

# The General Equilibrium Foundations of Electoral Regression Discontinuity Designs

Tara Slough\*

Tinghua Yu<sup>†</sup>

August 25, 2022

## Abstract

Under the assumption that close elections are decided randomly, electoral regression discontinuity designs (electoral RDs) are thought to recover the effect of electing politicians of a certain party or persuasion, holding all else constant. We argue that this interpretation ignores strategic behavior on the part of political actors, who routinely make decisions in anticipation of electoral results. Changes in a given outcome—bureaucratic turnover, for example—should not be attributed solely to actions taken by the politician once in office but also to actions taken by actors before election day. We develop this argument using a simple model of elections and bureaucratic turnover. But the core point generalizes beyond bureaucrats: close-election RD estimates should be interpreted not as partial equilibrium effects but as a reflection of what happens in (general) equilibrium when strategic actors make consequential decisions in anticipation of elections.

---

\*Assistant Professor, New York University. [tara.slough@nyu.edu](mailto:tara.slough@nyu.edu)

<sup>†</sup>Lecturer, Birkbeck, University of London [t.yu@bbk.ac.uk](mailto:t.yu@bbk.ac.uk).

Mayor Carlos<sup>1</sup> recently won a competitive election and has taken office in a Brazilian municipality. Aware that he barely won the election, he looks ahead to the next election with some apprehension. Within a four-year term, Mayor Carlos and his administration take many actions; we consider his bureaucrats' policy implementation decisions. In principle, these actions could influence his electoral fortunes in the next election in several ways. Voters may reward Mayor Carlos for his ability to deliver services they value or punish him if these services do not materialize.

At the conclusion of his first term, Mayor Carlos faces a formidable challenger, José.<sup>2</sup> José and Carlos wage a bitter campaign and José narrowly defeats Carlos. Scholars may be interested in understanding how this alternation of power affects turnover of municipal staffing and subsequent service provision (Akhtari, Moreira, and Trucco, 2022; Toral, 2022a). To understand these effects, scholars implicitly ask how would the bureaucratic turnover be different if Carlos had won a second term from the outcomes they observe under first-term mayor José?

In order to credibly answer this question—and others—about the effects of election outcomes, researchers since Lee (2008) have used close-elections regression discontinuity designs. Under comparatively weak assumptions, these designs facilitate unbiased estimation of the local average treatment effect (LATE) of an election outcome in “tied” elections. Returning to our example, José's narrow victory and those in other districts where an incumbent narrowly won or lost could be used to estimate LATEs of turnover on measures of bureaucratic composition/staffing or public service provision where the margin of victory was zero.

In this paper, we analyze how actors' dynamic considerations—and associated actions—affect the *interpretation* of LATEs estimated by electoral regression discontinuity designs. Concretely, we study how Mayor Carlos' bureaucrats' first-term policy implementation is “internalized” by LATE estimates. In particular, we suggest two ways that these anticipatory actions can influence the interpretation of the LATE. First, to the extent that first period actions are effective in influencing election outcomes, they influence the composition of the sample within the close elections

---

<sup>1</sup>We follow existing Brazilian vignette survey experiments in naming our hypothetical mayor “Mayor Carlos” (Weitz-Shapiro and Winters, 2016; Boas, Hidalgo, and Melo, 2019).

<sup>2</sup>José was the modal (first) name of mayoral candidates in Brazil in 2020.

sample from which the LATE is estimated. Second, first-term actions may determine the “status quo” that an incumbent or challenger must work from. Both dynamic considerations influence the extent to which we can generalize from the LATE to other effects *even within the sample*. Moreover, the second consideration—the role of the status quo—also influences whether we can measure “all else equal” effects in RDs.

To this end, we distinguish between “partial” and “general” equilibrium effects in electoral RDs and argue that the LATE in electoral RDs should be interpreted as a measure of a general equilibrium effect.<sup>3</sup> In the context of markets, Mas-Colell, Whinston, and Green (1995: p. 538) define partial equilibrium models as assuming “no feedback effects from endogenous [prices, profits, productions, and other variables of interest] to the underlying demand or cost curves that are specified in advance.” These assumptions facilitate “all else equal” comparisons. General equilibrium models do not impose these assumptions, but similarly do not afford “all else equal” interpretation. Translating these concepts to the study of elections, the “feedback effects” present in general equilibrium models are introduced when multiple actors—like politicians and voters—act in consideration of the other actors’ behavior.

We first present an analysis of electoral RDs to illustrate how these dynamic considerations manifest in the LATEs that we estimate. Our analysis is generic to many settings that are united by a common sequence. Crucial to this sequence, a political actor at time  $t - 1$  (immediately preceding the election that determines treatment assignment) anticipates how their actions will affect the next election outcome or the choices available to future incumbents. This class of settings is far broader than cases of incumbents contesting re-election (like Mayor Carlos). It also includes settings where an incumbent might seek to increase the likelihood of another victory by their party or hamstringing a future incumbent from an opposition party.

We benchmark our analysis of electoral RDs to two (hypothetical) experiments to show why general equilibrium considerations emerge. When introducing electoral RDs Lee (2008) made explicit the analogy between the two research designs by titling his paper: “Randomized experiments

---

<sup>3</sup>Banerjee and Duflo (2009) object to the use of “general” equilibrium outside the context of markets, preferring instead simply “equilibrium.” We use the terms interchangeably.

from non-random selection in U.S. house elections.” Subsequent work has assessed the correspondence between RD estimates and experimental estimates (Green et al., 2009). While it is generally not possible or advisable to randomize election outcomes (Slough, 2022), a hypothetical comparison to two experimental designs shows how the interpretation of relevant estimands is different in RDs. We show that if a researcher could simply randomize election outcomes (unbeknownst to the candidates), they would limit concerns about selection into close elections but would not eliminate “status quo” considerations. If a researcher could deploy and randomize elections where incumbent leaders and their opposition did not think they would face the voters, both general equilibrium considerations would be eliminated. These hypothetical benchmarks illustrate under-developed *differences* between the treatment effects estimated by electoral RDs and experiments.

We illustrate these considerations with a simple model corresponding to the Mayor Carlos/José vignette. Our model seeks to understand post-election bureaucratic retention and its implications for bureaucratic quality in a patronage system. We show that pre-election actions by the bureaucrat have two effects which are incorporated in relevant LATEs. Under the assumption that voters value implementation of policies or public goods, bureaucrats’ first-period actions help determine which constituencies are more likely to have close elections. Additionally, the bureaucrat’s first-period actions determines the extent of politician learning about the bureaucrat’s (unobserved) type, constituting the status quo. Because incumbents and challengers use information in different ways when making retention decisions, this learning from first-period actions feeds into the LATE. We illustrate how equilibrium considerations should influence our interpretation of the LATE.

Our paper makes three contributions. First, we show that conventional wisdom that electoral RDs identify partial equilibrium effects—as implied since the seminal work of Lee (2008)—is inaccurate in the presence of the forward-looking behavior characteristic of elections. Our analysis shows that LATEs instead incorporate two classes of indirect general equilibrium effects. Ultimately, it is a matter of debate when measures of partial versus general equilibrium effects are more valuable in applied research. Our contribution is to show that LATEs in electoral RDs should be viewed as measures—however stylized—of general equilibrium effects. Interpretation

of LATEs as general equilibrium effects is presently limited (though see Eggers (2017) and Hofstetter and Sheen (2011) for exceptions). Importantly, we show that this property is a general feature of electoral RDs, not RDs in general.

Second, we contribute to a growing literature on the theoretical implications of empirical models (TIEM) (e.g., Bueno de Mesquita and Tyson, 2020; Prato and Wolton, 2019; Slough and Tyson, 2022; Abramson, Koçak, and Magazinnik, 2021). Our focus on electoral RDs is closest in approach to Eggers (2017) and Marshall (2022), but our argument is distinct. Eggers (2017) provides equilibrium-based explanations for differences in candidate quality which may underpin findings of incumbency advantage in electoral RD designs. Our argument focuses on equilibrium effects realized *prior to close elections*. Eggers instead focuses on equilibrium effects that emerge *after the close election* in RDs on incumbency advantage. Marshall (2022) argues that in LATES of a politician characteristic (e.g., race or gender) on political outcomes, typically reflect both the effect of that characteristic and differences in candidate quality that drive sorting into close elections. However, Marshall does not discuss why such compensating differentials emerge in the first place. In this respect, our results on selection into close races both microfound and generalize the concerns in Marshall (2022). Our argument considers a wider range of electoral RDs (inclusive of incumbency advantage and politician characteristic RDs) and shows how forward-looking behavior influences interpretation of the LATE.

Finally, our illustrative example focuses on bureaucratic turnover and the composition of the public sector in patronage democracies. In so doing, it engages a growing literature that studies the function and effects of patronage using electoral RDs in Brazil.<sup>4</sup> Colonnelli, Prem, and Teso (2020) and Brollo, Forquesato, and Gozzi (2017) use close elections RDs to show the measure the extent of patronage hiring of politically-aligned/affiliated individuals in Brazil (conditional on close elections) and also measure effects on the quality of appointees. Akhtari, Moreira, and Trucco (2022) and Toral (2022b) consider the implications of bureaucratic turnover on service delivery in Brazilian schools. Our model provides predictions about the effect of turnover on

---

<sup>4</sup>See also Toral (2022b) and Frey and Santarrosa (2022) for other excellent electoral RDs on bureaucratic politics in Brazil.

bureaucratic retention (resp. turnover) and quality with implications for the quality of subsequent policy implementation. We show how these mechanisms manifest in the LATEs estimated by electoral RDs.

## 1 What causal effects do electoral RDs identify?

RDs permit identification of some form of local treatment effect under relatively weak assumptions. We will focus on RDs that identify the local average treatment effect (LATE) consistent with almost all applications of this design.

In electoral district,  $d$ , we will denote possible election outcomes (i.e., victory of a party, candidate, or proposal) by  $i$ . For simplicity, we will refer to these election outcomes as a victory for a “candidate”  $i$ . Each candidate earns a vote share  $V_{id} \in [0, 1]$ . Suppose further that the candidate indexed by  $j \neq i$  is the candidate that wins the most votes aside from candidate  $i$  (and thus could be the winner or the marginal loser). We can define the margin of victory of candidate  $i$  over candidate  $j$  as:  $M_{id} \equiv V_{id} - V_{jd} \in [-1, 1]$ . The treatment—an election outcome defined relative to candidate  $i$ —can be expressed as:

$$Z_{id} = \begin{cases} 1 & \text{if } M_{id} \geq 0 \\ 0 & \text{if } M_{id} < 0. \end{cases} \quad (1)$$

Given this notation of treatment, observed outcomes are given by:  $Y_{id} = Y_{id}(1)Z_{id} + Y_{id}(0)(1 - Z_{id})$ , where  $Y_{id}(1)$  and  $Y_{id}(0)$  are treated and untreated potential outcomes, respectively. RDs typically seek to measure the LATE in a tied election (at the point of discontinuity), i.e., when  $V_{id} = 0$ :

$$\tau_{id} = E[Y_{id}(1) - Y_{id}(0) | M_{id} = 0]. \quad (2)$$

This quantity is typically estimated by evaluating limits on both sides of the discontinuity:

$$\tau_{id} = \lim_{M_{id} \downarrow 0} E[\widehat{Y}_{id} | M_{id} = m] - \lim_{M_{id} \uparrow 0} E[\widehat{Y}_{id} | M_{id} = m]. \quad (3)$$

In this “standard” RD set-up, researchers compare realized outcomes for both the winning and (marginally) losing outcome/candidate,  $i$  and  $j$ . Marshall (2022) points out that this is not the case in many electoral RD designs.<sup>5</sup> For example, if researchers seek to measure effects on policies set by a winning politician or party, losing candidates do not have the power to set policy, so these outcomes are not realized. As a result, most electoral RDs compare the realized outcomes as a function of attributes of marginal winners across districts.

Standard RD estimators rely on a continuity framework for identification of LATEs in an RD (Cattaneo and Titiunik, 2022). This framework invokes two assumptions (Imbens and Lemieux, 2008; Hahn, Todd, and Van der Klaauw, 2001). First, it assumes that the expectation of potential outcomes  $E[Y(0) | M_d = m]$  and  $E[Y(1) | M_d = m]$  are continuous in  $m$ . Second, it assumes positive density around the threshold.

It is worthwhile to note that we distinguish between *identification* and *interpretation* of the of the LATE. The issues that we discussion are primarily concerns of interpretation. Specifically, we ask what the LATE,  $\tau_d$ , tells us about the causal effect of some electoral outcome within the RD framework. The issues that we discuss can imply sorting, which may imply an endogenous selection into treatment (see Grimmer et al. (2011) for discussion in the context of close elections). Such sorting can introduce bias in estimated LATEs. Fortunately, there exist straightforward statistical tests for sorting as elaborated by McCrary (2008) and Cattaneo, Jansson, and Ma (2020).

---

<sup>5</sup>Marshall (2022) focuses on the setting of politician characteristic regression discontinuity designs that seek to estimate the effect of a candidate with a given characteristic winning office. This comparison of marginal winners across districts extends beyond the class of RDs that he characterizes.

## 2 General Framework

Our general framework is guided by the insight that political actors take actions in an effort to improve the future electoral prospects of candidates or parties that they support. This insight is either assumed or observed in many studies of democratic politics. To maintain a sufficient level of abstraction, we will consider a class of theories united by a common sequence of actions. Consistent with our guiding intuition, we assume that among other possible goals, a candidate seeks to be elected (or re-elected) in election  $t = 1$ .<sup>6</sup> Without loss of generality and for ease of exposition, we will refer to this actor as the  $t = 0$  incumbent.<sup>7</sup> We will denote the parameter of  $q_0 \in [0, 1]$  as the first-period incumbent's *perception* of their likelihood of retention in election  $t = 1$ . This sequence is given by:

1. Nature decides the election  $t = 0$ , determining the incumbent.
2. An actor takes some action,  $a_0(q_0)$  which may be conditioned on anticipated prospect of the incumbent's re-election,  $q_0$ .
3. Election  $t = 1$  occurs. The incumbent's margin of victory or defeat is given by  $m_1(a_0)$ . The treatment,  $Z$ , is realized:

$$Z = \begin{cases} 1 & \text{if } m_1(a_0) \geq 0 : \text{The incumbent wins re-election} \\ 0 & \text{if } m_1(a_0) < 0 : \text{The incumbent loses re-election.} \end{cases} \quad (4)$$

4. The second-period incumbent takes some action  $a_1(Z, q_1, a_0)$  which is measured by realized outcome  $Y(Z)$ .

From this sequence, three assumptions are worth note. First, the first-period incumbent or another actor may be able to affect their electoral fortunes through a first-period action. In other

---

<sup>6</sup>One could alternatively assume that an actor seeks a different electoral goal, i.e., for a party to be re-elected in  $t = 1$ .

<sup>7</sup>Note that our example in Section 3 instead on a bureaucrat at  $t = 0$ . Both can be analyzed using the same framework.



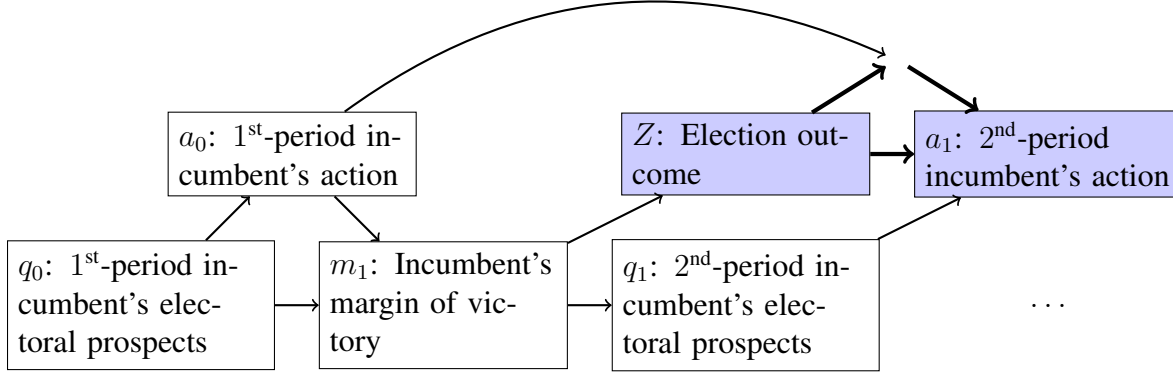


Figure 1: DAG illustrating class of models of interest. The causal relationship of interest is shaded in blue and the thick arrow indicates the causal effect that the analyst seeks to estimate.

words, the incumbent's margin of victory,  $m_1$ , can vary in their first-period action,  $a_0$ . These actions might include a policy choice or some type of learning or information revelation. Second, this incumbent's actions can vary in her anticipated likelihood of re-election, as  $a_0$  is a function of  $q_0$ . This suggests that a first-period incumbent facing a close race might take a different action than an incumbent that anticipates a landslide victory. For example, an incumbent facing a nail-biter might be more reticent to implement an unpopular policy than an incumbent who is confident that she will win. Finally,  $a_1$  may be a function of  $a_0$ . This means that actions may be constrained by a status quo. For example, if the outcome of interest is some measure of policy change, the existing policy ( $a_0$ ) may influence whether the second-period incumbent has an incentive to pursue a different policy.

This class of models can be represented by a directed acyclic graph (DAG), as depicted in Figure 1. Two nodes of the graph are shaded to highlight the causal relationship of interest: the effect of election outcomes on the subsequent action of an incumbent. Close election RDs are routinely invoked to measure this causal relationship. It is useful to consider the path from  $a_0$  (the status quo) to  $a_1$ . Of course, if the RD is valid, the conditional expectation of any measure of  $a_0$  should be smooth around the threshold. However, the status may differentially constrain the second-period incumbent as a function of treatment. Hence, we depict an interaction between  $a_0$  and  $Z$  in the DAG.

As with many theoretical models, the sequence of actions corresponds to the political process in a single district/electoral contests. Quantitative research designs, like RDs, that seek to estimate some form of aggregate causal effects leverage observations from multiple contests. For this reason, we impose a general statistical model across observations. To do so, we assume that the margin of victory,  $m_1$  can be expressed as:

$$m_1 = g(a_0(q_0)) + \epsilon. \quad (5)$$

The function  $g(\cdot)$  is a reduced-form representation of voters’ voting behavior that maps an incumbent’s action,  $a_0$  into her subsequent margin of victory,  $m_1$ .  $\epsilon \sim f(\cdot)$ , where  $f(\cdot)$  is a symmetric, mean-zero probability density function. This noise can be interpreted as a valence shock which is independently and identically distributed across districts (contests).

## 2.1 Two Experimental Benchmarks

Our goal is to trace the implications of a politician’s forward-looking considerations for the interpretation of the LATE estimand in regression discontinuity designs. To this end, it is useful to ask the question: under what conditions can we “break” or eliminate this type of forward-looking consideration? Because researchers can choose the research designs they use but cannot generally control the behavior of political actors, it is useful to consider what could, in principle, be achieved through research design alone. To this end, we consider two experimental benchmarks. Both benchmarks should be understood as *hypothetical*. Researchers turn to electoral RDs precisely because it is impossible or unethical to randomly manipulate electoral outcomes consistent with these experimental designs.

**Benchmark #1:** In experimental benchmark #1, we consider an experiment in which an experimenter randomly assigns election outcomes unbeknownst to the first-period incumbent. Formally, this can be achieved by setting  $g(a_0(q_0)) = 0$  for all  $q_0$ . Here, the margin of victory—and thus treatment—is solely a function of random noise ( $\epsilon$ ). Given our distributional assumptions on  $f(\cdot)$ , this implies that the incumbent wins re-election with probability  $\frac{1}{2}$ . In this variant, the first-period

incumbent anticipates there will be an election but does not know that the result will be randomized. As such, they choose  $a_0$  to maximize their vote margin given  $q_0$ .

**Benchmark #2:** In experimental benchmark #2, we consider a more extreme experiment in which the first period incumbent does not realize that will be faced with an election to maintain power. One could imagine that the incumbents are dictators with long time horizons. An experimenter both (i.) introduces the election as a means for retaining power; and (ii.) randomizes the outcome of the election ( $m_1$ ). Formally, this can be expressed through two parametric restrictions on the general model. First, fix  $q_0 = 1$  for all districts, which means that all incumbents anticipate staying in office with probability 1. Second, as in Benchmark #1, we will set  $g(a_0(q_0)) = 0$  for  $q_0 = 1$ .

These experimental designs are represented as a DAG in Figure 2. Importantly, both benchmarks can be viewed as restrictions of the main model in which causal paths are removed. Randomizing election outcomes omits the paths from first-period perceptions of likelihood of victory ( $q_0$ ) and incumbent actions ( $a_0$ ) to the realized margin of victory  $m_1$ . Because  $m_1$  is randomized in both experiments, second-period incumbents should not use this outcome to inform their anticipated probability of victory,  $q_1$ .<sup>8</sup> For this reason, we omit the arrow from  $m_1$  to  $q_1$  in the benchmark. The difference between benchmarks #1 and #2 is that the first-period incumbent may take a different action  $a_0$ . Different first-period actions can generate different effects when  $a_0$  “interacts” with treatment,  $Z$ . While this difference in behavior is not observably different from the Benchmark #1 DAG in Figure 2, we show that is indeed consequential for interpretation of the LATE.

## 2.2 Causal Estimands

We use this framework to characterize relevant estimands formally. We compare the LATE associated with the RD design to the average treatment effects (ATEs) associated with both experimental benchmarks. Because we assume that researchers randomly assign margin of victory  $m_1$  in both experiments, we can also characterize the LATEs from both experiments (when  $m_1 = 0$ ). While

---

<sup>8</sup>Note, however, that this is not consequential for our argument.

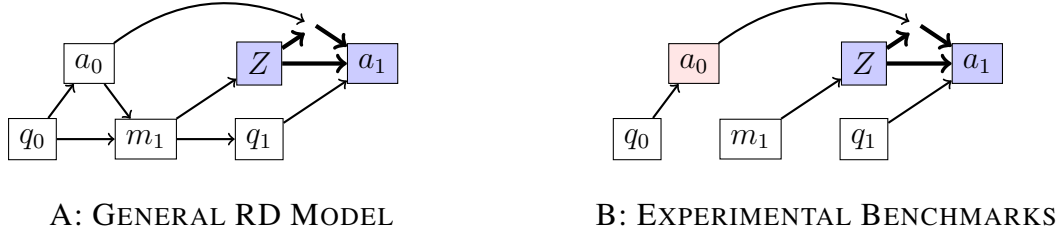


Figure 2: DAGs illustrating differences between our “full” RD model and the experimental benchmarks. Note that  $a_0$  is apt to differ across the two experimental benchmarks.

this is a quantity that experimentalists would not routinely estimate, it allows us to isolate the effects of the design—experiment versus RD—from the estimand.

**Remark 1. *LATE in a RD.*** The *LATE* of an incumbent victory on the second-period incumbent action is given by:

$$LATE_{RD} = E[a_1(Z = 1, q_1, a_0)|m_1(a_0) = 0] - E[a_1(Z = 0, q_1, a_0)|m_1(a_0) = 0]$$

From this expression, it is important to note that the first-period incumbent’s actions enter the LATE estimand in two ways. First and most obviously, the existing policy  $a_0$  constitutes the status quo. As such, the second-period action,  $a_1$ , may be a function of this pre-existing policy choice. This is the case regardless of whether the incumbent wins re-election. Second, and more subtly, the incumbent’s margin of victory is a function of their first-period action. Conditioning the LATE at  $m_1(a_0) = 0$  is fundamental to identification in the RD design. Yet, this also conditions the effective sample on first-period behavior.

To illustrate the sample-conditioning aspect of the LATE, consider the probability that  $m_1(a_0) \in [-\delta, \delta]$ , for arbitrarily small  $\delta > 0$ . This is the probability that a given election falls arbitrarily close to the  $m_1(a_0) = 0$  discontinuity. We can express this conditional probability, given that some  $q_0 = q$  is:

$$\Pr(m_1(a_0) \in [-\delta, \delta]|a_0 = a) = \Pr(\epsilon \in [-\delta - g(a_0 = a), \delta - g(a_0 = a)]).$$

This expression provides intuition that when researchers focus on the local estimand, they condition on behavior realized before the discontinuity of interest. When this behavior affects election outcomes, the LATE “internalizes” this anticipatory behavior in part through the sample conditioning.

We proceed by examining the ATEs produced by our experimental benchmarks in Remark 2. First, these estimands obviously do not condition on the realized margin of victory. Instead, they evaluate the treatment effect across all elections in a given sample. This eliminates one of the two channels through which anticipatory behavior of politicians enters the estimand. Second, the other channel—the “status quo” action—varies (and thus enters) the observed potential outcomes  $a_1(\cdot)$  in Benchmark #1. In an experiment in which researchers—unknownst to politicians—randomized the election, anticipatory actions by politicians could still influence the status quo. As such, this channel remains open in Benchmark #1. In Benchmark #2, in contrast, no politician anticipates an election and they do not act accordingly. While  $a_1$  is still a function of  $q_1$ , we note that  $Z$  is independent of  $q_1$  in both benchmarks.

**Remark 2. ATEs with experimental benchmarks.** *The ATEs of each benchmarking experiment are given by:*

$$\begin{aligned}ATE_{B1} &= E[a_1(Z = 1, q_1, a_0)] - E[a_1(Z = 0, q_1, a_0)] \\ATE_{B2} &= E[a_1(Z = 1, q_1)] - E[a_1(Z = 0, q_1)]\end{aligned}$$

The ATEs in the experimental benchmarks obviously relax the sample-conditioning channel through which the anticipatory behavior of an incumbent could enter the reduced-form causal effect. Additionally, when the incumbent does not anticipate any possibility of turnover as in Benchmark #2, the “status quo” channel is also closed off. We now ask whether this is solely a function of the difference in estimand (ATE versus LATE). Comparison of the LATEs in Remarks 1 and 3 suggest that these properties of the benchmark ATEs are maintained when considering the LATEs. Because both benchmark experiments randomize the margin of victory, it means that the

margin of victory must be independent of the incumbent’s first period action ( $m_1 \perp a_0$ ). As such, the sample conditioning aspect of  $LATE_{RD}$  is eliminated. As in the ATE analysis, the anticipatory behavior can generate different status quos in Benchmark #1 but not Benchmark #2.

**Remark 3. *LATEs with experimental benchmarks.*** *The LATEs in each benchmarking experiment are given by:*

$$LATE_{B1} = E[a_1(Z = 1, q_1, a_0)|m_1(a_0) = 0] - E[a_1(Z = 0, q_1, a_0)|m_1(a_0) = 0] \text{ and } m_1 \perp a_0.$$

$$LATE_{B2} = E[a_1(Z = 1, q_1)|m_1(a_0) = 0] - E[a_1(Z = 0, q_1)|m_1(a_0) = 0] \text{ and } m_1 \perp a_0.$$

### 2.3 Interpretation and identification of the LATE in Electoral RDs

We argue that the two channels through which anticipatory behavior “enters” the LATE estimand in electoral regression discontinuity designs pose important considerations for the interpretation. The standard description of the LATE estimated by electoral regression discontinuity designs is the effect of some class of electoral outcomes ( $Z$ ) on some post-treatment behavior ( $a_1$ ) in tied races (where  $m_1(a_0) = 0$ ).<sup>9</sup> Discussion of mechanisms typically focuses on why some electoral outcome might affect post-treatment behavior by positing measured or unmeasured channels/mediators. We argue that the LATE generally incorporates two additional mechanisms worthy of consideration.

The likelihood of selection into close elections should encourage scholars to consider the political process that generates close elections. When incumbent or candidate behavior can affect election outcomes—a desirable feature from the perspective of electoral accountability—the actions and circumstances preceding close elections may be very different from those preceding landslide elections. This introduces the possible limits to the generalizability of LATEs. While the generalizability of LATEs is a known concern, it typically focuses on the possibility that marginal winners and losers will choose different actions than non-marginal winners and losers, respectively

---

<sup>9</sup>While rarer in existing applications, one could also study the effect of a class of election outcomes on beliefs or attitudes.

(de la Cuesta and Imai, 2016; Cattaneo et al., 2020). However, the possibility for status quo effects presents another explanation for limited ability to generalize. Suppose that marginal and non-marginal candidates take different actions in anticipation of an election. The “status quo” mechanism enters the LATE when there exists an interaction between this policy choice and treatment. In this case, different pre-electoral choices can generate problems for extrapolation or generalizability within sample. As in Benchmark #2, this can occur even if pre-electoral actions do not selection into close elections. As a result, researchers should consider both explanations for heterogeneity when discussing the generalizability of a LATE.

The selection channel may also have implications for *identification* of the LATE in electoral RDs. Suppose that incumbents could completely determine electoral outcomes through their first-period actions, they could “sort” into winning re-election (or ensuring victory for their party). In our formalization, if  $Var(\epsilon)$  is sufficiently small relative to how electoral outcomes change in first-period actions ( $\frac{\partial g(a_0(q_0))}{\partial a_0(q_0)}$ ), incumbents can choose  $a_0$  to win re-election at disproportionate rates. Sorting can introduce bias into the LATE. This potential identification problem is, however, more straightforward than problems of interpretation. McCrary (2008) and Cattaneo, Jansson, and Ma (2020) provide tests for sorting (differences in density at the threshold) for RDs. Because this possibility is ultimately quite straightforward to address using existing statistical tests, we instead focus on the more challenging questions of interpretation.

Reduced-form LATEs are most commonly motivated as “partial equilibrium effects” that isolate the effect of treatment on the decision of a single player, e.g., the second-period incumbent. Proponents of the partial equilibrium approach argue that partial equilibrium effects offer “cleaner” (or more easily interpretable) tests of a mechanism or set of mechanisms by considering only the behavior of a single actor. Skeptics, however, argue that equilibrium results may provide a clearer characterization of “how the world works.” We have shown that in the electoral context LATEs generally “internalize” equilibrium considerations. The comparison to Benchmarks #1 and #2 suggests that these considerations likely to be more prevalent in cleanly-identified regression discontinuity LATEs than in analogous experimental estimands. As such, researchers analyzing electoral

RDs need to invoke equilibrium considerations for interpretation of the LATE estimand.

### 3 Illustration: A model of bureaucratic selection and performance

We now provide an applied illustration of the properties of electoral RDs that we describe. Consistent with our motivation of Mayor Carlos and his challenger José, we develop a simple two-period model of bureaucratic selection and effort. To do so, we consider an environment with two periods, indexed  $t \in \{1, 2\}$  and two competing politicians,  $P_A$  and  $P_B$ . The politician in power in each period  $t$  appoints a bureaucrat who chooses an action  $e_t \in \{0, 1\}$ . Each bureaucrat is of type  $\theta = \{L, H\}$ , where  $\Pr(\theta = H) = \pi$ .

In each period, the state of the world  $s_t \in \{0, 1\}$  is realized and observed by only the bureaucrat. Both states are equally likely:  $\Pr(s_t = 1) = \frac{1}{2}$ . The politician in power makes a policy benefit of 1 if the bureaucrat's action matches the state (i.e.  $e_t = s_t$ ). A bureaucrat is either high type ( $\theta = H$ ) or low type ( $\theta = L$ ) and his type is his private information. A high type receives a payoff of 1 when  $e_t = s_t$ . In contrast, when  $e_t \neq s_t$  a low type receives a payoff of  $r_t$ , which is uniformly distributed on the support  $[0, R]$ . These payoffs can be viewed as “policy benefits” for the low type bureaucrat who doesn't share the policy goals of the politician. A bureaucrat of either type additionally earns an “office benefit” of  $w$  for each period they are employed. The per-period utility of an employed bureaucrat is thus:

$$U_B(e_t) = \begin{cases} w + \mathbb{I}(s_t = e_t) & \text{if } \theta = H \\ w + r_t \times \mathbb{I}(s_t \neq e_t) & \text{if } \theta = L. \end{cases} \quad (6)$$

If a bureaucrat is not employed, their utility is normalized to 0.

The politician shares an affinity with a pool of potential bureaucrats. While in office, if a bureaucrat and politician share this affinity, the politician earns a benefit of  $b$ , which is uniformly distributed on the support  $[0, B]$ .  $b$  can be interpreted as a measure of the (non-electoral) returns to patronage hiring, including but not limited to the welfare of affiliates. If a politician is not in office, their utility is normalized to 0. Without loss of generality, we assume that  $P_A$  is in office in



period 1 and that the first-period bureaucrat shares an affinity with  $P_A$ .

At the beginning of period 2, an election between  $P_A$  and  $P_B$  takes place. We will denote by  $q \in [0, 1]$  the entrenchment of  $P_A$  in the first period. We interpret this as the incumbent's (perceived) probability of re-election. We allow for good policy performance ( $e_1 = s_1$ ) of  $P_A$  to be rewarded. This means that the performance of the bureaucrat in the first period contributes to the re-election of the incumbent. If the bureaucrat matches her action to the state, the probability that the incumbent is re-elected is  $\max\{q + \alpha, 1\}$ .  $\alpha$  measures the degree to which good performance of the bureaucrat contributes to the incumbent's electoral fortune. When they fail to do so, the probability of re-election is  $q$ .

The elected politician decides to re-appoint the first-period bureaucrat or replace him with a potential bureaucrat whose type is private information. The politician and bureaucrat do not discount.

The sequence mirrors our general framework, and proceeds as follows:

1. The bureaucrat observes the state of the world,  $s_1$  and  $r_1$
2. The bureaucrat chooses  $e_1$  and first-period utilities are realized.
3. The election occurs.
4. Politicians observe  $b$ .
5. The winner appoints (or re-appoints) the second-period bureaucrat.
6. The bureaucrat observes the state of the world,  $s_2$  and  $r_2$ .
7. The bureaucrat chooses  $e_2$  and second-period utilities are realized.

We characterize the unique perfect Bayesian equilibrium of the model. We impose several parametric restrictions to direct focus to the interesting cases of the model. First, we assume that  $\frac{\omega}{R} < 12$  to ensure that there exists  $r$  such that a type  $L$  bureaucrat in the first period could choose  $e_t \neq s_t$ . Second, we assume that  $B < 1 - \pi$ . By limiting the benefit from employing a

bureaucrat with whom the politician shares affinity in this way, we accommodate the possibility that a bureaucrat's strong first-period performance could incentivize a  $P_B$  to keep the bureaucrat in the second period.

### 3.1 Equilibrium

We use perfect Bayesian equilibrium. In the second period, a bureaucrat of type  $H$  will clearly choose  $e_2 = s_2$  whereas a bureaucrat of type  $L$  will choose  $e_2 = 1 - s_2$ .

In order to characterize the second-period politician's retention decision, let the posterior belief about the first-period bureaucrat's type be denoted  $\mu$ . Consider first the case in which  $P_A$  has won re-election. They must choose between the first-period bureaucrat or candidate from the pool of potential bureaucrats. Because the politician will obtain the benefit from employing a bureaucrat from the pool of potential bureaucrats,  $b$  with either candidate, they compare their posterior belief that the incumbent bureaucrat is of type  $H$ ,  $\mu$ , to the share of type  $H$  bureaucrats in the pool of candidates,  $\pi$ . Clearly then, if  $\mu \geq \pi$ ,  $P_A$  will retain the first-period bureaucrat. If not, they will appoint a new bureaucrat.

Now consider the case in which  $P_B$  has won office in period 2. If  $P_B$  keeps the incumbent bureaucrat, the expected policy benefit is  $\mu$ . If they replace the bureaucrat, they can earn the payoff from affinity,  $b$ , and their expected policy benefit is  $\pi$ . As such,  $P_B$  will retain the bureaucrat if and only if  $\mu \geq \pi + b$ . Given the density of  $b$ , the probability of retention by  $P_B$  can be expressed as:

$$\sigma \equiv \Pr(b < \mu - \pi) = \frac{\mu - \pi}{B}. \quad (7)$$

We now turn to the first-period behavior of the bureaucrat. A bureaucrat of type  $H$  will always choose  $e_1 = s_1$ . Here, the bureaucrat's preferred policy aligns with the policy that is most likely to improve the odds of retention. In contrast, a bureaucrat of type  $\theta = L$  faces a tradeoff between her policy objective ( $e_t \neq s_t$ ) and the policy that maximizes the probability of retention ( $e_t = s_t$ ). Denote by  $\lambda$  the probability that type  $L$  chooses  $e_1 = s_1$  in equilibrium. By Bayes' rule, then, upon realization of payoffs that indicates that  $e_1 = s_1$ , the politicians' posterior belief that the

first-period bureaucrat is of type  $H$  is:

$$\mu(\lambda) = \frac{\pi}{\pi + (1 - \pi)\lambda} \quad (8)$$

It is useful to note that  $\mu(\lambda) \geq \pi$ . This means that by choosing  $e_1 = s_1$ , the bureaucrat can ensure that they will be retained if  $P_A$  wins the election. When does a bureaucrat of type  $L$  choose  $e_1 = s_1$ ? We first consider the case in which  $q + \alpha \geq 1$ . Here, by choosing  $e_1 = s_1$ , the bureaucrat can ensure that the politician will win the election. The bureaucrat of type  $L$  thus selects  $e_1 = s_1$  if:

$$\underbrace{2w + E[r_2]}_{\text{Utility if } e_1 = s_1} \geq \underbrace{w + r_1}_{\text{Utility if } e_1 \neq s_1}$$

$$r_1 \leq \frac{2w + R}{2}.$$

The probability that a type- $L$  bureaucrat chooses  $e_1 = s_1$  is thus:

$$\lambda = \lambda(\sigma) = \frac{1}{2} + \frac{w}{R}.$$

We now consider the case in which  $q + \alpha < 1$ . Here, the bureaucrat cannot ensure the incumbent's victory by choosing  $e_1 = s_1$ , so they must consider their likelihood of retention under both  $P_A$  and  $P_B$ . In this case, the bureaucrat of type  $L$  thus selects  $e_1 = s_1$  if:

$$\underbrace{w + (q + \alpha + (1 - q - \alpha)\sigma)(w + E[r_2])}_{\text{Utility if } e_1 = s_1} \geq \underbrace{w + r_1}_{\text{Utility if } e_1 \neq s_1}$$

$$r_1 \leq (q + \alpha + (1 - q - \alpha)\sigma) \frac{2w + R}{2}.$$

The probability that type  $L$  chooses  $e_1 = s_1$  is then:

$$\lambda = \lambda(\sigma) = (q + \alpha + (1 - q - \alpha)\sigma) \left( \frac{1}{2} + \frac{w}{R} \right).$$

Upon receiving a first-period payoff of 1, given  $L$ 's strategy  $\lambda$ , the probability that  $P_B$  retains the period 1 bureaucrat is thus:

$$\sigma = \sigma(\lambda) = \frac{1}{B} \left( \frac{\pi}{\pi + (1 - \pi)\lambda} - \pi \right).$$

This analysis yields the following equilibrium:

**Proposition 1.** *There exists a unique equilibrium where a bureaucrat of type  $H$  chooses,  $e_1 = s_1$ , a bureaucrat of type  $L$  chooses  $e_1 = s_1$  with probability  $\lambda(\sigma^*)$ ,  $P_A$  retains the first-period bureaucrat only if it receives a first-period policy payoff of 1, and  $P_B$  keeps the incumbent with probability  $\sigma(\lambda^*)$  if it receives a payoff of 1. Both  $P_A$  and  $P_B$  replace the incumbent bureaucrat when if the first-period politician receives a policy payoff of 0.*

*(Proof in Appendix.)*

This equilibrium suggests that different election outcomes could have implications for several important behavioral outcomes: bureaucratic retention and second-period policy by the bureaucrat. Note that expected second-period policy is a direct function of the share of high and low types in the bureaucracy. To study these outcomes in an RD design, it is important to consider the incumbent politician's equilibrium probability of victory. To clarify the structure of the cases, we will denote equilibrium strategies explicitly when  $q + \alpha \geq 1$  and implicitly (with  $\lambda^*(q, \alpha)$  and  $\sigma^*(q, \alpha)$ ) when  $q + \alpha < 1$ .

We denote the equilibrium probability of victory as  $q^*$ . Clearly this probability depends on the share of high types among first-period bureaucrats,  $\pi$ . It also depends on the rate at which low types choose  $e_1 = s_1$  in equilibrium,  $\lambda^*(q, \alpha)$ . As such the equilibrium probability of victory is therefore:

$$q^* = \begin{cases} q + \alpha(\pi + (1 - \pi)\lambda^*(q, \alpha)) & \text{if } q + \alpha < 1 \\ \pi + (1 - \pi) \left( \frac{1}{2} + \frac{w}{R} \right) & \text{if } q + \alpha \geq 1. \end{cases}$$

It is important to reiterate that the incumbent's probability of victory depends on the first period

behavior of the bureaucrat. Moreover, the bureaucrat purposely chooses this behavior to improve their likelihood of retention. To the extent that  $q^*$  provides information about how close the election will be, this means that first period actions will influence selection into any form of close-elections bandwidth.

### 3.2 Empirical implications for an electoral RD

We focus on two important outcomes captured by our model: (i.) the probability of bureaucratic retention, and (ii.) bureaucratic quality, or the share of second-period bureaucrats of type  $H$ . This can be measured behaviorally by assessing whether  $e_2 = s_2$ , given our expectations of second period behavior. Our model yields testable predictions for each outcome:

**Proposition 2.** *Relative to when the challenger ( $P_B$ ) wins the election, when the first-period incumbent ( $P_A$ ) is re-elected:*

- (i) *The first-period bureaucrat is retained at a higher probability.*
- (ii) *The second-period bureaucrat is more likely to be of type  $\theta = H$ .*

Inspection of Table A1 reveals two theoretical implications of our model for an electoral RD. The first result in 2 is relatively unsurprising. Bureaucratic retention is more likely when the incumbent stays in office. Less intuitively, re-election of the bureaucrat yields a higher quality second-period bureaucracy. The logic underpinning both results is similar. Recall that the first-period politician shares an affinity with the bureaucrat. As such, when re-elected, the incumbent need only consider bureaucratic quality when deciding whether to retain the bureaucrat. If elected, a challenger needs to consider both quality and affinity. Because they do not share an affinity with the “incumbent” bureaucrat, they are more likely to replace them, reducing the extent of positive selection. Importantly, this quality result does not depend on bureaucratic learning or expertise, it is driven entirely by selection.

Figure 3 illustrates the results in Proposition 2 under one set of parameter values. Importantly, it shows that while treatment effects maintain the predicted sign, treatment effects are vary in the equilibrium probability of victory (on the  $x$ -axis). This occurs because, in equilibrium, the bureau-

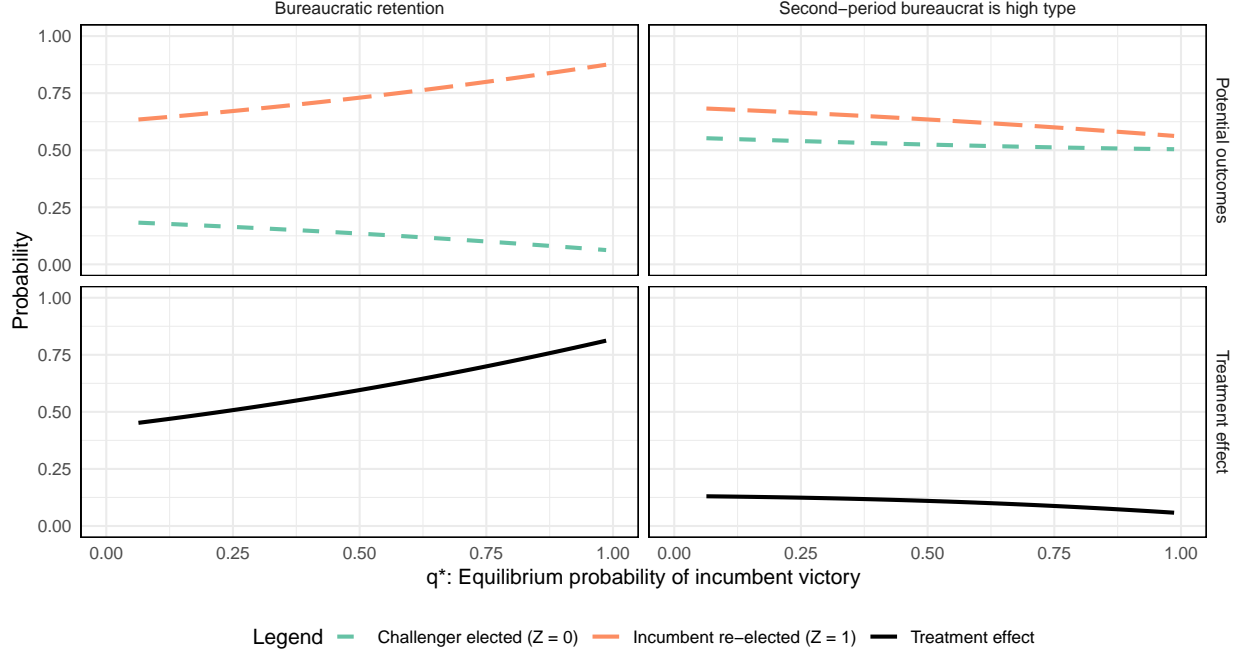


Figure 3: Potential outcomes and treatment effects under the following parametric assumptions:  $q \in [0, 0.9]$ ,  $\alpha = 0.1$ ,  $B = 1$ ,  $R = 1$ ,  $w = 0.25$ ,  $\pi = 0.5$ .

crat's first period actions influence both the probability of victory and the politician's second-period decision.

How do the effects we observe vary from those that might be observed under either benchmark? Under Benchmark #1, in which election outcomes are randomized, the difference between  $LATE_{RD}$  and  $ATE_{B1}$  is simply the differences in weights afforded to each observation when calculating upper and lower limits. The RD affords weight to a sample of units in close elections in order to calculate the effect when an election is tied. Because effects vary in  $q^*$ , this reweighting and sample conditioning is likely to drive differences in the two estimands. Yet, it is impossible to ascertain the sign or magnitude of these estimates without the density of  $q^*$ .

Under Benchmark #2, when no re-election is anticipated (when  $q = 1$ ), the behavior of first-period bureaucrats does not vary in their anticipated prospect of victory, since all actors anticipate that the incumbent will be retained with certainty. Here, the difference in  $LATE_{RD}$  and  $ATE_{B2}$  incorporates differences in the degree of politician learning about the bureaucrat in addition to the compositional differences in the density of  $q^*$  discussed with reference to Benchmark #1.

### 3.3 Simulation: Comparing estimands to experimental benchmarks

We emphasize the difference in interpretation of RD estimates and both experimental benchmarks through a simulation. In order to conduct a simulation, we construct distribution of the probability of victory ( $q$  in the model) and the margin of victory (the running variable for the RD). To do so, we use data on the margin of victory in Brazilian mayoral races over 5 cycles (2000, 2004, 2008, 2012, and 2016) to calibrate a distribution of probabilities of victory. To this end, we propose a mapping from  $q$  to the realized margin of victory, which we describe in Appendix B. With this data, and the parametric assumptions imposed in Figure 3, it is straightforward to simulate observed potential outcomes following the equilibrium of the model.

We simulate both outcomes of interest: an indicator for bureaucratic retention and an indicator for a high-type bureaucrat in the second period. In particular, we simulate potential outcomes under:

1. The equilibrium of the model: this corresponds to the observational data used in the RDs.
2. Benchmark #1: We randomize the margin of victory, assuming that all incumbents have a  $\frac{1}{2}$  probability of victory.
3. Benchmark #2: We assume that  $q_0 = 1$  and randomize the margin of victory, following #2.

With each iteration of these outcomes (under each model), we estimate treatment effects of an incumbent's re-election of the incumbent on both outcomes of interest.

In Figure 4, we report estimates of treatment effects estimated by difference-in-means the bias-corrected regression discontinuity estimator described by Cattaneo and Titiunik (2022).<sup>10</sup> Consider first the observational data consistent with the data used in electoral RDs. We see that the difference-in-means and RD estimates diverge. This is because re-election is confounded by the anticipated likelihood of victory and bureaucrat's first-period actions. Low-type bureaucrats exert effort at higher rates when they believe the incumbent is more likely to be re-elected which leads

---

<sup>10</sup>For the RD estimates, we implement the default kernel and bandwidth-selection implemented by the `rdrobust` package.

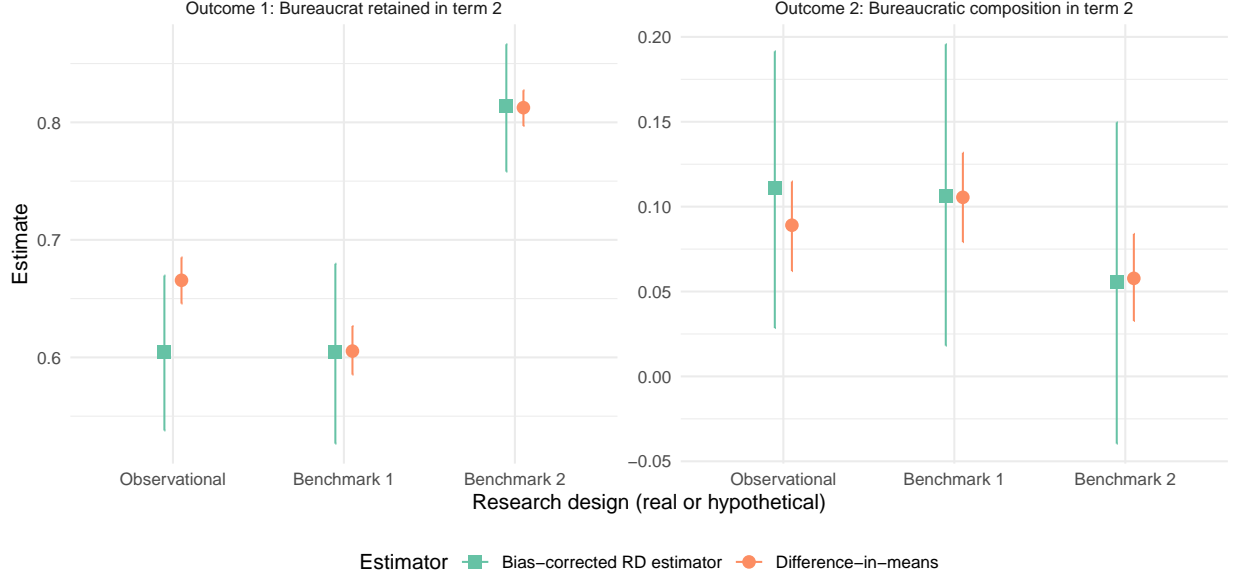


Figure 4: Estimates and 95% confidence intervals (constructed from  $n = 2,000$  realizations of the data) under three different theoretical data generating processes and two estimators. To simulate this data, we set  $\alpha = 0.1$ ,  $B = 1$ ,  $R = 1$ ,  $w = 0.25$ ,  $\pi = 0.5$ , and draw  $q$  from a distribution calibrated from the universe of Brazilian mayoral elections from 2000-2016.

to an exaggeration of the treatment effect on bureaucratic retention and an attenuation of treatment effects on bureaucratic quality, following Figure 3. We see both biases manifest when comparing the (biased) difference-in-means estimates to the (unbiased) RD estimates in the “observational” data in Figure 4.

In contrast, with both experimental benchmarks, the difference-in-means estimator estimates an ATE which is, by construction, equivalent to the LATE estimated by the RD. Unlike in the observational data, we see that there is no evidence of bias in these estimates. Moreover, like Stommes, Aronow, and Sävje (2021), our simulation reveals that the RD estimates are quite noisy.

More importantly than discussions of bias, however, is the difference between the observational data and the experimental benchmarks. While the LATE estimates of Benchmark #1 are similar to the observational data, the LATE of Benchmark #2 is substantially larger (for bureaucratic retention) and smaller (for bureaucratic composition). The magnitude of this difference between *estimands* under the present parametric assumptions is *larger* than the bias that motivates the adoption of the electoral RD. Of course, the magnitude of indirect (equilibrium) effects relative to bias



will depend on the model. Nevertheless, the differences between RD estimates and experimental benchmarks that omit the dynamic considerations we emphasize holds substantial implications for how we interpret the LATEs measured by electoral RDs.

## 4 Applications

We now turn to assess the prevalence of these general equilibrium considerations in RDs in political science. To do so, we examine all of the published RDs surveyed by Stommes, Aronow, and Sävje (2021). We first note that RDs outside of the context of elections generally do not present the dynamic concerns that we discuss. Most of these “non-electoral” RDs explore the settings where policies are targeted on the basis on some threshold in a running variable like an individual’s age or a community’s population. Four common running variables that are unlikely to raise these concerns are: age (e.g., Cavaille and Marshall, 2019), population thresholds (e.g., Larreguy, Marshall, and Querubin, 2016), performance metrics (e.g., Holbein, 2016), and geographic boundaries (e.g., Rozenas, Schutte, and Zhukov, 2017).<sup>11</sup> Consider the use of age of a running variable. When a treatment like mandatory education is assigned discontinuously a function of a threshold in age, students generally cannot change or manipulate their age in order to change their probability of being treated (or avoiding treatment). In this case, the dynamic considerations we analyze do not obviously arise.

Our concerns are salient, however, in electoral RDs on various topics. This design has routinely been used to measure incumbency effects, the effects of partisan alignment between politicians of different levels of government, the effects of partisan control of government, the value of office, and the effects of incumbent ideology, among others. In addition to the electoral RD studies that exploit close elections in democracies, a few studies use electoral RDs in different settings. We classify these applications in Table A3.

We now connect our general framework to three specific applications where these concerns arise. We consider three articles representative of the applications in Table A2: Fourinaies and

---

<sup>11</sup>See Table A2 for a list of running variables in non-electoral RDs where the equilibrium considerations we raise are unnecessary for interpretation.

Hall (2014), Bohlken (2018), and Eggers and Hainmueller (2009). Fourniaies and Hall (2014) use a regression discontinuity design in US House and state legislative elections to estimate the incumbency advantage on campaign contributions. They argue that access-motivated interest group has more incentive to invest in incumbents' campaigns. Bohlken (2018) measures the effects of partisan alignment between the state and national legislators in India on the characteristics of development projects proposed under a development scheme. She argues that elite cooperation facilitates partisan favoritism in the allocation of these projects. Eggers and Hainmueller (2009) employ a regression discontinuity design to estimate the effect of serving in Parliament on the wealth of British politicians. They argue that the primary mechanism behind the wealth effects of political office is that office provides Members of Parliament (MPs) the political connections and knowledge that is valuable to firms.

As in our general framework, it is straightforward to identify a forward-looking behavior ( $a_0$ ) that might affect both election outcomes ( $m_1$ ) and the outcome of interest in the subsequent (post-election) period ( $a_1$ ). In Table 1, we show how our framework links to each of these settings. In the appendix, we elaborate on the logic of the arguments in Table 1, drawing on wider literatures.

## **5 Practical guidance for electoral RD practitioners**

The effects of election outcomes are central to many research agendas in political science. Given the importance of electoral RDs in this pursuit, we consider the implications of our argument for these efforts. We argue that answers to three questions can guide researchers.

### **5.1 Question #1: Do we want to know partial or (general) equilibrium effects?**

Our first question considers the desirability of measuring partial versus general equilibrium effects. Scholars favoring partial equilibrium effects care about “all else equal” comparisons between marginal winners and marginal losers that hold fixed the behavior of all others. As a test of whether one specific mechanism is present, partial equilibrium effects can be quite useful. For example, we may wonder if “all else equal” winning office increases politicians' wealth (Eggers and Hainmueller, 2009). But if we want to make predictions about observed effects in the world,

	INCUMBENCY EFFECTS Fouirnaies and Hall (2014)	PARTISAN ALIGNMENT Bohlken (2018)	VALUE OF OFFICE Eggers and Hainmueller (2009)
$a_0$ : Pre-electoral actor(s) and action(s)	An interest group decides on the amount of campaign donations to the incumbent.	National legislators decide on the amount of development funds to allocate to state legislators.	Politicians decide the amount of legislative service to firms to exchange for financial benefits.
$m_1$ : How does $a_0$ plausibly affect election outcomes?	Campaign donations help the incumbent to run a successful campaign and thus improve her chance of being re-elected.	Development funds allow state legislators to build a political network with brokers who improve the chance of winning state elections.	On the one hand, more service to firms leads to fewer services to constituencies and therefore hurts the electoral prospect of the politicians. On the other hand, in exchange for legislative service, firms provide donations which enhance politicians' electoral prospects.
$a_1$ : How does $a_0$ affect $a_1$ (the outcome of interest)?	Past contributions facilitate connections between interest groups and politicians, which facilitate donations in the subsequent term.	Development funds allocated to the state legislators, in part as a function of the size of their established co-partisan networks.	Past constituency service provides an informational signal about the politician's type, which induces firms to provide them with subsequent employment.

Table 1: Application of framework to three published electoral RDs.

we likely want to know about other actors responses to these effects. How do legislatures respond to the partisanship of the governor when passing budget bills? How do religious leaders respond to initial provocations of violence? And how do these reactions (or “feedback effects”) map into the ultimate outcomes we observe? While general equilibrium interpretations pose more challenges for interpretation (and potentially identification), they may provide more accurate description of “how the world works.”

Our take on partial- versus general-equilibrium effects is pragmatic: measuring either type of effects can be valuable in different circumstances (Banerjee and Duflo, 2009). However, understanding which class of effect a causal estimand measures is crucial to the interpretation of the effect of interest. We argue that electoral RDs should be understood as measuring a very specific general equilibrium effect. As such, researchers should aim to characterize the actions—before and after the election outcome is realized—that feed into this estimand.

Is there an alternative research design to credibly study the partial equilibrium effects of elections? Here, we are skeptical. It is precisely because candidates, parties, and voters act to pursue

their preferred outcomes that these issues emerge. We have shown, through the analysis of experimental benchmarks, that experiments could “break” some of these indirect effects. However, there are normative concerns about intervening in elections at a scale that could change outcomes (Slough, 2022). Even in electoral experiments that appear to have downstream consequences from random assignment, researchers affect outcomes by changing electoral behavior, they do directly randomize election results as in our hypothetical benchmarks (Casey, Kamara, and Meriggi, 2021; Gulzar and Khan, 2018; Ofosu, 2019). For these reasons, we argue that the general equilibrium interpretation of the effects of elections is not, in practice, a unique feature of electoral RDs.

## 5.2 How can we maximize learning from electoral RDs?

Our argument stresses the standard interpretation of the LATE in electoral RDs is often imprecise and may hinder our understanding of the estimand. To this end, we provide two additional qualifications—of theoretical and empirical import—to the standard interpretation of these LATEs:<sup>12</sup>

- Standard interpretation: “The local effect of [electoral outcome] is...”
- Our interpretation: “The total local effect of marginal [electoral outcome] victories is...”

Obviously the treatment—the “electoral outcome” of interest—varies across applications: it could be an incumbent victory, the victory of some type of candidate (Marshall, 2022), a measure of party turnover in office, or some other treatment. The qualification of a “total” effect acknowledges that there are multiple mechanisms that feed into the effect of an electoral outcome on a given outcome. We emphasize the focus on “marginal victories” to underscore the effect of selection into marginal races that is inherent to this design.

How can we learn about the multiple mechanisms underpinning the LATE? First, we advocate that authors propose explicit model for how selection into marginal races and status-quo effects may enter the LATE provides two crucial benefits. First, it helps us to form theoretical expectations about the LATE. Even if the partial equilibrium effect of an electoral outcome should be *positive*

---

<sup>12</sup>We thank Thomas Robinson for inspiring this language.

	Outcome 1: Bureaucratic retention	Outcome 2: Bureaucratic quality
Untreated (potential) outcomes are...	$\downarrow$ in $q^*$	$\downarrow$ in $q^*$
Treated (potential) outcomes are...	$\uparrow$ in $q^*$	$\downarrow$ in $q^*$

Table 2: Additional testable implications of the model.

(for example), clear specification of the indirect (equilibrium) effects can reveal whether these effects should increase or attenuate these partial equilibrium effects.

Second, most models provide more predictions than simply the LATE. For example, Figure 3 provides additional testable implications with respect to the behavior to the slope the conditional expectation function (CEFs) on each side of the cutoff, as described in Table 2. These CEFs are already estimated in the process of estimating the LATE, but researchers do not generally report their slope. However, these slopes are informative about both general equilibrium effects and the mechanisms at play under the specified model. While confounding is an obvious concern, we contend that reporting these slopes/comparative statics in addition to LATE estimates can be used to support the interpretation of the LATE.

### 5.3 What are the alternatives for measuring general equilibrium effects?

The use of LATEs from close races to measure general equilibrium effects has several disadvantages. First, there is generally not a way to decompose the effects of the equilibrium effects that we identify from more standard partial equilibrium effects described in the existing literature. In other words, the analysis in Figure 4 is only possible because the data is simulated. Second, we cannot generally isolate the dynamic channels even indirectly. It might be possible to measure a pre-treatment covariate measuring anticipatory pre-electoral actions,  $a_1$ . However, a comparison of LATEs among the subgroup where  $a_1 = 1$  to those where  $a_1 = 0$  can be misleading if different races are marginal when  $a_1 = 1$  than when  $a_1 = 0$ . Finally, in practice, electoral RDs tend to produce noisy estimates (Stommes, Aronow, and Sävje, 2021). Indirect methods to try to decompose LATEs across subgroups are very likely to be underpowered in most contexts given an upper bound on the number of elections.

Is there a different way to measure the different channels that feed into equilibrium effects of elections? The promise of other design-based approaches is more limited. Our experimental benchmarks are hypothetical precisely because it is hard or unethical to randomize election outcomes. One alternative is to adopt a more structural approach. Here, the idea is to posit a theoretical model and then derive an estimator to estimate its structural parameters. These parameter estimates permit researchers to measure the effects of different mechanisms and to conduct counterfactual simulations. Structural work in political science and political economy to date has focused less on the effect of election outcomes. However, Diermeier, Keane, and Merlo (2005) provide a related exercise when they estimate the (monetary) returns to congressional office.<sup>13</sup> Of course, the cost of this structural framework is its heavy reliance on assumptions. While these assumptions arguably facilitate learning from data, they contrast directly with the impulses that motivate the use of electoral RDs.

## 6 Conclusions

We argue that the causal effects estimated by electoral RD designs incorporate legacies of forward-looking behavior before the election that determines treatment. In particular, we identify selection into close races and “status quo” effects as mechanisms whose effects are incorporated into LATEs from electoral RDs. These channels suggest that electoral RDs should typically be viewed as general, rather than partial equilibrium estimands. While these observations do not necessarily portend problems for identification, they change the interpretation of existing and future electoral RD results.

By distinguishing between partial and general equilibrium interpretations of the LATE, we call for more explicit argumentation to justify the interpretation of measured effects. These arguments, and ideally theoretical models, are crucial to understanding when estimated LATEs allow us to assess theories and arguments empirically.

---

<sup>13</sup>As we have discussed earlier, Eggers and Hainmueller (2009) employ a regression discontinuity design to estimate the value of office in the British Parliament.

## References

- Abramson, Scott F., Korhan Koçak, and Asya Magazinnik. 2021. “What Do We Learn about Voter Preferences from Conjoint Experiments?” Working paper available at [https://www.dropbox.com/s/kracengw1mvz5my/Conjoint\\_AJPS\\_Submission%20%281%29.pdf?dl=0](https://www.dropbox.com/s/kracengw1mvz5my/Conjoint_AJPS_Submission%20%281%29.pdf?dl=0).
- Akhtari, Mitra, Diana Moreira, and Laura Trucco. 2022. “Political Turnover, Bureaucratic Turnover, and the Quality of Public Services.” *American Economic Review* 112 (2): 442–493.
- Ariga, Kenichi. 2015. “Incumbency disadvantage under electoral rules with intraparty competition: evidence from Japan.” *The Journal of Politics* 77 (3): 874–887.
- Banerjee, Abhijit, and Esther Duflo. 2009. “The Experimental Approach to Development Economics.” *Annual Review of Economics* 1: 151–178.
- Boas, Taylor C, and F Daniel Hidalgo. 2011. “Controlling the airwaves: Incumbency advantage and community radio in Brazil.” *American Journal of Political Science* 55 (4): 869–885.
- Boas, Taylor C., F. Daniel Hidalgo, and Marcus André Melo. 2019. “Norms versus Action: Why Voters Fail to Sanction Malfeasance in Brazil.” *American Journal of Political Science* 63 (2): 385–400.
- Boas, Taylor C, F Daniel Hidalgo, and Neal P Richardson. 2014. “The spoils of victory: campaign donations and government contracts in Brazil.” *The Journal of Politics* 76 (2): 415–429.
- Bohlken, Anjali Thomas. 2018. “Targeting ordinary voters or political elites? Why pork is distributed along partisan lines in India.” *American Journal of Political Science* 62 (4): 796–812.
- Brollo, Fernanda, Pedro Forquesato, and Juan Carlos Gozzi. 2017. “To the Victor Belongs the Spoils? Party Membership and Public Sector Employment in Brazil.” Available at <https://ideas.repec.org/p/wrk/warwec/1144.html>.
- Brollo, Fernanda, and Tommaso Nannicini. 2012. “Tying your enemy’s hands in close races: the politics of federal transfers in Brazil.” *American Political Science Review* 106 (4): 742–761.
- Broockman, David E, and Timothy J Ryan. 2016. “Preaching to the choir: Americans prefer communicating to copartisan elected officials.” *American Journal of Political Science* 60 (4): 1093–1107.
- Bueno de Mesquita, Ethan, and Scott A. Tyson. 2020. “The Commensurability Problem: Conceptual Difficulties in Estimating the Effect of Behavior on Behavior.” *American Political Science Review* 2 (375-391).
- Carson, Jamie L, and Joel Sievert. 2017. “Congressional candidates in the era of party ballots.” *The Journal of Politics* 79 (2): 534–545.
- Casey, Katherine, Abou Bakarr Kamara, and Niccoló Meriggi. 2021. “An Experiment in Candidate Selection.” *American Economic Review* Forthcoming.

- Cattaneo, Matias D., Luke Keele, Rocío Titiunik, and Gonzalo Vazquez-Bare. 2020. “Extrapolating Treatment Effects in Multi-Cutoff Regression Discontinuity Designs.” *Journal of the American Statistical Association* 116 (536): 1941–1952.
- Cattaneo, Matias D., Michael Jansson, and Xinwei Ma. 2020. “Simple Local Polynomial Density Estimators.” *Journal of the American Statistical Association* 115 (531): 1449–1455.
- Cattaneo, Matias D., and Rocio Titiunik. 2022. “Regression Discontinuity Designs.”. Available at <https://arxiv.org/abs/2108.09400>.
- Caughey, Devin, Yiqing Xu, and Christopher Warshaw. 2017. “Incremental democracy: The policy effects of partisan control of state government.” *The Journal of Politics* 79 (4): 1342–1358.
- Cavaille, Charlotte, and John Marshall. 2019. “Education and anti-immigration attitudes: Evidence from compulsory schooling reforms across Western Europe.” *American Political Science Review* 113 (1): 254–263.
- Clinton, Joshua D, and Michael W Sances. 2018. “The politics of policy: The initial mass political effects of medicaid expansion in the states.” *American Political Science Review* 112 (1): 167–185.
- Colonnelli, Emanuele, Mounu Prem, and Edoardo Teso. 2020. “Patronage and Selection in Public Sector Organizations.” *American Economic Review* 110 (10): 3071–3099.
- Coppock, Alexander, and Donald P Green. 2016. “Is voting habit forming? New evidence from experiments and regression discontinuities.” *American Journal of Political Science* 60 (4): 1044–1062.
- Dahlgaard, Jens Olav. 2018. “Trickle-up political socialization: The causal effect on turnout of parenting a newly enfranchised voter.” *American Political Science Review* 112 (3): 698–705.
- de Benedictis-Kessner, Justin. 2018. “Off-cycle and out of office: Election timing and the incumbency advantage.” *The Journal of Politics* 80 (1): 119–132.
- de Benedictis-Kessner, Justin, and Christopher Warshaw. 2016. “Mayoral partisanship and municipal fiscal policy.” *The Journal of Politics* 78 (4): 1124–1138.
- de la Cuesta, Brandon, and Kosuke Imai. 2016. “Misunderstandings About the Regression Discontinuity Design in the Study of Close Elections.” *Annual Review of Political Science* 19 (375-396).
- Diermeier, Daniel, Michael Keane, and Antonio Merlo. 2005. “A Political Economy Model of Congressional Careers.” *American Economic Review* 95 (1): 347–373.
- Eggers, Andrew C. 2017. “Quality-Based Explanations of Incumbency Effects.” *Journal of Politics* 79 (4): 1315–1328.
- Eggers, Andrew C, and Arthur Spirling. 2017. “Incumbency effects and the strength of party preferences: Evidence from multiparty elections in the United Kingdom.” *The Journal of Politics* 79 (3): 903–920.



- Eggers, Andrew C, and Jens Hainmueller. 2009. "MPs for sale? Returns to office in postwar British politics." *American Political Science Review* 103 (4): 513–533.
- Ferwerda, Jeremy, and Nicholas L Miller. 2014. "Political devolution and resistance to foreign rule: A natural experiment." *American Political Science Review* 108 (3): 642–660.
- Fiva, Jon H, and Daniel M Smith. 2018. "Political dynasties and the incumbency advantage in party-centered environments." *American Political Science Review* 112 (3): 706–712.
- Folke, Olle, and James M Snyder. 2012. "Gubernatorial midterm slumps." *American Journal of Political Science* 56 (4): 931–948.
- Fourinaies, Alexander, and Andrew B Hall. 2014. "The financial incumbency advantage: Causes and consequences." *The Journal of Politics* 76 (3): 711–724.
- Frey, Anderson, and Rogerio Santarrosa. 2022. "The Politicization of Bureaucracy: Evidence from Brazil." Working paper.
- Galasso, Vincenzo, and Tommaso Nannicini. 2011. "Competing on good politicians." *American political science review* 105 (1): 79–99.
- Gerber, Elisabeth R, and Daniel J Hopkins. 2011. "When mayors matter: estimating the impact of mayoral partisanship on city policy." *American Journal of Political Science* 55 (2): 326–339.
- Green, Donald P., Terence Y. Leong, Holger L. Kern, Alan S. Gerber, and Christopher W. Larimer. 2009. "Testing the Accuracy of Regression Discontinuity Analysis Using Experimental Benchmarks." *Political Analysis* 17: 400–417.
- Grimmer, Justin, Eitan Hersh, Brian Feinstein, and Danel Carpenter. 2011. "Are Close Elections Random?" Available at <https://www.dropbox.com/s/e92fezcldgfvvl2k/cef2.pdf?dl=0>.
- Gulzar, Saad, and Benjamin J Pasquale. 2017. "Politicians, bureaucrats, and development: Evidence from India." *American Political Science Review* 111 (1): 162–183.
- Gulzar, Saad, and Muhammad Yasir Khan. 2018. "Motivating Political Candidacy and Performance: Experimental Evidence from Pakistan." Working paper.
- Hahn, Jinyoong, Petra Todd, and Wilbert Van der Klaauw. 2001. "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica* 69 (1): 201–209.
- Hainmueller, Jens, Dominik Hangartner, and Giuseppe Pietrantuono. 2017. "Catalyst or crown: Does naturalization promote the long-term social integration of immigrants?" *American Political Science Review* 111 (2): 256–276.
- Hall, Andrew B. 2015. "What happens when extremists win primaries?" *American Political Science Review* 109 (1): 18–42.

- Hall, Andrew B, and Daniel M Thompson. 2018. "Who punishes extremist nominees? Candidate ideology and turning out the base in US elections." *American Political Science Review* 112 (3): 509–524.
- Hidalgo, F Daniel, and Simeon Nichter. 2016. "Voter buying: Shaping the electorate through clientelism." *American Journal of Political Science* 60 (2): 436–455.
- Hofstetter, Selina, and Greg Chih-Hsin Sheen. 2011. "Partisan Incumbency Disadvantage: An Empirical Strategy to Exclude an Alternative Explanation." Available at [https://www.dropbox.com/s/49yv42vtyuin12d/pid\\_bjps\\_sep2021.pdf?dl=0](https://www.dropbox.com/s/49yv42vtyuin12d/pid_bjps_sep2021.pdf?dl=0).
- Holbein, John. 2016. "Left behind? Citizen responsiveness to government performance information." *American Political Science Review* 110 (2): 353–368.
- Holbein, John B, and D Sunshine Hillygus. 2016. "Making young voters: The impact of preregistration on youth turnout." *American Journal of Political Science* 60 (2): 364–382.
- Imbens, Guido W, and Thomas Lemieux. 2008. "Regression discontinuity designs: A guide to practice." *Journal of Econometrics* 142 (2): 615–635.
- Klašnja, Marko. 2015. "Corruption and the incumbency disadvantage: Theory and evidence." *The Journal of Politics* 77 (4): 928–942.
- Klašnja, Marko, and Rocío Titunik. 2017. "The incumbency curse: Weak parties, term limits, and unfulfilled accountability." *American Political Science Review* 111 (1): 129–148.
- Larreguy, Horacio, John Marshall, and Pablo Querubin. 2016. "Parties, brokers, and voter mobilization: How turnout buying depends upon the party's capacity to monitor brokers." *American Political Science Review* 110 (1): 160–179.
- Lee, David S. 2008. "Randomized experiments from non-random selection in U.S. House elections." *Journal of Econometrics* 142: 675–697.
- Lopes da Fonseca, Mariana. 2017. "Identifying the source of incumbency advantage through a constitutional reform." *American Journal of Political Science* 61 (3): 657–670.
- Marshall, John. 2022. "Can Close Election Regression Discontinuity Designs Identify Effects of Winning Politician Characteristics?" *American Journal of Political Science* Forthcoming.
- Mas-Colell, Andreu, Michael D. Whinston, and Jerry R. Green. 1995. *Microeconomic Theory*. New York, NY: Oxford University Press.
- McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics* 142 (2): 698–714.
- Mo, Cecilia Hyunjung, and Katharine M Conn. 2018. "When do the advantaged see the disadvantages of others? A quasi-experimental study of national service." *American Political Science Review* 112 (4): 721–741.

- Ofosu, George Kwaku. 2019. "Do Fairer Elections Increase the Responsiveness of Politicians?" *American Political Science Review* First View: 1–17.
- Palmer, Maxwell, and Benjamin Schneer. 2016. "Capitol gains: the returns to elected office from corporate board directorships." *The Journal of Politics* 78 (1): 181–196.
- Prato, Carlo, and Stephane Wolton. 2019. "Electoral Imbalances and their Consequences." *Journal of Politics* First View: 1–15.
- Rozenas, Arturas, Sebastian Schutte, and Yuri Zhukov. 2017. "The political legacy of violence: The long-term impact of Stalin's repression in Ukraine." *The Journal of Politics* 79 (4): 1147–1161.
- Schickler, Eric, Kathryn Pearson, and Brian D Feinstein. 2010. "Congressional parties and civil rights politics from 1933 to 1972." *The Journal of Politics* 72 (3): 672–689.
- Slough, Tara. 2022. "The Ethics of Electoral Experiments: Design-Based Recommendations." Available at <https://taraslough.com/assets/pdf/eee.pdf>.
- Slough, Tara, and Scott A. Tyson. 2022. "External Validity and Meta-Analysis." *American Journal of Political Science* Forthcoming.
- Snyder Jr, James M. 1990. "Campaign contributions as investments: The US House of Representatives, 1980-1986." *Journal of Political Economy* 98 (6): 1195–1227.
- Stommes, Drew, P. M. Aronow, and Frederik Sävje. 2021. "On the reliability of published findings using the regression discontinuity design in political science." Available at <https://arxiv.org/abs/2109.14526>.
- Szakonyi, David. 2018. "Businesspeople in elected office: Identifying private benefits from firm-level returns." *American Political Science Review* 112 (2): 322–338.
- Toral, Guillermo. 2022a. "How patronage delivers: Political appointments, bureaucratic accountability, and service delivery in Brazil." Working paper available at [https://www.guillermotoral.com/benefits\\_of\\_patronage.pdf](https://www.guillermotoral.com/benefits_of_patronage.pdf).
- Toral, Guillermo. 2022b. "Turnover: How lame-duck governments disrupt the bureaucracy and service delivery before leaving office." Available at <https://www.guillermotoral.com/turnover.pdf>.
- Weitz-Shapiro, Rebecca, and Matthew S. Winters. 2016. "Can Citizens Discern? Information Credibility, Political Sophistication, and the Punishment of Corruption in Brazil." *Journal of Politics* 79 (1): 60–74.
- Xu, Yiqing, and Yang Yao. 2015. "Informal institutions, collective action, and public investment in rural China." *American Political Science Review* 109 (2): 371–391.

## A Proofs

### Proof of Proposition 1

*Proof.* Rewrite  $\sigma(\lambda) = \frac{1}{B} \left( \frac{\pi}{\pi + (1-\pi)\lambda} - \pi \right)$  as:

$$\lambda = \left( \frac{1}{\pi + \sigma B} - 1 \right) \frac{\pi}{1 - \pi}.$$

We show that there exist a unique  $\sigma^*$  and  $\lambda^*$  such that  $\lambda = \left( \frac{1}{\pi + \sigma B} - 1 \right) \frac{\pi}{1 - \pi}$  and  $\lambda = (q + \alpha + (1 - q - \alpha))\sigma \left( \frac{1}{2} + \frac{w}{R} \right)$ . It is equivalent to show that there exists a unique  $\sigma^* \in [0, 1]$  such that  $f(\sigma^*) = 0$ , where  $f(\sigma) \equiv (q + \alpha + (1 - q - \alpha))\left(\frac{1}{2} + \frac{w}{R}\right) - \left(\frac{1}{\pi + \sigma B} - 1\right)\frac{\pi}{1 - \pi}$ .  $f(\sigma)$  is continuous and increasing in  $\sigma$ .  $f(0) = (q + \alpha)\left(\frac{1}{2} + \frac{w}{R}\right) - 1 < 0$  and  $f(1) = \frac{1}{2} + \frac{w}{R} + \frac{\pi^*}{1 - \pi^*} > 0$ . Therefore, there exists a unique  $\sigma^* \in [0, 1]$  such that  $f(\sigma^*) = 0$ .  $\square$

### Proof of Proposition 2

*Proof.* The proof is based on the potential outcomes presented in Table 2. Because  $\sigma^*(q, \alpha) < 1$ , it is obvious that if  $q + \alpha < 1$ , the first-period bureaucrat is retained at a higher probability when the incumbent is re-elected. Notice that  $\frac{2R\pi}{R(1+\pi)+2w(1-\pi)} < 1$ . If  $q + \alpha > 1$ , the retention rate conditional on the re-election of the incumbent is clearly higher. Because  $\sigma^*(q, \alpha) < 1$ , it is obvious that if  $q + \alpha < 1$ , the bureaucratic quality is higher when the incumbent stays in office. Notice that  $(\pi - \pi^2)(1 - \frac{1}{2} - \frac{w}{R}) > 0$ . It is clear that if  $q + \alpha \geq 1$ , the bureaucratic quality is higher when the incumbent stays in office.  $\square$

Outcome	$E[\text{Outcome}   P_A \text{ re-elected}]$	$E[\text{Outcome}   P_B \text{ elected}]$
<b>Bureaucratic retention</b>		
Case #1: $q + \alpha < 1$	$\pi + (1 - \pi)\lambda^*(q, \alpha)$	$(\pi + (1 - \pi)\lambda^*(q, \alpha))\sigma^*(q, \alpha)$
Case #2: $q + \alpha \geq 1$	$\pi + (1 - \pi)\left(\frac{1}{2} + \frac{w}{R}\right)$	$(\pi + (1 - \pi)\left(\frac{1}{2} + \frac{w}{R}\right))\left(\frac{2R\pi}{R(1+\pi)+2w(1-\pi)}\right)$
<b>Bureaucratic quality</b>		
Case #1: $q + \alpha < 1$	$\pi + (\pi - \pi^2)(1 - \lambda^*(q, \alpha))$	$\pi + (\pi - \pi^2)\sigma^*(q, \alpha)(1 - \lambda^*(q, \alpha))$
Case #2: $q + \alpha \geq 1$	$\pi + (\pi - \pi^2)\left(1 - \frac{1}{2} - \frac{w}{R}\right)$	$\pi$

Table A1: Potential outcomes implied by the equilibrium behavior characterized in Proposition 1.

## B Simulation of model

When considering electoral outcomes in election  $t = 1$ , electoral RDs use the margin of victory to measure the running variable. However, this is different than the *probability of victory* that actors consider when determining their first-period behavior. We treat the margin of victory of a realization of a random variable (following (5)). Roughly consistent with the distribution of margin of victory in 5 cycles of Brazilian elections, we will assume that the margin of victory in each race,  $m_i$ , is drawn from a normal distribution with mean  $\mu_i$  and variance  $v^2$ .

$$m_i \sim \mathcal{N}(\mu_i, v^2) \quad (9)$$

Call this race-specific density  $f_i(\cdot)$ . We estimate the variance from the Brazilian data and set  $v = 0.228$ . We know that the incumbent wins when  $m_i > 0$ . As such, the probability of victory is  $q_i = 1 - F_i(0)$ . We can then estimate the distribution of  $q_i$  that generated the observed data.

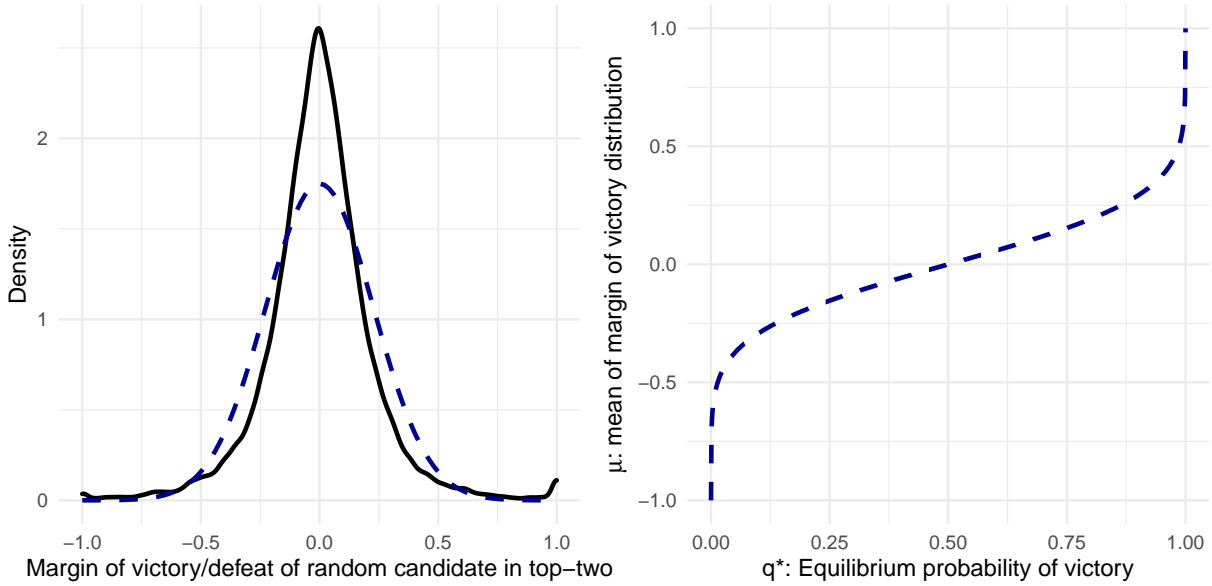


Figure A1: Left: Comparison of normal distribution with mean zero and  $v = 0.228$  (dashed) and the empirical distribution of margin of victory in five cycles of Brazilian mayoral elections (solid). Right: The mapping between  $q_i$  and  $\mu_i$  under (9).

The simulation proceeds as follows:

1. Parametric assumptions:  $\alpha = 0.1$ ,  $\pi = 0.5$ ,  $R = 1$ ,  $w = 0.25$ , and  $B = 1$ .
2. Draw a sample of  $n = 5,565$  from the distribution of  $q_i$  implied by the five cycles of Brazilian electoral data. We assume that these represent an equilibrium probabilities of victory. However these probabilities of victory are endogenous to first-period actions. As such we construct  $q_0 = q_i - 0.07$ . We will denote the  $n$ -element vector of  $q_0$  as  $\mathbf{q}$ .
3. We repeat the following steps  $K = 2000$  times:

- (a) Draw an  $n$ -element vector of first-period types:

$$\boldsymbol{\theta}_1 \sim \text{Bernoulli}(\pi).$$

- (b) Draw an  $n$ -element vector of first-period actions consistent with the equilibrium implications of the model. Note that the vector of first period actions is indexed by observational, **b1** (Benchmark #1), or **b2** (Benchmark #2):

$$\mathbf{e}_1^{o,b1} = \begin{cases} 1 & \text{if } \theta_1 = 1 \\ \text{Bernoulli}(\mathbf{q} + \alpha + (1 - \mathbf{q} - \alpha)\sigma^*(\frac{1}{2} + \frac{w}{R})) & \text{if } \theta_1 = 0 \text{ and } q_i + \alpha < 1 \\ \text{Bernoulli}(\frac{1}{2} + \frac{w}{R}) & \text{if } \theta_1 = 0 \text{ and } q_i + \alpha \geq 1 \end{cases}$$

$$\mathbf{e}_1^{b2} = \begin{cases} 1 & \text{if } \theta_1 = 1 \\ \text{Bernoulli}(\frac{1}{2} + \frac{w}{R}) & \text{if } \theta_1 = 0 \end{cases}$$

- (c) Draw an  $n$ -element vector of margin of victory,  $\mathbf{m}$ , and define associated  $n$ -element treatment indicators,  $\mathbf{Z}$

$$\begin{aligned} \mathbf{m}^o &= \mathcal{N}(\mathbf{q} + \alpha \mathbf{e}_1^{o,b1}, v^2) & \mathbf{Z}^o &= \mathbb{I}(\mathbf{m}^o \geq 0) \\ \mathbf{m}^{b1,b2} &= \mathcal{N}(0, v^2) & \mathbf{Z}^{b1,b2} &= \mathbb{I}(\mathbf{m}^{b1,b2} \geq 0) \end{aligned}$$

- (d) Draw an  $n$ -element vector,  $\mathbf{r}_2$ , to capture the second-period incumbent's retention decision. Note that the equilibrium  $\sigma^*$  is different in benchmark 2 than from  $\sigma^*$  in the observational data and benchmark 1.

$$\begin{aligned} \mathbf{r}_2^o &= \mathbf{Z}^o \mathbf{e}_1^{o,b1} + (1 - \mathbf{Z}^o) \text{Bernoulli}(\sigma^*) \mathbf{e}_1^{o,b1} \\ \mathbf{r}_2^{b1} &= \mathbf{Z}^{b1,b2} \mathbf{e}_1^{o,b1} + (1 - \mathbf{Z}^{b1,b2}) \text{Bernoulli}(\sigma^*) \mathbf{e}_1^{o,b1} \\ \mathbf{r}_2^{b2} &= \mathbf{Z}^{b1,b2} \mathbf{e}_1^{b2} + (1 - \mathbf{Z}^{b1,b2}) \text{Bernoulli}(\sigma^*) \mathbf{e}_1^{b2} \end{aligned}$$

- (e) Draw an  $n$ -element vector,  $\mathbf{c}_2$  to capture the distribution of high-types in the second-period bureaucracy.

$$\begin{aligned} \mathbf{c}_2^o &= \mathbf{r}_2^o \boldsymbol{\theta}_1 + (1 - \mathbf{r}_2^o) \text{Bernoulli}(\pi) \\ \mathbf{c}_2^{b1} &= \mathbf{r}_2^{b1} \boldsymbol{\theta}_1 + (1 - \mathbf{r}_2^{b1}) \text{Bernoulli}(\pi) \\ \mathbf{c}_2^{b2} &= \mathbf{r}_2^{b2} \boldsymbol{\theta}_1 + (1 - \mathbf{r}_2^{b2}) \text{Bernoulli}(\pi) \end{aligned}$$

- (f) Estimate and record difference-in-means estimates  $\beta_y^j$  as well as regression discontinu-

ity estimates  $\tau_y^j$  for each  $y^j \in \{\mathbf{r}^j, \mathbf{c}^j\}$  and  $j \in \{o, b1, b2\}$ .

$$y^j = \kappa + \beta^j \mathbf{Z}^j$$

$$\tau_j = \lim_{m^j \downarrow 0} E[\widehat{y}^j | m^j = 0] - \lim_{m^j \uparrow 0} E[\widehat{y}^j | m^j = 0].$$

## C Classification of published regression discontinuity designs in political science

In Tables A2-A3, we divide the RDs surveyed by Stommes, Aronow, and Sävje (2021) into those that do not use electoral results to construct the running variable (Table A2) and those that do use electoral results to construct the running variable (Table A3).

Author(s) Year	Cutoff or Boundary Types
Cavaille and Marshall (2019)	Age
Coppock and Green (2016)	Age
Dahlgaard (2018)	Age
Holbein and Hillygus (2016)	Age
Hidalgo and Nichter (2016)	Population or percentage of a specific category
Larreguy, Marshall, and Querubin (2016)	Population or percentage of a specific category
Holbein (2016)	Performance measures
Mo and Conn (2018)	Performance measures
Clinton and Sances (2018)	Geographic or administrative boundaries
Ferwerda and Miller (2014)	Geographic or administrative boundaries
Gulzar and Pasquale (2017)	Geographic or administrative boundaries
Rozenas, Schutte, and Zhukov (2017)	Geographic or administrative boundaries

Table A2: Non-electoral RDs in political science journals (*AJPS*, *AJPS*, and *JoP*). Our sample comes from Stommes, Aronow, and Sävje (2021).



Author(s) Year	Effects of Interest
Ariga (2015)	Incumbency effects
Boas and Hidalgo (2011)	Incumbency effects
de Benedictis-Kessner (2018)	Incumbency effects
Eggers and Spirling (2017)	Incumbency effects
Fiva and Smith (2018)	Incumbency effects
Fouirnaies and Hall (2014)	Incumbency effects
Klašnja (2015)	Incumbency effects
Klašnja and Titunik (2017)	Incumbency effects
Lopes da Fonseca (2017)	Incumbency effects
Bohlken (2018)	Partisan alignment effects (between politicians)
Brollo and Nannicini (2012)	Partisan alignment effects (between politicians)
Carson and Sievert (2017)	Partisan alignment effects (between politicians)
Folke and Snyder (2012)	Partisan alignment effects (between politicians)
Broockman and Ryan (2016)	Partisan alignment effects (between citizens and politicians)
Caughey, Xu, and Warshaw (2017)	Effects of partisan control
de Benedictis-Kessner and Warshaw (2016)	Effects of partisan control
Galasso and Nannicini (2011)	Effects of partisan control
Gerber and Hopkins (2011)	Effects of partisan control
Schickler, Pearson, and Feinstein (2010)	Effects of partisan control
Boas, Hidalgo, and Richardson (2014)	Value of office
Eggers and Hainmueller (2009)	Value of office
Palmer and Schneer (2016)	Value of office
Szakonyi (2018)	Value of office
Hall (2015)	Effects of ideology of primary election winner
Hall and Thompson (2018)	Effects of ideology of primary election winner
Hainmueller, Hangartner, and Pietrantuono (2017)	Effects of naturalization
Xu and Yao (2015)	Effects of leader's connection to family clans

Table A3: Electoral RDs in three political science journals (*AJPS*, *AJPS*, and *JoP*). Our sample comes from Stommes, Aronow, and Sävje (2021).

## D Elaboration of framework application

In this section, we elaborate the logic of the arguments in Table 1.

### D.1 Incumbency effects

Fourirnaies and Hall (2014) use a regression discontinuity design in U.S. House and state legislative elections to estimate the incumbency advantage on campaign contributions. They discuss three possible mechanisms behind the financial incumbency advantage. An access-motivated interest group has more incentive to invest in incumbents' campaigns. Office comes with a direct benefit that attracts contributors. Incumbents gain experience in fundraising and build a valuable connection to potential contributors. They find supportive evidence for the theory of interest group access. Building on the insight of the theory of interest group access, we suggest that dynamic concerns exist in the RD model in the context of Fourirnaies and Hall (2014).

Consider an interest group and an incumbent. Applying our framework, we consider two-period  $t = 0$  and  $t = 1$  where RD design is based on the election at  $t = 1$ . The interest group decides on the amount of campaign donation to the incumbent at  $t = 0$  (corresponding to  $a_0$  in our framework). The more likely the incumbent stays in office, the higher return to the investment for the interest group. In other words, the anticipated prospect of the incumbent's re-election,  $q_0$ , affects the donation decision of the interest group,  $a_0$ . The campaign donation helps the incumbent to throw a successful campaign and improves her chance of being re-elected. Campaign donations are effective in influencing election outcomes. As we have discussed in our general framework, this dynamic affects the sample's composition within the close bandwidth from which the LATE is estimated.

Snyder Jr (1990) points out, "contributors must develop a relationship of mutual trust and respect with officeholders to receive tangible rewards for contributions." We conceptualize this process of trust-building between donors and officeholders as a process in which the officeholder updates their beliefs about whether donors are trustworthy. The interest group's period 0 donation serves as a signal of its trustworthiness. This information will affect the campaign contribution dynamics between the incumbent if she stays in office after the election at  $t = 1$  and, therefore, the donation amount at  $t = 1$  (corresponding to  $a_1$  in our framework). The first period's forward-looking behavior by the interest group shapes the status quo that conditions the second period's actions. Because of such a dynamic, RD LATE in this context includes general equilibrium effects.

### D.2 The effects of partisan alignment

Using a regression discontinuity design, Bohlken (2018) examines the causal effects of partisan alignment between the state and national legislators in India on the characteristics of development projects proposed under a scheme known as the Member of Parliament Local Area Development Scheme (MPLADS). The paper suggests a theory of elite cooperation to explain partisan favoritism. The key motive for national legislators to favor co-partisan state legislators is to win over the state legislators' cooperation in implementing development projects. The state legislator, in turn, benefits from her constituency's receiving allocations from the development. In particular, a state legislator could use the project to reward lower-level brokers, which would help her to build her political network. This type of elite cooperation is likely to happen among national and state legislators who share partisanship because of the likelihood of repeated interaction and the role of the party leader in facilitating the cooperation. The theory of elite cooperation, we argue, suggests dynamic concerns in the RD model in Bohlken (2018).

To apply our framework, we consider a two-period setup with two players – a national legislator and a state legislator from the same party. At  $t = 0$ , the national legislator decides on the amount of development funds (i.e.  $a_0$  in our framework) to allocate to the state legislator at  $t = 0$ . The development funds allow the state legislator to build a political network with lower-level brokers who would improve her chance of winning the state election at  $t = 1$ . Development funds allocated to the state legislator affect election outcomes. This is the type one dynamic that conditions the sample from which LATE is estimated.

Though not discussed in the paper, like many instances of cooperation, the elite cooperation between the national and the state legislators would involve much asymmetric information, including each party's trustworthiness and political capital. Period 0's development funds allocation serves as a signal of these traits. This information eventually affects the cooperation dynamics after the election at  $t = 1$  and the resulted amount of funds at  $t = 1$  (i.e.  $a_1$  in our framework). The national legislator's forward-looking action shapes the status quo and conditioned action in the next period. Due to this dynamic, RD LATE in this context includes general equilibrium effects.

### **D.3 The value of office**

Eggers and Hainmueller (2009) employ a regression discontinuity design to estimate the effect of serving in Parliament on the wealth of British politicians. They argue that the primary mechanism behind the wealth effects of political office is that office provides Members of Parliament (MPs) the political connections and knowledge that is valuable to firms. As a result, firms hire them as directors and consultants. They find that significant financial benefits are only among the Conservative MPs. They attribute this differential in office benefit to the differences in how the parties were financed and organized. While a handful of trade unions funded the Labor Party, the Conservative Party gathered its financial support from diffuse contributors. Compared to Labor MPs, Conservative MPs have more autonomy to forge relationships with various firms that compete for their legislative services. Building on the idea of legislative service, we develop a framework that demonstrates dynamic concerns in Eggers and Hainmueller (2009)'s RD setup.

Consider a two-period environment with two players: a firm and a politician. At  $t = 0$ , the politician decides the amount of legislative service to the firm. Though not discussed in Eggers and Hainmueller (2009), there should be implications of the legislative services to the firm on the politician's electoral prospect at  $t = 1$ . On the one hand, the literature on legislative services would suggest that more services to firms might lead to fewer services to the constituency and therefore hurt the electoral prospect of the politicians. On the other hand, in exchange for legislative services, firms might provide donations which enhance politicians' electoral prospects. Both mechanisms suggest that the amount of legislative service to the firms impacts election outcomes. This type of dynamics conditions the sample based on which LATE is estimated.

Similar to the previous discussion on relationship building between donors and officeholders and cooperation between national and state legislators, asymmetric information is prevalent in the collaboration between politicians and firms. Period 0's legislative service to the firm provides valuable information which eventually affects the dynamics between the politician and the firm after the election at  $t = 1$  and thereby the amount of service offered by the politician at  $t = 1$  (i.e.  $a_1$  in our framework). In this environment, legislative service to the firm shapes the status quo and conditioned action in the next period. General equilibrium effects are thus included in RD LATE in this context.