8113)	0.10726 (0.7400)	0.459	[-0.185436, 0.410420]	883	386	0.089
Income per capita	0.35014** (2.4841)	0.17377 (1.5844)	0.113	[-0.048416, 0.456866]	887	351	0.079
Operating costs	0.56466*** (5.1096)	0.12603 (1.5087)	0.131	[-0.040392, 0.310443]	1121	622	0.114
Operating ratio	0.43711*** (2.8441)	-0.01972 (-0.1568)	0.875	[-0.213319, 0.181714]	1121	444	0.078
Tax revenue	0.53333*** (4.6424)	0.09150 (1.2547)	0.210	[-0.053927, 0.245818]	1121	580	0.105
Tax rate	0.48171*** (4.6864)	0.37923 (0.7462)	0.456	[-0.758479, 1.691020]	962	569	0.123
Employment ratio	0.47431*** (3.4913)	0.00551 (0.2383)	0.812	[-0.033228, 0.042428]	1286	519	0.082
Government grants	0.48263*** (3.3052)	2661.20 (1.1332)	0.257	[-2689.35, 10062.20]	1121	497	0.090

Notes: Entries report fuzzy RD estimates from `rdrobust` using the running variable X^*_{mt}, cutoff $c = 0$, triangular kernel, local linear estimation, and MSE-optimal bandwidth selection. The sample excludes municipalities where neither bloc holds a seat majority. Standard errors are clustered at the municipality level. The first-stage and second-stage columns report point estimates with robust z-statistics in parentheses. Effective observations refer to observations within the selected bandwidth. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The results are similar to the baseline findings. All second-stage estimates are statistically insignificant in the defined-majority sample, suggesting that the main results are not driven by municipalities where neither bloc holds a majority. The first stage remains positive and statistically significant across outcomes, although it is weaker than in the full sample. This is expected, since excluding undefined majorities removes many of the observations that contribute to the shift from non-right majority to right-bloc majority at the cutoff.

G Incumbency advantage

Table 13: Effect of right-bloc majority on right-bloc incumbency at the next election

	Fuzzy RD (p=1)
First stage	0.76600***
	(0.05600)
	[0.67739, 0.85461]
Second stage	-0.01252
	(0.02597)
	[-0.29394, 0.17788]
Total observations (N)	2027
Effective observations (within h)	812
Bandwidth h (mserd)	0.072
Polynomial order p	1
Kernel	Triangular

Notes: Robust 95% confidence intervals are reported in brackets. Robust standards errors are reported in parenthesis. The table reports fuzzy RD estimates from `rdrobust` with cutoff $c = 0$, running variable X^*_{mt}, treatment status is realized right bloc seat majority, and outcome realize right bloc seat majority at the subsequent election. Standard errors are clustered atthe municipality level. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

This regression tests for an incumbency advantage by examining whether a right-bloc majority obtained in a close election increases the probability that the right bloc also wins the subsequent election. In effect, this compares municipalities where the right bloc barely won a majority to municipalities where it barely failed to do so, and uses this quasi-random variation to estimate the causal effect of current right-bloc control on the probability of future right-bloc control. A positive second-stage estimate would indicate incumbency advantage, whereas a null estimate would suggest little or no such persistence.

H Bandwidth sensitivity analysis

Table 14 presents the bandwidth sensitivity analysis for all outcome variables, illustrating how the estimated treatment effects and effective sample sizes vary with alternative bandwidth choices around the baseline MSE-optimal bandwidth.

Table 14: Bandwidth sensitivity analysis across outcome variables

Bandwidth multiple	h	Point estimate	Std. error	95% CI	Eff. obs.
Panel A: Income per capita					
$0.5h^*$	0.022	-0.10817	0.08729	[-0.371792, 0.083757]	198
$0.75h^*$	0.033	-0.07914	0.05557	[-0.333529, 0.052819]	270
$1.25h^*$	0.055	-0.02707	0.01933	[-0.235834, 0.039259]	449
$1.5h^*$	0.066	-0.01286	0.01086	[-0.197447, 0.048749]	535
$2h^*$	0.088	-0.02310	0.05562	[-0.125306, 0.081485]	695
Panel B: Crime reports					
$0.5h^*$	0.022	0.05782	0.07779	[-0.211556, 0.470040]	235
$0.75h^*$	0.033	0.04506	0.08823	[-0.212104, 0.361604]	324
$1.25h^*$	0.055	0.05194	0.13726	[-0.173093, 0.255928]	537
$1.5h^*$	0.066	0.07387	0.27059	[-0.163549, 0.216493]	646
$2h^*$	0.088	0.06980	0.08737	[-0.095760, 0.227540]	836
Panel C: Tax rate					
$0.5h^*$	0.0190	-0.29739	0.74909	[-2.35239, 1.55992]	184
$0.75h^*$	0.0285	-0.18054	0.43577	[-1.99354, 1.29782]	276
$1.25h^*$	0.0475	-0.16010	0.54923	[-1.36050, 1.00824]	435
$1.5h^*$	0.0570	-0.14005	0.37031	[-1.22798, 0.830715]	526
$2h^*$	0.0770	-0.08026	0.15781	[-1.03016, 0.605702]	708
Panel D: Operating costs					
$0.5h^*$	0.0265	-0.07645	0.15302	[-0.365450, 0.216978]	274
$0.75h^*$	0.0398	-0.05612	0.08339	[-0.314536, 0.153743]	387
$1.25h^*$	0.0663	-0.02020	0.02889	[-0.224919, 0.106622]	647
$1.5h^*$	0.0795	-0.01853	0.03702	[-0.185956, 0.110294]	763
$2h^*$	0.1060	-0.03122	0.13723	[-0.138832, 0.109954]	991
Panel E: Tax revenue					
$0.5h^*$	0.0225	-0.10301	0.11999	[-0.341734, 0.133559]	240
$0.75h^*$	0.0338	-0.07749	0.06445	[-0.326665, 0.078278]	332
$1.25h^*$	0.0563	-0.03292	0.02500	[-0.243559, 0.047831]	555
$1.5h^*$	0.0675	-0.02176	0.01939	[-0.206755, 0.056218]	659
$2h^*$	0.0900	-0.03983	0.11399	[-0.133438, 0.093063]	849
Panel F: Operating ratio					
$0.5h^*$	0.0270	-0.05803	0.09290	[-0.183200, 0.180970]	278

Continued on next page

Table 14 continued

Bandwidth multiple	h	Point estimate	Std. error	95% CI	Eff. obs.
$0.75h^*$	0.0405	-0.06728	0.07232	[-0.188690, 0.094800]	400
$1.25h^*$	0.0675	-0.02921	0.05447	[-0.187219, 0.026321]	659
$1.5h^*$	0.0810	-0.01362	0.04983	[-0.167197, 0.028143]	774
$2h^*$	0.1080	-0.01584	0.04356	[-0.114552, 0.056200]	1007
Panel G: Government grants					
$0.5h^*$	0.0225	1193.80	4088.92	[-6439.63, 9588.93]	240
$0.75h^*$	0.0338	1043.30	3407.84	[-5193.70, 8165.02]	332
$1.25h^*$	0.0563	644.80	2457.06	[-3561.90, 6069.78]	555
$1.5h^*$	0.0675	403.05	2164.82	[-3162.85, 5323.23]	659
$2h^*$	0.0900	461.52	1791.57	[-2754.25, 4268.72]	849
Panel H: Education expenditure					
$0.5h^*$	0.0390	-0.09213	0.13115	[-0.391367, 0.122721]	312
$0.75h^*$	0.0585	-0.05907	0.09836	[-0.302482, 0.083097]	477
$1.25h^*$	0.0975	-0.05776	0.06884	[-0.191795, 0.078058]	762
$1.5h^*$	0.1170	-0.05606	0.06048	[-0.178027, 0.059055]	900
$2h^*$	0.1560	-0.05020	0.05048	[-0.160660, 0.037219]	1080
Panel I: Employment ratio					
$0.5h^*$	0.034	-0.01167	0.01908	[-0.046979, 0.027814]	333
$0.75h^*$	0.051	-0.00811	0.01568	[-0.046095, 0.015369]	486
$1.25h^*$	0.085	-0.00861	0.01165	[-0.030318, 0.015340]	800
$1.5h^*$	0.102	-0.00862	0.01026	[-0.027982, 0.012224]	932
$2h^*$	0.136	-0.00663	0.00863	[-0.026678, 0.007152]	1187

Notes: Bandwidths are manual choices intended as multiples of the MSE-optimal bandwidth from the baseline local-linear specification. Standard errors are clustered at the municipality level. Robust 95% confidence intervals are shown in brackets. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

I Estimates from subsample 1993–2006

Table 15: Fuzzy RD estimates, subsample 1993–2006

	First stage			Second stage			Effective obs.	Bandwidth h
Outcome	Estimate	SE	95% CI	Estimate	SE	95% CI		
Income per capita	0.76377***	0.07052	[0.62555, 0.90198]	-0.00009	0.04434	[-0.08807, 0.08574]	301	0.090
Crime reports	0.81601***	0.04959	[0.71882, 0.91320]	-0.04927	0.08702	[-0.21001, 0.13111]	457	0.095
Education	0.70043***	0.09361	[0.51695, 0.88390]	-0.02900	0.06017	[-0.16597, 0.06989]	239	0.070
Government expenditure	0.78975***	0.05755	[0.67696, 0.90255]	0.05310	0.05692	[-0.05508, 0.16806]	400	0.078
Operating ratio	0.73980***	0.07495	[0.59290, 0.88671]	-0.08101	0.07030	[-0.24774, 0.02784]	301	0.057
Tax rate	0.67730***	0.09832	[0.48459, 0.87002]	0.21492	0.53739	[-0.96724, 1.13934]	214	0.060
Tax revenue	0.80980***	0.05143	[0.70900, 0.91060]	0.01509	0.05649	[-0.09101, 0.13045]	444	0.090
Government grants	0.79573***	0.05570	[0.68656, 0.90490]	1530.10	1430.27	[-1065.29, 4541.36]	410	0.081
Employment ratio	0.72951***	0.05793	[0.61597, 0.84306]	-0.00317	0.01287	[-0.02667, 0.02379]	429	0.065

Notes: The table reports fuzzy RD estimates for the subsample covering 1993–2006. All specifications use the running variable X^*_{mt}, cutoff $c = 0$, MSE-optimal bandwidth selection, local linear estimation, and a triangular kernel. Standard errors are clustered at the municipality level. Robust 95% confidence intervals are shown in brackets. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

J Summary statistics

Table 16: Summary statistics for main outcome and fiscal variables

Variable	Obs.	Mean	Std. dev.	Min	Max
`Income per capita`	1450	12.0235	0.1870	11.6120	12.9029
`Crime reports`	1737	9.1369	0.2892	8.0005	10.0425
`Tax rate`	1740	32.4977	1.2190	28.6500	35.1500
`Operating ratio`	1739	-1.4818	0.1784	-2.0633	-0.3767
`Operating costs`	1739	10.8851	0.2700	10.2109	11.6028
`Tax income`	1739	10.4446	0.2360	9.8181	11.2288
`Government grants`	1739	10338.42	6544.49	-16451.75	35980.00
`Education expenditure`	1444	11.3434	0.2384	10.8118	12.3571
`Employment ratio`	1734	0.7880	0.0432	0.5472	0.8943