Supplementary Materials for "Legislature Size and Welfare: Evidence from Brazil"

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Contents

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B.1 A Model of Legislature Size and Service Provision

B.1.1 Primitives of the Model

Consider an strategic interaction between a mayor (M) and $N \geq 3$ city councilors, indexed in $i \in \{1, 2, \cdots, N\}$. The mayor is the head of the local executive branch, and has the prerogative of proposing a vector of policies, that is voted by the city councilors. Policy proposals are a combination of public goods provision g, rents for the mayor r, and a vector of transfer for city councilors $\mathbf{x}$.

The city council votes the mayor's proposal and if they accept it, it is implemented. Otherwise, a reversal policy takes place. Reversal policy in this context means that the mayor's proposal failed to secure majority of council votes. The decision leaves the mayor's hands, and either immediately stops or it is transferred to the council. The types of reversal policies are crucial for our argument, as it changes the relative strength of the councilors in their bargaining with the mayor. We investigate three types of reversal policies: a *non-partisan* reversal mechanism, where city councilors have no partisan concerns; a *partisan* reversal mechanism, where the council is divided in supporters and non-supporters of the mayor; and finally a *hybrid* reversal mechanism, that combines partisan concerns with non-partisan rent extraction. In the main paper, we focus on the hybrid model, as it is the more realistic scenario. Despite that, comparing the partisan and non-partisan reversal mechanisms illustrate clearly our argument that the partisan a mechanism is what drives our results.

To govern, the mayor has to convince at least half of the city councilors to support her policy proposals. Similar to selectorate models (Bueno De Mesquita et al. 2005), we assume that the selectorate, i.e. the individuals that have a say in the mayor's policy proposals, are the N city councilors. The winning coalition, which is the minimum number of members that have to be convinced to implement the policy, is equal to half of the selectorate $N/2$. City councilors are moved by policy concerns p and transfers received from the mayor $\mathbf{x}$. The use of transfers to affect the councilors' choices generate a cost of govern, C_G, that vary with the reversal mechanism and the legislature size.

Finally, after paying the costs of govern, the mayor proposes a level of public goods provision (g) and rents (r). Public goods provision help the mayor to get reelected. Rents are for direct mayor's consumption, and does not contribute to the mayor's electoral success. This makes the mayor's expected utility a sum of the gains from rents and the benefits from reelection. Both the utilities from rents $u(r)$ and the probability of reelection $\pi(g)$ are concave functions, meaning that more rents or public goods increase the utility at a decreasing rate.[1] The probability of reelection is multiplied by the benefit from holding office $B_M > 0$. This benefit captures the tangible and intangible gains that the mayor perceives from holding the public office. We assume that the benefits are high enough to rule out an equilibrium with only rents and transfers. If the mayor's policy proposal is approved, then the expected utility for the mayor is:

[1] In technical terms, $u' > 0$ and $\pi' > 0$; and $u'' < 0$ and $\pi'' < 0$.

$$\mathbb{E}U(r, g) \;=\; u(r) + B_M \pi(g)$$

The policy choices of the mayor are subject to the municipal budget constraint. The municipality has $R > 0$ resources, and cannot run debts, meaning that the budget has to be balanced. We also assume that there are enough resources for the mayor to govern.

Let C_G be the costs of govern. The budget balance constraint requires the offers that the mayor makes to the city councilors satisfy the following inequality:

$$r + g + C_G \;\leq\; R$$

The expected utility for the city councilors depend on the reversal policy. As discussed, we explore three different mechanisms. First, in *non-partisan* reversal mechanism, when the council rejects the mayor's proposal, a reversal stage starts with resources diminish by a factor of $\delta \in (0, 1)$ and the selection of one councilor randomly. The randomly selected councilor becomes now the proposer. If her proposal is accepted, it is implemented. If it is rejected, the budget shrinks again by a factor δ, and another city councilor is recognized to make the proposal. The process repeats, until one proposal is finally accepted.

In the second reversal mode, the *partisan* reversal mechanism, we assume that each councilor has a party affiliation. Party affiliations are mutually exclusive (a councilor cannot belong to two parties at once). If a city councilor is affiliated to a party aligned with the mayor, we say that she belongs to the government coalition $G \subset \{1, 2, \cdots, N\}$. Otherwise, the city councilor belongs to the opposition $O \not\subset G$. The mayor's policy generate a political value of $p > 0$ for the councilors, and whether it adds or subtracts from the councilor's utility depends of the party affiliation of the councilor. If the mayor's proposal is rejected, a city councilor aligned with the mayor, that would receive p from the policy, now receives zero. On the other hand, a city councilor that belongs to the opposition would receive $-p$ from the mayor's policy, and now receives zero. Although these assumptions are strong, they capture the underlying political dynamics of partisanship: the political preferences in terms of public goods provision are shared by councilors aligned with the mayor. Voting against the mayor's proposals would represent to defy the partisan brand.

In the last reversal mechanism, the *hybrid* reversal mechanism, we combine partisanship with legislature bargaining rents. Councilors in this mechanism receive a partisan utility from the mayor's policy, but if they reject the policy, a reversal policy similar to the non-partisan reversal mechanism takes place. In the mayor's proposal stage, city councilors that belong to the mayor's coalition receive a gain of $p > 0$ when the mayor's policy is implemented. Opposition members, on the other hand, receive $-p$ when the mayor's proposal is implemented. If the mayor's proposal is rejected, as city councilors do not control the Executive when the two branches are separated, no public goods are provided ($p = 0$). Moreover, in equilibrium, we assume that city councilors would prefer to receive rents instead of providing public goods. This aligns with the empirical evidence in most developing economies.

The timeline of the game is as follows:

1. The mayor learns how many government $|G|$ and opposition $|O|$ legislators were elected.

2. The mayor proposes a policy vector $(r, g, x)_M$.

3. The city council votes the proposal.

   - If the council accepts, the policy is implemented and the game ends.

   - Otherwise, the reversal policy is implemented.

4. (Reversal Policy) Depends on which mechanism, we have three different reversal policies. The details are in the following sections.

This is a bargaining game, along the lines of Rubinstein (1982). For a political science models similar to this model, see Baron and Ferejohn (1989) and, to some extent, Bueno De Mesquita et al. (2005).

The solution concept we use in this model is the Sub-game Perfect Nash Equilibrium. A Sub-game Perfect Nash Equilibrium requires that the strategies played are a Nash Equilibrium in each of the subgames of the game. The solution is found using backward induction. In the model with infinitely repeated proposals, we extend the equilibrium concept to require that the equilibrium is Stationary. A Stationary Sub-game Perfect Equilibrium is based on the assumption that, at each given point of the game, if a politician accepts an offer at time $k + 1$, she should accept the same offer at time k. Stationarity gives us an objective of finding an offer that would be accepted at any stage in the game. Applying this reasoning, we can find a sequence of offers for each point k in time. For the mayor the optimal offer is at $k = 0$, and this represents a no delay in policy implementation.

Definition 1. *The equilibrium for the game consists in a sequence of strategic policy vectors* $(g^*, r^*, x^*)_k$*, indexed in* $k \in \{0, 1, \cdots, \infty\}$*, such that, at every stage of the game, the mayor and the councilors have no profitable deviations from the equilibrium.*

Moreover, we assume that the players play no dominated strategies, even when there is less than sufficient support for implement the policy.

B.1.2 The Mayor's Decision Stage

Solving the game by backward induction would require us to start with the councilor's strategies. However, for simplicity, assume at this point that we solved the the game for the councilors, and found the equilibrium cost of govern $C_G(N)$ when there are N city councilors. Below we define $C_G(N)$ for each of the mechanisms, but for now we use this assumption to simplify the exposition.

We now derive the optimal rents (r) and public goods provision (g) proposed by the mayor. The mayor benefits from public goods provision, as it increases the chances of her reelection. However, she prefers to contribute as minimum as necessary for the public goods, and extract the remaining resources as political rents. The mayor's objective is to maximize the following expected utility, subject to the municipal budget constraint.

$$\begin{aligned} \max_{r,g} \quad & u(r) + B_M \pi(g) \\ \text{s.t.} \quad & r + g + C_G(N) \leq R \end{aligned}$$

In equilibrium the budget binds, and the public goods provision maximizes:

$$\max_{g} \{u(R - g - C_G(N)) + B_M \pi(g)\}$$

The first order condition for an optimal public goods provision makes the marginal benefits to increase rents, in this case, marginal costs of providing the public goods, equals to the marginal benefits of reelection:

$$u'(R - g - C_G(N)) = B_M \pi'(g)$$

This is sufficient for the equilibrium, as the second order condition reassures the concavity of the mayor's expected utility: $u''(R - g^* - C_G(N)) + B_M \pi''(g^*) < 0$.

Proposition 1. *The provision of public goods increase with legislature size when the costs of govern decrease with legislature size.*

Proof. To prove this result we need to show that the mayor's expected utility satisfy the increasing differences in g and N. This would mean that when increasing the size of the council, the optimal solution g^* would also increase. However, as N increases discretely, we cannot take a the cross-partial derivative on N or even use the implicit function theorem. Instead, we use monotone comparative statics to derive these results (Milgrom and Shannon 1994).

The mayor's expected utility satisfy the increasing differences in g and N when, for $g > g'$ and $N + 1 > N$,[2] we have:

$$\mathbb{E}\text{U}(g, N + 1) - \mathbb{E}\text{U}(g', N + 1) \geq \mathbb{E}\text{U}(g, N) - \mathbb{E}\text{U}(g', N)$$

[2]Moreover, the requirement that the decision and type space is a complete lattice, is satisfied for $\mathbb{R} \times \mathbb{N}$.

After substituting, we have:

$$u(R-g-C_G(N+1))+B_M\pi(g)-u(R-g'-C_G(N+1))-B_M\pi(g') \geq u(R-g-C_G(N))+B_M\pi(g)-u(R-g'-C_G(N))-B_M\pi(g')$$

Which is equal to:

$$u(R-g-C_G(N+1)) - u(R-g'-C_G(N+1)) \geq u(R-g-C_G(N)) - u(R-g'-C_G(N))$$

Multiplying both sides by $\frac{1}{g-g'}$ and taking the limit when $g \longrightarrow g'$, we have:

$$u'(R-g-C_G(N+1)) \geq u'(R-g-C_G(N))$$

And the function satisfies the increasing differences in g and N when the costs of govern an $N+1$-sized city council is lower than the costs of govern an N-sized city council:

$$C_G(N+1) \leq C_G(N)$$

□

This provides us our first empirical hypothesis to test: if the cost of govern decreases when the legislature increases, then the public goods provision increase when the legislature size increase.

At this stage, we need to solve for the costs of govern to determine whether this hypothesis is true or not. Below we show that it is true for the *partisan* and *hybrid* mechanisms, but it is not true for the *non-partisan* mechanism.

B.1.3 Non-Partisan Reversal Mechanism

In the *non-partisan reversal* mechanism, we assume that when the council rejects the mayor's office, a reversal stage starts with the selection of one councilor randomly. To find the stationary sub-game perfect Nash equilibrium, suppose that there were $k-1$ rejections, and we are at the k-th stage in the game. A councilor accepts the proposer's offer if, and only if, to accept the offer is better than to wait until the next stage. If the offer is x_i, then:

$$x_i \geq \frac{1}{N}\left[\delta^{k+1}R - \frac{N}{2}x_i\right] + \left(1-\frac{1}{N}\right)\left(\frac{1}{2}\right)[x_i]$$

In the left-hand side, we place the offer. In the right-hand side, there are two components. The first is the amount that the councilor i gets when she is the proposer. It is equal to the budget in the next round minus the offers that she has to make to convince half of the councilors. The second part is the gains if when the councilor happens to reject the current received offer, but still receives an offer in the following round. Note that the offer the councilor makes as the proposer is the same as the offer that she wants to receive. This because the city councilors are exchangeable, and the solution is symmetric for all councilors receiving an offer (this means that we could have dropped the i in the solution). After some algebra, the offer x_i has to be greater than or equal to:

$$x_i \geq \frac{2\delta^{k+1}R}{2N+1} \equiv \underline{x}(k)$$

The proposer always offer the minimum possible to get the proposal approved. In the case, the offer at any given stage k is going to be equal to $\underline{x}(k)$.

Proceeding backwards, at the mayor's proposal stage $k = 0$ and the mayor is going to offer $\underline{x}(0, N) = \dfrac{2\delta R}{2N+1}$ to half of the councilors. In this context, the costs of govern are equal to:

$$C_G(N) = \frac{\delta RN}{2N+1}$$

Proposition 2. *In the non-partisan reversal mechanism, the costs of govern increase as the size of the legislature increases.*

Proof. The difference in costs when there are $N+1$ city councilors versus when there are N city councilors is equal to:

$$\begin{aligned} C_G(N+1) - C_G(N) &= \frac{(N+1)\delta R}{2(N+1)+1} - \frac{N\delta R}{2N+1} \\ &= \frac{\delta R}{(2N+3)(2N+1)} \\ &> 0 \end{aligned}$$

Therefore, the costs always increase when the size of the legislature increases.

□

Corollary 1. *In the non-partisan reversal mechanism, public goods provision decreases as the legislature size increases.*

This result is crucial to understand the role of the politics in our model. If the costs of govern increase with legislature size, then any increase in public goods provision associated with legislature size has come from a different mechanism. In our case, we show that there is a *political* reason for increasing the public goods provision.

B.1.4 Partisan Reversal Mechanism

We now look into the other extreme, which is a fully politicized legislature. In this case, when the mayor implements the policy, it generates a value of $p > 0$. A legislator aligned with the mayor receives a benefit p while an opposition legislator receives a cost of $-p$. Moreover, as in the case of the non-partisan reversal, the mayor can choose to distribute x_i private goods for a legislator i. In this case, when the policy is implemented, the utility of an aligned legislator is equals to $x_i + p$. When the legislator is not aligned with the mayor, her utility is equals to $x_i - p$. If the policy is not implemented, then we assume a reversal policy of zero in all choice vectors $g = x_i = p = 0$.

A legislator aligned with the mayor always support the mayor's policy, regardless of receiving or not private goods. This because as $p > 0$, the councilor always receive positive benefits from the policy.[3] However, politicians not aligned with the mayor require a compensation when their votes are needed. In this case, if the mayor's proposal requires the support of an opposition politician, then the mayor compensates the councilor for the policy costs. In such case:

$$x_i \geq p$$

Optimality dictates that the mayor offers $x_i = p$ to the opposition legislator. Let γ be the ex-ante probability of electing a politician aligned with the mayor. Then, the costs of govern when the legislature size is N is equal to the expected number of politicians in the opposition that the mayor has to compensate minus the amount needed to implement the policy, in this case, half of them:

$$C_G(N) \ = \ p\left(\frac{N}{2} - \gamma N\right)$$

Proposition 3. *In the partisan reversal mechanism, if the chance of electing a mayor-aligned politician is greater than one-half, then the costs of govern decrease as the legislature size increases.*

Proof. The differences in costs when there are $N + 1$ versus when there are N legislators are equal to:

$$\begin{aligned} C_G(N+1) - C_G(N) &= p\left(\frac{N+1}{2} - \gamma(N+1)\right) - p\left(\frac{N}{2} - \gamma N\right) \\ &= p\left(\frac{1}{2} - \gamma\right) \end{aligned}$$

[3] The mayor could even impose a cost to the city councilors aligned with the government. Although this seem unrealistic, it is mostly the case when government-aligned legislators are recognized to draft legislation for the mayor's office, or even take the popular fallout from a failed policy.

Therefore, when $\gamma < \frac{1}{2}$, the costs of govern increase. Otherwise, the costs decrease.

$\square$

Corollary 2. *In the partisan reversal mechanism, if the chance of electing a mayor-aligned politician is greater than* $1/2$*, then public goods provision increase as the legislature size increases.*

This result shows that when the chances of electing an aligned politician is sufficiently high, then the costs of govern decrease. As a response, the public goods provision (and also rents), increase. The rate distribution between these two vectors is determined by the marginal change in the utility from rents and the utility from reelection. In any case, the amount allocated for both increase.

B.1.5 Hybrid Partisan and Non-partisan Reversal Mechanism

The *hybrid reversal* mechanism combines both partisan and non-partisan motivations. In the hybrid mechanism, a city councilor aligned with the government favors the mayor's offer if:

$$x_i \geq \frac{2\delta R}{2N+1} - p$$

An opposition politician, on the other hand, favors the mayor's offer if, and only if:

$$x_i \geq \frac{2\delta R}{2N+1} + p$$

We can decompose these costs in two components. First, the costs in terms of rents. Second, the costs (or benefits) from political alignment. In this context, the governing costs depend on the mayor's offers for councilors in the mayor's coalition versus the mayor's offers for councilors in the opposition. The ex-ante chance of a opposition member be elected is equal to $1-\gamma$, and taking the weighted averages, these costs become:

$$C_G(N) \;=\; \frac{N}{2}\left(\frac{2\delta R}{2N+1} - p\right) + \left(\frac{N}{2} - \gamma N\right) p$$

Proposition 4. *In the hybrid reversal mechanism, if* $\gamma \leq \frac{1}{p}\left[\frac{1}{(2N+1)(2N+3)}\right]$*, then the costs of govern increase as the legislature size increases. Otherwise, the costs decrease when the legislature size increases.*

Proof. The difference in costs when there are $N+1$ city councilors versus the costs when there are N councilors is equal to:

$$
\begin{aligned}
C_G(N+1) - C_G(N) &= \frac{N+1}{2}\left(\frac{2\delta R}{2(N+1)+1} - p\right) + \left(\frac{N+1}{2} - \gamma(N+1)\right)p \\
&\quad - \frac{N}{2}\left(\frac{2\delta R}{2N+1} - p\right) - \left(\frac{N}{2} - \gamma N\right)p \\
&= \frac{\delta R}{(2N+3)(2N+1)}\left[1 - \gamma(2N+1)(2N+3)p\right]
\end{aligned}
$$

The conditions for the differences in governing costs be decrease when the legislature size increases is:

$$
\gamma \geq \frac{1}{p}\left[\frac{1}{(2N+3)(2N+1)}\right] \equiv \underline{\gamma}
$$

□

Corollary 3. *In the hybrid reversal mechanism, public goods provision decreases as the legislature size increases.*

Also, the threshold $\underline{\gamma}$ for the probability of electing a politician aligned with the government is decreasing in both the political value of the mayors policy p and the size of the legislature N. This means that higher political values and larger legislatures makes it easier to satisfy the electoral threshold.

B.1.6 Main Hypotheses

Our model provides two empirically testable hypotheses. Consider a city council comprised of N legislators, that increase its council size to $N+1$. Then:

H1. The provision of public goods increase when the governing costs decrease with legislature size.

H2. The governing decrease with legislature size when the chance of electing a government aligned legislator is sufficiently high.

In the paper, we show that the provision of education and healthcare improves in Brazil, and this results from the fact that the chances of electing a city councilor aligned with the mayor is of 91%, thanks to the rule of distributing seats adopted in the country.

B.2 Variable Sources and Descriptive Statistics

We use three information sources, either from Brazilian governmental agencies or available online. Table 1 displays the primary sources and their respective URLs.

Now let us define each variable.

B.2.1 Outcomes Aggregated at the Municipal Level

- **Number of Seats 2000**: Number of councilors in the municipality by the 2000 elections (source: TSE).
- **Population 2000**: Municipal population according to the 2000 Brazilian Census (source: IBGE).
- **Per-Capita GDP Census 2000 (in millions)**: Municipal per-capita GDP computed by the IBGE Census (source: IPEA).
- **Proportion of Poverty Census 2000**: Proportion of poverty in the municipality defined as people living on less than R$ 70.00 a day (source: MDS).
- **Number of Seats 2004**: Number of city-councilors in a municipality according to the Electoral Justice decision (source: TSE).
- **Infant Mortality 2005–2008**: Infant mortality computed as the number of children born alive that died before reaching one year of age divided by the number of children born alive, multiplied by 1,000 (source: DataSUS)
- **Post-Natal Mortality Rate 2005–2008**: Infant mortality computed as the number of children born alive that successfully lived 28 days, but died before reaching one year of age, divided by the number of children born alive, multiplied by 1,000 (source: DataSUS)
- **Coverage Family Health Program 2005–2008**: Number of families covered by the Family Health Program (source: DataSUS).
- **Prop. Born w. 7+ Pre-Natal 2005–2008**: Proportion of children born who went to seven or more prenatal care consultations (source: DataSUS)
- **Enrollment Elementary School 2005–2008**: Number of children enrolled in elementary schools, averaged by classroom size (K–4) (source: INEP).
- **Enrollment Middle School 2005–2008**: Number of children enrolled in middle schools, averaged by classroom size (5–9) (source: INEP)

Table 1: Data Sources

Source Code	Source Name	Description	URL
DataSUS	Brazilian Health Ministry Data Service	Collects data on Health Care	http://www.datasus.gov.br
IBGE	Brazilian Institute of Geography and Statistics	Collects data Geography, Economics and Demography	http://www.ibge.gov.br
INEP	Data Service of Ministry of Education	Collects data on performance of Education	http://www.inep.gov.br
InterLegis	Senate Legislative Data Service	Collect Data on Legislative in Brazil	http://www.interlegis.leg.br
IPEA	Brazilian Institute of Applied Economics	Collects data on the Economy	http://www.ipeadata.gov.br
MDS	Social Security Ministry	Collect data on Social Coverage and Effectiveness of Social Programs	http://www.mds.gov.br
TSE	Superior Electoral Tribunal	Collects data on elections results	http://www.tse.jus.br

- **Quality of Elementary School Index 2005–2008**: IDEB score averaged by schools in the municipality. IDEB scores are composed by the student's grades in math and language, multiplied by an indicator of the distortion between the year the child is supposed to be and the year the child is. The estimators are cleaned to avoid influences of schools and classroom specific effects (source: INEP, years: 2005, 2007).
- **Quality of Middle School Index 2005–2008**: IDEB score averaged by schools in the municipality. IDEB scores are composed by the student's grade in math and language, multiplied by a score capturing the distortion between the year the child is supposed to be in, and the year the child is studying. The estimators are cleaned to avoid the influence of schools and classrooms specific effects. (source: INEP, years: 2005, 2007).
- **Mayoral Pre-Electoral Coalition Size 2004**: Number of elected councilors in the mayoral pre-electoral coalition (source: TSE)
- **Number of Appointed Bureaucrats 2005–2008**: Number of bureaucrats of the direct administration that were appointed to jobs in the municipality (source: IBGE, years: 2005, 2006, and 2008)
- **Number of Career Bureaucrats 2005–2008**: Number of career bureaucrats in the municipality. Career bureaucrats cannot be fired by politicians and earn considerably better pensions after retirement (source: IBGE, years: 2005, 2006, and 2008)
- **Number of Councilor's Appointed Assistants 2005**: Number of assistants appointed for the councilor cabinet (source: InterLegis)
- **Number of Females Elected 2004**: Number of females elected to city council (source: TSE)
- **Number of Non-Whites Elected 2004**: Number of non-whites elected to city council. We collected this data for municipalities less than 10,000 inhabitants away from the cutoffs (source: our compilation based on the TSE candidate pictures).
- **Competition per Seat 2004**: Number of people running for city-councilor divided by the number of city councilors in the municipality (source: TSE)
- **Proportion Approved Legislation 2005**: Number of approved legislation in 2005 divided by the number of proposed legislation (source: InterLegis)

B.2.2 Legislation Dataset

We coded the legislation approved by the councilors in 64 municipalities that are 10 thousand inhabitants away from the council size thresholds. We classified the legislation in five categories:

- **Local Public Goods**: Legislation that provides a service targeted at the individual, group, or geographical level.
- **Public Goods or General Legislation**: Legislation that provides a service for the entire municipality, or approves a law that does not discriminate among citizens.
- **Oversight**: Legislation that requests information to the mayor's office or the bureaucracy about the provision of services.
- **Others**: Legislation that is not classified as Local Public Goods, Public Goods, or Oversight. Usually honors or procedures.
- **Education and Health Care**: Legislation about Education or Health Care provision.

B.2.3 Online 2016 Former City Councilors Survey

We run an online survey asking 174 councilors whether helping with [...service...] gives [none, few, some, or many] votes. The services were:

- **Help Hospital Admission**: We asked former councilors whether helping with hospital admission gives [none, few, some, many] votes (source: survey).
- **Help School Admission**: We asked former councilors whether helping with school admission gives [none, few, some, many] votes (source: survey).
- **Help Getting Medication**: We asked former councilors whether helping voters get medication gives [none, few, some, many] votes (source: survey).
- **Fix Street Potholes**: We asked former councilors whether pressing the bureaucracy to fix street potholes gives [none, few, some, many] votes (source: survey).
- **Other Local Public Goods**: We asked former councilors whether helping voters with local public goods and services such as traffic rules, sewage constructions, potable water, and others, gives [none, few, some, many] votes (source: survey).
- **Legislative Duties**: We asked former councilors whether the act to discuss, propose, and vote on legislation gives [none, few, some, many] votes (source: survey).
- **Honors Legislation**: We asked former councilors whether proposing bills to praise local celebrities gives [none, few, some, many] votes (source: survey).

- **Oversee School Quality**: We asked former councilors whether overseeing school quality gives [none, few, some, many] votes (source: survey).
- **Oversee Construction**: We asked former councilors whether overseeing public construction gives [none, few, some, many] votes (source: survey)

In this Appendix, we present other questions we asked in the survey. They are meant to improve the knowledge we have about the city councilors perceptions about their representation. We have not added these to the main paper because they were more complimentary and not of general interest. The descriptive statistics of the variables used in the main paper follow in Table 2.

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Table 2: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
	Municipal Characteristics				
Number of Seats 2000	5,521	10.76	2.70	9	21
Population 2000	5,474	22,341.96	44,573.67	697	567,728
Per-Capita GDP Census 2000 (in thousands)	5,474	0.13	0.46	0.00	13.57
Proportion of Poverty Census 2000	5,474	46.58	22.82	2.89	93.02
Number of Seats 2004	5,527	9.22	0.94	9	21
	Health Care Outcomes				
Infant Mortality 2005-2008	13,329	20.64	13.45	1.28	209.30
Post-Natal Mortality Rate 2005-2008	6,113	9.29	8.64	0.59	200.00
Coverage Family Health Program 2005-2008	20,577	76.97	27.39	0.00	100.00
Prop. Born w. 6+ Pre-Natal 2005-2008	16,578	54.02	24.30	0.57	100.00
	Education Outcomes				
Enrollment Elementary School 2005-2008	10,842	20.62	5.32	1.00	57.80
Enrollment Middle School 2005-2008	7,887	25.18	7.25	1.00	92.50
Quality of Elementary School Index 2005-2008	9,267	3.73	0.94	0.70	8.10
Quality of Middle School Index 2005-2008	5,566	3.21	0.76	0.30	6.60
	Resources and Patronage				
Mayoral Coalition Size 2004	5,522	4.86	1.71	0.00	17.00
Number of Appointed Bureaucrats 2005-2008	16,564	67.78	126.68	0	2,894
Number of Career Bureaucrats 2005-2008	16,558	436.13	698.17	0	11,633
Number Councilor Assistants 2005	5,523	4.63	8.55	0	213
	Representation and Competition				
Proportion Female Elected 2004	5,526	1.12	1.20	0.00	8.00
Proportion Non-White Elected 2004	397	2.23	1.87	0.00	9.00
Competition per Seat 2004	5,527	6.27	3.82	1.00	25.83
Proportion Approved Legislation 2005	3,694	0.83	0.28	0.00	1.00
	Legislation Approved Data				
Legislation – Local Public Goods	346,553	0.73	0.44	0	1
Legislation – Public Goods	346,553	0.03	0.18	0	1
Legislation – Oversight	346,553	0.03	0.18	0	1
Legislation – Others	346,553	0.17	0.38	0	1
Legislation – Education and Health	346,553	0.11	0.31	0	1
	Survey – Electoral Attractiveness of Services				
Votes – Help Hospital Admission	174	0.82	0.38	0	1
Votes – Help School Admission	174	0.59	0.49	0	1
Votes – Help get Medication	174	0.71	0.46	0	1
Votes – Fix Street Potholes	174	0.49	0.50	0	1
Votes – Local Public Goods	174	0.81	0.39	0	1
Votes – Legislive Duties	174	0.22	0.42	0	1
Votes – Honors Legislation	174	0.18	0.38	0	1
Votes – Oversee School Quality	174	0.29	0.45	0	1
Votes – Oversee Construction	174	0.24	0.43	0	1

Notes: The legislation approved dataset has data on 64 of the 202 municipalities 10 thousand inhabitants from the council size thresholds. The survey summary statistics here are unweighted. Numbers of cases vary due to missingness.

B.2.4 Threshold Manipulation, Sorting, and Pre-treatment Consistency

The validity of our causal claim relies on three assumptions. First, the impossibility of municipalities to manipulate the placement at the left or right-hand side of the thresholds. Second, that we are consistently comparing municipalities in the same cutoffs. Lastly, that we have no pre-treatment differences in our sample.

For the first assumption, McCrary (2008) proposes a measure of the distributional imbalance around the discontinuity, testing whether cases are more abundant in the left or the right of the cutoff. For our research, the McCrary statistic is 0.391 (SE = 0.299), showing no evidence of manipulation.

We run the Cattaneo et al. (2019) manipulation test, which is based on the density of the local polynomial estimator. We use local polynomial orders from one to four. As the null hypothesis is no manipulation, the p-values for each polynomial order are: local linear (p-value = 0.442); quadratic (p-value = 0.740); cubic (p-value = 0.998); and quartic (p-value = 0.620). Therefore, there is no manipulation according to Calonico et al. (2019).

We also graph the McCrary (2008) test, pooling all the discontinuities. The results follow in Figure 1 and show no signs of manipulation.

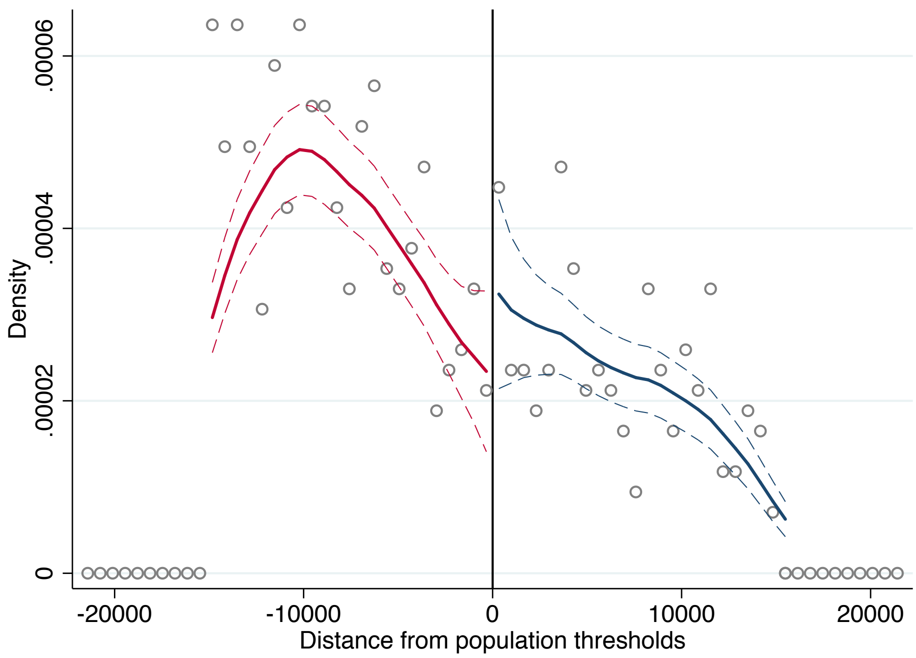

Figure 1: McCrary (2008) manipulation test

For the second assumption, we know that running RDD in multiple thresholds may lead to inconsistent estimates when there is an imbalance in the running variable's distribution around the cutoffs (Bertanha 2017; Cattaneo et al. 2016; Eggers et al. 2018). For example, in our dataset, we have twelve discontinuities, and at each threshold, we have a sharp increase of one councilor, from 9 to 21. When running an RDD on these discontinuities, a consistent estimator should fit an exact change of one councilor. However, if we pool all the discontinuities together with no correction, we find an increase in council size of 1.63 councilors. This imbalance is because when we combine all the discontinuities, we are implying that changes in municipalities right below the 47,619 cutoffs (9 to 10 councilors) are comparable to changes in towns right above the 571,428 cutoffs (20 to 21 council members). That is incorrect, not only because we could be comparing a municipality with nine councilors with one with 21 councilors, but also because these municipalities diverge in many other aspects, such as population to GDP per capita.

To avoid this problem, we show by simulation that adding controls, especially the variables responsible for the multiple threshold assignments (in our case, the population in 2003), improves the consistency and efficiency of the estimates. Relying on this fact, we add five controls to our estimates: population in 2003; GDP per capita; the number of seats in 2000; year; and a dummy for the northeast municipalities. The reason for the first variable is the multiple threshold assignments. We add GDP per capita because wealthier cities tend to be more productive and have better public services. The number of seats in 2000 intends to control for the fact that some municipalities could experience a change provoked by the previous council size, confounding our estimates. The year and the northeast dummies serve to improve efficiency. Adding controls improves efficiency in RDD (Calonico et al. 2019) and also enhances the consistency in the multiple thresholds RDD. See the following section for further tests and simulations.

For the last assumption, we should have no changes in pre-treatment covariates. These pre-treatment covariates are variables measured before the primary outcomes. As the threshold rule selected by the Brazilian Electoral Court was unpredictable, it should not detect any variation before the thresholds were in place.

Table 3 displays the results of the validity checks. At the top of the table, we run the first-stage regressions, both using and not using the controlling covariates strategy proposed, for both real and the placebo cutoffs. At the bottom, we estimate the pre-treatment covariate balance.

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Notice that without controls, the first stage overestimates the change in the primary treatment and underestimates the effect in the placebo regressions. After adding covariates, the results improve, and the point estimates are now one for the primary model and zero for the placebo regressions, which are the correct values. Moreover, Panel B of Table 3 shows that the pre-treatment covariates remain unchanged around the thresholds. These tests reinforce the validity of our research designs.

Table 3: Research Design Validity Check

	Panel A: Validity Check — First Stage			
	(1) Additional Num. Seats 2004 (Without Controls)	(2) Placebo Add. Num. Seats 2004 (Without Controls)	(3) Additional Num. Seats 2004 (With Controls)	(4) Placebo Add. Num. Seats 2004 (With Controls)
LATE	1.63***	-0.31**	1.00***	-0.00
	(0.51)	(0.14)	(0.0004)	(0.0000)
N Left	5184	4621	5184	4621
N Right	343	906	343	906
Eff N Left	199	638	49	886
Eff N Right	145	385	51	477
BW Loc Poly	8.717	6.700	3.008	8.629
BW Bias	13.620	12.114	5.008	13.466
	Panel B: Validity Check — Pre-Treatment Variables			
	(5) Number of Seats 2000	(6) Population 2000 Census	(7) GDP 2000 Census	(8) % of Poverty 2000 Census
LATE	0.17	-2057.60	0.06	-9.72
	(0.67)	(1926.33)	(0.18)	(6.27)
N Left	5178	5131	5131	5131
N Right	343	343	343	343
Eff N Left	208	219	200	194
Eff N Right	147	157	145	142
BW Loc Poly	8.970	9.368	8.739	8.530
BW Bias	13.860	15.029	14.752	13.035

Note: *** $p < .01$; ** $p < .05$; * $p < .1$. RD local linear estimates using Calonico et al. (2019) optimal bandwidth quadratic selection and triangular kernel. Robust standard errors, clustered at the municipal level, in parentheses. Controls: population, GDP per capita, number of seats in 2000, year, and dummy for northeast region. *N Left* and *N Right* represent the total number of observation in the left and right sides of the thresholds (untreated). *Eff N Left* and *Eff N Right* are the number of cases within the bandwidth. *BW Loc Poly* is the Bandwidth used to compute the Local Average Treatment Effect (LATE). *BW Bias* is the Bandwidth used to compute the standard errors.

B.3 Population Thresholds in Brazil Before and After the 2003 Supreme Court Decision

The identification strategy requires exogenous changes in the city council size thresholds. Previous to the changes we exploit here, the 1988 Brazilian Constitution states the following distribution of seats per population size, presented in Table 4.

Table 4: City council size from the 1988 Constitution

	Min. Leg.	Max. Leg.	Min. Pop.	Max. Pop.	Num. Mun. Bin (2001-2004)
1	9	21	0	1,000,000	5,537
2	33	41	1,000,001	5,000,000	11
3	42	55	5,000,001	∞	2

These cutoffs were in place until 2003, when the municipality of Mira Estrela decided to decrease the council size from eleven to nine councilors. The two losing councilors started a judicial contestation that made it to the Supreme Court. The court ruled that the Constitution meant that council size should vary proportionally with population, starting with nine legislators, until the maximum number of councilors, 55.

Following this decision, the Superior Electoral Tribunal (TSE) decided the thresholds that should be followed in the 2004 election (Resolution 21,702/2004). According to their decision, the legislature should increase as displayed in Table 5.

Table 5: City council size thresholds according to TSE resolution 21,702/2004

	Num. Leg.	Min. Pop.	Max. Pop.	Num. Mun. Bin (2003 pop. proj.)
1	9	0	47,619	5,029
2	10	47,620	95,238	317
3	11	95,239	142,857	89
4	12	142,858	190,476	43
5	13	190,477	238,095	30
6	14	238,096	285,714	21
7	15	285,715	333,333	13
8	16	333,334	380,952	13
9	17	380,953	428,571	6
10	18	428,572	476,190	6
11	19	476,191	523,809	4
12	20	523,810	571,428	5
13	21	571,429	1,000,000	13
14	33	1,000,001	1,121,952	1
15	34	1,121,953	1,243,903	2
16	35	1,243,904	1,365,854	1
17	36	1,365,855	1,487,805	2
18	37	1,487,806	1,609,756	1
19	38	1,609,757	1,731,707	1
20	39	1,731,708	1,853,658	0
21	40	1,853,659	1,975,609	0
22	41	1,975,610	4,999,999	4
23	42	5,000,000	5,119,047	0
24	43	5,119,048	5,238,094	0
25	44	5,238,095	5,357,141	0
26	45	5,357,142	5,476,188	0
27	46	5,476,189	5,595,235	0
28	47	5,595,236	5,714,282	0
29	48	5,714,283	5,833,329	0
30	49	5,833,330	5,952,376	0
31	50	5,952,377	6,071,423	1
32	51	6,071,424	6,190,470	0
33	52	6,190,471	6,309,517	0
34	53	6,309,518	6,428,564	0
35	54	6,428,565	6,547,611	1
36	55	6,547,612	∞	0

As there are few municipalities above one million inhabitants, we decided to restrict our attention to municipalities with less than $571,429 + 47,619 = 619,048$ inhabitants. Table 6 displays the frequency of municipality by thresholds used in our paper.

The Supreme Court ruled that the electoral courts should use the 2003 population projection to assign municipalities to cutoffs. In Brazil, the Brazilian Institute of Geography and Statistics (IBGE) propose the official population projections. Sorting is not an issue for our identification strategy, as IBGE is an insulated bureaucracy, insulated from local influences (see also the McCrary test above).

To provide an overview of the data dispersion, Figure **??** plots the municipalities by their proximity from the cutoffs. In the map we provide the contour of every Brazilian municipality, coloring it by the proximity to the council size thresholds. The darker the color, the closer to the population thresholds. In regression

Table 6: City council thresholds used in the paper

	Legislature Size	Min. Population	Max. Population	Num. Mun. Bin (2003 pop.)
1	9	0	47,619	5,029
2	10	47,620	95,238	317
3	11	95,239	142,857	89
4	12	142,858	190,476	43
5	13	190,477	238,095	30
6	14	238,096	285,714	21
7	15	285,715	333,333	13
8	16	333,334	380,952	13
9	17	380,953	428,571	6
10	18	428,572	476,190	6
11	19	476,191	523,809	4
12	20	523,810	571,428	5
13	21	571,429	1,000,000	13

discontinuity, the closer a municipality is to the cutoff, the more influential it is in the estimation. Figure 2 also shows that our municipalities are reasonably distributed around the whole country.

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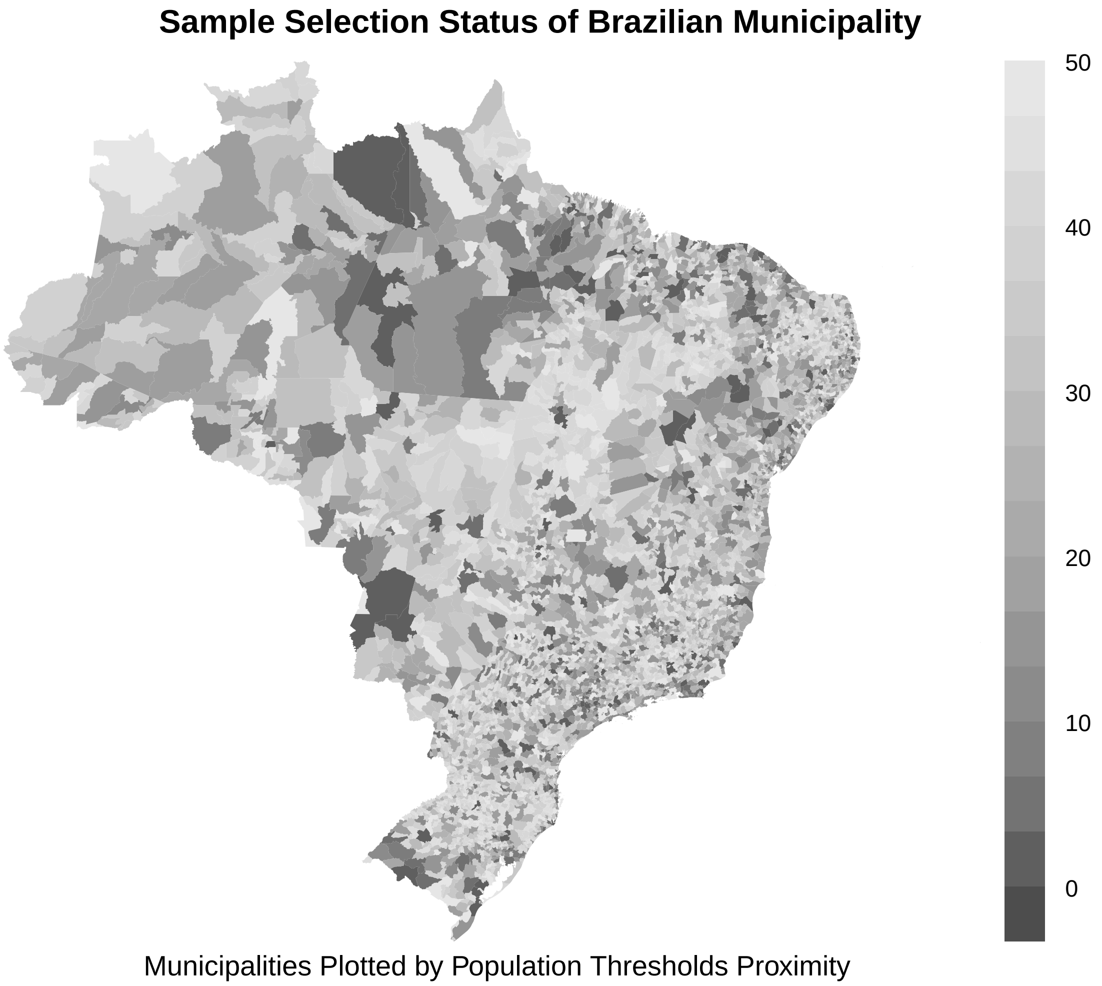

Figure 2: Municipalities by their Selection Status

Moreover, we could question whether the discontinuities studied here are sharp or fuzzy. In order to be fuzzy, the laws passed by the Supreme Court had to be contested or not be perfectly enforced. For instance, Cavalcanti (2017) shows in his second chapter that there has been a misuse of these discontinuities. However, this problem was absent in our RDD, as the problem identified by Cavalcanti was based on the use of thresholds approved in 2009. In our case, we use the thresholds approved in 2003, and until a period before 2009 (in our case, 2008).

B.4 Identification Strategy in Multiple Thresholds Regression Discontinuity Design

Usually when carrying out regression discontinuity with multiple cutoffs, authors pool all the discontinuities together and estimate the effect of council size on outcomes. Let $C = \{c_0, c_1, \cdots, c_n, c_{n+1}\}$ be the set of cutoffs thresholds associated with the running variable X. The outcome variable is assumed to be Y. In the pooled model, the Local Average Treatment Effect is:

$$LATE_{nocontrols} = \mathbb{E}\left[\lim_{x\downarrow c_i}\mathbb{E}[Y_i|X_i] - \lim_{x\uparrow c_i}\mathbb{E}[Y_i|X_i]|\forall c_i \in C\right]$$

Nevertheless, this may be problematic as the municipal characteristics in each cutoff may vary from cutoff to cutoff. The population imbalance certainly carries out heterogeneity, in the way shown by Cattaneo et al. (2016). To ensure we have comparability, and that we estimate the average LATE for all cutoffs, avoiding most of the heterogeneity, we propose to add a set of controls denoted by Z_i that vary between cutoffs. For instance, the population that determines the change from one threshold to another also determines all the cutoffs. As the population varies smoothly around each cutoff, it makes it a perfect control to add. The new estimator is:

$$LATE_{controls} = \mathbb{E}\left[\lim_{x\downarrow c_i}\mathbb{E}[Y_i|X_i] - \lim_{x\uparrow c_i}\mathbb{E}[Y_i|X_i]|\forall c_i \in C, Z_i\right]$$

We control for Population, GDP, a dummy for the Northeast States, seats before the 2003 decision, and year. All these control variables are smooth in each cutoff, but are significantly different from cutoff to cutoff, making them adequate control variables for our case (Calonico et al. 2019). To perform the estimations, we use a triangular kernel, which places more weight on the municipalities that are closer to each cutoff. To compute the optimal bandwidth, we use the Calonico et al. (2014) method. To study the sensitivity to the bandwidth choice, we vary the bandwidth from 50% to 200% of the optimal bandwidth size. We also use cluster-robust standard errors at the municipal level.

However, as it is uncommon for a regression discontinuity to use control variables, some readers may ask why in this case is it advisable? To answer this question, we run a series of simulations to show that controlling for the variable that determines the assignment not only helps improve the consistency of our estimates but also improves their efficiency.

Consider the data distributed in one of the four forms depicted by Figure 3. We add nine thresholds, at 0.1, 0.2, and so on until 0.9, and we create six types of outcomes. The first three outcomes are sharp changes. In the first, we add variation from one to 10, with one step for each change. In the second, we add 1 in the first cut, zero in the second cut, and -1 in the third cut, repeating this pattern for the remaining cutoffs. The estimated change should be equal to zero. In the third model, we add one in the first cut, $1 + 0.9$ in the second cut, $1 + 0.9 + 0.8$ in the third cut, and so on until the last cutoff. This is intended to simulate a diminishing effect from one cutoff to the other. The last three outcomes are the same as depicted here, added by a Normal random disturbance, mean zero, and variance 0.01. We then run a thousand Monte Carlo simulation for each combination, $4 \times 6 \times 2 = 48$ in total, fitting a regression with and without the running variable as a control.

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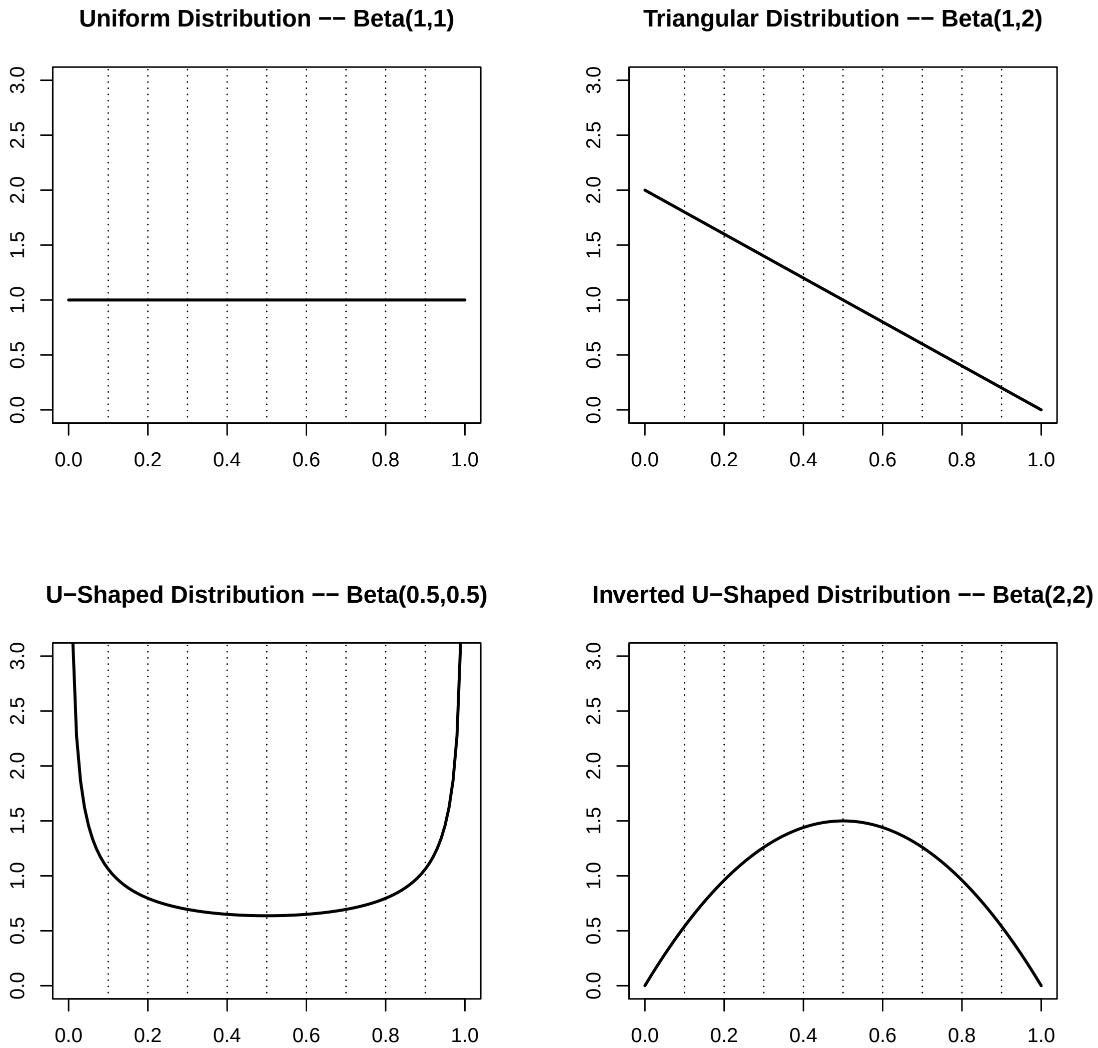

Figure 3: Simulation RD with Multiple Thresholds – Data Distribution

A consistent estimator should fit one in the first and fourth data distribution. In the second and fifth, the change should be of $(1, 0, -1, 1, 0, -1, 1, 0, -1)$, and the average change here is equal to zero. In the third and sixth models the change should be equal to $(0.9, 0.8, 0.7, 0.6, 0.5, 0.4, 0.3, 0.2, 0.1)$, and the average change should be equal to 0.5. The first three models should present a more efficient estimation than the last three models. We display the results in Figure 4, showing that the controlled models are more consistent and efficient.

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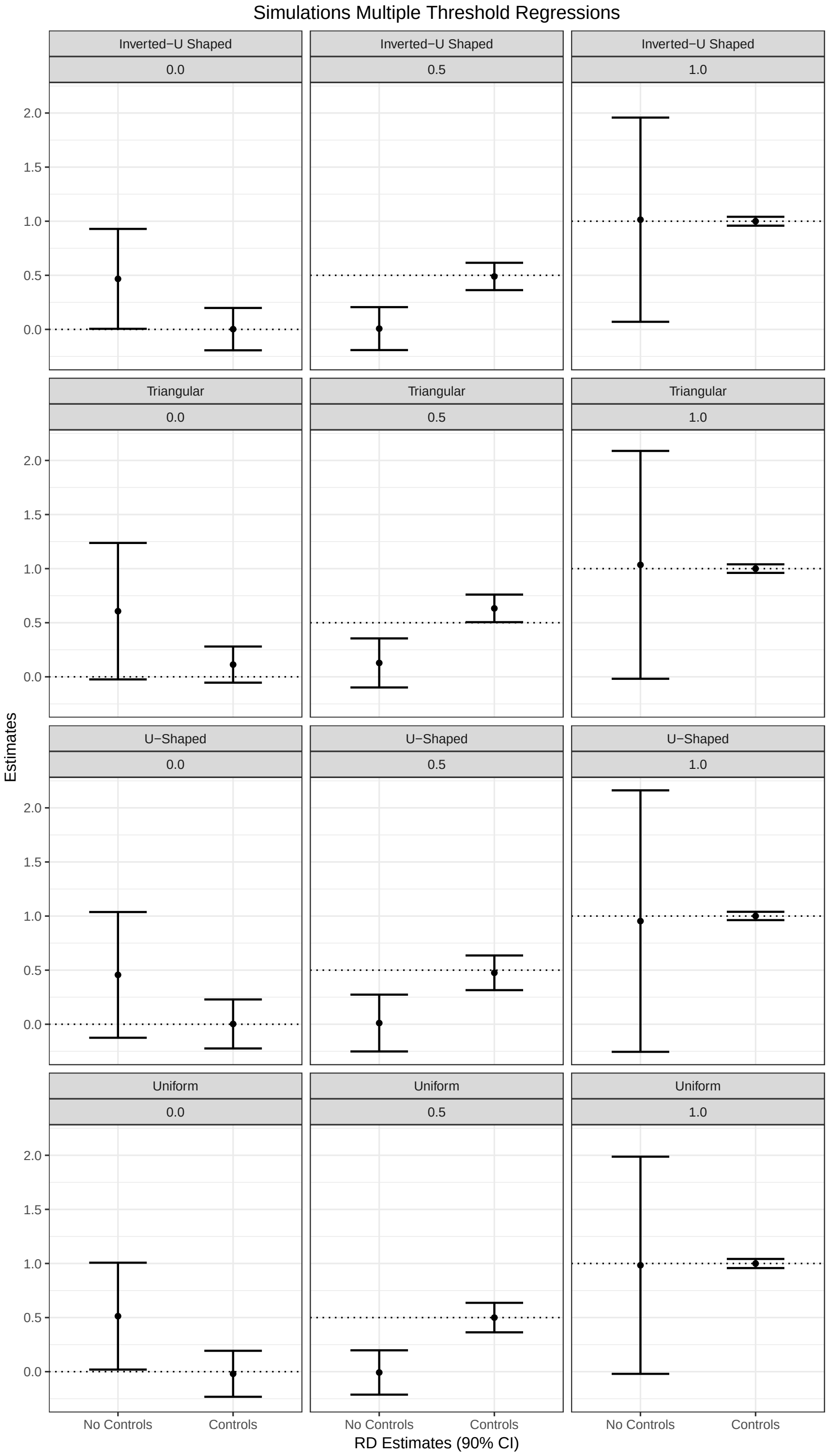

Figure 4: Estimations for the Different Data Generating Processes

B.5 Placebo Regressions for the Aggregated Mechanism Outcomes

Here we present the placebo regressions for the mechanism-aggregated outcomes. In Panel A of the main paper, we run the regression on alternative explanations. As we can see, the only female changed significantly, but the change is statistically expected in at least 10% of cases. For non-white, we have not collected data on non-white legislators in the placebo cutoffs; therefore, we leave it blank. In Panel B, we have the placebo regressions for the primary mechanism: resource access and patronage. As expected, the placebo regressions were not statistically significant.

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Table 7: Mechanism – Aggregate Municipal Level Regressions

	Panel A: Representation and Elections – Placebo Cutoffs			
	Representation		**Elections & Leg. Productivity**	
	Num. Female Councilors	Num. Non-white Councilors	Candidates Per Seat	Prop. Laws Approved Council
LATE	0.65**		-0.72	-0.001
	(0.27)		(0.48)	(0.06)
N Left	4620		4621	2991
N Right	906		906	703
Eff N Left	412		567	356
Eff N Right	297		347	229
BW Loc Poly	4.861		6.113	5.58
BW Bias	8.775		10.345	8.896

	Panel B: Resource Access and Patronage – Placebo Cutoffs			
	Mayoral Coalition Size	Num. Politically Appointed Empl.	Num. Career Bureaucrats	Num. Councilor Assistants
LATE	0.12	3.77	115.67	-2.17
	(0.34)	(17.20)	(79.96)	(1.81)
N Left	4618	13855	13850	4618
N Right	904	2709	2708	905
Eff N Left	644	1123	1464	592
Eff N Right	388	825	957	361
BW Loc Poly	6.730	4.475	5.461	6.279
BW Bias	10.421	7.600	9.999	10.944

Note: ***p < .01; **p < .05; *p < .1. RD estimates using Calonico et al. (2014) optimal bandwidth selection and triangular kernel. Robust standard errors, clustered at the municipal level, in parenthesis. Controls: population, GDP per capita, number of seats in 2000, year, and dummy for northeast region.

B.6 Sensitivity Analysis for Bandwidth Selection

We present here the sensitivity to the bandwidth selection. Following the suggestions of Bueno and Tuñón (2015), we vary the bandwidth from 50% to 200% of the Calonico et al. (2014) estimate.

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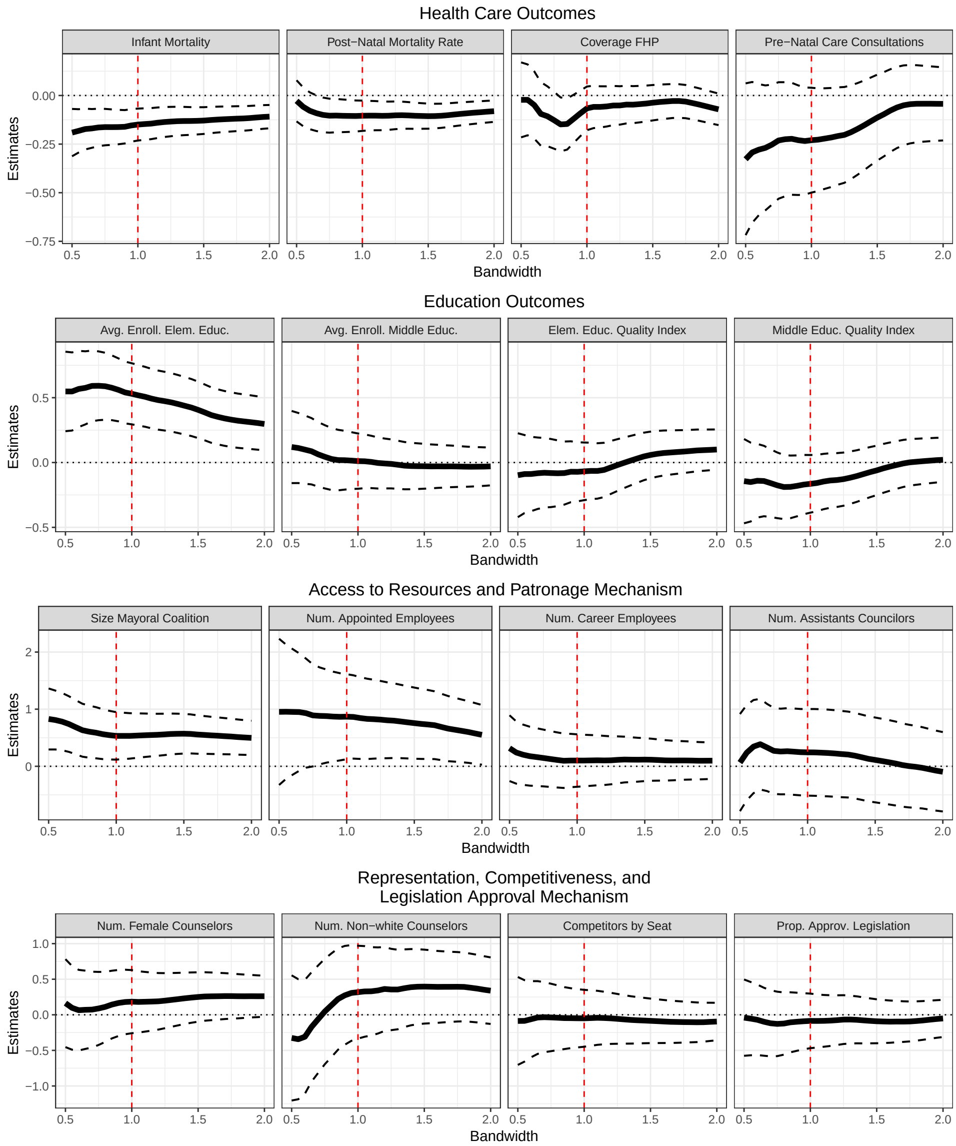

Figure 5: Bandwidth Sensitivity – Main Models

In Figure 6, we run the same bandwidth analysis for the placebo outcomes. The results, as expected, show mostly insignificant results.

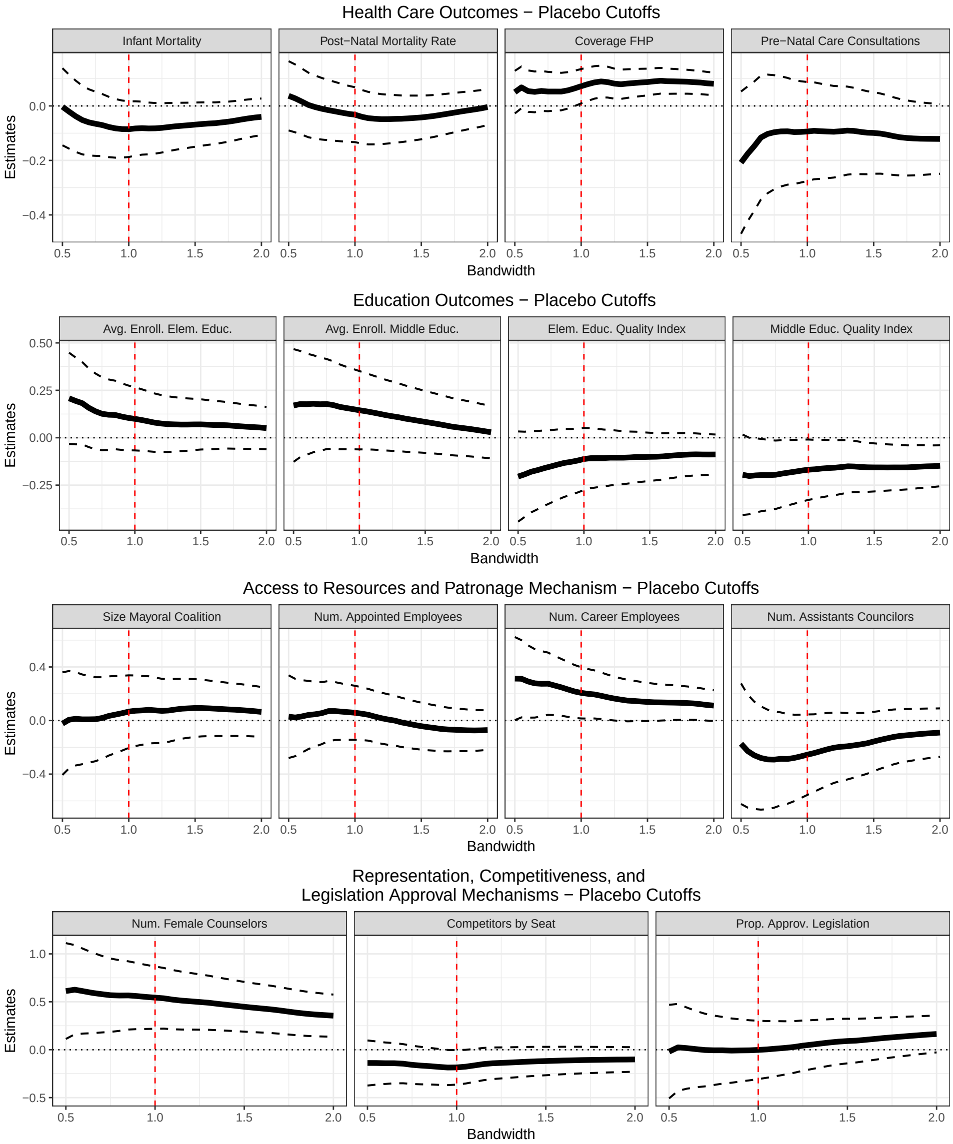

Figure 6: Placebo Regressions – Bandwidth Sensitivity

B.7 Sensitivity to Different States in the Analysis

The literature considers that some states are more prone to inefficiencies than the rest of the country. Moreover, some states had such poor Health Care and Education systems that they could be driving the results by themselves. As a consequence, our results could be a byproduct of structural changes in crucial states, instead of in council size. We run the analysis dropping one state at a time, to investigate this statewide heterogeneity. The results are in Figures 7,8, 9, and 10.

To interpret the results, first, note that most coefficients remain around the same value as before the robustness checks. The exceptions are with the Family Health Program, which improves without Para'{i}ba but decreases significantly without Bahia, both northeastern states. In the placebo, the FHP coverage increases when we remove Bahia and decrease when we add S~{a}o Paulo. Otherwise, note that all the other variables remain unchanged, except for gender. Gender changes because there were laws during this period mandating a minimum amount of female candidates. It is expected that one variable changes as a response to randomness.

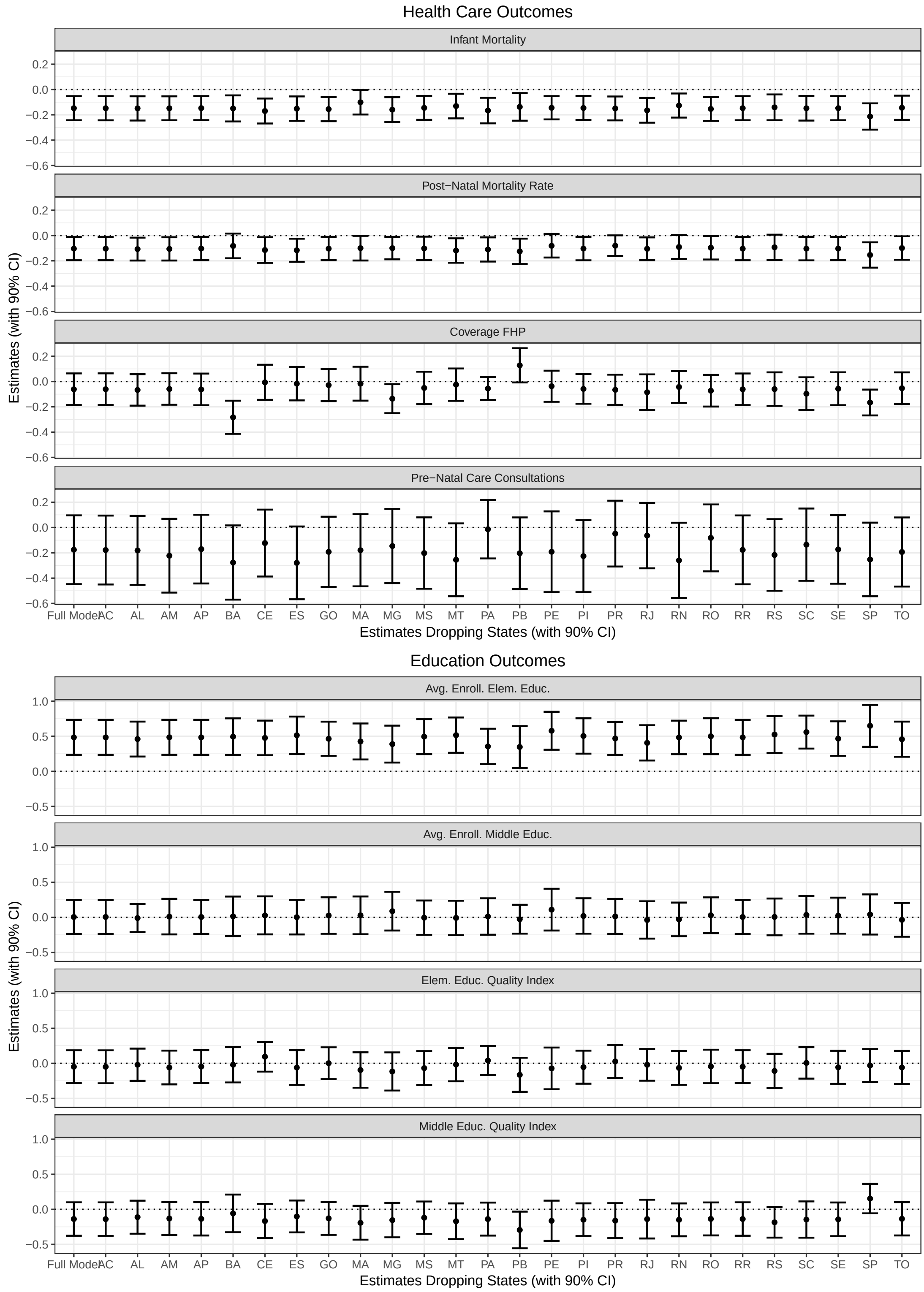

Figure 7: Sensitivity Analysis for the States in the Sample – Welfare Outcomes

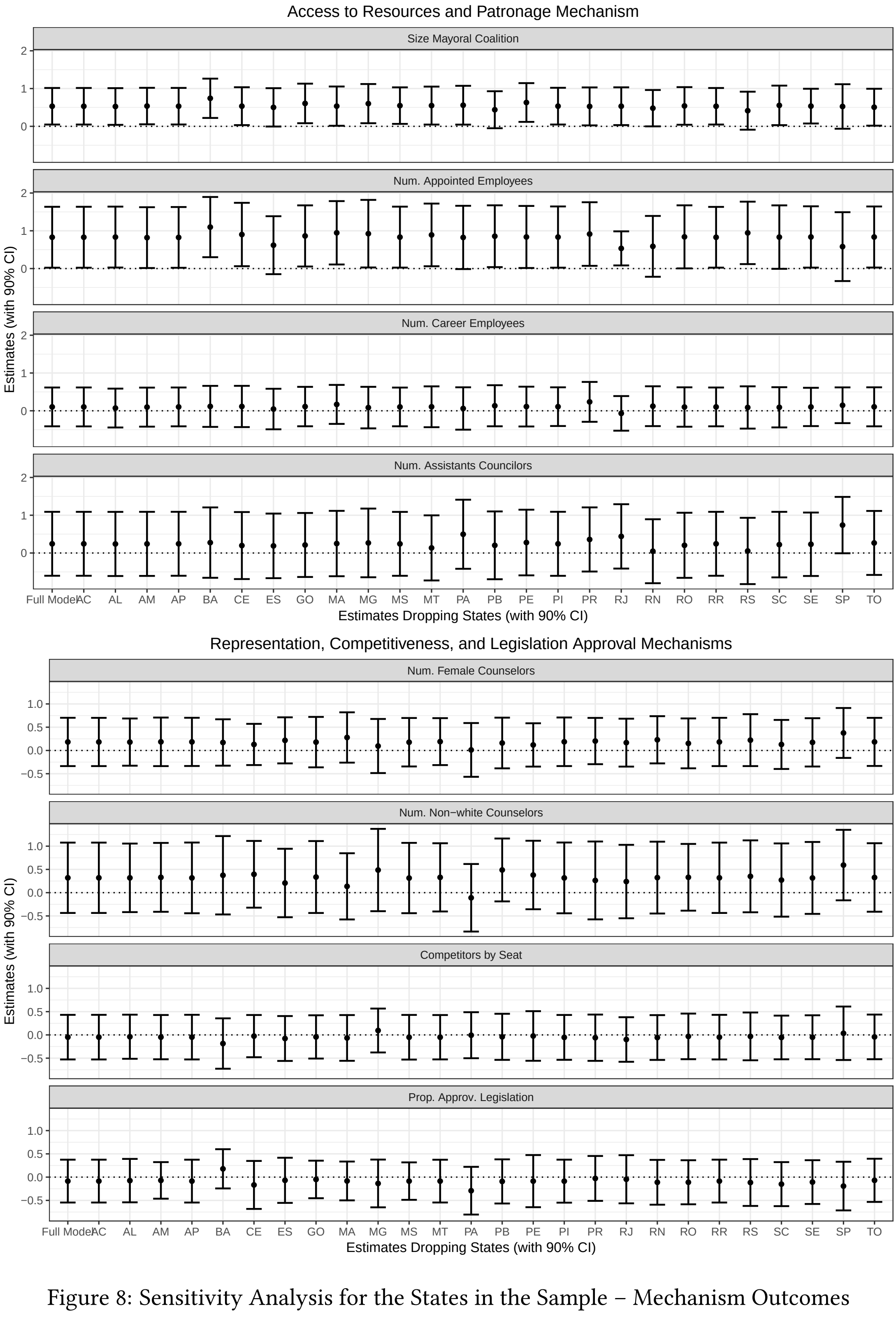

Figure 8: Sensitivity Analysis for the States in the Sample – Mechanism Outcomes

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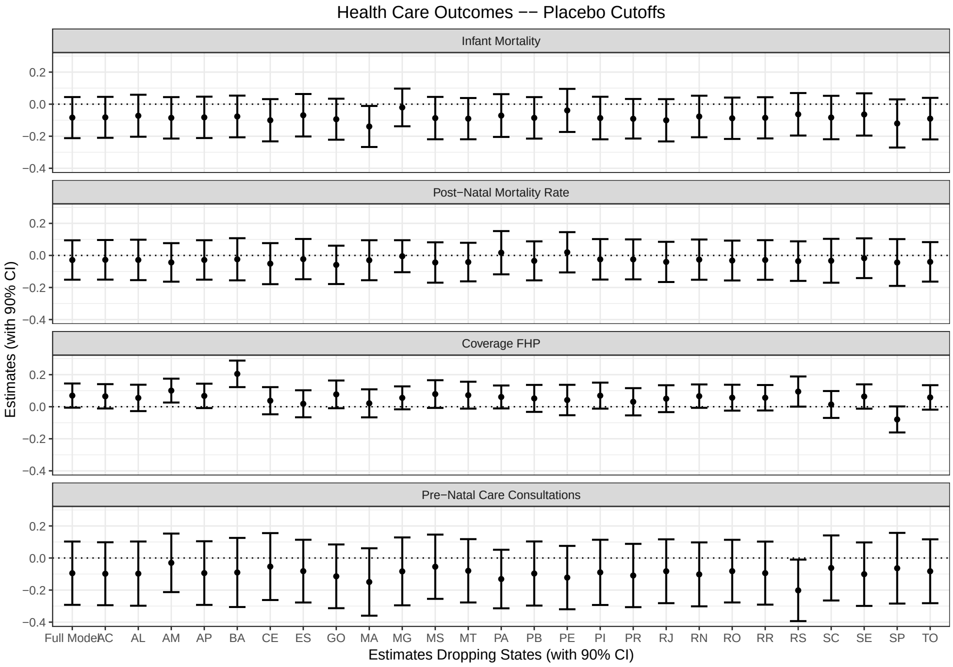

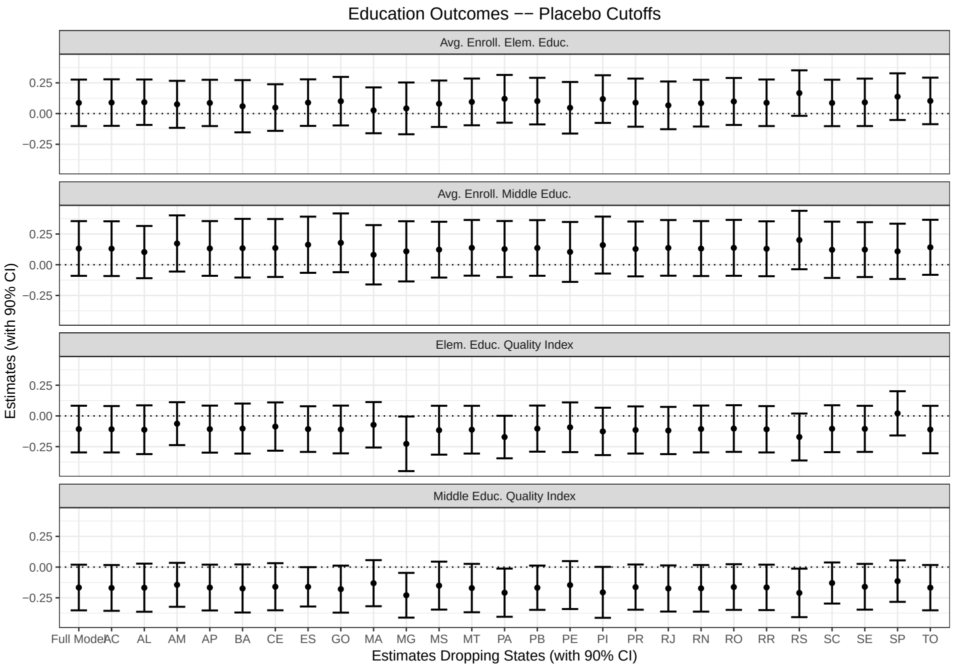

Figure 9: Sensitivity Analysis for the States in the Placebo Cutoffs – Welfare Outcomes

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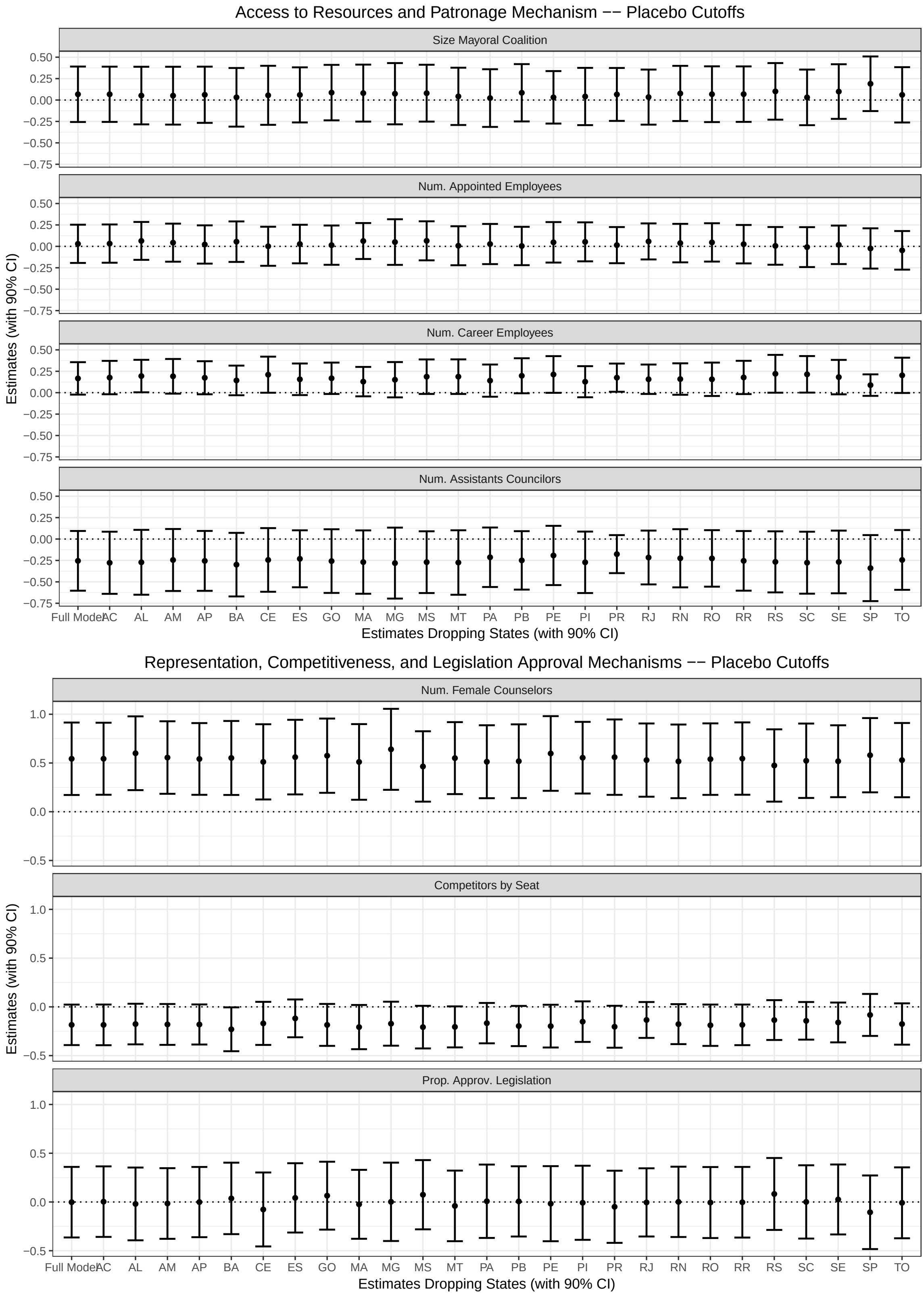

Figure 10: Sensitivity Analysis for the States in the Placebo Cutoffs – Mechanism Outcomes

B.8 Sensitivity to the Functional Form Analysis

In the paper, we run all the regressions using local linear polynomials. Gelman and Imbens (2014) show that local linear and quadratic polynomials are better in terms of consistency than cubic and quartic.

However, we run here all the models using polynomials from local linear to a quartic, showing that our results are robust to different regression functional forms. With two exceptions, the results are mostly consistent. First, quadratic and cubic polynomials make the significance go away in some models. Second, quartic polynomials seem to make the Family Health Program (FHP) significant and negative, which goes in line with our theoretical predictions. Figures 11 and 12 present the results for the real and the placebo cutoffs.

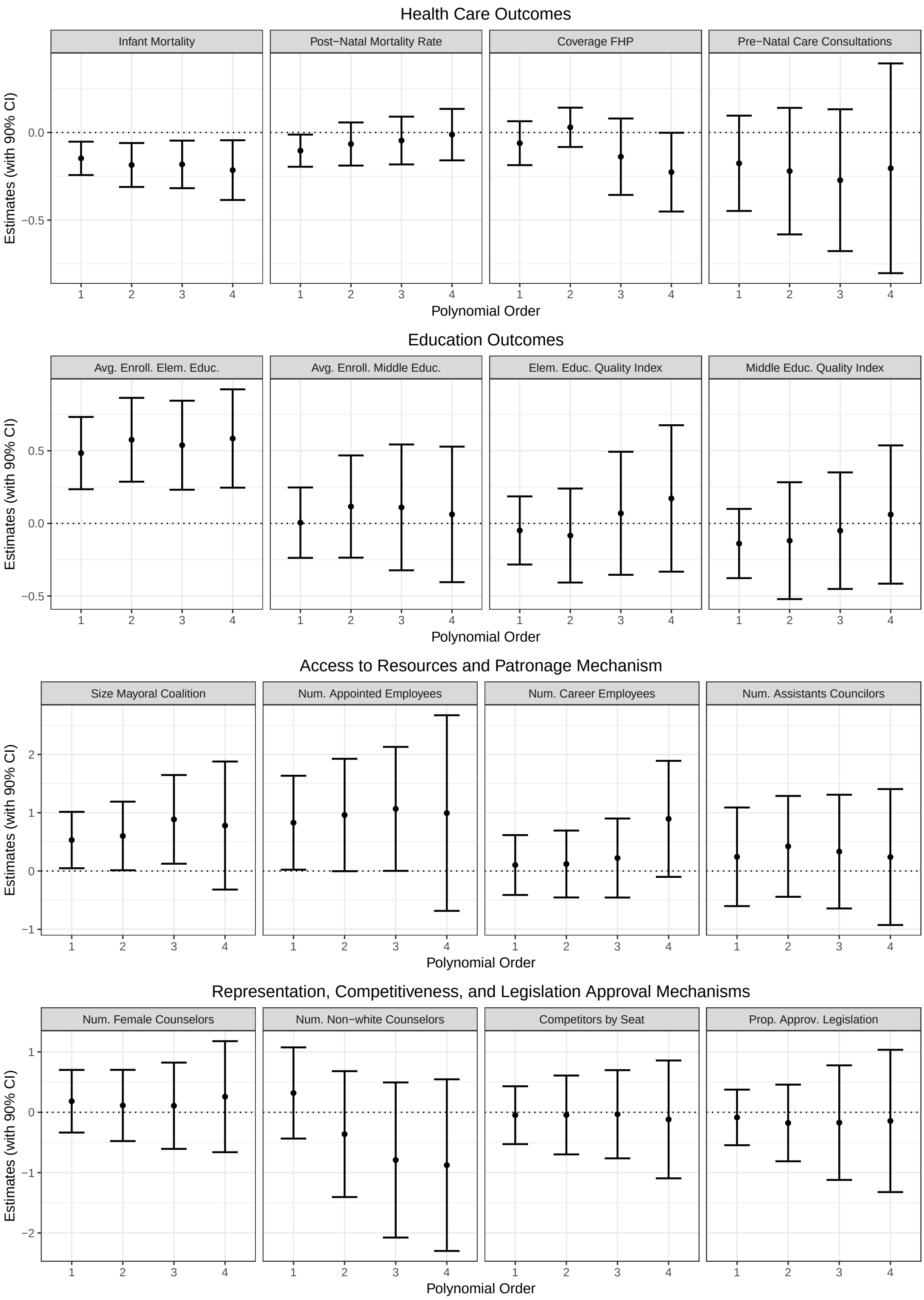

Figure 11: Sensitivity Analysis for the Functional Form

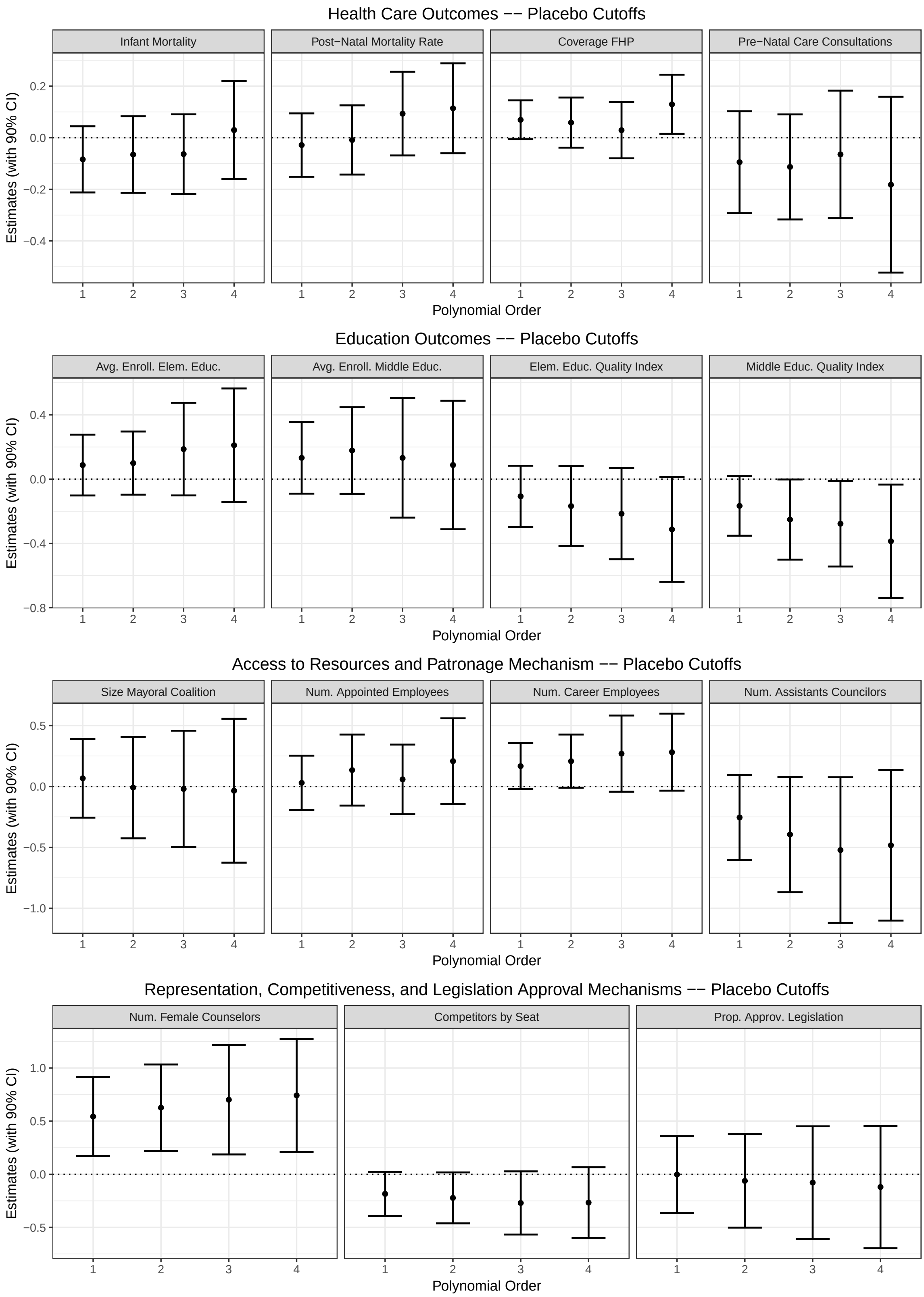

Figure 12: Sensitivity Analysis for the Functional Form – Placebo Cutoffs

B.9 Sensitivity to Covariates

The control variables play an essential role in our model. As we argued before, without controls, the first stage is inconsistent, and we may be capturing differences *between* cutoffs, instead of *within* cutoffs.

However, if the selection of controls makes the models overestimate quantities in the directions our narrative emphasizes, this would pose a considerable credibility issue for our analysis. To study the sensitivity to controls, we run the same regressions for all possible control combinations. Figure 13 displays the results for this sensitivity test.[4]

[4] In the Figure, *NC* stands for No Controls. The controls used are *gdp*, the municipal GDP in a given year; *nseats2000*, the number of seats before the 2003 Supreme Court decision; *northeast*, a dummy variable for Northeast Brazil; *pop2003_2*, population in 2003 (underscore 2 means that is population by thousands); and *year*.

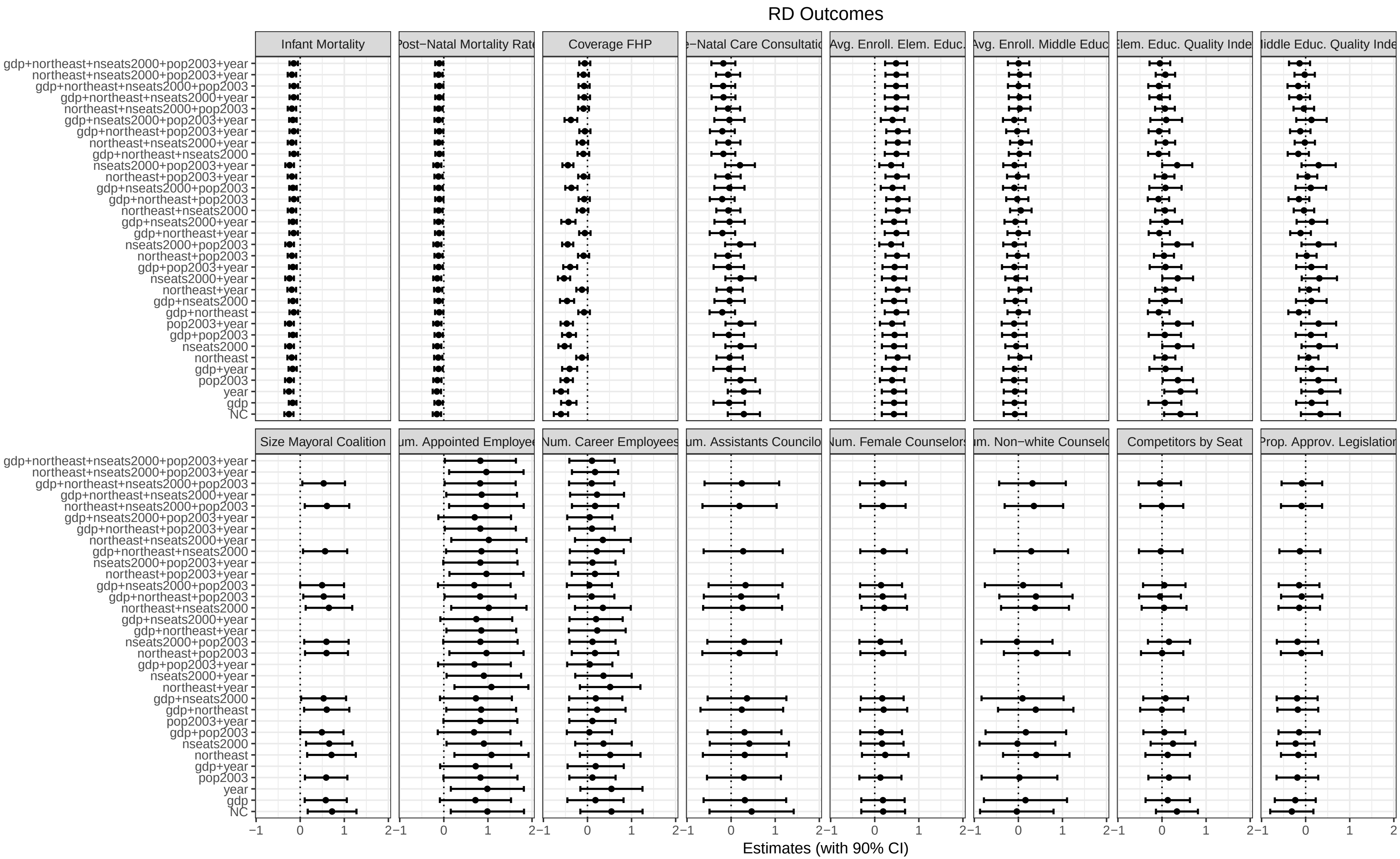

Figure 13: Sensitivity Analysis for the Control Variable's Choices

For instance, changes in controls have a substantial effect on the detected differences in the Family Health Program coverage, as well as in Elementary Education Quality.

In our view, our choice was the most conservative, as the Full Model is significant when most of the combinations also are significant. Otherwise, our model is not significant when most combinations are also insignificant. Regarding the point estimates, our analysis chooses the smaller coefficient sizes systematically for the significant variables.

We also plot sensitivity tests to covariates in the placebo regressions in Figures 14 and 15. The theoretical expectations here are that most of these outcomes would fail, and the graphs indeed confirm these expectations.

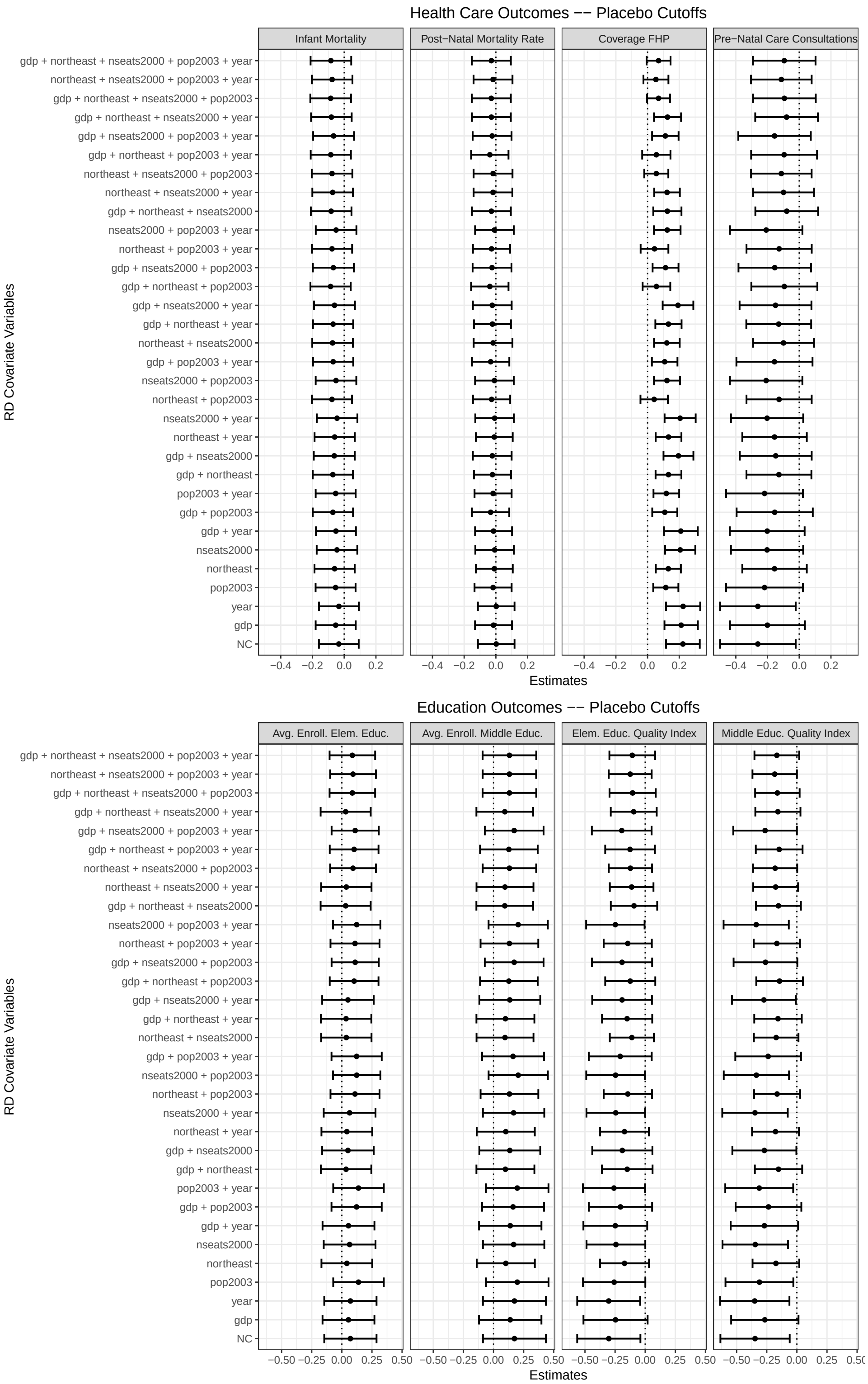

Figure 14: Placebo Regressions Outcomes – Sensitivity to Covariates

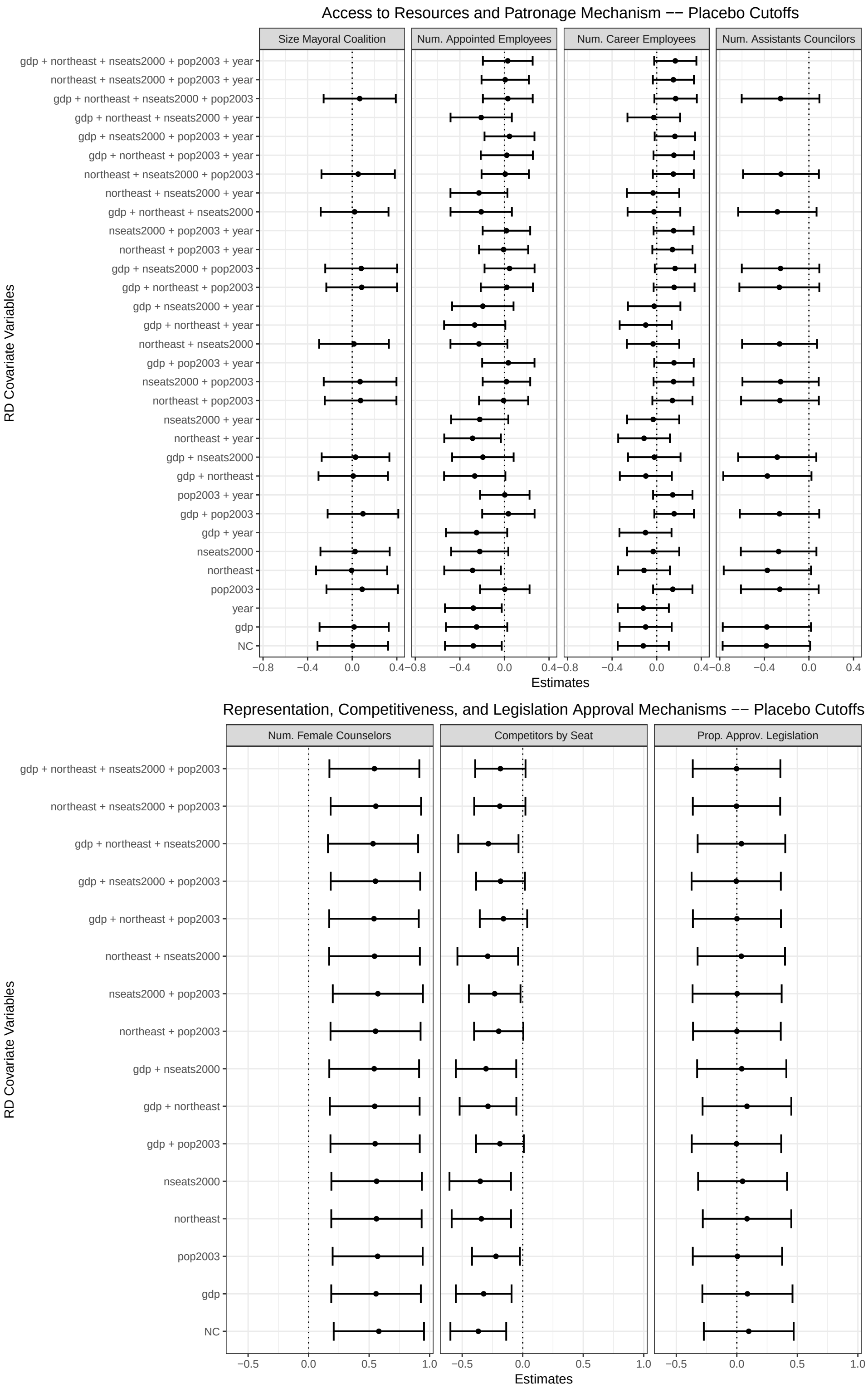

Figure 15: Placebo Regressions Mechanisms – Sensitivity to Covariates

B.10 Sensitivity to Additional Cutoffs

To study the sensitivity to the number of cutoffs, as well as a possibility of diminishing returns from the additional legislator, we run regressions limiting the population sizes to half the cutoffs, from the second cutoff to the last. This strategy is similar to adding one cutoff at a time. The results are stable and do not change our main conclusions. Moreover, there is no increasing or decreasing pattern, which would indicate that differential returns are occurring. Figures 16 and 17 display the results for the main cutoffs, while Figures 18 and 19 present the same results for the placebo cutoffs.

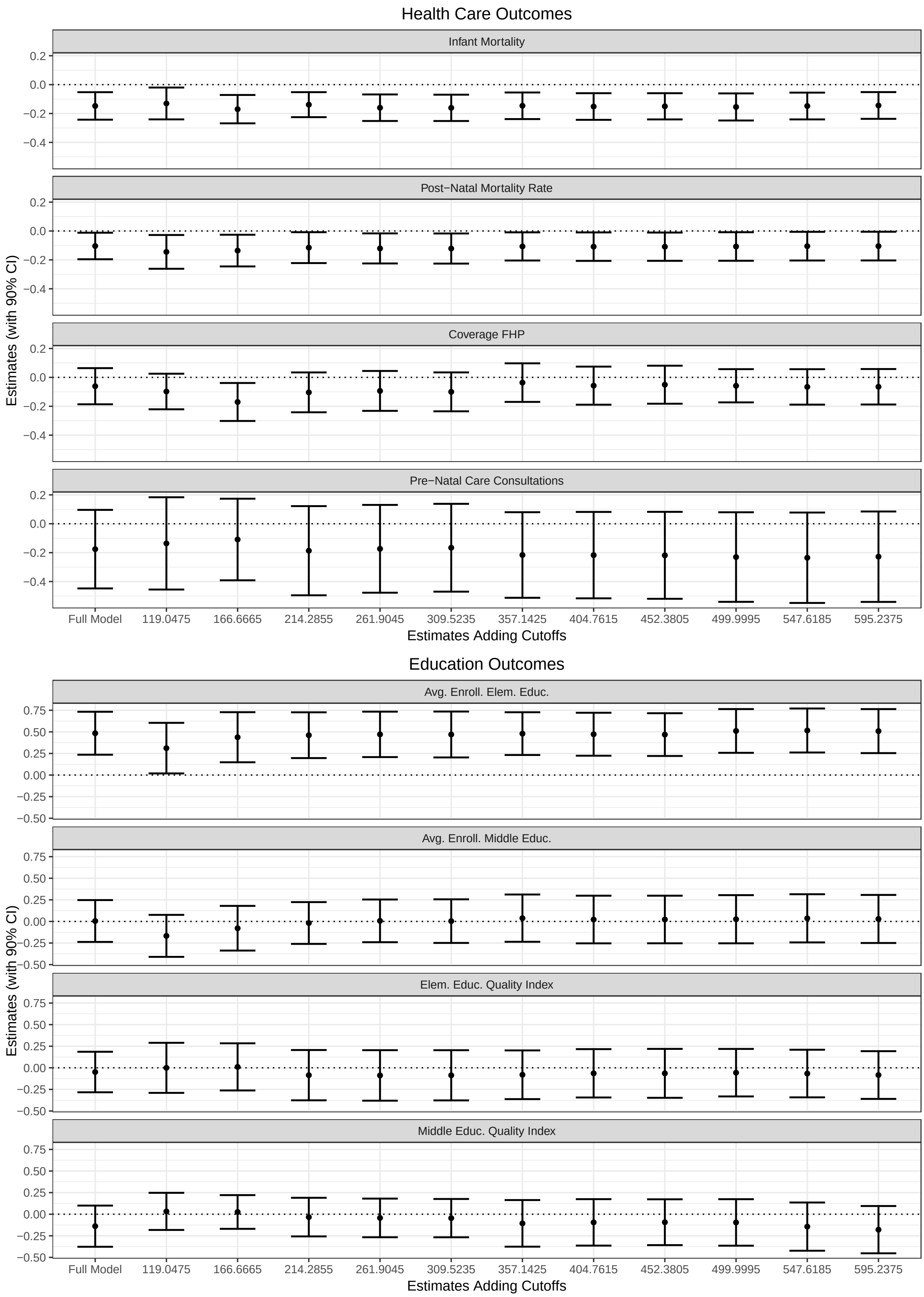

Figure 16: Sensitivity to Addition of Cutoffs – Welfare Outcomes

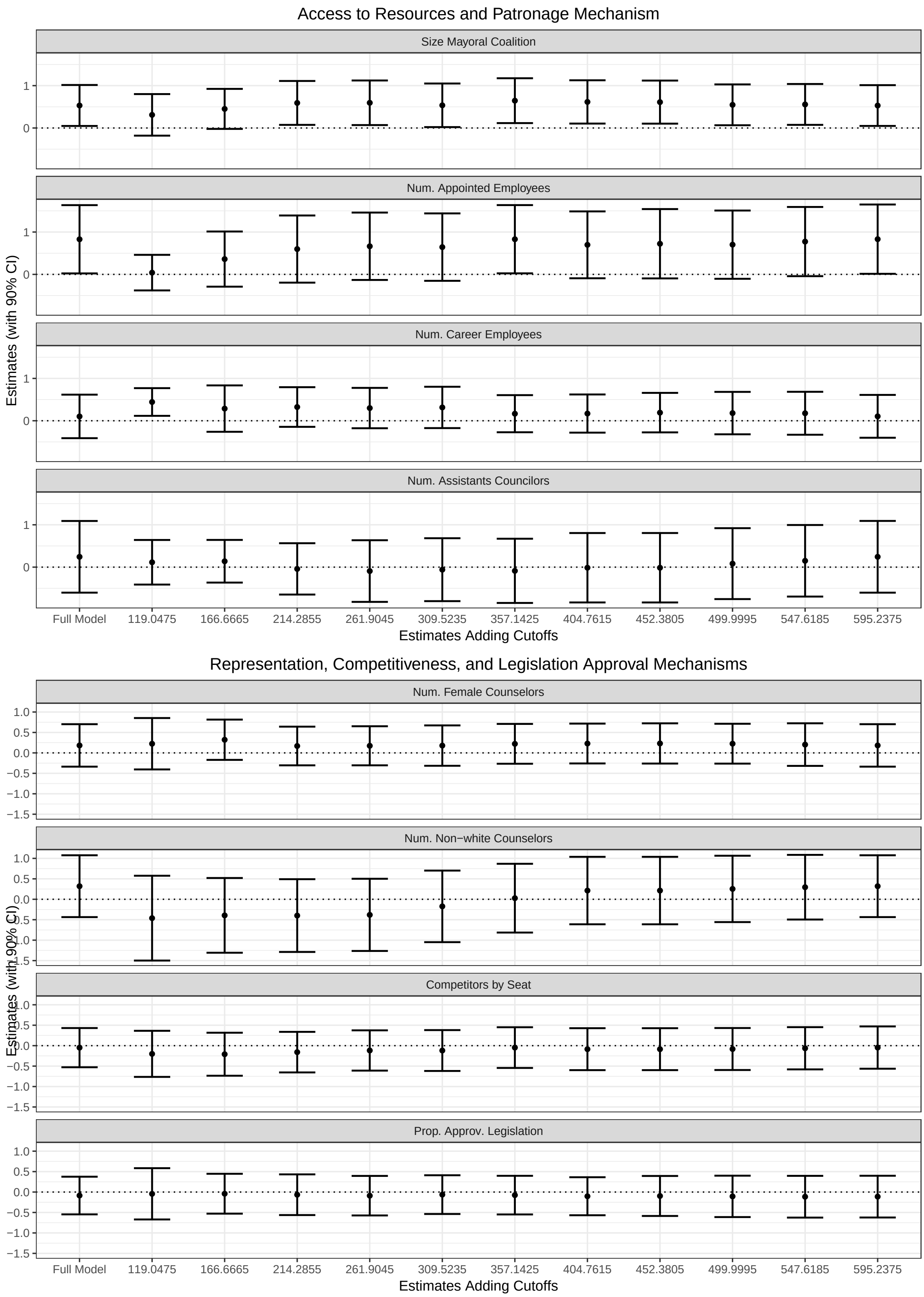

Figure 17: Sensitivity to Addition of Cutoffs – Mechanism Outcomes

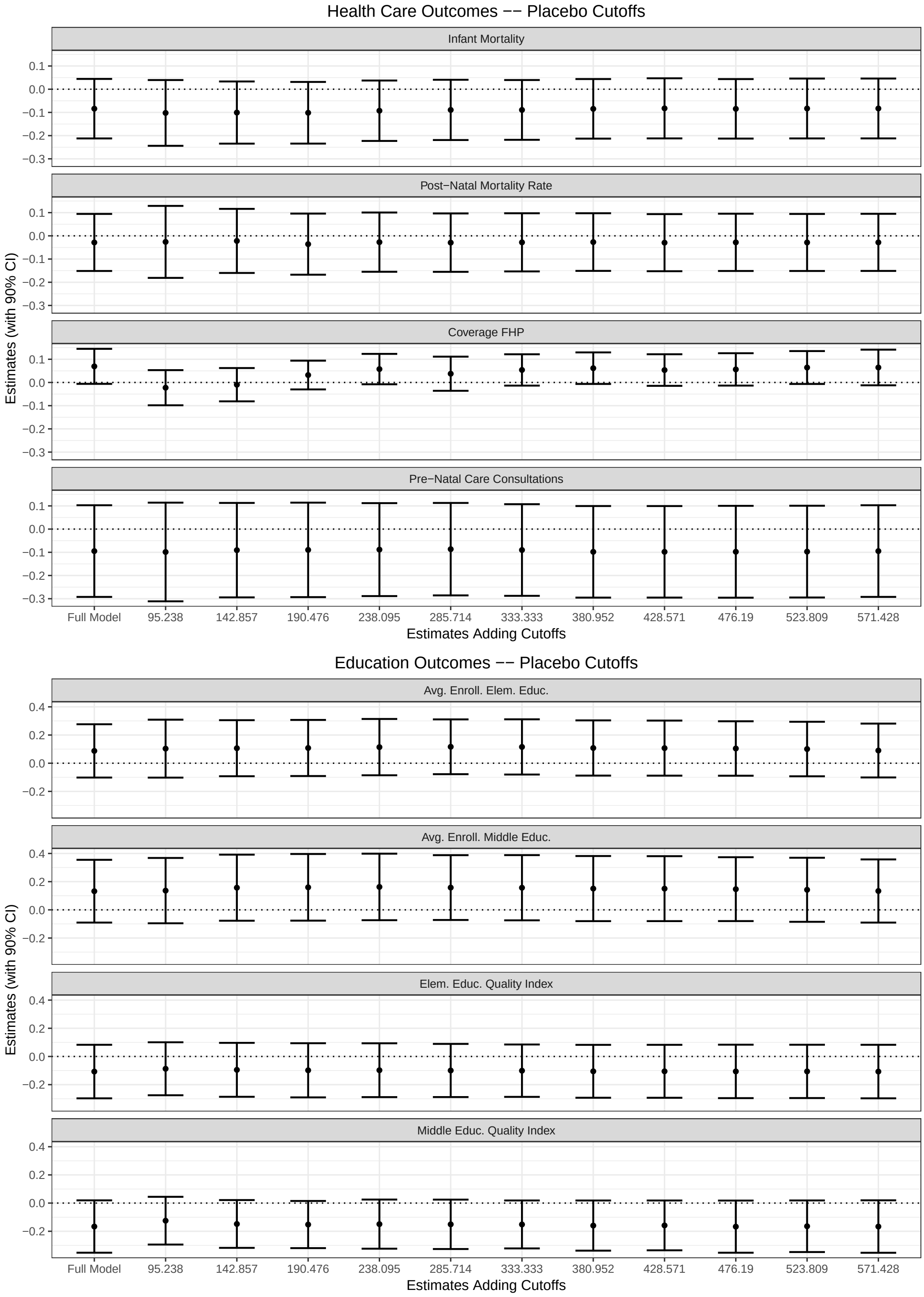

Figure 18: Sensitivity to Addition of Cutoffs – Placebo Welfare Outcomes

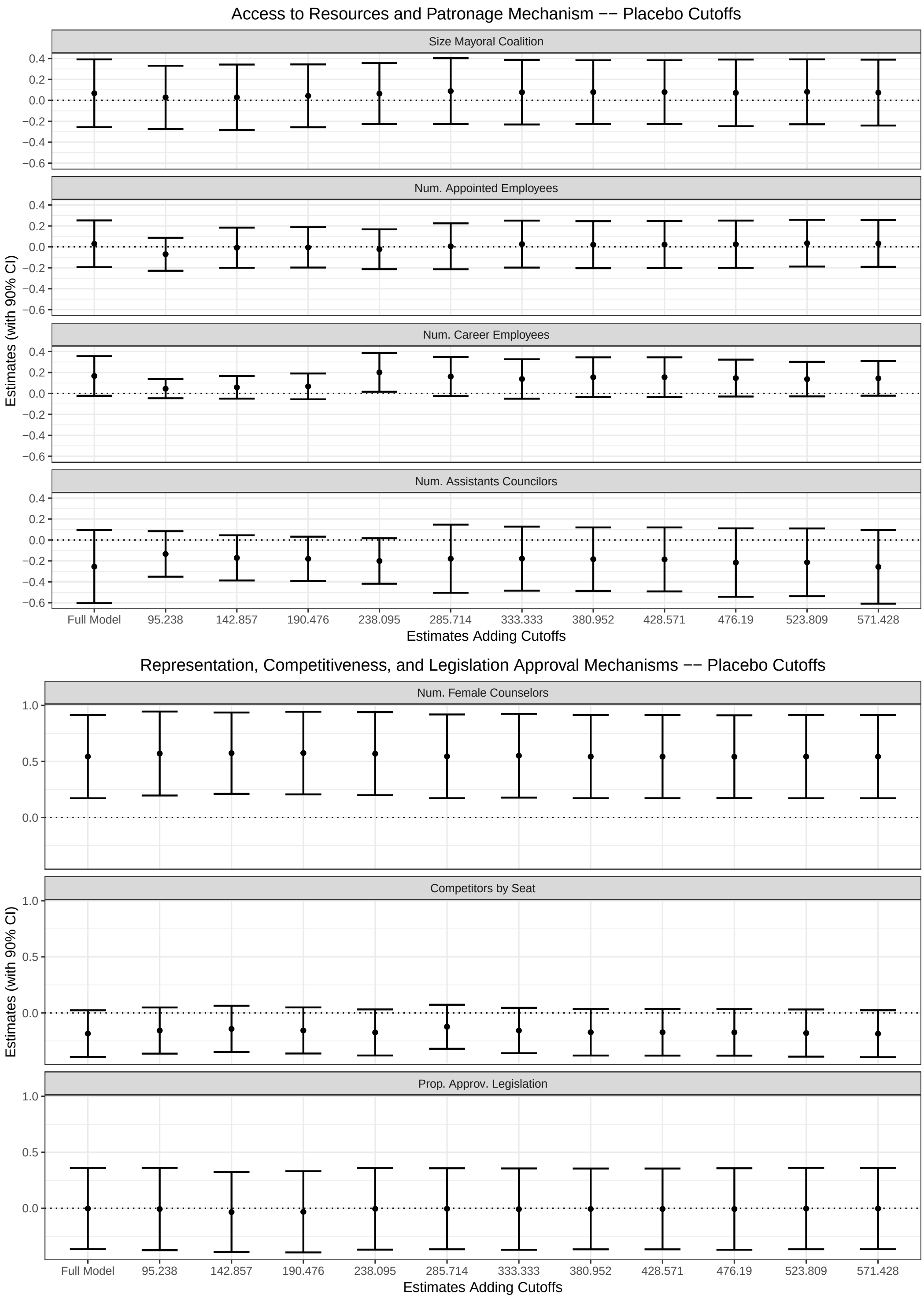

Figure 19: Sensitivity to Addition of Cutoffs – Placebo Mechanism Outcomes

B.11 Mayor's Characteristics, Revenues, and Transfers from the Central Government

Two alternative explanations could confound our results. First, the selection of the mayor could be affected by the city council size, and then the welfare improve by the quality of the mayor, and not by the partisan mechanism described. Second, the partisan mechanism could be confounded by an argument similar to the Weingast et al. (1981) "law of 1/n' ', and the improvements in welfare could have been a result of higher revenues. This section shows that none of these concerns are true in our case.

B.11.1 Mayoral characteristics

We select four characteristics: gender, schooling, whether the mayor was reelected in 2004, and whether the mayor was reelected in 2008. The results show no variation on council size and mayoral characteristics.

Table 8: Mayoral Characteristics

	Female Mayor	**Mayor w. College Degree**	**Reelected Mayor 2004**	**Reelected Mayor 2008**
LATE	0.07	-0.06	-0.09	0.06
	(0.08)	(0.13)	(0.10)	(0.14)
N Left	5069	5069	5184	5183
N Right	335	335	343	343
Eff N Left	299	226	192	231
Eff N Right	184	154	141	159
BW Loc Poly	11.738	9.655	8.508	9.596
BW Bias	17.104	15.762	13.338	14.741

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Local linear RD Estimates using CCT Optimal Bandwidth Selection and Triangular Kernel.

Quadratic Robust Standard Errors in Parentheses.

Controls: population, GDP per capita, number of seats in 2000, year, and dummy for northeast region.

Table 9: Mayoral characteristics – Placebo

	Female Mayor	**Mayor w. College Degree**	**Reelected Mayor 2004**	**Reelected Mayor 2008**
LATE	0.03	0.04	0.08	-0.07
	(0.05)	(0.08)	(0.07)	(0.08)
N Left	4526	4526	4621	4620
N Right	878	878	906	906
Eff N Left	567	707	683	759
Eff N Right	342	406	397	427
BW Loc Poly	6.197	7.413	7.006	7.71
BW Bias	10.241	12.64	13.536	12.648

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Local linear RD Estimates using CCT Optimal Bandwidth Selection and Triangular Kernel.

Quadratic Robust Standard Errors in Parentheses.

Controls: population, GDP per capita, number of seats in 2000, year, and dummy for northeast region.

B.11.2 Revenues and Transfers

We run the regressions on federal transfers and revenue raised within the municipalities. There is only a small effect on education transfer, barely significant at 10%. The other indicators remain insignificant. Note that the placebo are significant, and this actually reinforce the claim that the education effect may be spurious.

Table 10: Transfers and Revenue

	Total Transfers	**FPM Transfers**	**Education Transfers**	**Total Revenue**
LATE	0.03	-0.03	0.18^{*}	0.06
	(0.04)	(0.03)	(0.10)	(0.04)
N Left	15555	15555	15460	14668
N Right	1029	1029	1028	998
Eff N Left	804	258	349	626
Eff N Right	516	273	333	442
BW Loc Poly	10.617	4.568	6.005	9.253
BW Bias	16.447	8.243	11.412	15.092

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Local linear RD Estimates using CCT Optimal Bandwidth Selection and Triangular Kernel.

Quadratic Robust Standard Errors in Parentheses.

Controls: population, GDP per capita, number of seats in 2000, year, and dummy for northeast region.

Table 11: Transfers and Revenue – Placebo

	Total Transfers	**FPM Transfers**	**Education Transfers**	**Total Revenue**
LATE	0.07^{**}	0.04	0.03	0.07^{**}
	(0.03)	(0.03)	(0.06)	(0.03)
N Left	13865	13865	13776	13103
N Right	2719	2719	2712	2563
Eff N Left	807	636	1928	848
Eff N Right	619	544	1163	611
BW Loc Poly	3.418	3.028	6.738	3.67
BW Bias	6.021	5.36	10.871	6.222

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Local linear RD Estimates using CCT Optimal Bandwidth Selection and Triangular Kernel.

Quadratic Robust Standard Errors in Parentheses.

Controls: population, GDP per capita, number of seats in 2000, year, and dummy for northeast region.

Table 12: Accuracy SVM Classifier (tested in 20% of the data)

Variable	**Accuracy**
Local Public Goods	93.8
Public Goods and Legislation	94.2
Oversight	94.9
Others	93.5
Education and Health Care	92.5

B.12 Legislation Dataset

In the Fall of 2018, we collected the data on the legislation approved by the city council of 64 municipalities 10 thousand inhabitants away from the thresholds. There are 202 municipalities at a 10 thousand inhabitants distance from the cutoff, but only 64 had information on the legislation for the period between 2005 to 2008.

To classify the legislation, we hand-coded 1% of the dataset (3,466 cases) and applied a Supporting Vector Machines algorithm to the remaining 99% cases. First, we train and test the accuracy of the SVM classifier on 80% of the dataset. Then, we ran the training on the full hand-coded data and predicted the remaining.[5]

We hand-coded using five characteristics:

1. **Local Public Goods**: Whether the bills provided a local public good or service. Bills here are, for example, proposals to fix street potholes, staff a given health clinic, purchase equipment to a given school. To lay on this category, the service should have been targeted for a fraction of the population.

2. **Public Goods and Legislation**: Whether a bill is about a municipal public good (e.g., improving education), or about legislation that does not discriminate against any citizen within the polity (general law).

3. **Oversight**: Legislation requesting information on the status of service provision.

4. **Others**: Legislation that is not categorized as any of the previous three listed. Legislation here includes honors to notable citizens and procedure legislation, among others.

5. **Health Care and Education**: Bills on education and health care, broadly defined.

We run the SVM on the hand-coded data. Table 12 shows the classification accuracy for each of the variables that we hand-coded. In all models, we set cost equals 10 to avoid overfitting.

After we classify all the bills, the frequency in each category was added to the main paper. We also add the productivity per legislator, consisting of a ratio of the legislation approved in the municipality in the four-year term, divided by the council size. As expected, the results changed little from when we considered the absolute values.

[5] We tested SVM, Naive-Bayes, Random Forests, and Neural Networks. We choose SVM as it gives the highest prediction rate. We are using a simple bag-of-words classifier, with the R package RTextTools (Collingwood et al. 2013).

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