The More the Merrier? The Effects of Council Size on Representation and Local Governance

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Abstract

Does broader political representation improve local governance? I use population thresholds in Mexican state law that fix the size of a municipal council, comparing municipalities just above a threshold with those just below, and link novel data on the characteristics of every council member to the output that council produces in office. Crossing a threshold seats 12 percent more representatives, and they are negatively selected: councils gain more members with a basic level of education and with no prior political experience, while parity rules hold the share of women fixed. Governance does not improve. Larger councils approve about half as many initiatives, a third fewer per member, across a narrower agenda, and their alignment with the preferences of citizens decreases; spending is unchanged in level and composition. I interpret this as negative selection at the margin of the party list: the supply of candidates is smooth at the threshold, but lists are closed and ranked, and parties concentrate their strongest candidates where the return to quality is highest.

Keywords local politics, political representation, governance quality
JEL D72, H72, H75, O12

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1 Introduction

Having broad political representation may improve local governance by narrowing the information and accountability gaps between citizens and representatives. For instance, more representatives per resident can increase the responsiveness of a council to citizen preferences, since each one answers to fewer residents and draws on information the others do not hold (Tiebout, 1956; Oates, 1972). Each is also subject to social as well as electoral sanction by the residents among whom they live (Seabright, 1996; Boffa et al., 2016). However, adding representatives can also raise the cost of reaching agreement and increase the number of members whose assent a decision requires (Tsebelis, 2002), and it divides the credit for any initiative among more members. The two forces run in opposite directions, so the effect of adding representatives need not be monotone and may turn negative once a council is large enough (Narasimhan and Weaver, 2026).

There is little evidence on how expanding representation affects governance at the local level, or on the conditions under which one of these forces dominates the other. Although most of the work on the size of local councils looks at outcomes such as spending (Egger and Koethenbuerger, 2010) or the core functions of local government (Narasimhan and Weaver, 2026), the mechanisms behind those outcomes are rarely examined. The composition of the council itself is seldom observed, although a council's size can only operate through the members it seats.

In this paper I estimate the effect of council size on political representation and on the quality of local governance. I focus on Mexico, where state law fixes the number of council members by municipal population. The thresholds at which that number changes support a regression discontinuity design, comparing municipalities just above a threshold with otherwise similar municipalities just below. I collect novel data on the characteristics of local representatives and link them to the outcomes their council produces in office. This allows me to estimate how descriptive representation changes and whether that change carries through to the quality of local governance.

The first set of results shows that descriptive representation does change at the threshold. Councils above it seat 1.17 more representatives, which confirms that the rule binds. The additional members are less educated and less politically experienced than those already in place: there are 1.20 more members with a basic level of education, and more who arrive from the private sector or are in their first job. Because a larger council holds more members of every kind, I benchmark each estimate against the number it would seat if the additional members resembled those already in place. The number with a

basic level of education is close to four times that benchmark, and the number holding a university degree falls where the benchmark implies a gain, so the council is recomposed rather than only enlarged. Gender is the exception, because Mexican law requires party lists to alternate between women and men: the number of women rises with the council while their share of it does not move.

I interpret this as negative selection at the margin of the list. If the supply of candidates is the same on either side of the threshold and parties rank their lists by quality, then the marginal seat is the cheapest a party wins, since it requires the fewest votes, and it goes to a candidate ranked below those already seated. This is consistent with evidence that parties concentrate their strongest candidates where the return to quality is highest (Galasso and Nannicini, 2011), and that selection into office improves with the prominence of the post (Dal Bó et al., 2017).

The second set of results shows that this change does not improve the quality of governance, and on most margins reduces it. Councils above the threshold approve about half as many initiatives, and about a third fewer per member. Their agenda covers a narrower range of subjects. The topics they take up also align less closely with residents' priorities. I measure this as the share of the issues residents submitted to the municipality that the council's approved agenda addressed. Councils below the threshold address 44 percent of those issues, and councils above address 16 percentage points fewer. Neither the level nor the allocation of spending moves, with any reallocation towards public goods bounded within four percentage points, and residents' own assessment of the services they receive does not change either. The comparison therefore holds a municipality's resources fixed and isolates the body that governs it: two municipalities of the same population, drawing the same revenue, and differing only in the number of representatives seated.

Two channels are consistent with this pattern. The first is the quality of the marginal representative, who is less educated and less experienced than the members already seated. The second is the division of responsibility, since the credit for any initiative is shared among more members and each member's return to drafting one falls. Both are consistent with lower output, a narrower agenda and weaker alignment, so telling them apart would require observing which members draft the initiatives a council approves, which the data do not record. The effect is the same across the size distribution, which is what a mechanism common to every municipality would produce rather than one working through local conditions.

This paper contributes to work on how the composition of a representative body shapes its output. A first literature changes that composition directly, most often through quotas,

and asks what a government then provides: reserving council seats for women in India shifts public goods towards those the women themselves prioritise (Chattopadhyay and Duflo, 2004), and the procedural knowledge councillors bring with them shapes their effectiveness once seated (Auerbach et al., 2023), while responsiveness in these settings runs through the individual representative rather than the body (Bussell, 2019). A second literature uses population thresholds that change council size, and estimates their effect on the budget (Egger and Koethenbuerger, 2010; Pettersson-Lidbom, 2012; Höhmann, 2017) or on corruption (Britto and Fiorin, 2020), with a meta-analysis placing the fiscal effect close to zero (Freire et al., 2023). This paper departs from both. It measures the composition of the body rather than holding it fixed, it examines the output of the council rather than only its budget, and it finds that composition and output move in opposite directions.

The closest work to this paper is Narasimhan and Weaver (2026), who exploit discontinuities in the number of hyperlocal representatives across Indian local governments and report precisely estimated nulls on the core functions of local government, concluding that expanding representation has limits where the capacity of representatives is weak. I measure that capacity. Descriptive representation does change here, and it changes downward, through negative selection at the margin of the party list. My results on spending agree with theirs, but legislative output falls by about half and the share of residents' issues the council addresses falls by 16 percentage points. The mechanism I propose for this is that the marginal seat is the cheapest a party wins, so it goes to a candidate the party had ranked below those it already seated.

The paper proceeds as follows. Section 2 describes the municipal setting and the rule that fixes council size. Section 3 describes the data and the sample. Section 4 sets out the design and tests the assumptions it requires. Section 5 reports the effects on political representation and on the quality of governance, and Section 6 discusses what they imply.

2 Institutional background

Mexico is a federation of 31 states and Mexico City, whose states are divided into 2,453 municipalities, each governed by an elected municipal council. Local councils are responsible for the provision of services such as drinking water, streets, parks and gardens, and preventive policing and traffic. The council approves the annual budget of expenditures and the public accounts that the state legislature then audits, so a council member votes on the decisions that bind the municipality. It has no authority over education, health care,

or criminal investigation and prosecution, which the states and the federal government run. Between 2010 and 2021 federal transfers supplied roughly 70 percent of municipal revenue, and the rest came from other sources such as their own taxes and fees, or state transfers. Municipalities raise little of their own: the property tax base is thin outside the largest cities, and the state legislature, not the council, approves the municipal revenue law and the rates and valuation tables that apply to it. A council therefore decides how a largely predetermined budget is spent and how the municipality is regulated, rather than its revenue.

Council members are elected by the municipality as a whole. Voters cast a single vote for a party list carrying the candidate for mayor and a closed list of candidates for the council seats; the party with the most votes takes the mayoralty and a majority of the seats, and the rest go to the losing parties in proportion to their vote.[1] The lists are closed, and party leaders control both the nomination and the rank (Blanes and Lopez-Moctezuma, 2026). Since 2015 every list must be half women and half men and must alternate by gender down the list, and a party must head half the slates it registers in a state with a woman, within blocs of municipalities graded by its vote share in the previous election so that women are not confined to races it expects to lose. Terms run three years, and consecutive reelection for one additional term became possible under the 2014 reform and was first used in 2017.[2] States hold municipal elections on their own calendars rather than together.

State law fixes how many representatives a council seats, and it fixes that number by municipal population, through a step function written into each state's electoral code or its organic municipal law. The national statistical agency measures population through the decennial census and the intercensal survey, and each state electoral institute applies the most recent published count when it calls an election. Municipal boundaries rarely change, so the population a municipality carries into an electoral cycle is the one the census recorded years earlier. Both the thresholds and the number of representatives attached to them vary across states, which generates the discontinuities this paper exploits. Crossing a threshold adds between one and six representatives, three at the median cell, and several states share a threshold while seating different numbers, so it is the rule rather than the

[1] The formula varies by state. In most, a party entering the proportional allocation must clear a minimum share of the municipal vote, commonly three percent, and the seats are distributed by natural quotient with any remainder assigned by largest remainder. Each state's electoral code sets how many seats are allocated this way and how many go to the winning slate.

[2] An incumbent seeking the additional term must be nominated by the party that nominated them originally, or by one of the parties in the coalition that did, unless they left it before the midpoint of the term. Access to the ballot therefore stays with the party leadership, and reelection does not loosen its control over candidacies.

threshold alone that defines a comparison. Table A1 in Appendix A lists the rule for every state and threshold.

3 Data

Combining three sources, I assemble original data linking the composition of a municipal council to its output in office. The first covers municipal characteristics and the population thresholds in state law that fix council size. The second covers the characteristics of the individual representatives. The third covers governance quality: effort in office, the allocation of the budget, and the alignment of the council's agenda with residents' priorities. Of the 2,453 municipalities in the country, the design only considers those whose representatives are chosen through party-based elections and whose state ties council size to population brackets. The first requirement excludes the 476 municipalities that select their representatives by customary law, and the second excludes Oaxaca and Veracruz, where state law sets council size by a different rule.[3] The period runs from 2013 to 2018, and because states hold municipal elections on their own calendars a municipality enters the panel in more than one cycle, on average 2.03 times, leaving 1,391 municipalities in 28 states and 2,819 municipality-cycles.

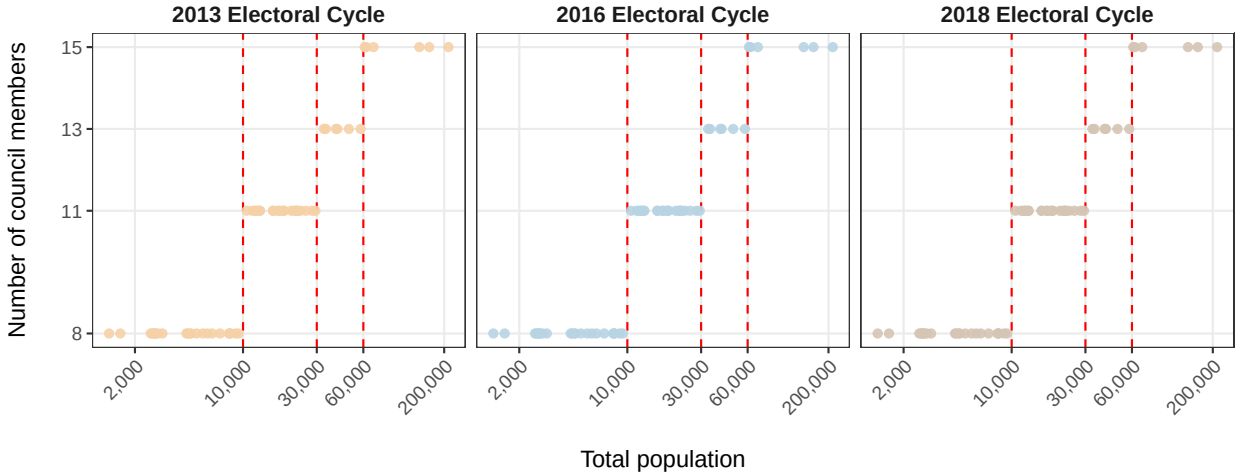
Threshold rules and council size. For each state and every electoral year in the sample I collect data on the population thresholds that determine the council size from the state's electoral code or organic municipal law.[4] Population comes from the national statistical agency, through the 2010 and 2020 Population and Housing Censuses and the 2015 Intercensal Survey, which is the same count a state electoral institute applies when it calls an election. Municipal election dates and results come from the state electoral institutes and fix the cycle each council belongs to, since states do not vote together. The number of members actually seated comes from the Census of Municipal Governments, which enumerates every municipal government in waves from 2013 to 2021. Appendix Table A4 sets the number of members the census records against the number state law assigns. The two have a correlation of 0.87. As an illustration of the variation, Figure 1 shows the number of council members in the state of Zacatecas by their population across three different electoral cycles. Council size moves in discrete steps as population crosses the cutoffs state law sets, and municipalities sit on those steps. Every state in the sample

[3] Municipalities governed by local rules, *usos y costumbres*, appear in fourteen states and concentrate in Oaxaca, Puebla, Chiapas, and Guerrero.

[4] There are no changes in the thresholds. Within every state and every population bracket, the number of representatives state law assigns is the same in each cycle the sample observes.

generates variation of this kind, each at its own cutoffs and with its own step sizes.

Figure 1. Descriptive example: Threshold rule and council size in Zacatecas



Notes. Each point is a municipality of Zacatecas in one electoral cycle, against the number of council members state law assigns it. Dashed lines mark the population cutoffs in state law, and population is on a logarithmic scale.

Table 1 reports how council size varies across the municipal population distribution: a council seats 9.51 members on average and ranges from 4 to 28, rising from 8.21 members below 25,000 inhabitants to 15.27 above 100,000. The spread within each band is wide, because every state sets its own thresholds and attaches its own council sizes to them, so population alone accounts for 45 percent of the variance in council size against 77 percent once state fixed effects are added.

Table 1. Number of representatives across the municipal population distribution

Municipal population	Observations	N representatives			
		Mean	SD	Minimum	Maximum
Under 25,000	863	8.21	1.98	4	13
25,000 to 50,000	253	9.75	2.03	4	16
50,000 to 100,000	138	11.47	2.56	6	17
Over 100,000	137	15.27	3.69	10	28
All municipalities	**1,391**	**9.51**	**3.14**	**4**	**28**

The unit is a municipality, covering 1,391 municipalities in 28 states across 2,819 municipality-cycles. The number of representatives is the number state law assigns, averaged over the cycles in which the municipality appears and rounded, and population is its median across those cycles.

Characteristics of representatives. I collect the characteristics of every council member from the records of the Census of Municipal Governments, which the national statistical

agency does not release publicly. For each representative I recover gender, education, the job held immediately before taking office, age, and the party that nominated them. Education is reported as a completed level, which I group into basic, intermediate and advanced,[5] and the previous job is reported as a category, which I group into public sector, private sector, party or union work, prior elected office, and no previous employment. I aggregate the individual records to the council, so each municipality-cycle carries the share of its members falling in each category. Table 2 reports the characteristics of the council members by quartile of municipal population. Across the sample a council is 48 percent women, which might suggest that the parity rules bind but that they do not carry women all the way to half the council, since parity binds candidacies rather than seats: a council with an odd number of members cannot divide evenly, and the winning slate takes its majority seats from the top of a list whose first position fixes which gender receives the extra one.[6] Advanced schooling rises from 20 percent of members in the smallest quartile of municipalities to 64 percent in the largest, while members in their first job fall from 18 percent to 5 percent. One interpretation is that the median education of residents barely moves across those quartiles (see Table A3 in Appendix), so a small municipality seats a council that looks much like the population electing it, while a large one draws from a far more selected group: council members are 1.9 times as likely as residents to hold advanced schooling in the smallest quartile against 3.4 times as likely in the largest, so the positive selection Dal Bó et al. (2017) document for politicians is much weaker where the municipality is small. Councils seat 3.6 parties on average and 2.5 by the effective-number measure, and here too size matters: the largest party holds 63 percent of the seats in the smallest quartile against 54 percent in the largest, so bigger councils are both more crowded and less dominated by a single party.

[5] Basic covers no schooling through lower secondary, intermediate covers technical or commercial training and upper secondary, and advanced covers a bachelor's degree or higher.

[6] The share of women rises from 41 percent in 2013 to 52 percent in 2018 as the parity rules bind, which the electoral-cycle fixed effects absorb. The share is 48 percent in every quartile of population, so the rules bind equally in small and large municipalities.

Table 2. Characteristics of representatives, by quartile of municipal population

	Quartile of municipal population				
	Q1	Q2	Q3	Q4	All
Panel A. Characteristics of representatives					
Women	0.48	0.48	0.48	0.48	0.48
Advanced schooling	0.20	0.34	0.45	0.64	0.41
Intermediate schooling	0.22	0.22	0.21	0.20	0.21
Basic schooling	0.55	0.40	0.30	0.13	0.35
Age, years	41.8	42.3	42.8	44.5	42.9
Panel B. Job held before taking office					
Private sector	0.44	0.45	0.42	0.38	0.42
Public sector	0.16	0.21	0.29	0.35	0.25
First job	0.18	0.14	0.11	0.05	0.12
Other	0.21	0.18	0.15	0.15	0.17
Panel C. Party composition					
Number of parties	2.8	3.3	3.7	4.7	3.6
Effective number of parties	2.1	2.4	2.6	2.9	2.5
Largest party, seat share	0.63	0.61	0.58	0.54	0.59
Representatives	7.6	8.2	9.3	12.7	9.5
Municipal population, mean	3,999	11,768	26,105	171,520	53,358
Municipal population, median	4,024	11,623	25,333	80,258	17,021
Municipality-cycles	705	705	704	705	2,819

The unit is a municipality-cycle. Quartiles of municipal population cut at 7,092, 17,021 and 40,016 inhabitants. Entries are shares of a council's members except age, the number of parties and the number of representatives. Age is available for 2,374 of the 2,819 municipality-cycles.

Governance quality. I collect administrative data on performance of the representatives over their term in office. The Census of Municipal Governments records the output of a council once in office, and I take three groups of outcomes. The first is legislative output: the initiatives placed before the council and those it approved, each classified by policy topic, together with the sessions it held and the standing committees it maintained. The topic classification lets me measure not only the volume of a council's approvals but the breadth of its agenda, which I summarise as the entropy of approved initiatives across topics, so that a council concentrating on a few subjects scores lower than one spreading its work. The second is the composition of public spending, which I take from the census's budget module and from the national statistical agency's municipal public finance statistics, reported by revenue source and by spending chapter. The third is representation: the census separately records the proposals residents submitted to the municipality, classified by topic, and matching those against the council's approved agenda gives the share of the

issues residents raised that the council acted on. The two modules classify topics on different catalogues, so I map both onto a common scheme before matching. Together these let me test whether a larger council differs in its legislative performance, in its spending and in its responsiveness to residents. Table 3 reports the level of each outcome across the population distribution. Councils grow with population and so does the number of initiatives they approve, which is the association the design is built to break.

Table 3. Governance outcomes, by quartile of municipal population

	Quartile of municipal population				
	Q1	Q2	Q3	Q4	All
Panel A. Legislative performance					
Initiatives approved	27	33	34	62	39
Log initiatives approved	2.52	2.68	2.66	3.13	2.75
Log initiatives per member	1.11	1.20	1.11	1.28	1.17
Topic diversity in agenda	1.46	1.58	1.61	1.62	1.57
Panel B. Composition of public spending					
Log spending	18.01	18.72	19.39	20.76	19.28
Unspent share of income	0.042	0.037	0.041	0.043	0.041
Particularistic share	0.084	0.076	0.079	0.088	0.082
Public goods share	0.308	0.351	0.355	0.256	0.316
Panel C. Representation					
Alignment with citizen preferences	0.050	0.069	0.091	-0.011	0.050
Citizen proposals received	198	630	777	3,968	1,529
Municipalities	340	350	356	345	1,391
Representatives	7.6	8.2	9.3	12.7	9.5
Municipality-cycles	705	705	705	707	2,822

The unit is a municipality-cycle and entries are means. Quartiles of municipal population cut at 7,092, 17,021 and 40,016 inhabitants. Outcomes are defined as in Table 6. Spending is the reported total revenue less the balance left unspent at the end of the year, and alignment is the share of the issues residents raised that the council's approved agenda addressed, expressed as an index. The table describes the sample. Municipalities in different quartiles face different statutory rules, and every estimate compares municipalities within the same state and threshold and within thirty percent of it.

Additional datasets. The baseline covariates come from the Population and Housing Census, which surveys the demographics of every municipality: the age structure of the population, education, labour force participation and employment, and the share without health coverage. I take these from the 2010 round, which precedes every electoral cycle in the sample, so no covariate can respond to the council it is used to describe. They serve two purposes: they test whether municipalities on either side of a threshold resemble each

other on characteristics fixed before a council takes office, which Table A5 in Appendix A reports, and they describe the municipalities the design compares. Municipal election results from the state electoral institutes supply the partisan composition of each council and the vote shares that determine how the proportional seats are allocated.

4 Empirical strategy

To estimate the effect of the number of representatives on descriptive representation and the quality of governance, I use a regression discontinuity design. I stack municipalities across states, thresholds and electoral cycles since thresholds vary across municipalities. In particular, for municipality i in state s and electoral cycle t I estimate

$$Y_{ist} = \tau \mathbf{1}[x_i \geq 0] + \beta_1 x_i + \beta_2 x_i \mathbf{1}[x_i \geq 0] + \gamma_{s,c(i)} + \mu_t + \varepsilon_{ist}. \quad (1)$$

The outcome Y_{ist} is a characteristic of the representatives or a measure of governance quality. The running variable $x_i = (P_i - c(i))/c(i)$ measures how far municipality i falls from the threshold that applies to it as a share of that threshold, where P_i is the population the national census records and $c(i)$ the nearest threshold in its state, so that $x_i = -0.10$ places a municipality ten percent below its own threshold whether that threshold is 5,000 or 500,000 inhabitants. The indicator $\mathbf{1}[x_i \geq 0]$ marks municipalities at or above the threshold, which seat more representatives than those below, and the terms β_1 and β_2 let the outcome trend with population and let that trend differ on each side, so that τ measures a jump at the threshold rather than a change in slope. μ_t is an electoral-cycle fixed effect to account for shocks common to all municipalities in one term of office. $\gamma_{s,c(i)}$ is a state-by-nearest threshold fixed effect. I estimate equation (1) by local linear regression with a triangular kernel, following Hahn et al. (2001) and Imbens and Lemieux (2008), weighting each municipality by $1-|x_i|/h$, and I cluster standard errors at the municipality, since state law fixes a municipality's council size in every cycle it appears and its residuals therefore correlate across cycles. τ identifies a weighted average of treatment effects across the stacked thresholds (Cattaneo et al., 2016) (Appendix D disaggregates). The weights are proportional to the kernel-weighted number of municipalities near each cutoff, so τ is not an unweighted average over the 77 state-by-threshold cells the window contains: the largest single cell carries 8 percent of the weight, the five largest carry 24 percent between them, and cells at cutoffs of 50,001 inhabitants or below carry 61 percent. The estimate therefore describes small municipalities compared over a few thousand inhabitants more than it describes the largest thresholds in the schedule.

I follow the normalizing-and-pooling estimator of Cattaneo et al. (2016). Each municipality's distance to its threshold is expressed as a share of that threshold, and the bandwidth is set in those units. I do this for three reasons. First, no window measured in inhabitants is local at every threshold: ten thousand inhabitants is twice the smallest threshold in the schedule and under one percent of the largest. Second, a single window in inhabitants would also impose one slope on brackets whose sizes differ by more than two orders of magnitude. Third, it would violate a property the design must satisfy, since population is a deterministic and continuous function of the running variable and its estimated discontinuity is therefore zero by construction: pooling distances in inhabitants produces jumps in population of 11 to 31 log points, where proportional distances produce at most four thousandths of one. Appendix Table A7 reports both.

I set $h = 0.30$, which keeps each municipality within thirty percent of its own threshold. In inhabitants this is a different window at every threshold: ±1,500 at the 5,000 threshold, ±12,000 at the median threshold of 40,001, and ±84,000 at 280,001, the largest threshold with municipalities on both sides, which Appendix Table A8 lists in full. The narrow windows carry the estimates, since 64 percent of the 407 municipalities inside the window sit at thresholds of 50,001 inhabitants or below. A data-driven rule would choose much the same level, as the mean squared error criterion of Calonico et al. (2014) selects between 0.27 and 0.31 for eight of the nine outcomes the paper reports, and I hold h common across outcomes so that estimates are comparable from table to table. Appendix Tables A16 and A18 re-estimate every outcome from $h = 0.10$ to $h = 0.50$ and after discarding the municipalities closest to the threshold, and Appendix Table A9 reports what the comparison cell does to each estimate.

4.1 Validity of the design

Two assumptions identify τ, and under them municipalities just below a threshold serve as controls for those just above. The first is that municipalities do not sort across the threshold, which would fail if a council near one could influence the population the census records for it. The institutional setting makes sorting difficult by construction, since the national statistical agency runs the census on its own calendar and publishes municipal counts years before councils take office, so a municipality seeking an extra seat would have to alter a federal statistical product in advance of knowing that it needed one. The second is that every other determinant of Y_{ist} is continuous at the threshold, so that any jump in the outcome is attributable to the seats the rule adds. In a population-threshold design this carries a specific requirement: council size must be the only thing

state law changes at these particular population cutoffs, because a state attaching other obligations to the same cutoff would load those into τ as well. I test each assumption in turn.

Municipalities must not sort across the threshold. The density of municipalities should be continuous at the cutoff if there is no manipulation. I test this with the local polynomial density estimator of Cattaneo et al. (2020), letting the procedure select its own bandwidth and using one observation per municipality, so that a municipality observed in several cycles counts once. The null is a continuous density at the threshold, and the test does not reject it: in proportional units $t = 0.73$ and $p = 0.465$, and in inhabitants $t = -1.72$ and $p = 0.085$. There is no evidence that municipalities sort across the cutoff. Table 4 reports both estimates.[7] Figure 2 shows the same cross-section directly: the histogram falls steadily from left to right with no spike on either side of zero, and the two fitted densities meet at the cutoff with overlapping confidence bands. Municipalities fall below their threshold about three times as often as above it because population is right skewed and most municipalities are small, not because any of them moved. Appendix Table A6 reports four further checks, testing one state-by-threshold cell at a time, restricting the pooled test to cells the nearest-edge assignment does not truncate, counting municipalities on each side within each cell, and testing for heaping at round populations. None rejects.

Table 4. Local polynomial density test at the population threshold

Running variable	t	p-value	
Relative distance	0.730	0.465	h = 0.326 / 0.427
Raw distance (inhabitants)	-1.722	0.085	h = 28,117 / 25,983

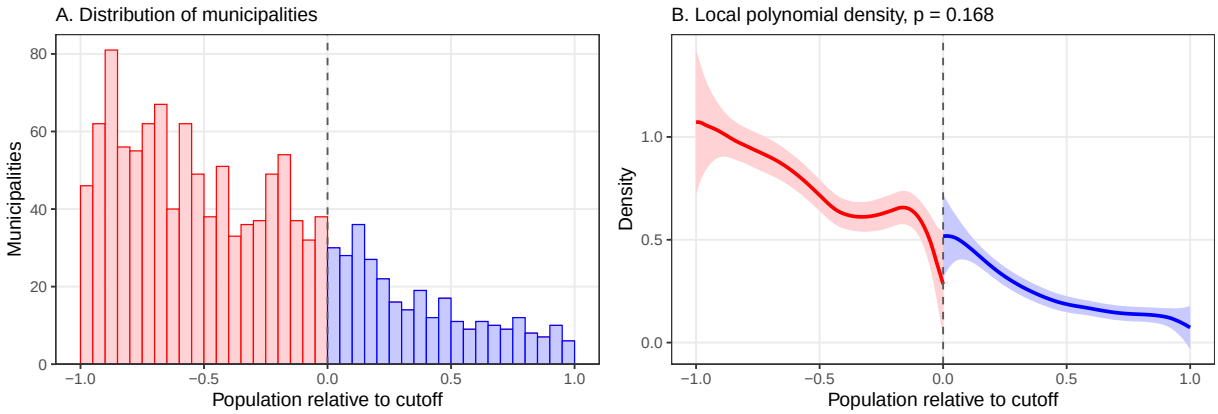
Notes. Local polynomial density test of Cattaneo et al. (2020), one observation per municipality, with the estimator selecting its own bandwidth on each side of the cutoff. The null is a continuous density at the threshold. Relative distance measures a municipality's distance to its threshold as a share of that threshold; raw distance measures it in inhabitants.

Every other determinant of the outcome must be continuous at the threshold. Appendix Table A5 reports the discontinuity in fourteen municipal characteristics the 2010 Population Census measures, every one of which precedes each electoral cycle in the sample and so cannot respond to a council. None differs at five percent, and the three that reach ten percent carry small point estimates, which is what chance delivers in fourteen

[7] Restricting the sample to a window before estimating lowers the p value to 0.021 at $|x_i| \leq 0.30$, but that is a property of the restriction rather than of the data, since truncation makes the density fall away at the edge of the retained window and so biases a boundary estimator evaluated at the cutoff. As the restriction tightens the selected bandwidth collapses from 0.40 to 0.07 and the statistic moves monotonically with it, from +1.38 to −2.31.

tests. A placebo moving the threshold to points where state law changes nothing gives four of thirty-two estimates at ten percent against the three chance predicts, in Appendix Table A19. More importantly, the money a municipality receives does not change at the threshold: federal transfers allocated by population formula are continuous through the cutoff, which Panel C of Appendix Table A7 reports. The design therefore compares municipalities with the same resources and a different number of council members.

Figure 2. Distribution of municipalities and estimated density around the population threshold



Notes. Cross-section of municipalities. Panel A counts municipalities by distance to their population threshold, in bins of five percent of the cutoff. Panel B plots the local polynomial density estimator of Cattaneo et al. (2020) with ninety five percent confidence bands on each side. Red covers municipalities below the cutoff and blue those at or above it.

5 Results

The results come in two parts. I begin with the composition of the council, comparing municipalities just above a threshold with otherwise similar municipalities just below. I then estimate the effect on the quality of governance, and read it against the change in composition the first part establishes.

5.1 Political representation

A larger council can broaden political representation by extending each party's allocation further into the list it registered. Lists are closed and ranked, so a party fills its seats in the order in which it registered its candidates. The design compares municipalities that are alike in everything but the number of seats state law assigns them, and that face the same

supply of potential candidates, so their councils are drawn from lists of the same kind and differ only in how far the allocation reaches. Whether this changes political representation depends on how parties order their lists. If the ordering is unsystematic with respect to a characteristic, a longer allocation seats more of the same people in expectation, so each group grows in proportion to the share it already holds and composition does not move; if parties rank on loyalty, experience, schooling or any attribute correlated with them, the additional positions are filled by candidates who differ from the members already seated. Partisan composition should not move under either ordering, since each voter casts a single vote for a party and that vote determines every seat the party obtains.

Table 5 reports the effect of crossing the population threshold on the size of the council and on its composition. Column 1 of Panel A is the first stage, the number of representatives the census records in office. The remaining columns of Panel A count the members holding each characteristic, so that every estimate is on the same scale as the first stage and reports how many of the additional seats a group receives. The last two columns of Panel A report the partisan structure, which neither ordering predicts will move. Panel B counts members by the job each held before taking office, which measures whether a wider council reaches beyond the pool already holding local power.

Councils above the threshold seat 1.17 more representatives, an increase of 12 percent relative to the control mean of 9.57, which confirms that the thresholds bind.[8] Of those, 0.40 are women, an increase of 9% relative to a mean of 4.61. Members with a basic level of education rise by 1.20, an increase of 55% relative to a mean of 2.18, while members holding a university degree fall by 0.29, a decrease of 6% relative to a mean of 5.15. The council therefore changes in composition as well as in size: members with a basic level of education grow far faster than the council does, while members holding a university degree do not increase at all.

A council with more seats holds more members of every kind, so a gain in any one group is not by itself evidence that the composition changed. I therefore benchmark each estimate against the number of members a council would seat if the additional ones resembled those already seated, which is a group's existing share of the council times the

[8]Two counts of council size are available: the number state law assigns, which I collect from the statutes, and the number the Census of Municipal Governments records in office. The statutory count rises by 2.31 at the threshold and the recorded count by 1.17, and Appendix Table A4 reports both, and Appendix Table A17 repeats them across bandwidths. The gap arises because state law applies whichever population figure was current when a council was seated, because several states top up councils with proportional-representation seats the population bracket does not fully determine, and because the census counts the members in office at the time of enumeration, so replacements and vacancies enter it. I scale the estimates by the recorded count throughout, which is the conservative choice.

1.17 seats the rule adds. Women gain 0.40 against a benchmark of 0.56, slightly fewer than proportional growth alone would deliver, and their share of the council does not change. Members with a basic level of education gain 1.20 against a benchmark of 0.31, close to four times as many. Members holding a university degree lose 0.29 where the benchmark implies a gain of 0.58. The council is recomposed and not merely enlarged: Appendix Table A10 reports that the share of members with basic schooling rises by 8.5 percentage points and the share holding a degree falls by 7.1, while the share of women does not move.

Table 5. Effect of crossing the population threshold on the number of representatives and the characteristics of the representatives

Panel A. Number of representatives, characteristics and partisan structure

	First stage	Characteristics of the representatives			Partisan structure	
	Representatives	Women	Basic education	Advanced education	Effective number of parties	Seat share of largest party
RD estimate	1.169***	0.404**	1.204***	-0.291	-0.200	0.030
	(0.244)	(0.193)	(0.313)	(0.400)	(0.150)	(0.019)
p-value	[0.000]	[0.037]	[0.000]	[0.468]	[0.185]	[0.115]
Dep. var. mean	9.57	4.61	2.18	5.15	2.76	0.55
Bandwidth (share of cutoff)	0.30	0.30	0.30	0.30	0.30	0.30
Observations	823	823	823	823	823	823

Panel B. Quality of the representatives, previous occupation

	Private sector	Own business	First job	Local office	Federal office	Party post
RD estimate	0.452*	0.312	0.421*	0.211	-0.361*	0.033
	(0.242)	(0.414)	(0.252)	(0.370)	(0.184)	(0.106)
p-value	[0.062]	[0.452]	[0.095]	[0.569]	[0.051]	[0.757]
Dep. var. mean	1.03	2.86	0.79	2.33	0.81	0.34
Bandwidth (share of cutoff)	0.30	0.30	0.30	0.30	0.30	0.30
Observations	798	798	798	798	798	798

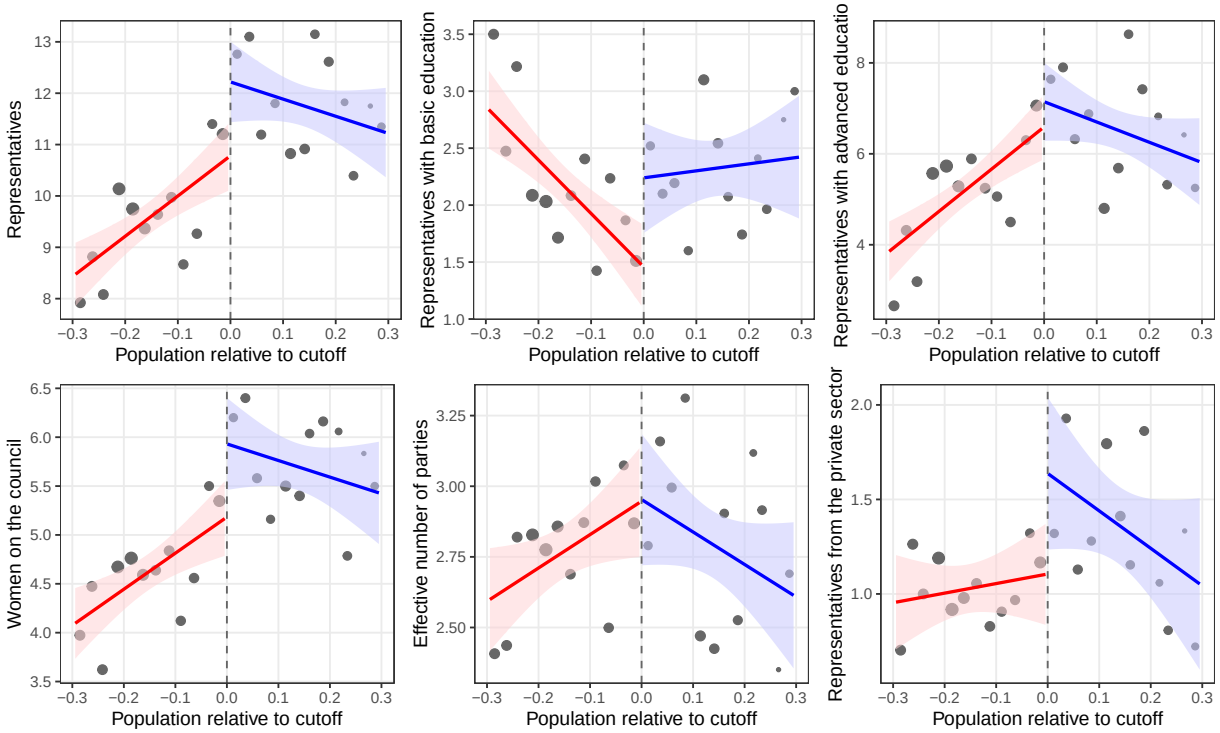
Each column is a separate regression on the number of representatives in that group, except the effective number of parties, which is the inverse Herfindahl index of seat shares, and the seat share of the largest party. Basic education covers no schooling through lower secondary and advanced covers a university degree or higher. Panel B reports the occupation each representative held before taking office, where first job means the representative had never worked and local office covers state and municipal government. Local linear regression with triangular weights inside a bandwidth of 0.30 of the population threshold, which is 12,000 inhabitants at the median cutoff and runs from 1,500 to 390,000 across the thresholds. State-by-threshold and electoral-cycle fixed effects are included. Standard errors clustered at the municipality in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Panel B reports the job each member held before taking office. Members arriving from the private sector rise by 0.45, an increase of 44% relative to a mean of 1.03, and those in their first job by 0.42, an increase of 53% relative to a mean of 0.79, while those arriving from federal office fall by 0.36, a decrease of 45% relative to a mean of 0.81. These are changes in composition rather than the by-product of a larger council: the benchmark would add 0.13 members from the private sector and 0.11 in their first job, against the

0.45 and 0.42 estimated, and would add 0.10 from federal office where the estimate is a loss of 0.36. The additional members are therefore drawn from outside political careers, though these estimates are less precise than those on schooling and do not survive the log specification of Appendix Table A11. The effective number of parties and the seat share of the largest party do not move, at −0.200 and 0.030, which is what closed-list allocation implies.

Figure 3 plots the same estimates without a regression, as bin means on either side of the threshold with local linear fits and confidence bands. The discontinuity is visible in the number of representatives, in the members whose schooling stopped at the basic level and in women, while advanced schooling and the effective number of parties run through the cutoff without a break. Appendix Figure A1 plots every outcome the table reports, together with the full set of previous occupations.

Figure 3. Number of representatives and political representation around the population threshold



Notes. Each panel plots bin means with point size proportional to the number of observations, together with local linear fits and ninety five percent confidence bands on each side of the cutoff. Counts are numbers of representatives. Red covers municipalities below the cutoff and blue those at or above it.

Between municipalities alike in everything but council size, the additional representatives are less educated and less politically experienced than the members already seated. Because the supply of candidates is the same on either side of the threshold, that difference

comes from where in the list the allocation stops rather than from who was available to stand: the last seat a party wins is cheaper than the first, since it requires fewer votes, and a party fills a cheaper seat with a candidate it would not place at the head of the list. Parties concentrate their strongest candidates where the return to quality is highest (Galasso and Nannicini, 2011) and selection into office improves with the prominence of the post (Dal Bó et al., 2017).

5.2 Quality of governance

The two forces set out in the introduction bear directly on what a council produces. A council of ten attributes to each member a smaller share of the return to drafting an initiative than a council of seven, which weakens the incentive to draft one and can lower the number the council approves, while the informational gain from more representatives works the other way.

Table 6. Effect of crossing the population cutoff on governance quality

Outcome	RD estimate		p-value	Control mean	N
Panel A. Office performance					
Log initiatives approved	-0.697***	(0.218)	[0.001]	2.787	773
Log initiatives approved per member	-0.449***	(0.131)	[0.001]	1.220	773
Topic diversity in agenda	-0.357**	(0.149)	[0.017]	1.545	719
Panel B. Composition of public spending					
Log spending	0.089	(0.066)	[0.177]	19.548	789
Unspent share of income	0.013	(0.009)	[0.154]	0.041	789
Particularistic share of spending	0.008	(0.010)	[0.391]	0.079	789
Public goods share of spending	-0.005	(0.021)	[0.809]	0.330	789
Panel C. Representation					
Alignment with citizen preferences	-0.161*	(0.091)	[0.079]	0.438	351

Each row is a separate regression. Topic diversity is the entropy of the approved initiatives across different policy topics, so a lower value means a narrower agenda. Panel C measures representation where alignment is the share of citizens' issues that reached the council's agenda. Local linear regression with triangular weights inside a bandwidth of 0.30 of the population threshold, which is 12,000 inhabitants at the median cutoff and runs from 1,500 to 390,000 across the thresholds. State-by-threshold and electoral-cycle fixed effects are included. Standard errors clustered at the municipality in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6 reports the effect of crossing the population threshold on the performance of a council. Panel A measures office performance: the initiatives the council approved, in logs, the same count per member, and the range of subjects they span, measured as the

entropy of approved initiatives across the census topic classification, so that a lower value marks a narrower agenda. A council above the threshold approves 0.70 log points fewer initiatives, about half as many as one below, and 0.45 log points fewer per member, both significant at one percent, while topic entropy falls by 0.36 on a control mean of 1.55. Larger councils therefore produce less office output in terms of policy proposals over a narrower range of subjects, and the fall per member indicates lower effort from each representative rather than the same work divided among more of them. The contraction is not spread evenly across the agenda: Appendix Table A12 estimates the effect on the share of initiatives falling in each policy area, and the two that fall significantly are social development, down 3.2 percentage points on a control mean of 2.3, and public security, down 2.6 points on a control mean of 3.5.

Panel B measures the level and the allocation of municipal spending: outlays in logs, the share of income the council leaves unspent, the particularistic share reaching identifiable recipients, and the share going to public goods. There is no effect on the measures of spending at conventional levels of significance. The share going to public goods is estimated at -0.005 with a standard error of 0.021 on a control mean of 0.330, which rules out any reallocation larger than four percentage points. Citizens' own assessment of the services they receive does not move either, on any of the five measures Appendix Table A13 reports, though that survey covers too few municipalities to place a tight bound on it.

Finally, I test whether a broader council is more responsive to residents' priorities. Panel C measures representation as the share of the issues residents submitted to the municipality that the council's approved agenda addressed. A council above the threshold acts on 16 fewer of every 100 issues residents raised, on a base of 44, an estimate significant at ten percent. The measure requires a council's approved agenda and its residents' proposals to be recorded in the same cycle, which holds for 351 of the 826 municipality-cycles in the window, so this is the one result in the paper estimated on fewer than half the sample. Whether a municipality-cycle is covered does not itself jump at the threshold, estimated at -0.100 with a standard error of 0.085 against a coverage rate of 0.40 below the cutoff, so the restriction narrows the sample without selecting on treatment. Appendix Figure A2 plots all eight outcomes around the threshold and shows the same pattern without a regression. Appendix Table A20 adjusts every reported estimate for multiple hypothesis testing by the false discovery rate of Benjamini and Hochberg (1995). The shift towards members with a basic level of education and the falls in approved initiatives, in output per member and in agenda breadth all survive adjustment across the full set of outcomes; the estimates on women and on alignment do not.

The costs of adding a representative outweigh the informational gains. Neither the level nor the allocation of spending moves, so a larger council is not spending the same money differently but producing less. There are two possible explanations consistent with this pattern. The first is the quality of the marginal representative, who is less educated and less politically experienced than the members already in place, and so is drawn from further down the distribution a party ranks on its capacity to carry a ticket. The second is the division of responsibility, since the credit for an initiative is shared among more members and each member's return to drafting one falls. Both are consistent with the fall in alignment alongside the fall in output, because a council that approves less over a narrower agenda covers fewer of the issues residents raise, and the design does not separate them.

The effect does not vary with the size of the municipality. Splitting the estimation window into terciles of municipal population, output per member falls by 0.42, 0.45 and 0.46 log points from the smallest tercile to the largest, and no outcome differs significantly across the three. Councils in the largest tercile seat 12.9 members on average against 8.1 in the smallest, and 64 percent of their members hold a university degree against 33 percent. An additional representative reduces output by the same amount in each. This is more consistent with the division of responsibility among more members than with a shortage of candidates. Appendix Figure A3 reports the estimates. Appendix Figures A4, A5 and A6 repeat the exercise grouping councils by the seats they hold below the cutoff, with median splits in Appendix Table A14 and the same split measured before the council takes office in Appendix Table A15, while Appendix Figure A7 groups by the number of seats the statute adds.

6 Discussion

This paper estimates the effect of council size on political representation and on the quality of local government. I focus on Mexico, where the council size is defined by municipal population thresholds, which allows me to compare municipalities that are similar on their baseline covariates and differ only in council size. I collect novel data on the characteristics of the representatives seated on each council and link them to its output, so that a change in the composition of a council can be traced to a change in its output. Municipalities above the threshold seat larger councils, but the marginal legislator is less educated and less politically experienced. The supply of candidates explains that selection: a party draws on a limited pool of people it is willing and able to put forward, and a council

with more seats to fill exhausts that pool, seating candidates a smaller council would not have reached. Those councils also produce less, over a narrower policy agenda that is less aligned with the preferences of their constituents. Together, these results suggest that widening representation does not by itself improve government, and can work against it when the additional seats are filled from a candidate pool a party has already exhausted.

Evidence on the size of a legislature concerns mostly its spending. Quasi-experimental work on municipal and regional governments has estimated the effect of legislature size on the budget (Egger and Koethenbuerger, 2010; Pettersson-Lidbom, 2012; Höhmann, 2017), and a recent meta-analysis places that effect close to zero (Freire et al., 2023); Britto and Fiorin (2020) look instead at corruption. Narasimhan and Weaver (2026) move beyond fiscal outcomes and report precisely estimated nulls on the core functions of local government in India. This paper looks at dimensions these do not capture: the volume of a council's legislative output, the breadth of its agenda, the correspondence between that agenda and the issues residents raise, and the composition of the council itself. Along each of them a broader council performs worse, and it also seats members who are less educated and less politically experienced than those already in place. A decline in the quality of the marginal representative and the division of responsibility among more members are both consistent with this pattern.

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A Summary statistics

Table A1. Statutory schedule of council size, by state and population bracket

State	Population bracket	Repre-sentatives	Municipality-cycles in the bracket
Aguascalientes	Up to 30,000	7	8
	30,000 to 700,000	9	12
	Over 700,001	16	2
Baja California	Up to 250,000	11	4
	250,001 to 500,000	14	2
	Over 500,001	16	4
Baja California Sur	Up to 20,000	7	2
	20,001 to 80,000	10	4
	80,001 to 280,000	14	2
	Over 280,001	14	2
Campeche	Up to 200,000	10	18
	Over 200,001	14	4
Chiapas	Up to 15,000	6	80
	15,001 to 100,000	9	139
	Over 100,001	10	16
Chihuahua	Up to 20,000	9	69
	20,000 to 60,000	13	27
	60,000 to 500,000	17	9
	Over 500,000	21	6
Coahuila de Zaragoza	Up to 15,000	8	18
	15,001 to 40,000	12	6
	40,001 to 80,000	16	7
	Over 80,001	19	7
Colima	Up to 25,000	9	8
	25,001 to 50,000	10	4
	50,001 to 100,000	11	0
	Over 100,001	12	8
Durango	Up to 15,000	8	44
	15,001 to 150,000	10	30
	150,001 to 500,000	16	2
	Over 500,000	18	2
Guanajuato	Up to 40,000	9	34
	40,000 to 140,000	11	38
	Over 140,001	14	20
Guerrero	Up to 24,999	7	92
	25,000 to 74,999	9	55
	75,000 to 114,999	11	4
	115,000 to 299,999	14	7
	Over 300,000	22	2
Hidalgo	Up to 30,000	10	57
	30,000 to 50,000	13	16
	50,001 to 100,000	17	6
	Over 100,001	21	5
Jalisco	Up to 50,000	12	162
	50,001 to 100,000	15	18
	100,001 to 500,000	18	12

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State	Population bracket	Repre-sentatives	Municipality-cycles in the bracket
	Over 500,001	22	2
Michoacan de Ocampo	Up to 40,000	8	173
	40,001 to 100,000	12	26
	Over 100,001	13	10
Morelos	Up to 30,000	4	34
	30,001 to 60,000	6	14
	60,001 to 100,000	8	10
	100,001 to 180,000	10	4
	180,001 to 350,000	12	2
	Over 350,000	16	2
Nayarit	Up to 15,000	8	8
	15,001 to 40,000	11	20
	40,001 to 80,000	14	6
	Over 80,001	17	6
Nuevo Leon	Up to 12,000	7	50
	12,000 to 50,000	11	26
	50,000 to 150,000	13	12
	150,000 to 250,000	14	0
	250,000 to 350,000	15	4
	350,000 to 450,000	16	4
	450,000 to 550,000	18	2
	550,000 to 650,000	19	0
	650,000 to 750,000	23	2
	750,000 to 850,000	24	0
	850,000 to 950,000	25	0
	950,000 to 1,050,000	26	0
	Over 1,050,000	28	2
Puebla	Up to 60,000	9	292
	60,000 to 90,000	12	12
	Over 90,000	13	14
	Over 1,300,000	24	2
Queretaro	Up to 60,000	9	20
	60,001 to 150,000	11	12
	Over 150,001	13	4
	Over 1,000,001	15	0
Quintana Roo	Up to 200,000	10	21
	Over 200,001	16	6
San Luis Potosi	Up to 90,000	7	104
	90,001 to 400,000	14	10
	Over 400,001	17	2
Sinaloa	Up to 50,000	11	27
	50,000 to 100,000	13	12
	Over 100,000	19	15
Sonora	Up to 30,000	6	84
	30,000 to 100,000	11	14
	100,000 to 999,999	21	12
	Over 1,000,000	26	0
Tabasco	Up to 100,000	11	18
	Over 100,001	12	16
Tamaulipas	Up to 30,000	5	87

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State	Population bracket	Repre-sentatives	Municipality-cycles in the bracket
	30,000 to 50,000	7	3
	50,000 to 100,000	10	6
	100,000 to 200,000	14	9
	Over 200,000	16	18
Tlaxcala	Up to 9,999	6	54
	10,000 to 29,999	7	44
	Over 30,000	8	22
Yucatan	Up to 5,000	6	108
	5,000 to 10,000	9	52
	10,000 to 100,000	12	50
	Over 250,000	20	2
Zacatecas	Up to 10,000	8	69
	10,000 to 30,000	11	69
	30,000 to 60,000	13	18
	Over 60,000	15	18

Every state and population threshold that contributes to the estimates, meaning every cell holding municipalities on both sides of a threshold inside the estimation window. Representatives below and above are the counts state law assigns on each side. Cycles count municipality-cycles.

Table A3. Demographics of the municipality, by quartile of municipal population

	Quartile of municipal population				
	Q1	Q2	Q3	Q4	All
Panel A. Age structure of the population					
Aged 0 to 17	0.36	0.39	0.40	0.38	0.38
Aged 18 to 24	0.12	0.12	0.13	0.13	0.12
Aged 60 and over	0.14	0.12	0.10	0.09	0.11
Panel B. Schooling of the population					
No schooling	0.08	0.09	0.09	0.06	0.08
Primary schooling	0.14	0.14	0.13	0.12	0.13
Secondary schooling	0.15	0.14	0.14	0.15	0.14
Advanced schooling	0.11	0.11	0.12	0.19	0.13
Years of schooling, mean	6.4	6.5	6.6	8.0	6.9
Panel C. Employment and health coverage					
Economically active	0.34	0.34	0.34	0.38	0.35
Employed	0.96	0.95	0.95	0.96	0.95
Women in employment	0.23	0.21	0.23	0.28	0.24
Unemployed	0.05	0.05	0.05	0.04	0.05
Without health coverage	0.32	0.35	0.37	0.32	0.34
Panel D. Advanced schooling, council against residents					
Council members	0.20	0.34	0.45	0.64	0.41
Residents	0.11	0.11	0.12	0.19	0.13
Ratio	1.9×	3.0×	3.9×	3.4×	3.1×
Municipal population, median	4,024	11,623	25,333	80,258	17,021
Municipality-cycles	705	705	704	705	2,819

The unit is a municipality-cycle. Quartiles of municipal population cut at 7,092, 17,021 and 40,016 inhabitants.

B Validity of identification assumptions

Two measures of the first stage appear in the paper: the count state law implies rises by 2.31 representatives at the threshold, and the count the Census of Municipal Governments records in office rises by 1.17. The gap arises because state law applies whichever population figure was current when a council was seated, because several states add proportional-representation seats the population bracket does not fully determine, and because the census counts the members in office at enumeration, so replacements and vacancies enter it. I scale the estimates by the recorded count, which is conservative, since attenuation there makes any per-representative effect larger rather than smaller. The design is a reduced form on crossing the threshold, so the gap affects how a per-representative magnitude should be read and not identification. Table A5 reports the discontinuity in fourteen municipal characteristics the 2010 Population Census measures,

every one of which precedes each electoral cycle in the sample. None differs at five percent, and the three that reach ten percent are what chance delivers in fourteen tests; a placebo at cutoffs where state law changes nothing gives four of thirty-two at ten percent against the three chance predicts, in Appendix Table A19. Federal transfers allocated by population formula are also continuous at the cutoff, which Panel C of Appendix Table A7 reports, so the design compares municipalities with the same resources and a different number of council members.

Table A4. First stage

Outcome	Control mean	Estimate		95% CI	*N*
Number of representatives, statute	9.27	2.31***	(0.18)	[1.95, 2.67]	823
Number of representatives, government census	9.57	1.17***	(0.24)	[0.69, 1.65]	823

30 of the statutory cutoff, with threshold and electoral-cycle fixed effects and standard errors clustered at the municipality. The first row uses the number of representatives that state law implies and the second the number of representatives that the National Census of Municipal Governments records.

Table A5 reports the discontinuity in fourteen municipal characteristics the 2010 Population Census measures, every one of which precedes each electoral cycle in the sample. None differs at five percent, and the three that reach ten percent are what chance delivers in fourteen tests; a placebo at cutoffs where state law changes nothing gives four of thirty-two at ten percent against the three chance predicts, in Appendix Table A19. Federal transfers allocated by population formula are also continuous at the cutoff, which Panel C of Appendix Table A7 reports, so the design compares municipalities with the same resources and a different number of council members.

Table A5. Continuity of predetermined municipal characteristics at the population threshold

Covariate	Control mean	Estimate		95% CI	*N*
Population aged 0-17	0.384	0.011	(0.008)	[-0.005, 0.027]	820
Population aged 18-24	0.126	0.002	(0.002)	[-0.002, 0.005]	820
Population aged 60+	0.104	-0.007	(0.005)	[-0.017, 0.003]	820
No schooling	0.082	0.009	(0.007)	[-0.005, 0.022]	820
Primary schooling	0.125	-0.004	(0.004)	[-0.013, 0.005]	820
Secondary schooling	0.142	0.009	(0.007)	[-0.005, 0.023]	820
Tertiary schooling	0.143	-0.023*	(0.012)	[-0.046, 0.000]	820
Average years of schooling	7.012	-0.374*	(0.200)	[-0.767, 0.019]	820
Economically active	0.359	-0.013	(0.008)	[-0.029, 0.003]	820
Employed	0.957	-0.002	(0.004)	[-0.011, 0.007]	820
Employed, women	0.246	-0.021*	(0.011)	[-0.042, 0.000]	820
Employed, men	0.711	0.017	(0.011)	[-0.004, 0.038]	820
Unemployed	0.044	0.002	(0.004)	[-0.007, 0.011]	816
No health coverage	0.331	-0.024	(0.019)	[-0.061, 0.012]	820

Covariates come from the 2010 Population Census, which precedes every electoral cycle in the sample, so no covariate can respond to a council in it. Shares are of the municipal population, except average years of schooling. 30 of the population threshold, The bandwidth is 0.30 of the population threshold, which is 12,000 inhabitants at the median cutoff and runs from 1,500 to 390,000 across the thresholds. Standard errors in parentheses.

Table A6. Local polynomial density test, diagnostics

Specification	t	p-value	
A. Sample pre-truncated before estimating			
Relative distance, $w = 1.0$	1.379	0.168	forced $h = 0.397$
Relative distance, $w = 0.5$	-1.751	0.080	forced $h = 0.134$
Relative distance, $w = 0.4$	-2.007	0.045	forced $h = 0.126$
Relative distance, $w = 0.3$	-2.313	0.021	forced $h = 0.073$
Relative distance, $w = 0.2$	-2.087	0.037	forced $h = 0.070$
Inhabitants, $w = 50,000$	-1.749	0.080	forced $h = 14{,}553$
Inhabitants, $w = 20,000$	-1.195	0.232	forced $h = 6{,}916$
Inhabitants, $w = 10,000$	-0.925	0.355	forced $h = 3{,}899$
B. One cell at a time, in inhabitants			
12_25000	0.605	0.545	
13_30000	0.614	0.539	
14_50001	0.140	0.889	
16_40001	-0.779	0.436	
21_60000	0.578	0.564	
31_5000	-0.780	0.435	
7_15001	0.016	0.988	
Combined (Stouffer)	0.148	0.882	7 cells
C. Cells with untruncated support			
Relative distance	1.488	0.137	28 of 80 cells
Within-cell counts, $\lvert x\rvert \leq 0.10$		1.000	23 below / 22 above
Within-cell counts, $\lvert x\rvert \leq 0.15$		0.526	65 below / 57 above

Density test of Cattaneo et al. (2020), one observation per municipality; Table 4 reports the headline test. Panel A pre-truncates the sample at $|x_i| \leq w$, Panel B tests one state-by-threshold cell at a time and combines p values by Stouffer's method, and Panel C uses only cells the nearest-edge rule does not truncate.

Table A7. Placebo. Discontinuity in log population and in federal money set by formula

Panel A. Raw distance, bandwidth in inhabitants Bandwidth	Estimate	SE	p-value	N
2,500	0.019	(0.021)	[0.375]	334
5,000	0.113***	(0.034)	[0.001]	585
10,000	0.142***	(0.036)	[0.000]	1,021
20,000	0.225***	(0.033)	[0.000]	1,567
50,000	0.314***	(0.035)	[0.000]	2,365
Panel B. Relative distance, bandwidth as a share of the cutoff Bandwidth	Estimate	SE	p-value	N
0.10	-0.000**	(0.000)	[0.026]	257
0.15	-0.000	(0.000)	[0.328]	405
0.20	-0.000	(0.000)	[0.253]	575
0.30	-0.002**	(0.001)	[0.017]	823
0.40	-0.004***	(0.001)	[0.000]	1,020
Panel C. Federal money set by a population formula Outcome	Estimate	SE	p-value	N
Log municipal strengthening fund, allocated by population	0.037	(0.043)	[0.385]	789
Log earmarked federal grants	0.057	(0.075)	[0.443]	789
Log formula transfers	0.112*	(0.058)	[0.052]	789

Population cannot jump at a population cutoff, so any estimated discontinuity measures specification error. Panel A pools every cutoff under one bandwidth in inhabitants; Panel B measures distance as a share of each municipality's own cutoff, which is the running variable the paper uses. Panel C repeats the exercise on federal transfers allocated by population formula, which must also be continuous at the cutoff.

Table A8. The estimation window in inhabitants at each population threshold

Statutory threshold	Municipalities	Window at $h = 0.30$ (inhabitants)	A common window of 10,000 inhabitants, as a share of the threshold
5,000	33	±1,500	2.00
10,000	35	±3,000	1.00
15,001	37	±4,500	0.67
25,000	23	±7,500	0.40
30,000	49	±9,000	0.33
40,001	24	±12,000	0.25
50,001	22	±15,000	0.20
100,001	37	±30,000	0.10
200,001	4	±60,000	0.05
500,000	1	±150,000	0.02
1,300,000	1	±390,000	0.01

Selected thresholds. The third column gives the estimation window in inhabitants implied by $h = 0.30$ at that threshold, which is what the design actually compares. The fourth column reports the reverse exercise: what a single window of ten thousand inhabitants, of the kind used when all thresholds are close in size, would represent as a share of each threshold.

Table A9. The comparison cell and the estimates

Outcome	Comparison cell	Estimate		p-value	Cells	N
Number of representatives, census						
	None	1.779*	(1.000)	[0.076]	—	823
	Threshold	1.508***	(0.391)	[0.000]	44	823
	State × threshold	1.169***	(0.244)	[0.000]	77	823
Log initiatives approved						
	None	-0.363	(0.254)	[0.154]	—	773
	Threshold	-0.582**	(0.229)	[0.011]	44	773
	State × threshold	-0.697***	(0.218)	[0.001]	77	773
Log initiatives per member						
	None	-0.296**	(0.146)	[0.043]	—	773
	Threshold	-0.404***	(0.139)	[0.004]	44	773
	State × threshold	-0.449***	(0.131)	[0.001]	77	773
Agenda diversity						
	None	-0.233	(0.161)	[0.148]	—	719
	Threshold	-0.389**	(0.155)	[0.012]	44	719
	State × threshold	-0.357**	(0.149)	[0.017]	77	719

Each block re-estimates one outcome under three comparison cells: none, the population threshold alone, and the state-by-threshold cell that equation (1) uses. Electoral-cycle fixed effects are included throughout. Because several states share a population threshold while seating different numbers of representatives, the threshold cell compares municipalities governed by different rules and the finer cell does not.

C Additional results

Unless a note states otherwise, every estimate below comes from a local linear regression with triangular weights inside a bandwidth of 0.30 of the population threshold, with state-by-threshold and electoral-cycle fixed effects and standard errors clustered at the municipality. Stars denote * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10. Size and composition components of the effect on council membership

Group	Effect on the number of members	Size component	Composition component	Inter-action	Effect on the share of the council
Women	0.404**	0.559	-0.113	-0.014	-0.012
	(0.193)				(0.016)
Basic schooling	1.204***	0.308	0.810	0.099	0.085**
	(0.313)				(0.034)
Intermediate schooling	0.308	0.240	-0.095	-0.012	-0.010
	(0.277)				(0.027)
Advanced schooling	-0.291	0.575	-0.679	-0.083	-0.071*
	(0.400)				(0.039)
Private sector	0.452*	0.129	0.234	0.029	0.024
	(0.242)				(0.024)
First job	0.421*	0.112	0.238	0.029	0.025
	(0.252)				(0.027)
Federal office	-0.361*	0.095	-0.281	-0.034	-0.029*
	(0.184)				(0.016)
Council size	1.169***				
	(0.244)				

Each effect on the number of members decomposes as $s_0 \Delta N + N_0 \Delta s + \Delta s \, \Delta N$, where s_0 and N_0 are the share and the council size just below the threshold and ΔN and Δs are the estimated discontinuities in council size and in the group's share.

Table A11. Effect of crossing the population threshold on the number of representatives and on the composition of the council, in logs

Panel A. Number of representatives, characteristics and partisan structure

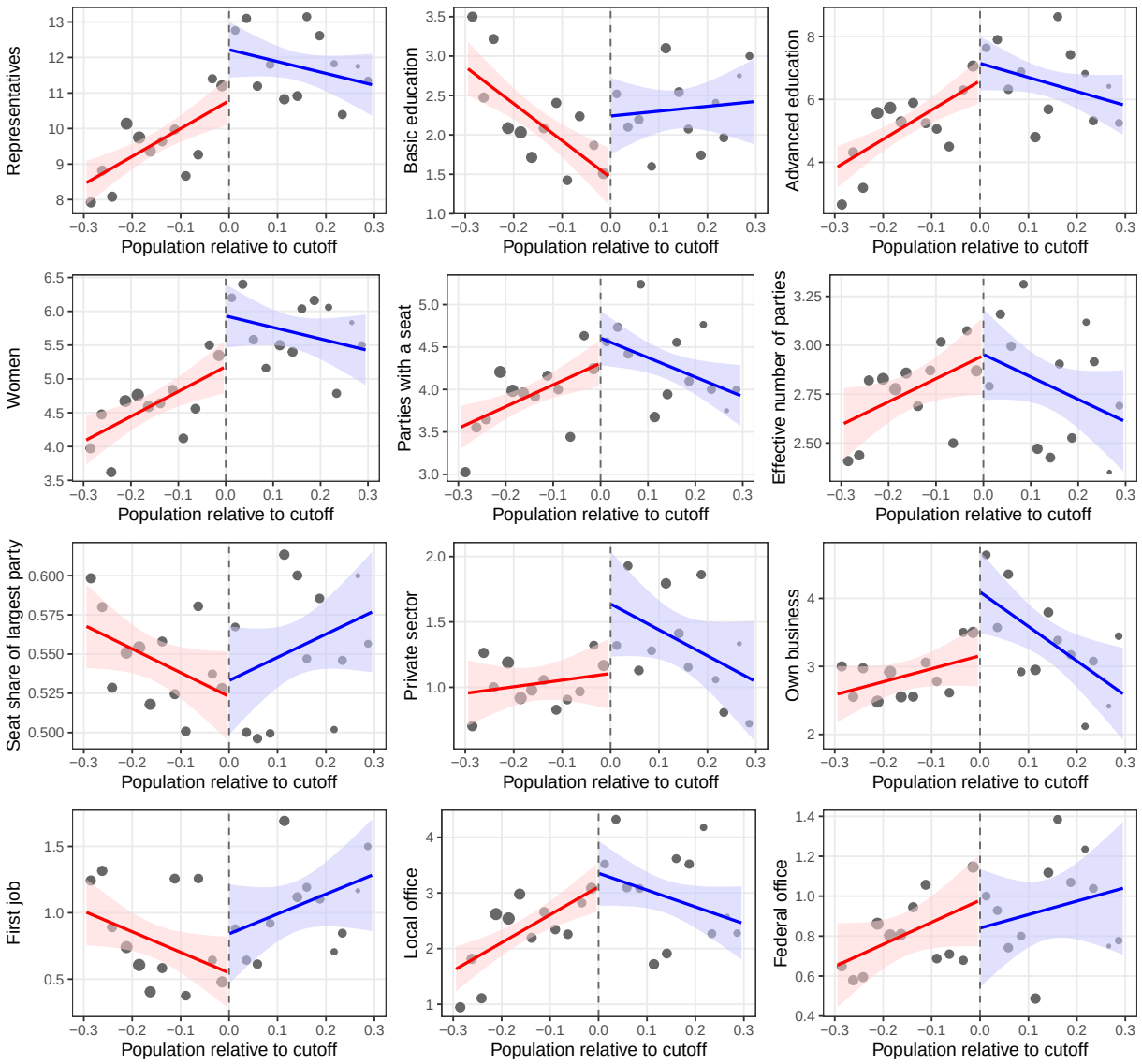
	First stage	Characteristics of the representatives			Partisan structure	
	Representatives	Women	Basic education	Advanced education	Effective number of parties	Seat share of largest party
RD estimate	0.115***	0.076*	0.355***	-0.005	-0.070	0.059
	(0.026)	(0.039)	(0.099)	(0.086)	(0.048)	(0.039)
p-value	[0.000]	[0.053]	[0.000]	[0.954]	[0.143]	[0.133]
Dep. var. mean	2.30	1.65	0.96	1.58	0.95	-0.65
Bandwidth (share of cutoff)	0.30	0.30	0.30	0.30	0.30	0.30
Observations	823	823	823	823	823	823

Panel B. Quality of the representatives, previous occupation

	Private sector	Own business	First job	Local office	Federal office	Party post
RD estimate	0.096	0.045	0.139	0.053	-0.108	0.021
	(0.093)	(0.118)	(0.099)	(0.102)	(0.081)	(0.051)
p-value	[0.304]	[0.703]	[0.162]	[0.599]	[0.183]	[0.673]
Dep. var. mean	0.51	1.14	0.39	0.96	0.42	0.18
Bandwidth (share of cutoff)	0.30	0.30	0.30	0.30	0.30	0.30
Observations	798	798	798	798	798	798

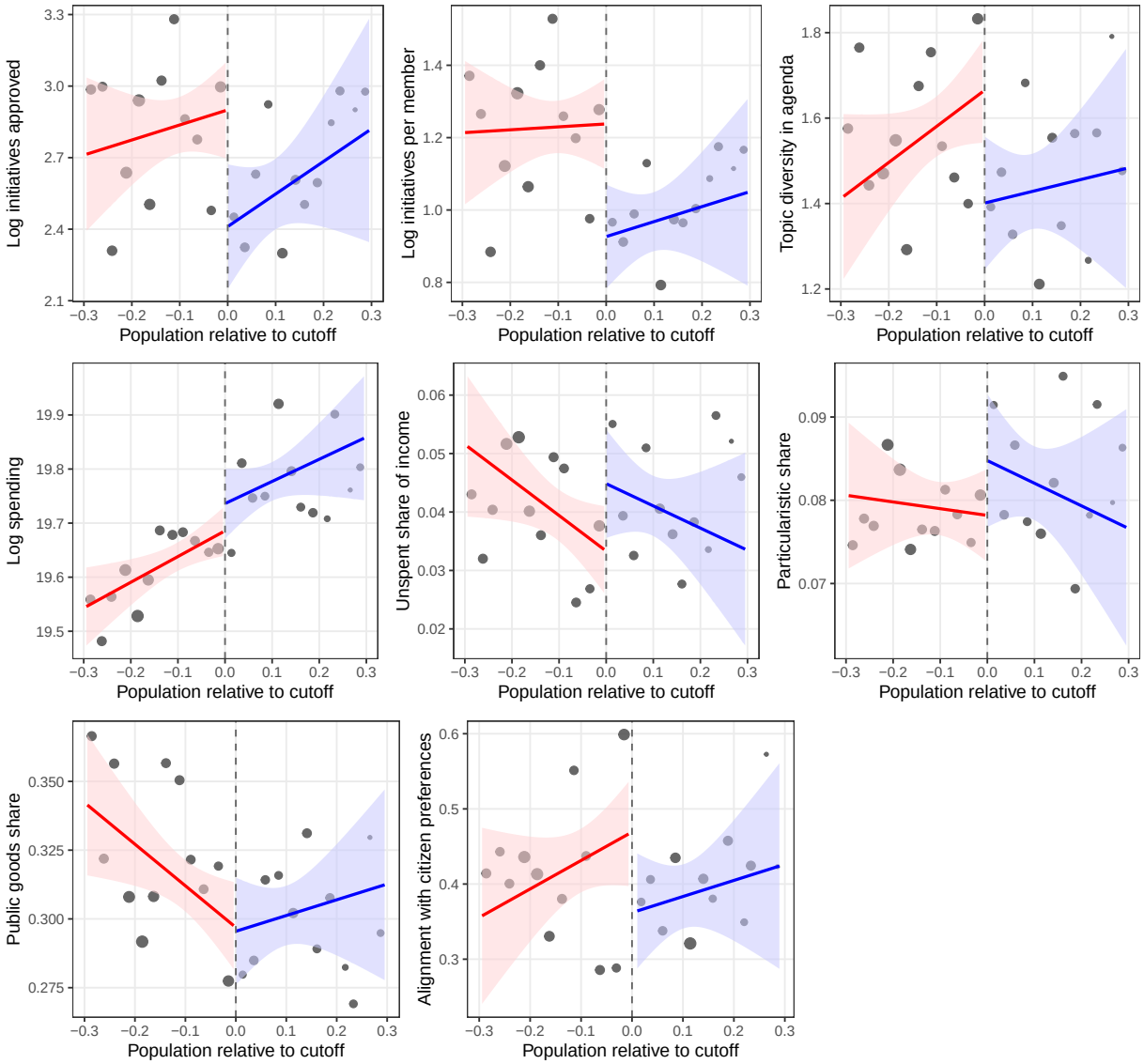
The same specification as Table 5 with every outcome in logs, using the log of one plus the count where the count can be zero, so estimates read as proportional changes.

Figure A1. Number of representatives, political representation and representative background around the population threshold



Every outcome that enters Table 5, together with the previous occupation of the representatives. Each panel plots bin means with point size proportional to the number of observations, together with local linear fits and ninety five percent confidence bands on each side of the cutoff. Counts are numbers of representatives. Red covers municipalities below the cutoff and blue those at or above it.

Figure A2. Effect of crossing the population cutoff on governance quality



Every outcome that enters Table 6. Each panel plots bin means with point size proportional to the number of observations, together with local linear fits and ninety five percent confidence bands on each side of the cutoff. The running variable is population relative to the population threshold, so zero is the cutoff and the window covers thirty percent of it on each side. Red covers municipalities below the cutoff and blue those at or above it.

Table A12. Effect of crossing the population threshold on approved initiatives, by policy topic

Outcome	RD estimate		p-value	Control mean	N
Gender and women's rights	-0.0127	(0.0093)	[0.174]	0.0132	771
Social development	-0.0316**	(0.0150)	[0.036]	0.0232	771
Health	-0.0100	(0.0101)	[0.321]	0.0189	771
Education	-0.0049	(0.0094)	[0.599]	0.0252	771
Public services	0.0264	(0.0248)	[0.286]	0.0383	771
Public works	-0.0268	(0.0265)	[0.312]	0.1078	771
Water and sanitation	-0.0084	(0.0054)	[0.121]	0.0199	771
Public security	-0.0262**	(0.0101)	[0.010]	0.0346	771

Each row is a separate regression on the share of the council's approved initiatives falling in that policy area, taken from the thirty topics of the INEGI catalogue. 30 of the population threshold,

Table A13. Residents' satisfaction with municipal services at the population threshold

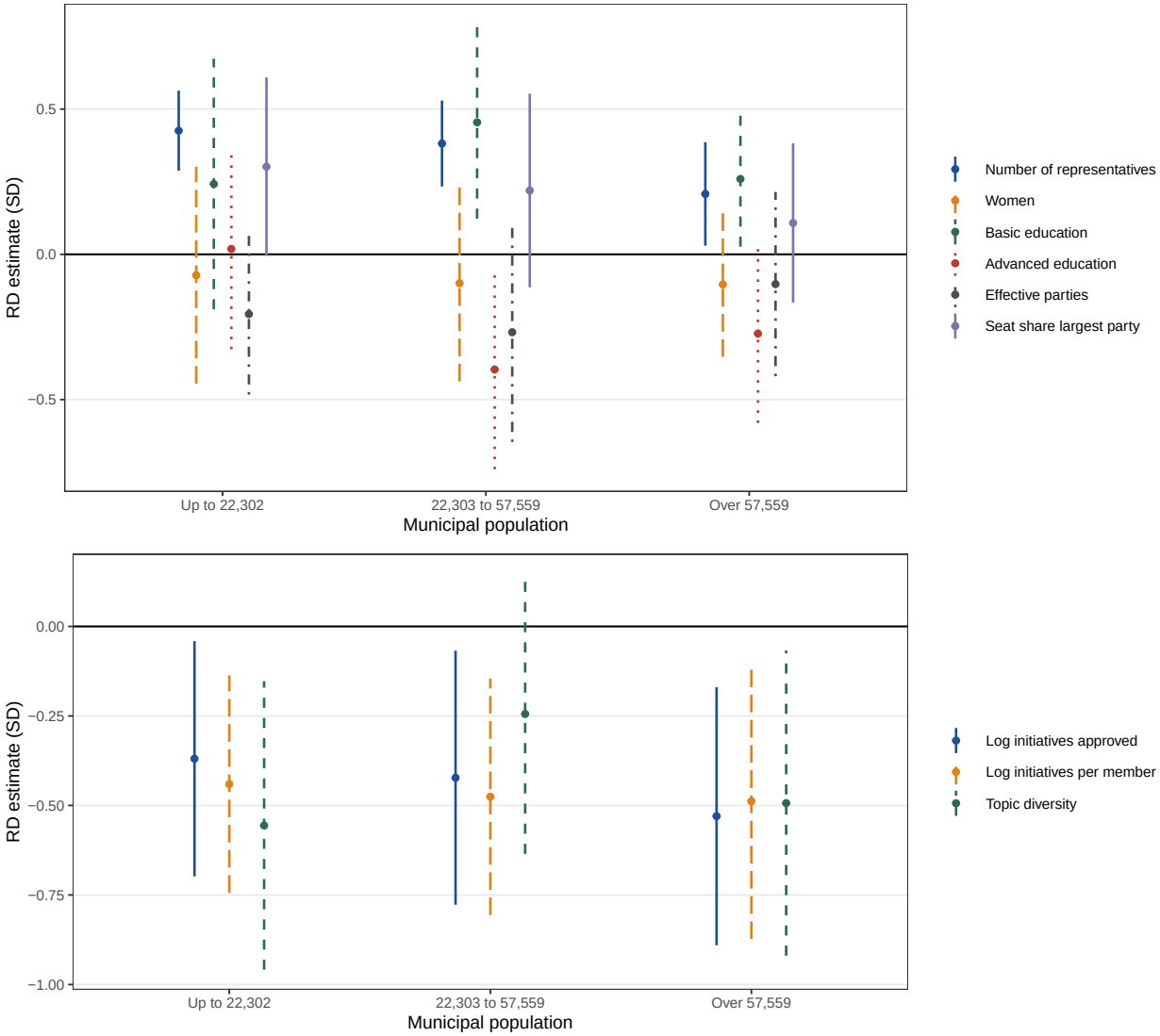
Service	RD estimate		p-value	95% CI	Control mean	N
All public services	0.214	(0.283)	[0.452]	[-0.35, 0.78]	6.12	141
Streets and roads	0.163	(0.533)	[0.761]	[-0.90, 1.23]	5.43	141
Street lighting	-0.128	(0.314)	[0.685]	[-0.75, 0.50]	5.97	141
Refuse collection	0.520	(0.396)	[0.194]	[-0.27, 1.31]	7.47	141
Municipal police	0.301	(0.206)	[0.148]	[-0.11, 0.71]	5.59	141

Satisfaction with the services the municipality delivers, reported by residents on a scale from zero to ten in the National Survey of Government Quality and Impact and averaged to the municipality. All public services is the mean across the four named services. The survey samples municipalities rather than enumerating them, so coverage is a fraction of the estimation window and the confidence intervals are correspondingly wide. 30 of the population threshold, with state-by-threshold and electoral-cycle fixed effects and standard errors clustered at the municipality.

D Heterogeneous treatment effects by quartile and by number of representatives

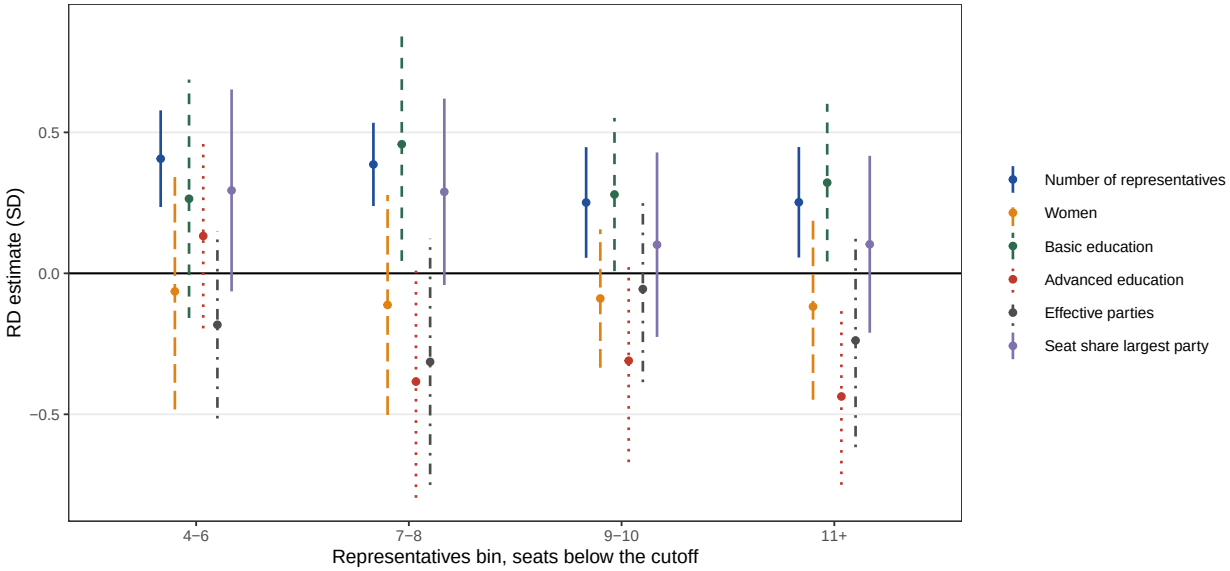
I split the estimation window two ways: into terciles of municipal population, and by the number of seats a council holds below the cutoff. The discontinuity is estimated separately within each group, with the running-variable slopes common across groups so that only the treatment effect varies. A Wald test cannot reject that the estimates are equal for any outcome under either split.

Figure A3. Effect of crossing the population threshold on political representation and on legislative output, by tercile of municipal population



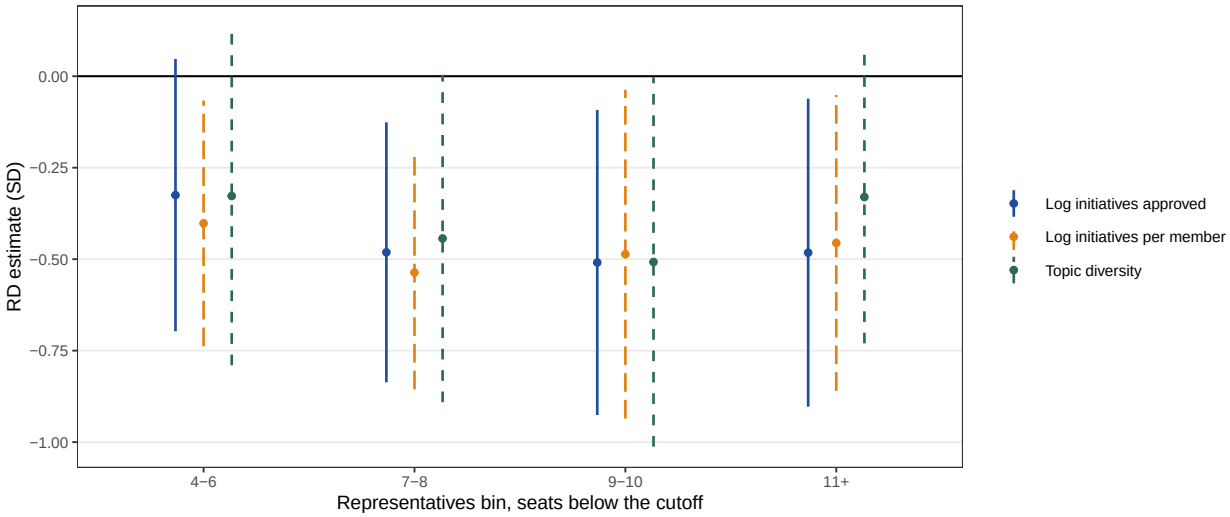
Municipalities in the estimation window are grouped into terciles of municipal population, cut at 22,302 and 57,559 inhabitants, and the discontinuity is estimated separately within each. The terciles are computed on the window rather than on the whole panel, so each holds about a third of the municipality-cycles the estimates use. Estimates are divided by the standard deviation of the outcome among municipalities below the cutoff, so the vertical axis is in standard deviations. Bars are ninety five percent confidence intervals.

Figure A4. Effect of crossing the population cutoff on number of representatives and political representation, by baseline number of representatives



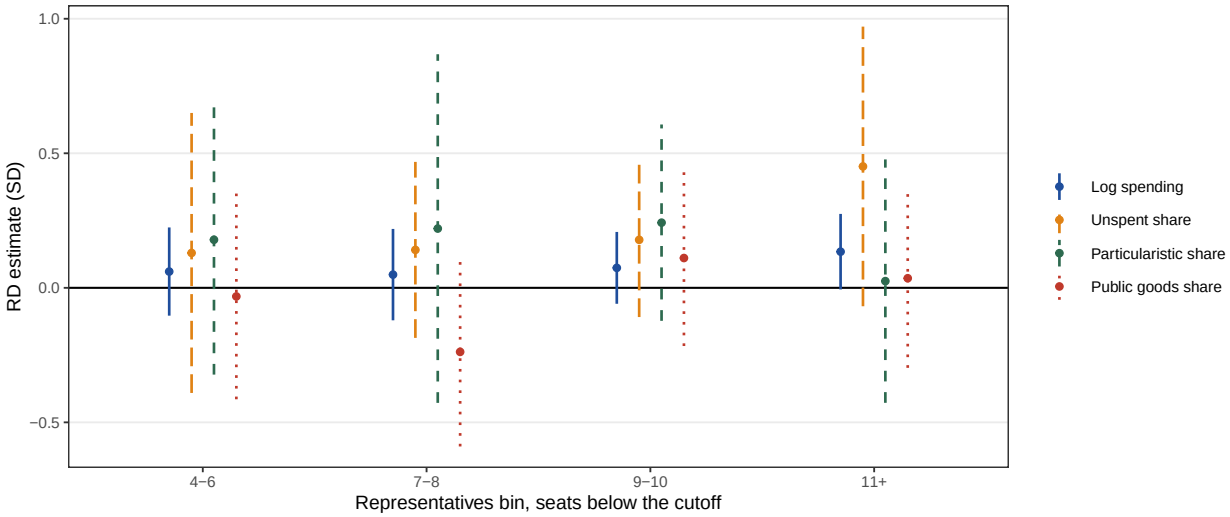
Estimates are divided by the standard deviation of the outcome among municipalities below the cutoff, so the outcomes share an axis. Bars are ninety five percent confidence intervals. Bins hold 174 to 240 municipality-cycles.

Figure A5. Effect of crossing the population cutoff on legislative output, by baseline number of representatives



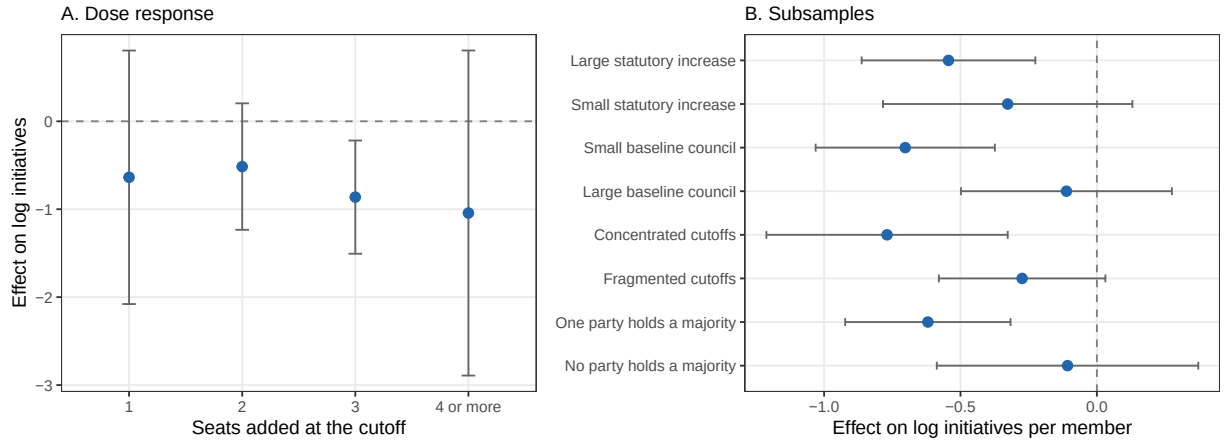
Estimates are divided by the standard deviation of the outcome among municipalities below the cutoff. Bars are ninety five percent confidence intervals. Bins hold 175 to 223 municipality-cycles.

Figure A6. Effect of crossing the population cutoff on public spending, by baseline number of representatives



Estimates are divided by the standard deviation of the outcome among municipalities below the cutoff. Bars are ninety five percent confidence intervals. Bins hold 182 to 226 municipality-cycles.

Figure A7. Effect of crossing the population cutoff on legislative output, by seats added and by subsample



Panel A estimates the effect on log approved initiatives separately by the number of seats the statute adds at the threshold. Panel B reports the effect on log initiatives per member on the subsamples used in the text. Bars are ninety five percent confidence intervals. The splits in the lowest two rows of Panel B condition on party control measured in the same term as the outcome; Table A15 replaces them with the previous term.

Table A14. Baseline council size and the fall in output per member

Specification	Group	Estimate		N
Median split on cell mean, separate slopes				
	Small	-0.561***	(0.148)	454
	Large	-0.320	(0.244)	268
Median split on baseline seats, separate slopes				
	Small	-0.539***	(0.139)	653
	Large	0.159	(0.390)	110
Median split on baseline seats, common slopes				
	Small	-0.479***	(0.137)	653
	Large	-0.274	(0.260)	110
Four bins on baseline seats, common slopes				
	4-6	-0.382**	(0.162)	223
	7-8	-0.509***	(0.154)	176
	9-10	-0.461**	(0.217)	189
	11+	-0.432**	(0.195)	175

The outcome is log initiatives per member. The first two blocks split the sample at the median and fit separate running-variable slopes on each side, once on the mean number of representatives on the control side of the cell and once on the modal number below the threshold. The last two blocks estimate one pooled regression in which the running-variable slopes are common and only the treatment effect varies by group, which is the specification the paper uses. The large-council group holds 110 municipality-cycles, so a separate slope fitted inside it is poorly determined and can absorb the level shift.

Table A15. Seat concentration measured before the council takes office

Outcome	Seat concentration measured	One party holds a majority		No party holds a majority	
Log initiatives approved					
Same term as the outcome		-1.007***	(0.271)	-0.004	(0.392)
Previous term		-0.578**	(0.293)	-0.812	(0.608)
Log initiatives per member					
Same term as the outcome		-0.620***	(0.154)	-0.108	(0.243)
Previous term		-0.381**	(0.164)	-0.569	(0.369)
Agenda diversity					
Same term as the outcome		-0.628***	(0.188)	0.142	(0.279)
Previous term		-0.097	(0.237)	-0.303	(0.471)

Each row is a separate pair of regressions on the two subsamples the column headings name. The first row of each block splits councils by whether one party holds at least half the seats in the same term as the outcome, which the election that seated the council determines and which is therefore not fixed in advance. The second row splits on the same measure taken in the previous term, which is. The pre-treatment split is available for 417 of the 826 municipality-cycles inside the window, since it requires the municipality to appear in a preceding cycle.

E Robustness

Table A16. Every reported outcome across bandwidths

	$h = 0.10$	$h = 0.15$	$h = 0.20$	$h = 0.30$	$h = 0.40$	$h = 0.50$
Representatives	2.110***	2.182***	2.265***	2.314***	2.366***	2.426***
	(0.393)	(0.316)	(0.246)	(0.183)	(0.152)	(0.132)
Log initiatives approved	-1.007**	-0.775**	-0.656**	-0.697***	-0.670***	-0.603***
	(0.418)	(0.322)	(0.267)	(0.218)	(0.196)	(0.178)
Log initiatives per member	-0.586**	-0.426**	-0.379**	-0.449***	-0.461***	-0.441***
	(0.247)	(0.198)	(0.164)	(0.131)	(0.119)	(0.107)
Topic diversity	-0.506*	-0.388*	-0.377**	-0.357**	-0.301**	-0.285**
	(0.288)	(0.220)	(0.182)	(0.149)	(0.133)	(0.119)
Log spending	0.079	0.088	0.086	0.089	0.065	0.043
	(0.162)	(0.111)	(0.086)	(0.066)	(0.057)	(0.051)
Unspent share of income	0.027	0.019	0.018*	0.013	0.010	0.005
	(0.017)	(0.013)	(0.011)	(0.009)	(0.008)	(0.007)
Particularistic share	0.017	0.015	0.010	0.008	0.007	0.007
	(0.031)	(0.018)	(0.013)	(0.010)	(0.008)	(0.007)
Public goods share	-0.022	-0.019	-0.016	-0.005	-0.008	-0.015
	(0.041)	(0.031)	(0.026)	(0.021)	(0.019)	(0.017)
Alignment with citizen preferences	-0.169	-0.210	-0.163	-0.161*	-0.123	-0.100
	(0.219)	(0.147)	(0.112)	(0.091)	(0.081)	(0.072)

The paper uses $h = 0.30$. Bandwidths are shares of each municipality's own cutoff.

Table A17. First stage across bandwidths

	$h = 0.10$	$h = 0.15$	$h = 0.20$	$h = 0.30$	$h = 0.40$	$h = 0.50$
Representatives, statute	2.110***	2.182***	2.265***	2.314***	2.366***	2.426***
	(0.393)	(0.316)	(0.246)	(0.183)	(0.152)	(0.132)
Observations	257	405	575	823	1,020	1,258
Representatives, government census	1.311***	1.232***	1.222***	1.169***	1.184***	1.295***
	(0.371)	(0.326)	(0.281)	(0.244)	(0.219)	(0.196)
Observations	257	405	575	823	1,020	1,258

The count set by state law comes from state electoral law and the census count from the number of representatives the Census of Municipal Governments records in office. Bandwidths are shares of each municipality's own cutoff.

Table A18. Discarding the municipalities closest to the threshold

Outcome	No donut	$\|x\| > 0.01$	$\|x\| > 0.02$	$\|x\| > 0.05$
Number of representatives, census	1.169***	1.036***	1.211***	1.275**
	(0.244)	(0.266)	(0.305)	(0.510)
	823	805	769	689
Log initiatives approved	-0.697***	-0.741***	-0.589**	-0.627*
	(0.218)	(0.248)	(0.269)	(0.356)
	773	756	724	644
Log initiatives per member	-0.449***	-0.486***	-0.419**	-0.600***
	(0.131)	(0.148)	(0.162)	(0.208)
	773	756	724	644
Agenda diversity	-0.357**	-0.445***	-0.249	-0.292
	(0.149)	(0.165)	(0.175)	(0.222)
	719	703	673	601

Each column re-estimates the discontinuity after dropping municipalities within the stated proportional distance of the threshold, so that any influence of observations at the edge is removed.

Table A19. Placebo cutoffs away from the population threshold

	−0.2 below	−0.15 below	0.15 above	0.2 above
Log initiatives approved	0.230	0.380	-0.083	0.601
	(0.275)	(0.328)	(0.377)	(0.389)
Log initiatives per member	0.196	0.239	0.010	0.321
	(0.173)	(0.203)	(0.242)	(0.210)
Topic diversity	0.050	0.373**	-0.290	0.035
	(0.179)	(0.181)	(0.221)	(0.254)
Log spending	-0.051	0.105*	-0.082	0.106
	(0.065)	(0.061)	(0.112)	(0.144)
Unspent share of income	0.001	0.007	-0.011	0.032**
	(0.012)	(0.010)	(0.010)	(0.016)
Particularistic share	-0.002	-0.007	0.006	0.021
	(0.010)	(0.012)	(0.010)	(0.015)
Public goods share	-0.036	0.060*	-0.055*	-0.064*
	(0.025)	(0.032)	(0.028)	(0.038)
Alignment with citizen preferences	0.003	-0.012	0.120	-0.108
	(0.136)	(0.140)	(0.138)	(0.198)

Each column moves the cutoff to a point where state law does not change the number of representatives, at fifteen or twenty percent of the cutoff away from it. Placebos are one-sided. A false cutoff below the true one draws only on municipalities below it, and a false cutoff above draws only on those above, so no placebo window straddles the real discontinuity. The bandwidth is 0.15.

Table A20. Benjamini-Hochberg adjusted significance across every reported outcome

Outcome	RD estimate		p-value	BH q within family	BH q all outcomes
Descriptive representation					
Women	0.404**	(0.193)	[0.037]	0.093	0.141
Basic education	1.204***	(0.313)	[0.000]	0.001	0.003
Advanced education	-0.291	(0.400)	[0.468]	0.468	0.556
Effective number of parties	-0.200	(0.150)	[0.185]	0.231	0.270
Seat share of largest party	0.030	(0.019)	[0.115]	0.192	0.219
Representative background					
Private sector	0.452*	(0.242)	[0.062]	0.187	0.170
Own business	0.312	(0.414)	[0.452]	0.678	0.556
First job	0.421*	(0.252)	[0.095]	0.190	0.200
Local office	0.211	(0.370)	[0.569]	0.682	0.636
Federal office	-0.361*	(0.184)	[0.051]	0.187	0.161
Party post	0.033	(0.106)	[0.757]	0.757	0.800
Legislative performance					
Log initiatives approved	-0.697***	(0.218)	[0.001]	0.002	0.009
Log initiatives per member	-0.449***	(0.131)	[0.001]	0.002	0.006
Topic diversity	-0.357**	(0.149)	[0.017]	0.017	0.081
Public spending					
Log spending	0.089	(0.066)	[0.177]	0.354	0.270
Unspent share of income	0.013	(0.009)	[0.154]	0.354	0.267
Particularistic share	0.008	(0.010)	[0.391]	0.521	0.530
Public goods share	-0.005	(0.021)	[0.809]	0.809	0.809
Representation					
Alignment with citizen preferences	-0.161*	(0.091)	[0.079]	0.079	0.187

Each row is a separate regression, on the same specification as Tables 5 and 6. The q values control the false discovery rate of Benjamini and Hochberg (1995), computed within each family of outcomes and across all nineteen hypotheses at once. The first stage is excluded, since number of representatives is not a hypothesis under test.