

# The Re-running Margin: Incumbency and Turnover in African Parliaments, 1963–2024\*

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## Abstract

Using newly digitized election results and a name-matching algorithm that tracks individual candidates across elections, we provide the first systematic estimates of the effect of winning a legislative seat on *re-running* in the next election across 17 African countries and 86 elections (1963–2024). Getting back on the ballot is the first gate to re-election. Incumbent re-running rates average 47%, turnover averages around 70%, and barely winning raises the probability of re-running by a median of 21 percentage points and of winning the next election by only 9—far below the US and the UK, where re-running is near-universal, turnover is far lower, and incumbency effects on re-running and winning are far larger. We look at country-specific academic literature, news sources, cross-country comparisons of indexes of parliamentary power, and text analysis of Hansard parliamentary activity to study the mechanism through which re-running is determined. The results are mostly consistent with re-running suppression by the executive. Monitoring incumbent re-nomination rates may help evaluate elections before a single ballot is cast.

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# 1 Introduction

African parliaments are weak. The average Parliamentary Powers Index (PPI) score across our 17 African countries is 0.40—ranging from 0.25 in Seychelles to 0.53 in Lesotho—compared with 0.63 in the US and 0.78 in the UK (Fish and Kroenig, 2009). The 13 closed-list PR and non-partisan countries in our extended sample average 0.48, also well below these benchmarks. Legislatures lack the institutional power, budgetary authority, and political independence that characterize their counterparts in established democracies (Barkan, 2009a; Opalo, 2019). Without effective legislative oversight, executives govern with limited accountability and democratic consolidation stalls. High turnover compounds the problem: longer tenures are associated with a more institutionalized and professionalized parliament (Polsby, 1968; Mayhew, 1974; Cirone et al., 2021). Opalo (2019) shows that the short, interrupted legislative careers typical of Africa prevent the accumulation of the experience on which institutionalization depends.

Two recent explanations on why African parliaments are relatively weaker in relation to the executive compared to other established democracies have been put forward.<sup>1</sup>

The first is supply-side (i.e., it relates to the appointment of candidates): the executive suppresses re-running. Barkan (2009a), synthesizing case studies of six African legislatures (Benin, Ghana, Kenya, Nigeria, South Africa, and Uganda), documents that executives “seek to defeat reformers in the next election by denying them the party’s nomination for reelection, or by liberally financing their opponents” (p. 19). Opalo (2019) formalizes this account, arguing that “overdeveloped” executive branches give “chief executives strong incentives to engineer high turnover rates in legislative elections in order to prevent legislators from developing strong and permanent bases of political support” (pp. 4, 136); whether through bureaucratic monitoring, party control of nominations, or informal patronage, the executive controls who runs, and legislators who become “too popular” are sanctioned (Opalo, 2019, p. 216). The suppression operates through two

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<sup>1</sup>The study of African legislatures has been relatively neglected compared to executive politics. Barkan (2009a) is a notable exception; see also Mamdani (1996) on colonial legacies that created “overdeveloped” executive branches relative to legislatures, Bratton and van de Walle (1997) on neopatrimonial rule, and Lindberg (2006) on African elections more broadly.

distinct channels. The executive may control nominations directly, denying renomination to independent-minded incumbents; or it may leave the contest open but withhold the resources and patronage an incumbent would receive in more established democracies. Barkan and Matiangi (2009) contrast the two in Kenya: under Kenyatta, “parliamentary elections were intraparty contests within the ruling party KANU, but they were largely free and fair contests with as many as ten candidates vying for each seat,” whereas under Moi “voters were also encouraged to vote for candidates considered ‘loyal’ by the president,” and “where they did not, the outcome was often manipulated” (p. 37). In this interpretation African parliaments’ high turnover is not a byproduct of weak legislatures; it is a deliberately maintained feature of executive dominance.

A further indication of executive dominance comes from contrasting two levels of incumbency. Descamps et al. (2026) estimate the effect of a national electoral victory on the probability that winning parties and leaders retain power beyond their scheduled term, and find that this national incumbency advantage is largest in Africa: election winners are 42 percentage points more likely to hold national power past their term, almost tripling the retention probability of a close runner-up, with the effect concentrated in less democratic settings and operating partly through the manipulation of the fairness and timing of elections. The companion cross-country study (De Magalhães and Hirvonen, 2026) shows that the corresponding *parliamentary* upper-bound incumbency advantage for African legislatures averages roughly +0.08, with several countries negative. The same regimes that never surrender national power thus preside over legislatures whose individual members enjoy little or no electoral advantage — the two objects move in opposite directions precisely where executives are most entrenched. That asymmetry is difficult to reconcile with a purely demand-side account of legislative turnover, and is exactly what the supply-side account predicts: the executive’s grip on national power is maintained, in part, by keeping legislators expendable.

The second theory is demand-side (i.e., it relates to voters’ choices and preferences): a self-fulfilling equilibrium of voter pessimism. Bowles and Marx (2023) formalize a model in which “voters’ beliefs about politicians are self-fulfilling: *because* turnover is high, lower-

quality candidates are more likely to seek reelection, and incumbents extract more rents in expectation of a short term” (p. 1). By adverse selection, the precarious single-term career attracts candidates with few outside options, who, anticipating a short tenure, invest less in policy and more in short-term rent extraction, and voters rationally punish incumbents for under-delivering. What voters want from a legislator shapes this channel: in a conjoint experiment with Ghanaian citizens, Ofosu (2024) finds that respondents put more weight on constituency-related activities than on parliamentary work, and, within the constituency, value political representation above constituency service.

Both interpretations are claims about the overall likelihood of reelection. Here we separate the process in two steps. First, a candidate must get back on the ballot, i.e. win re-nomination. Second, the candidate must win the vote. In the US and the UK and other established democracies the first gate is open: incumbents re-run almost as a matter of course—about 87% in the US House (Lee, 2001), with reselection rates near 92% in Britain (Norris and Lovenduski, 1995) and seniority-based renomination in Norway (Cirone et al., 2021). In these settings the incumbency advantage is understandably measured, and conceptualized, at the second gate.

In the African setting, it is helpful to think in terms of the supply side (supply of incumbents for renomination) as well as the demand side (voters rewarding or punishing incumbents). What is observed of course is the supply-demand equilibrium. By simply observing the low re-running rate we cannot say whether the supply of incumbents is being curtailed by executive suppression (supply-side argument), or whether incumbents find it not to be worth standing because they foresee that voters will punish them (a demand-side argument). Both predict the same aggregate outcomes: low incumbency effects, high turnover, weak legislatures. We will show that in African legislatures the evidence indicates that the supply-side argument is dominant in most cases (but not all). Candidate selection is fundamental, which we read through the comparative selectorate framework of Rahat and Hazan (2001) and Hazan and Rahat (2010).

The focus on re-running suppression is an angle that the literature on African legislative development has left largely unexamined. Barkan (2009a, pp. 18–19) explains

institutional strength as the product of an internal “coalition for change”—a small core of reform-minded legislators, “as few as 5 to 10 percent of all MPs,” who push to build the committee system, the budget authority, and the professional staff on which a strong legislature depends. But a coalition for change cannot form or persist unless its members return to the chamber election after election: institution-building is the work of legislators who accumulate experience, seniority, and committee standing over several terms (Polsby, 1968; Cirone et al., 2021). Barkan (2009a) notes that reformers “lose their seats in the next election” (p. 19), yet treats their non-return as a side-effect of their attempt at reform rather than the central obstacle. We place it at the center. If re-running is held low, no matter the underlying reason, no reform coalition can establish itself and the legislature stays weak.

In this paper we make four contributions. First, we compile and digitize candidate-level election results and link individuals across elections with a fuzzy name-matching algorithm, considerably expanding—by digitizing election results previously unavailable in machine-readable form—the samples studied by Opalo (2019) and Bowles and Marx (2023). Second, we provide the first systematic estimates of the effect of winning a legislative seat on *re-running* in the next election across African legislatures using regression discontinuity design. The effect of incumbency on re-running and re-running rates are the margins on which African legislatures differ most sharply from established democracies; existing work focuses on the incumbency effects on winning (Opalo, 2017; Bowles and Marx, 2023). Third, we provide runner-up RDD estimates for African legislatures, replicating Anagol and Fujiwara (2016) in all 17 countries. The runner-up design is informative in its own right and also serves as a benchmark/placebo for our cross-country evidence (developed in Section 5). Fourth, we characterize how candidates are selected in each country, i.e., who holds the first gate (Table 2), using cross-country variation in legislative institutions, individual-level parliamentary activity, and country-case evidence. To measure individual activity we assemble a cross-country corpus of verbatim parliamentary debates (Hansard) for eight countries (Ghana, Kenya, Mauritius, Nigeria, Tanzania, Uganda, Zambia, and Zimbabwe), distinguishing floor speeches from formal questions to

government. We do find that activity is associated with an incumbent clearing the first gate, but the electoral return is small relative to the UK (developed in Section 6) and that oversight (questions) and visibility (speeches) carry different electoral returns.

This paper speaks to several literatures. It takes up the study of the relative weakness of African legislatures and its sources (Lindberg, 2006; Barkan, 2009a; Opalo, 2019; Bowles and Marx, 2023), asking whether that weakness originates with the executive. It departs from existing estimates of African incumbency effects—Opalo (2017), Sishuwa (2022), and De Magalhães and Hirvonen (2026), which measure the advantage conditional on re-running or the effect on winning—by asking the prior question of who re-runs at all. It extends the literature on runner-up and rank effects (Anagol and Fujiwara, 2016; Song, 2022) to African legislatures. It connects to the literature on detecting electoral manipulation (Ichino and Schündeln, 2012; van Ham and Lindberg, 2015; Asunka et al., 2019; Cantoni and Pons, 2021; Casey et al., 2021; Das, 2023): most of that work is timed at or after the vote, whereas we measure a pre-election margin—opposition exclusion, candidate disqualification, control of who is allowed to stand—from the certified candidate list, before a single ballot is cast. Finally, it builds on the text-as-data study of parliamentary debate (Grimmer and Stewart, 2013; Proksch and Slapin, 2015; Wahman et al., 2021; Frantzeskakis, 2023; Kronborg, 2025): we add countries and elections to the African Hansard record and separate parliamentary *questions* from speeches, which prior African work does not.

Determining what governs the first gate, i.e., whether or not incumbents re-run, has important policy implications. Where a gatekeeper is present, reforms that protect incumbents’ right and precedence to re-run and keep primary expenditures in check could matter, as the strong re-nomination norms of Norway, the US, and Britain illustrate (Norris and Lovenduski, 1995; Hirano and Snyder Jr., 2019; Cirone et al., 2021); where voter punishment binds at the second gate, candidate-selection reform will not move outcomes, and deeper changes such as allocating power by seniority and clear budget authority to parliament may be needed (Bowles and Marx, 2023).

Because re-running rates reflect the environment facing legislators, sharp deviations

from a country’s baseline can signal democratic backsliding or manipulation weeks or months before a single ballot is cast. We therefore propose measuring re-selection rates of incumbents as a pre-emptive electoral monitoring tool (Section 7): a simple rule of thumb that flags an election when incumbent re-running falls below the country’s own baseline, which we validate against V-Dem election-quality benchmarks.

The paper proceeds as follows. Section 2 describes the data; Section 3 presents the re-running, turnover, and RDD results; Section 4 classifies countries by which gate binds and reads the country-specific evidence on why the first gate binds; Section 5 turns to cross-country correlates of legislative institutions; Section 6 examines individual-level parliamentary activity; Section 7 develops the monitoring proposal; and Section 8 concludes.

## 2 Data and Institutional Context

### 2.1 Election Data

Our primary data source is the Constituency-Level Elections Archive (CLEA; Kollman et al., 2025), which compiles constituency-level legislative election results from national election commissions worldwide. We extract all available elections for 17 sub-Saharan African countries that use constituency-based electoral systems and supplement with the Election Passport (Lubin, 2023). For several countries, we go beyond these databases by digitizing constituency-level results from primary sources. For Kenya, we transcribe seven elections (1963–1992) from the *National Elections Data Book Kenya 1963–1997* and the 1997 election from Omeri (2014), whose candidate names we validate against the vote counts and winners already recorded in CLEA. For Zimbabwe we compile results for eight elections (1990–2023) from academic monographs, newspaper reports, and official election commission data, replacing the CLEA records which contain only surnames for some years. For Tanzania, we digitize constituency-level results for the 1965 and 1970 single-party elections from Cliffe (1967) and Proctor, ed (1974). CLEA only records candidate names for Tanzania in 2005, 2015, and 2020; we recover the 2010 constituency results from Adam Carr’s Election Archive (Carr, n.d.). For Nigeria, we supplement

CLEA with the 2007 legislative results from Adam Carr’s Election Archive (Carr, n.d.). For Botswana, we digitize the official Supervisor of Elections reports on the 1974, 1979, and 1984 general elections (Steenkamp, 1974, 1980; Mogae, 1985), which provide complete candidate names and vote counts for three elections that CLEA records only partially or not at all. Every election that we digitize from primary sources is manually reviewed in full by the authors after the initial OCR or vision-based extraction: Kenya 1963, 1969, 1974, 1979, 1983, 1988, 1992, and 1997; Tanzania 1965, 1970, and 2010; Zimbabwe 1990 and 1995; Botswana 1974, 1979, and 1984. See the Online Appendix for details on all sources by country.

In many African countries legislators frequently switch parties between elections, and parties merge or dissolve often.<sup>2</sup> We therefore link individual candidates across consecutive elections by name and estimate incumbency effects at the individual level, as in De Magalhães (2015).

To track individual candidates across elections, we develop a fuzzy name-matching algorithm tailored to the spelling, ordering, and transliteration variation characteristic of African candidate names. The algorithm scores each candidate pair on given-name and surname similarity and went through four iterations of refinement, each driven by a specific failure mode identified during validation against hand-coded ground-truth samples; at each iteration we manually checked samples of the resulting matches—including matches above the auto-acceptance threshold—and adjusted the component weights and thresholds until the checks converged. Ambiguous cases that fall below the auto-acceptance threshold are resolved through manual review by the authors: across the 17 countries, we manually reviewed 918 candidate pairs flagged as medium-confidence, confirming 481 and rejecting 437. This 52% confirm rate is the yield of the manual review, not the accuracy of the matching. Validated against hand-coded ground truth in Malawi, Ghana, and Zambia, no clear false matches were identified among the auto-

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<sup>2</sup>In Liberia 2017, “a remarkable 31 of 38 incumbents in the House of Representatives ran for different parties than those they had stood for in prior elections” (Pailey and Harris, 2020). Opalo (2019, p. 224) concludes for Kenya and Zambia that “party identification carries very little information beyond signaling the credibility of promises of patronage and clientelistic benefits.” Section 4 provides additional country-specific evidence.

accepted links during manual checks, and no true matches among the auto-rejected pairs, so measurement error from the matching in  $\text{run}_{t+1}$  and  $\text{win}_{t+1}$  is minimized. Full details of the matching procedure, the steps to improve the algorithm, validation statistics, and per-country review counts are reported in Online Appendix A.1.2 and Table A.3.

## 2.2 Sample

We use two samples. The *main sample* comprises 17 countries with individual candidate-level voting data for winners and runners-up, covering 113 elections from 1963 to 2024, of which 86 elections contribute estimable RDD transitions (the remainder are used only to assign treatment in the first cycle, to measure outcomes in the last), for a winner–runner-up estimation sample of 26,657 candidate-elections (Table 1); for these countries we estimate all four outcomes of interest: the re-running rate, the turnover rate, the effect of winning on re-running ( $\hat{\tau}_{\text{run}}$ ), and the effect of winning on winning the next election ( $\hat{\tau}_{\text{win}}$ ). The *turnover sample* comprises 13 closed-list PR or non-partisan countries—South Africa, Namibia, Mozambique, Rwanda, Senegal, Cape Verde, Angola, Benin, Burkina Faso, Niger, Togo, Djibouti, and Burundi—for which complete candidate lists including losing candidates are not available. For these countries we observe only the elected legislators across consecutive parliaments, and we estimate turnover only. The turnover sample covers 32 inter-parliament transitions and 4,784 incumbents from 1990 to 2024.

Several of the early country-years are one-party, single-party-dominant, or no-party elections (for example, Kenya before 1992, Uganda’s no-party Movement elections, and Tanzania’s single-party elections in 1965 and 1970). From 1995 onwards most of our countries have multiparty systems, and the cross-country institutional correlations in Section 5 are computed on post-1995 estimates. The digitized Tanzania 1965 and 1970 single-party elections are reported as a historical example in Table 1 and Figures 3–4, but—because they predate the multiparty era—are excluded from the country classification (Table 2) and from the cross-country analysis; Tanzania’s pooled RDD estimate uses the multiparty candidate panel (2005–2020).

Coverage varies across countries with data availability. Botswana has the longest panel

(11 elections, 1965–2019) because we supplement CLEA with three digitized Supervisor of Elections reports—Steenkamp (1974) for 1974, Steenkamp (1980) for 1979, and Mogae (1985) for 1984—that provide complete winner and runner-up names previously coded as missing in or absent from CLEA. Tanzania, Kenya, Zimbabwe, and Nigeria similarly extend back via the digitized sources listed above. For country-years where parts of the panel rely on Wikipedia or Election Passport (Botswana 1969, 1999, 2004; Côte d’Ivoire 2011), we cross-checked candidate names and vote counts against any available official election commission reports and against academic monographs covering the same elections. Country-years and elections excluded from the analysis sample, the precise sources used, and the verification procedure are documented in Online Appendix Section A.1.1 and Table A.2.

## 2.3 Key Variables

For each election in period  $t$  and constituency, we identify the winner and the runner-up. The *running variable* for the RDD is the vote margin between the winner and the runner-up as a share of the total vote in the constituency. We measure two outcomes in election  $t + 1$ :

- **Re-running** ( $\text{run}_{t+1}$ ): Whether the candidate runs for any constituency in the next election ( $= 1$ ) or does not appear as a candidate ( $= 0$ ).<sup>3</sup>
- **Winning** ( $\text{win}_{t+1}$ ): Whether the candidate wins a seat in any constituency in the next election ( $= 1$ ) or does not win ( $= 0$ , including candidates who do not re-run).

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<sup>3</sup>Across the 14,549 matched candidate pairs in our sample, 83% re-run in the same or fuzzy-renamed constituency, 9% move to a seat that already exists in the  $t+1$  map, and 8% face a constituency that has been redrawn or renumbered. The same proportions hold among near-losers ( $|\text{margin}| \leq 5\text{pp}$  at  $t$ ): 84%, 11%, and 5%. Tracking candidates across constituencies (rather than only within the same constituency) is necessary because of frequent boundary changes; the demand-side interpretation of our results requires that voters in  $t+1$  have observed the same legislator’s behaviour in  $t$ , an assumption that holds for the modal candidate but is weaker for the small share who switch constituencies. At the country level, most countries have at least 80% of matched candidates re-running in the same or renamed constituency; the exceptions are Sierra Leone (42%, from the 2017 renumbering of every constituency), Tanzania (62%), Kenya and Liberia (both 77%, from the 2010 Kenyan redistricting and post-conflict Liberian boundary changes), and Mauritius (69%, an artifact of slate reassignment under the three-member block vote rather than of boundary change).

This is the individual unconditional effect; candidates who do not re-run are coded as not winning.

We measure the effect of an election in  $t$  on outcomes in  $t + 1$ . For each electoral cycle  $t$  to  $t + 1$ , we display the estimated effect and label it under the year  $t + 1$ . We also estimate the upper-bound incumbency advantage following De Magalhães and Hirvonen (2026): the unconditional estimate rescaled by the rate of re-running, partialling out random attrition. Details in Section 3.

## 2.4 Summary Statistics

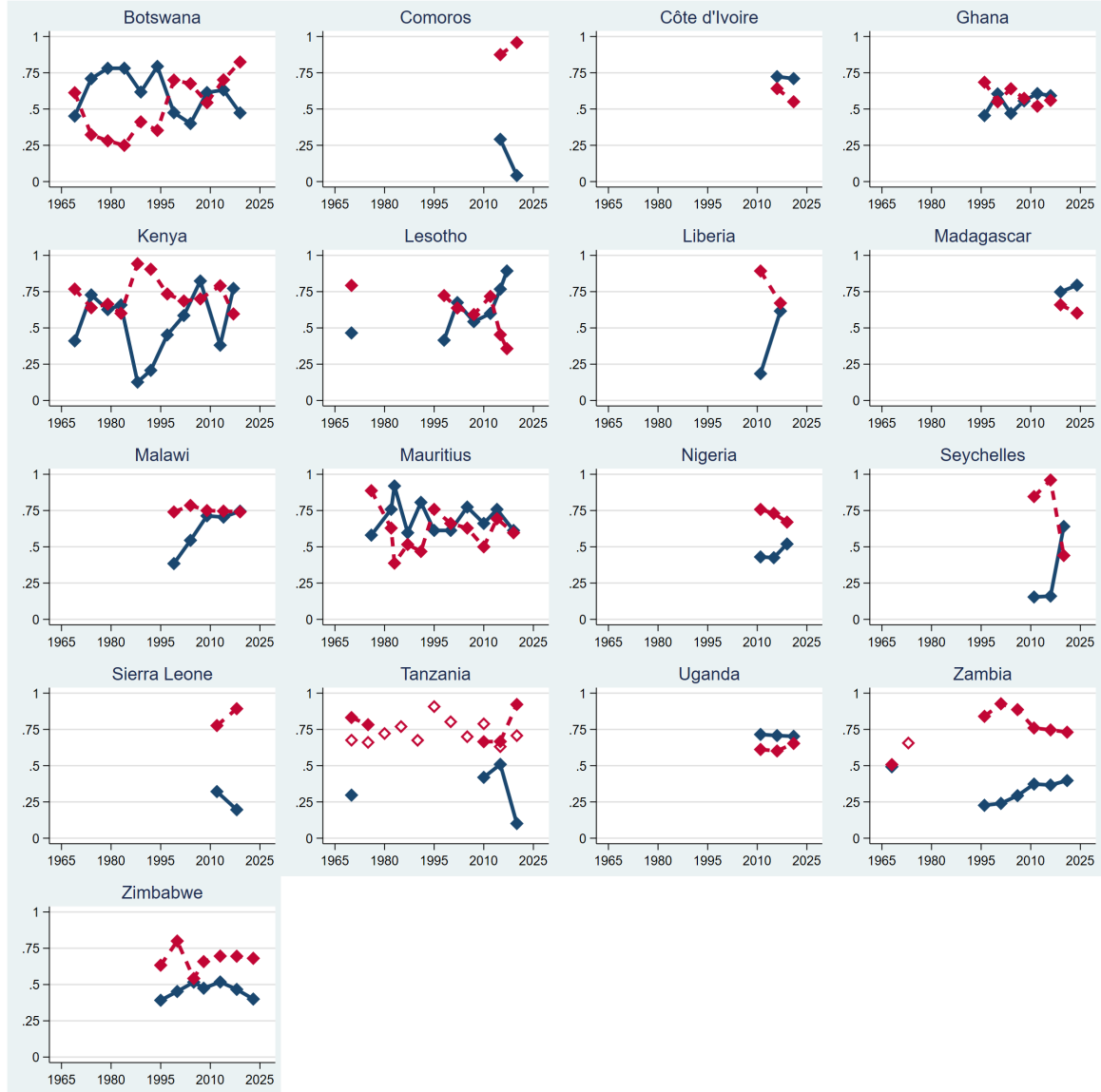
Figure 1 plots incumbent re-running rates and turnover rates over time for each country. Two features are immediately apparent. First, there is enormous cross-country heterogeneity: Uganda’s re-running rates hover around 70–75%, while Tanzania’s are below 20%. Second, within-country variation over time is also substantial: Malawi’s re-running rate rose from 38% in 1994 to 74% in 2014, Lesotho’s climbed from 41% in 1993 to 93% in 2015, and Zambia’s rose from 23% in 1991 to 40% in 2016, with a mirror-image decline in turnover. Figure 2 plots the resulting turnover series for the sample of countries with closed-list systems for which we only have measures of turnover. Per-country sources, year coverage, and roster completeness are documented in Online Appendix Section A.1.1.

Table 1 reports summary statistics and RDD estimates for each country. Raw incumbent re-running rates, i.e., the share of district winners who appear as candidates in  $t+1$ , range from 16.7% (Comoros) to 77.3% (Madagascar). Turnover rates, i.e., the share of incumbents who do not win in  $t+1$ , range from 56.0% (Botswana) to 91.7% (Comoros). For comparison, parliamentary turnover across 25 industrialized democracies averages about 32% (Bowles and Marx, 2023).<sup>4</sup>

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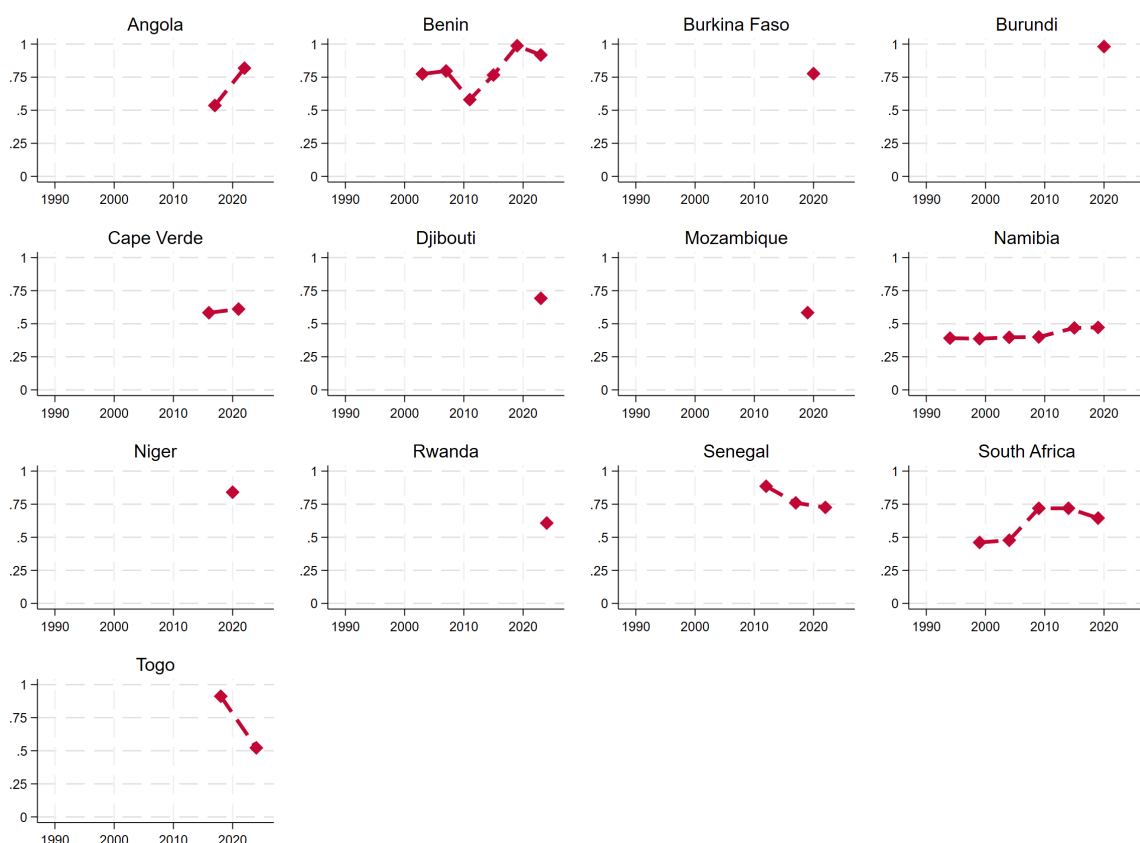
<sup>4</sup>The low re-running reaches the top of the seniority distribution. Even cabinet ministers re-run at a rate below 70% (67% versus 51% for backbench winners): one in three African cabinet ministers does not run again, well below the near-universal re-candidacy of sitting legislators in established democracies. Executives and party gatekeepers use their control even against senior figures.

Figure 1: Incumbent Re-Running Rates and Turnover Rates Over Time (Main Sample)



*Notes:* Each panel shows one country in the main sample. Solid line: share of district winners (incumbents) who re-run in the next election. Dashed line: share of incumbents who do not win the next election ( $\text{turnover} = 1 - \Pr(\text{win}_{t+1} | \text{won}_t)$ ). Each data point at year  $t$  shows the rate from the previous election into  $t$ . Hollow diamond markers indicate turnover rates computed from aggregate parliamentary records rather than individual candidate-level matching: Tanzania (*Wabunge wa Tanzania na Nafasi Zao*, Ofisi ya Bunge 2023, 1965–2023) and Zambia 1973 (Baylies and Szeftel, 1984). Source construction is documented in Online Appendix Section A.1.1. The corresponding turnover series for the turnover-sample countries are plotted in Figure 2.

Figure 2: Turnover Rates Over Time: Closed-List PR and Non-Partisan Countries (Turnover Sample)



Each marker shows turnover ( $1 - \text{share of legislators returning}$ ) for one consecutive-parliament transition. Sources: official parliament rosters, Wikipedia, EveryPolitician, and national newspapers (see Online Appendix).

*Notes:* Each panel shows one country in the turnover sample. Each marker reports turnover (the share of elected legislators in parliament  $t$  who do not return to parliament  $t+1$ ) for one consecutive-parliament transition; lines connect transitions within the same country. For these countries, complete candidate lists including losing candidates are not available, so we estimate turnover only and cannot estimate the RDD. Per-country sources are documented in Online Appendix Section A.1.1.

Table 1: Incumbency Effects in African Legislatures

| Country       | Years     | Elec. | $N$   | All Incumbents |          | RDD Estimates             |                           |
|---------------|-----------|-------|-------|----------------|----------|---------------------------|---------------------------|
|               |           |       |       | Rerun          | Turnover | $\Delta \text{Run}_{t+1}$ | $\Delta \text{Win}_{t+1}$ |
| Botswana      | 1965–2019 | 11    | 876   | 0.600          | 0.551    | 0.242**<br>(0.114)        | −0.147<br>(0.092)         |
| Comoros       | 2009–2020 | 2     | 96    | 0.167          | 0.917    | −0.012<br>(0.107)         | 0.038<br>(0.033)          |
| Côte d’Ivoire | 2011–2021 | 2     | 797   | 0.717          | 0.596    | 0.218*<br>(0.128)         | 0.168*<br>(0.102)         |
| Ghana         | 1992–2016 | 6     | 2,483 | 0.552          | 0.585    | 0.152*<br>(0.081)         | 0.054<br>(0.064)          |
| Kenya         | 1963–2017 | 11    | 3,902 | 0.543          | 0.723    | 0.024<br>(0.056)          | 0.050<br>(0.044)          |
| Lesotho       | 1993–2017 | 6     | 936   | 0.659          | 0.574    | 0.218<br>(0.136)          | 0.002<br>(0.125)          |
| Liberia       | 2005–2017 | 2     | 274   | 0.413          | 0.775    | 0.427**<br>(0.167)        | 0.303**<br>(0.132)        |
| Madagascar    | 2013–2024 | 2     | 466   | 0.773          | 0.629    | 0.299***<br>(0.103)       | 0.177<br>(0.108)          |
| Malawi        | 1994–2019 | 5     | 1,889 | 0.623          | 0.753    | 0.105<br>(0.085)          | 0.011<br>(0.081)          |
| Mauritius     | 1967–2019 | 11    | 4,736 | 0.699          | 0.611    | 0.186**<br>(0.073)        | 0.070<br>(0.062)          |
| Nigeria       | 2003–2019 | 3     | 2,133 | 0.458          | 0.719    | 0.231***<br>(0.087)       | 0.138**<br>(0.060)        |
| Seychelles    | 2007–2020 | 3     | 151   | 0.316          | 0.750    | 0.340<br>(0.218)          | 0.122<br>(0.194)          |
| Sierra Leone  | 2007–2018 | 2     | 448   | 0.259          | 0.835    | 0.282<br>(0.210)          | 0.085<br>(0.117)          |
| Tanzania      | 2005–2020 | 3     | 1,405 | 0.331          | 0.760    | 0.086<br>(0.091)          | 0.139*<br>(0.072)         |
| Uganda        | 2006–2021 | 3     | 2,006 | 0.708          | 0.625    | 0.129<br>(0.088)          | −0.065<br>(0.086)         |
| Zambia        | 1964–2021 | 7     | 1,915 | 0.329          | 0.794    | −0.025<br>(0.105)         | 0.132*<br>(0.068)         |
| Zimbabwe      | 1990–2023 | 7     | 2,144 | 0.461          | 0.676    | 0.209**<br>(0.102)        | 0.109<br>(0.085)          |

*Notes:* “Years” shows the first and last election year in the data. “Rerun” and “Turnover” report raw rates for all district winners (incumbents), pooled across elections. The most recent election is excluded as  $t + 1$  outcomes are unavailable.  $\text{Rerun} = \Pr(\text{run}_{t+1} \mid \text{won}_t)$ ;  $\text{Turnover} = 1 - \Pr(\text{win}_{t+1} \mid \text{won}_t)$ . RDD columns report bias-corrected estimates from local linear regressions with CER-optimal bandwidths and triangular kernel.  $N$  is the number of top-two (winner and runner-up) candidates with non-missing  $t + 1$  outcomes. Robust standard errors in parentheses. Tanzania’s 1965/1970 single-party elections and Lesotho’s stand-alone 1965 election are shown in Figures 1, 3 and 4 but excluded from the pooled estimates here for lack of continuity with the modern series. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

### 3 Empirical Strategy and Results

#### 3.1 Regression Discontinuity Design

We estimate the causal effect for an individual of winning a legislative election using the standard regression discontinuity framework (Calonico et al., 2014; Cattaneo et al., 2020). The RDD adds to the raw re-running and turnover rates of Section 2 the estimation of a causal effect: in the potential-outcomes framework, it compares the same individual’s outcomes when that individual barely wins versus when that individual is the runner-up, so the estimate isolates the effect of holding the seat from everything else that differs between winners and losers. The treatment is winning in election  $t$  (versus being the runner-up), and the running variable is the vote margin between the winner and runner-up as a share of total constituency votes. We estimate local linear regressions with CER-optimal bandwidths, triangular kernel weights, and robust bias-corrected inference; reported point estimates are the bias-corrected estimates (Calonico et al., 2014, 2020).<sup>5</sup>

The identifying assumption is that potential outcomes are continuous at the cutoff—i.e., that candidates who barely won are comparable, on average, to candidates who barely lost. Because the main specification compares only the winner and the runner-up, the cutoff is symmetric and a density (manipulation) test is not informative; we assess the assumption with balance tests on pre-determined covariates (party affiliation, prior incumbency status), reported in Online Appendix Section A.2.4. A density test is informative and is reported once we add the remaining losers in the analysis as a robustness check (Online Appendix Section A.2.3); we find no discontinuity in density.

We focus on two outcomes. The first,  $\hat{\tau}_{\text{run}}$ , is the effect of winning on the probability of running in the next election. The second,  $\hat{\tau}_{\text{win}}$ , is the effect of winning on the probability of winning in the next election, where candidates who do not re-run are coded as not winning.

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<sup>5</sup>The triangular kernel is the `rdrobust` default and is boundary-optimal for local polynomial estimation at a cutoff (Cattaneo et al., 2020). Re-estimating with a uniform kernel yields the same qualitative conclusions—the same countries carry the significant effects, with cross-country medians of +0.15 (re-running) and +0.08 (winning) against +0.21 and +0.09 under the triangular kernel; the comparison is available in the replication package.

### 3.2 Main Results

**Re-running ( $\hat{\tau}_{\text{run}}$ ).** The effect of barely winning on re-running, reported in Table 1, is positive in 15 of 17 countries and statistically significant at the 10% level in 8 (Botswana: +0.24,  $p < 0.05$ ; Côte d’Ivoire: +0.22,  $p < 0.10$ ; Ghana: +0.15,  $p < 0.10$ ; Liberia: +0.43,  $p < 0.05$ ; Madagascar: +0.30,  $p < 0.01$ ; Mauritius: +0.19,  $p < 0.05$ ; Nigeria: +0.23,  $p < 0.01$ ; Zimbabwe: +0.21,  $p < 0.05$ ). Point estimates range from  $-0.03$  (Zambia) to +0.43 (Liberia). The median effect across 17 countries is +0.21.<sup>6</sup> For comparison, De Magalhães and Hirvonen (2026) estimate a re-running effect of +0.33 in the US, with comparably large effects in the UK. The African median is roughly two-thirds of the US benchmark (+0.33).

**Winning ( $\hat{\tau}_{\text{win}}$ ).** The effect on winning in the next election, also in Table 1, is positive in 15 of 17 countries but statistically significant at the 10% level in only 5, all positive (Côte d’Ivoire: +0.17,  $p < 0.10$ ; Liberia: +0.30,  $p < 0.05$ ; Nigeria: +0.14,  $p < 0.05$ ; Tanzania: +0.14,  $p < 0.10$ ; Zambia: +0.13,  $p < 0.10$ ). The median effect is +0.09.<sup>7</sup> De Magalhães and Hirvonen (2026) report unconditional winning effects—the lower bounds of the incumbent-runner-up advantage—of +0.41 in the US and +0.34 in the UK. The African median is substantially smaller.

**The two gates.** The effect of incumbency on re-running  $\hat{\tau}_{\text{run}}$  and on winning  $\hat{\tau}_{\text{win}}$  are positively correlated across the 17 countries, though imprecisely ( $r = +0.37$ ,  $p = 0.15$ ). For most countries it is the first gate that binds; in a few, an incumbency advantage is present at both gates. These patterns are organized and discussed in Section 4.

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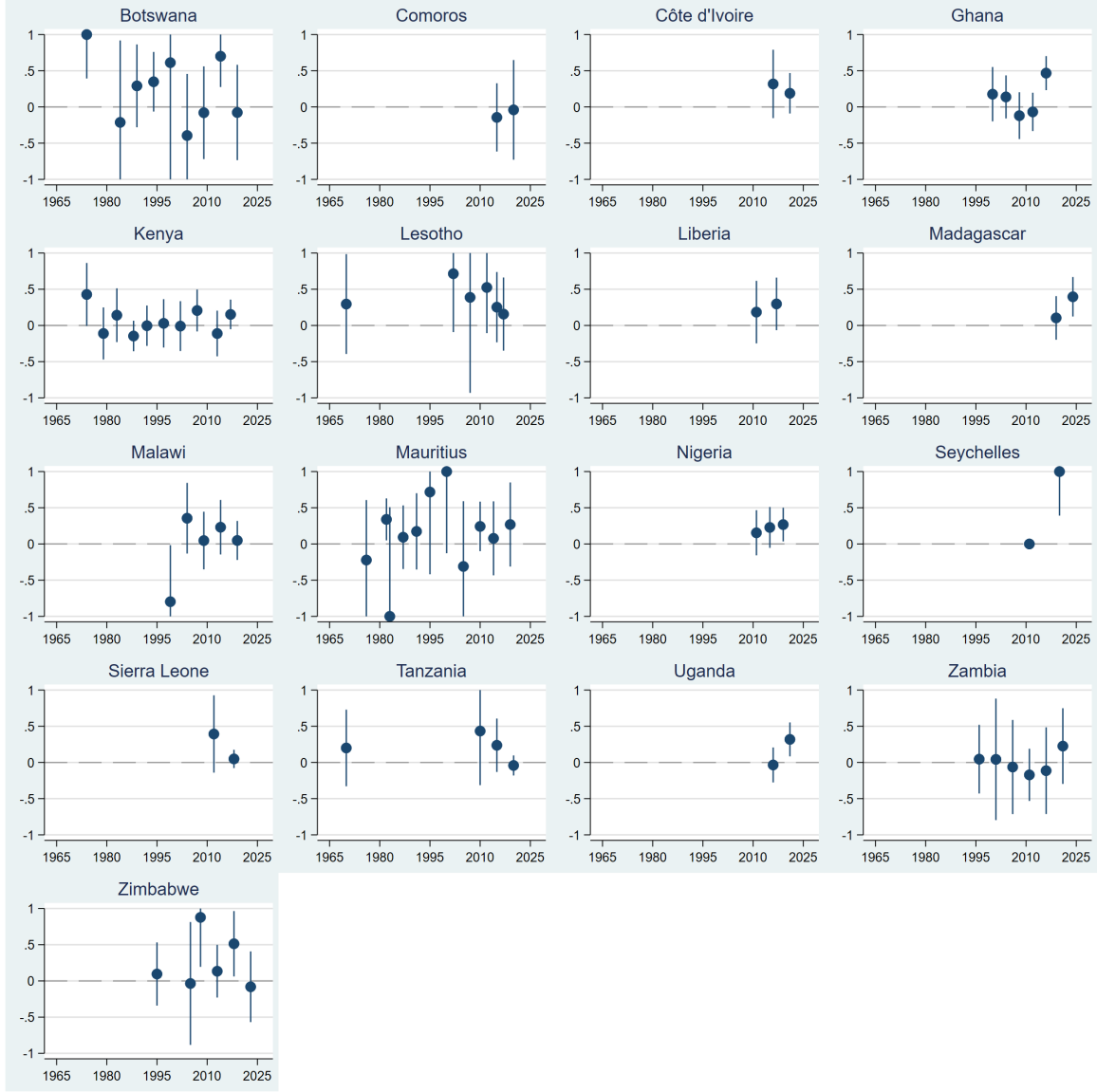
<sup>6</sup>These medians are robust to the sample. Dropping the de-jure one-party election-years in our data (Kenya 1969–1988), where the winner–runner-up contest is intra-party rather than across parties, leaves the re-running median at +0.21 and moves the winning median from +0.09 to +0.11; restricting to post-1995 elections gives +0.17 and +0.11, the higher winning figure reflecting the truncation of the longer country panels rather than regime type. All 17 countries remain estimable in each sample.

<sup>7</sup>The unconditional  $\hat{\tau}_{\text{win}}$  is the *lower bound* of the incumbent–runner-up advantage, since it codes incumbents who do not re-run as not winning. Following De Magalhães and Hirvonen (2026) (building on the partial-identification logic of Anagol and Fujiwara, 2016) we also construct the *upper bound*, which rescales the unconditional estimate by the re-running rate of whichever group (winners or runners-up) re-runs more, partialling out the share of non-re-runners lost to random attrition. Because the first-gate effect leaves the two re-running rates close in most countries, the two bounds are close as well; Online Appendix Table A.9 reports them by country, and Section 4 uses the bounds to sort the second gate.

**RDD effects over time.** Figure 3 plots the RDD effect on re-running ( $\hat{\tau}_{\text{run}}$ ) by election year for each country. The re-running effect is the key channel in our theoretical framework, since it mediates the effect on winning. The estimates are noisy, as expected given the small within-election samples, and in most countries the effect fluctuates around zero without a clear trend. Figure 4 plots the corresponding effect on winning ( $\hat{\tau}_{\text{win}}$ ), which similarly fluctuates around zero, consistent with persistently weak incumbency advantage.

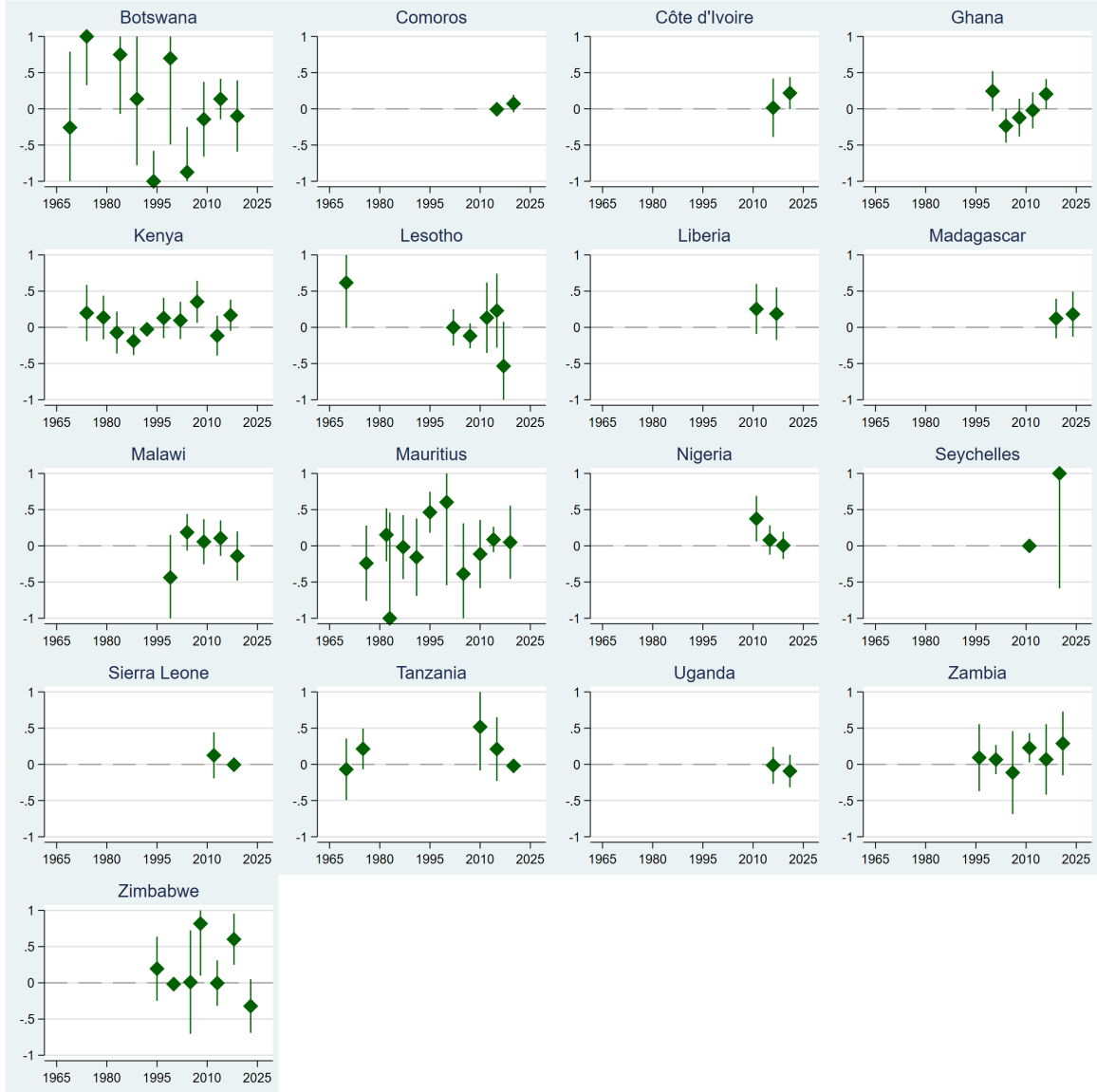
The by-year estimates are not stable in every country. Within-country sample sizes per electoral cycle are small (often 100–300 close races), and the political environment shifts across cycles—anti-incumbency waves, executive transitions, opposition boycotts, and changes in the executive’s control over nominations explain variations in the estimates. We treat the by-year instability itself as informative: incumbency effects in African legislatures do respond to the political dynamics of electoral cycles. Section 4 returns to specific country-years in which a sharp deviation from the country’s own baseline lines up with documented institutional or political changes.

Figure 3: RDD Effect on Re-Running ( $\hat{\tau}_{\text{run}}$ ) by Election Year



*Notes:* Each panel shows one country. Points are bias-corrected RDD point estimates from local linear regressions with CER-optimal bandwidths and triangular kernel. Error bars show 95% bias-corrected robust confidence intervals; point estimates and intervals are truncated at  $[-1, 1]$  for display (see Online Appendix Table A.5). Horizontal dashed line at zero. Each estimate is plotted at the outcome election year ( $t + 1$ ): the point at year  $t$  is the effect of barely winning the previous election on *re-running* in the election held at  $t$ .

Figure 4: RDD Effect on Winning ( $\hat{\tau}_{\text{win}}$ ) by Election Year



*Notes:* Each panel shows one country. Points are bias-corrected RDD point estimates from local linear regressions with CER-optimal bandwidths and triangular kernel. Error bars show 95% bias-corrected robust confidence intervals; point estimates and intervals are truncated at  $[-1, 1]$  for display (see Online Appendix Table A.5). Horizontal dashed line at zero. Each estimate is plotted at the outcome election year ( $t + 1$ ): the point at year  $t$  is the effect of barely winning the previous election on *winning* the election held at  $t$ .

### 3.3 Robustness of RDD Results

To assess the robustness and validity of our RDD results, we report election-year estimates for each country (Online Appendix Section A.2.2). Balance tests on pre-determined covariates (incumbent status in  $t$  and the full set of party indicators) show 6 rejections out of 108 country-by-party tests at the 5% level, close to the 5.4 expected by chance. We re-estimate the main specifications using the full candidate sample rather than only the marginal-two finishers (Online Appendix Section A.2.6); within the full candidate sample the density test is informative, and it does not reject the null of *no* manipulation in any of the 17 countries (Online Appendix Section A.2.3). We also re-estimate with a uniform rather than triangular kernel (see the note in Section 3) and with standard errors clustered at the constituency level (Online Appendix Section A.2.1). All eight countries with re-running effects that are significant at the 10% level retain their significance under constituency-clustered standard errors. A small number of election-years produce estimates outside  $[-1, 1]$ ; winsorizing them leaves the by-year summaries unchanged (Online Appendix Section A.2.2). All robustness checks return qualitatively similar conclusions.

As a separate identification exercise, we also replicate the runner-up RDD of Anagol and Fujiwara (2016) for each of the 17 countries: the running variable is the rank-2 versus rank-3 vote-share margin, and the outcome is whether the rank-2 finisher runs, and wins, in  $t+1$ . Neither finisher holds office, so any effect is informational rather than institutional. Of the 16 countries with estimable effects, 12 show a positive  $\hat{\tau}_{\text{run}}$ , with median  $+0.04$ , in the range of the Anagol and Fujiwara (2016) Brazilian and Indian estimates, and the corresponding rank-3 versus rank-4 placebo is a precise null. The three margins line up in the order one expects, and that Anagol and Fujiwara (2016) find: the winner versus runner-up effect (median  $+0.21$ ) exceeds the rank-2 versus rank-3 effect (median  $+0.04$ ), which in turn exceeds the rank-3 versus rank-4 effect (a precise null)—reassuring for the design, since each step down the ranking carries less office and less information. We return to this runner-up design in Section 5, where it offers a placebo for the institutional interpretation of the incumbency effect.<sup>8</sup>

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<sup>8</sup>The placebo is underpowered; see Online Appendix Section A.3.

## 4 Why the First Gate Binds

### 4.1 Classifying Countries by Which Gate Binds

The RDD estimates the effect of becoming the incumbent at two different gates. We read  $\hat{\tau}_{\text{run}}$  as the effect at the first gate (re-running) and the bounds on  $\hat{\tau}_{\text{win}}$  as the effect at the second (winning). These two estimates, together with the re-running rates of all incumbents, allow us to sort the 17 countries into four groups. This quantitative sorting is based on the following rule of thumb. A country's first gate is *open* if  $\hat{\tau}_{\text{run}}$  is positive and significant at the 10% level and the incumbent re-running rate exceeds one half; *partly open* if  $\hat{\tau}_{\text{run}}$  is positive and significant but the re-running rate stays below one half; and *closed* otherwise. Where the first gate is open, the bounds on  $\hat{\tau}_{\text{win}}$  sort the second gate: a negative upper bound identifies a disadvantage at the polls, while a positive upper-bound indicates an advantage that survives both gates. The resulting quantitative classification is reported in the last column of Table 2; the underlying RDD estimates are in Table 1 and the bounds in Online Appendix Table A.9.

In most countries the first gate binds. In nine of the 17—Comoros, Kenya, and Zambia with estimates at or below zero, together with the positive-but-insignificant set of Lesotho, Malawi, Seychelles, Sierra Leone, Tanzania, and Uganda— $\hat{\tau}_{\text{run}}$  is not significantly different from zero at the 10% level. Where an incumbent gains nothing at re-nomination, the supply- and demand-side accounts are observationally equivalent: a candidate whose margin of victory does not raise the probability of standing again looks the same whether a gatekeeper blocks the way or the incumbent forfeits, foreseeing a defeat. The RDD cannot separate the two in these countries, and we say only that the first gate binds.

In one country the first gate is open yet the second binds. In Botswana,  $\hat{\tau}_{\text{run}} = +0.24$  ( $p < 0.05$ ) and 60% of incumbents re-run—incumbents clear the first gate—yet the effect on winning is negative ( $\hat{\tau}_{\text{win}} = -0.15$ ,  $p = 0.11$ ) and even the upper bound of the incumbent–runner-up advantage is negative, at  $-0.24$ . Barely-winning incumbents are more likely to stand again and less likely to win when they do: voters punish them at the second gate. Botswana is the one country where the evidence points to an incumbency

disadvantage at the second gate, though the point estimate on winning is significant only at the 11% level and the identification rests on the negative upper bound.

In three countries the first gate is only partly open. In Liberia, Nigeria, and Zimbabwe  $\hat{\tau}_{\text{run}}$  is positive and significant, but the re-running rate stays below half (41%, 46%, and 46%): barely-winning incumbents are more likely to stand again, yet most incumbents still do not return, so the gate opens at the margin while remaining a binding constraint overall.

In a handful the incumbency advantage survives both gates. In Côte d’Ivoire, Ghana, Madagascar, and Mauritius  $\hat{\tau}_{\text{run}}$  is positive and significant, the re-running rate is above half, and the upper-bound incumbency advantage is positive: barely winning raises re-running and carries through toward winning the next election. These countries look most like the established democracies in which incumbency is studied at the second gate, though the magnitudes remain well below the US and UK benchmarks (Section 3). Qualitatively, these are also chambers where selection is competitive and institutionalized rather than executive-run—Mauritius, the strongest parliament in our sample by the Parliamentary Powers Index, is the clearest case—as the country paragraphs below describe.

The time dimension refines the sort, and explains two of the disparities between the literature reading and the pooled estimate. In Ghana, the by-year effect on re-running is indistinguishable from zero in every cycle through 2012 (+0.18, +0.14, −0.12, −0.07) and jumps to +0.47 ( $p < 0.01$ ) into 2016; in Uganda it is zero into 2016 (−0.04) and +0.32 ( $p < 0.01$ ) into 2021 (Figure 3). In both, the pooled estimate averages a gate that was closed for most of the sample with one that has recently opened, so Table 2 marks them Closed→Open. No other country shows this path: Nigeria’s positive effect is stable across all three cycles, and Madagascar’s two cycles are both positive—gates that are open throughout, not gates that opened.

Where the first gate binds, the RDD cannot say who holds it. The rest of this section, and Table 2, read from the country-specific literature and the press who holds the first gate where it binds—who selects candidates, and how the selectorate treats a

sitting incumbent. What follows describes who the literature and press identify as the selectorate.

## 4.2 How Candidates Are Selected: Who Holds the First Gate

Table 2 summarizes how candidates are selected in each of the 17 countries: who the effective selectorate is, whether a sitting incumbent is renominated by default, and how open the first gate is to an incumbent who wishes to stand again. In the language of the comparative candidate-selection literature, the question is how small and centralized the selectorate is (Gallagher and Marsh, eds, 1988; Rahat and Hazan, 2001; Hazan and Rahat, 2010). In most of our countries the selectorate is small, and it is the party leadership or the executive—not voters—that controls who appears on the ballot. Sitting legislators are frequently screened out, so the nomination stage, rather than the general election, is the binding constraint on incumbents’ political survival.

The literature-based classification is coded as follows. For each country we read the country-specific scholarship and press and code four features of candidate selection: who the effective selectorate is (the executive, the central party leadership, a party primary, local patrons, or an open field); whether a rule or norm renominates a sitting incumbent by default or the incumbent must re-contest the nomination; who funds the campaign (the party, a patron, or the candidate); and whether the party label is carried over from one election to the next. Together these four features yield the literature’s verdict on how open the first gate is to a sitting incumbent—the second-to-last column of Table 2. How each feature feeds the coding is not always obvious from the labels, so we discuss the coding country by country below. The quantitative classification of the last column is computed from the RDD without reference to this reading, so the two columns can be compared: they agree outright in seven of the 17 countries, the Closed→Open time paths of Ghana and Uganda reconcile two more (the literature describes the cycles before the recent opening), and the remaining disagreements are themselves informative. The paragraphs below go through the 17 countries in alphabetical order, giving for each country the literature reading and the quantitative sort.

Table 2: How Legislative Candidates Are Selected: The First Gate by Country

| Country       | Selectorate                                    | Re-runs by default | Funding          | Party label | First gate (lit) | First gate (quant) |
|---------------|------------------------------------------------|--------------------|------------------|-------------|------------------|--------------------|
| Botswana      | Central party primary ( <i>Bulela Ditswe</i> ) | Contested          | Party            | Yes         | Open             | Open               |
| Comoros       | Constitutional island rotation                 | n/a                | n/a              | n/a         | Closed           | Closed             |
| Côte d’Ivoire | Mixed (post-crisis competitive)                | Yes                | Mixed            | Yes         | Partly           | Open               |
| Ghana         | Party primaries in safe seats                  | No                 | Mixed            | Yes         | Closed           | Closed→Open        |
| Kenya         | Executive / central party                      | Contested          | Party            | Yes         | Closed           | Closed             |
| Lesotho       | Mixed (MMP; party list + constituency)         | Yes                | Mixed            | Yes         | Partly           | Closed             |
| Liberia       | Local / community elites (post-conflict)       | Yes                | Self             | Mixed       | Partly           | Partly             |
| Madagascar    | Leader-centred parties                         | Yes                | Self             | Yes         | Partly           | Open               |
| Malawi        | Open; independents prominent                   | Yes                | Self             | No          | Open             | Closed             |
| Mauritius     | Central party / alliance (block vote)          | Yes                | Party            | Yes         | Partly           | Open               |
| Nigeria       | Godfathers / governors + party elite           | No                 | Patron           | Yes         | Closed           | Partly             |
| Seychelles    | Ruling party                                   | Contested          | Party            | Yes         | Partly           | Closed             |
| Sierra Leone  | Central party leadership (handpicked)          | No                 | Party            | Yes         | Closed           | Closed             |
| Tanzania      | CCM Central Committee                          | No                 | Party            | Yes         | Closed           | Closed             |
| Uganda        | Open field / permeable primaries               | Yes                | Self / patronage | No          | Open             | Closed→Open        |
| Zambia        | Executive / party (nomination control)         | No                 | Party            | Yes         | Closed           | Closed             |
| Zimbabwe      | ZANU–PF Central Committee (factional)          | No                 | Party            | Yes         | Closed           | Partly             |

*Notes.* Candidate-selection regime for each of the 17 main-sample countries, read from the country evidence in Section 4 and the press quotes in Online Appendix Section A.6. Closed→Open marks a first gate whose by-year re-running effect is zero in every cycle except the most recent, which is positive and significant (Ghana into 2016, Uganda into 2021; see Figure 3). **Selectorate:** the effective selectorate that controls access to the ballot. **Re-runs by default:** whether a sitting incumbent is renominated as a matter of course (Yes), is routinely screened out (No), or faces a genuinely uncertain renomination (Contested). **Funding:** whether campaigns are financed by the party, by candidates themselves (Self), or by patrons (Patron). **Party label:** whether a party nomination is required to stand (independents are common where No). **First gate:** a summary of how open candidate selection is to a sitting incumbent — Open, Partly open, or Closed. The **First gate (quant)** column is the RDD classification (Open =  $\hat{\tau}_{run} > 0$  and significant with re-running above 50%; Partly = significant but re-running below 50%; Closed =  $\hat{\tau}_{run}$  not positive and significant), to be compared with the literature-based **First gate** column; full estimates in Table 1 and bounds in Online Appendix Table A.9. “n/a” marks Comoros, where constitutional rotation across the islands overrides individual candidacy.

**Botswana.** Our reading of the literature is that Botswana’s first gate is open: candidates are selected through the BDP’s primary system (*Bulela Ditswe*), renomination is contested rather than automatic, the party funds campaigns, and the party label carries over. The quantitative sort agrees that the first gate is open, and adds that the second gate binds:  $\hat{\tau}_{\text{run}} = +0.24^{**}$ —incumbents clear the first gate—while the effect on winning is the most negative in the sample ( $\hat{\tau}_{\text{win}} = -0.15$ ,  $p = 0.11$ ) and even the upper bound of the incumbent–runner-up advantage is negative, at  $-0.24$ . Barely-winning legislators are more likely to seek re-election, and *less* likely to retain their seats when they do: the binding constraint operates at the polls, and Botswana is the one country where the evidence points to an incumbency disadvantage at the second gate. Although *Bulela Ditswe* has been criticized for elite manipulation and BDP primaries regularly replace some incumbents, the dominant story over the most recent cycles is constituency-level voter rejection of long-serving incumbents in a context of rising youth unemployment and economic stagnation (Brown, 2020; Seabo and Nyenhuis, 2021). The BDP, which had won every election since independence in 1966, lost its parliamentary majority for the first time in 2014, retained government only through coalition arrangements in 2019, and lost outright to the Umbrella for Democratic Change (UDC) in 2024.

**Comoros.** Our reading of the literature is that the first gate is closed, by institutional design rather than by a single gatekeeper: the constitution rotates the presidency among the three islands (Grande Comore, Anjouan, Mohéli), party organizations are extremely fragmented and short-lived, and no durable rule or party exists to renominate a sitting deputy (Sanches et al., 2022; Ramachandran, 2024). The quantitative sort agrees:  $\hat{\tau}_{\text{run}} = -0.25$  (the most negative point estimate in the sample, though imprecise), and the re-running rate of 16.7% is the lowest of the 17 countries, reaching 0% in 2015—the most extreme value in our dataset. Opposition boycotts compound the rotation politics: the Juwa party boycotted the 2024–25 legislative elections, arguing that no fair electoral framework was possible under the incumbent regime, and observers reported polling agents expelled without justification (Online Appendix Section A.6). We read Comoros as institutional disruption: turnover is near-total for reasons unrelated to the performance

of individual legislators.

**Côte d’Ivoire.** Our reading of the literature is that the first gate is partly open: post-crisis elections are competitive and incumbents may stand again, but nominations in the government camp remain executive-driven and the party system is unsettled by the 2010–11 crisis and the opposition boycott that followed (Crook, 1997; Bassett, 2011; van Baalen and Gbala, 2023). The quantitative sort says open, with an advantage at both gates ( $\hat{\tau}_{\text{run}} = +0.22$ ,  $p < 0.10$ ;  $\hat{\tau}_{\text{win}} = +0.17$ ,  $p < 0.10$ ; re-running rate 72%): an individual incumbent who stands again is not blocked, broadly in line with the literature reading. Note that the panel is short. The Ivorian Popular Front (FPI) boycotted the 2011 parliamentary election, accusing the electoral commission of bias and the army of intimidating its supporters, which produced near-total opposition turnover for institutional rather than performance-based reasons; with only two estimable transitions, both shaped by the post-crisis reset, the estimates rest on the government-camp contests that remained competitive.

**Ghana.** Our reading of the literature is that Ghana’s first gate is closed, held by party establishment control over local primaries. More than 60 percent of Ghana’s 275 parliamentary seats are “safe” for either the NPP or the NDC, making the party primary tantamount to winning the general election: Brierley and Ofori (2014) note that parliamentary candidates are chosen by party delegates in constituency-level primaries and that, where one of the two parties is dominant, “winners are almost guaranteed of victory should they win the primary” (p. 383). Party leaders can preempt primaries by anointing candidates, but Ichino and Nathan (2012) show that canceling primaries alienates local party members who expect material inducements from competing aspirants, so leaders allow patronage-driven nomination contests in which wealthy challengers unseat incumbents. Ichino and Nathan (2013) find that these primaries generate a “primary penalty” for the ruling party—high-stakes competition produces intra-party conflict and disgruntled losers who defect or withhold support in the general election—and a “primary bonus” for the opposition, where lower stakes produce less severe conflict and patronage spending

mobilizes the local party organization. In our data, incumbent attrition at the primary stage is substantial: in 2020, roughly 40 NPP incumbent MPs lost their primaries, and 19 more voluntarily chose not to contest. The screening is visible on the ballot itself. Brierley and Ofori (2014) report that the independents elected in 2012 were aspirants “disqualified from parliamentary primaries for various, sometimes dubious, reasons, or candidates who had lost primaries but decided to run without party backing” (p. 384), and name Joseph Ofori among those who lost an NDC primary. Our candidate panel picks up the same member on both sides of the gate: Joseph Kwadwo Ofori won Akan for the NDC in 2008, and in 2012 the NDC label in Akan went to another candidate while Ofori returned to Parliament as an independent. Ofori cleared the second gate after being turned away at the first. A complementary supply-side channel operates through the cabinet. Lindberg and Zhou (2009) document how Ghana’s hybrid constitution, which requires the president to draw most ministers from Parliament, has been used “to recruit the most competent MPs to join the government, thus depleting the legislature of some of its best talent, and for enforcing loyalty to the executive among those MPs left behind” (p. 169); by 2005, 74% of the cabinet were sitting MPs (Table 5.8, p. 170), and MPs who fail to secure a ministerial post “are perceived by voters as useless for bringing resources back to the constituency” (p. 169). Lindberg and Zhou (2009) judge that the autonomy of Ghana’s Parliament strengthened through the 1990s but “declined significantly since” the NPP’s capture of both house and presidency in 2000, attributing the reversal partly to “the liberal dispensation of patronage by the executive to vulnerable MPs” and naming “a high turnover among MPs” among the contributing causes (p. 148). The quantitative sort reads the gate as open at the margin ( $\hat{\tau}_{\text{run}} = +0.15$ ,  $p < 0.10$ ; re-running rate 55%), but the by-year path locates the opening in time and reconciles the sort with the literature: the effect on re-running is zero in every cycle through 2012 and  $+0.47$  ( $p < 0.01$ ) into 2016 (Figure 3). The gate the literature describes—a primary that screens incumbents without regard to electoral strength—is the gate we estimate through 2012; in the most recent cycle electoral strength has begun to clear it, and Table 2 marks Ghana Closed→Open. The demand side also appears in Ghana: for example, in 2008, “skirt-

and-blouse” voting—supporting the opposition presidential candidate while retaining a local incumbent—represented voter-level protest against the ruling party. Ofosu (2019) shows that Ghanaian legislators act on the expectation of that sanction: members elected from constituencies randomly assigned intensive election-day observation in 2012 spent 19 percentage points more of their Constituency Development Funds over the following term, and the mechanism tests point to incumbents’ expectation of electoral sanction rather than to the selection of better candidates.

**Kenya.** Our reading of the literature is that Kenya’s first gate is closed, held by the executive and the central party establishment, and the quantitative sort agrees ( $\hat{\tau}_{\text{run}} = +0.02$ , indistinguishable from zero). Executive control of recruitment long predates the current period. Writing on the 1974 semi-competitive elections, Barkan and Okumu (1978) describe a four-tier patron–client pyramid with President Kenyatta at its apex, in which “the main function of elections in Kenya” was “to recruit talented political entrepreneurs into a national system of clientelist networks which link the periphery to the centre, *and* contain their ambition so that the entrepreneurs pose no challenge to the regime” (Barkan and Okumu, 1978, p. 97). Opalo (2019) provides the institutional narrative for the later period: “Thomas Mboya and Josiah M. Kariuki were assassinated in 1969 and 1975, respectively. Both were independent legislators with presidential ambitions” (pp. 216–217). Across Kenya’s competitive multiparty cycles the RDD effect on re-running is close to zero through the transitions that span Moi’s rule—insignificant into the 1997 and 2002 elections—and the point estimates turn positive only in the democratic period, into 2007 and 2017 (discussed below). Opalo (2017) reports that incumbency advantage in Kenya was negative under autocracy and positive under democracy; his estimates, however, condition on re-running, which generates a selection problem: if the executive prevents certain incumbents from re-running, the selected sample of re-runners is not comparable across regime types. Our unconditional estimates—which code non-re-runners as having lost—are closer to zero throughout, consistent with the view that executive control of candidacy, not voter evaluation, is the binding constraint. Opalo (2020) shows that periods of legislative ascendancy in Kenya are associated with reduced executive rule-making,

giving the executive a tangible incentive to demobilize active legislators ahead of the next nomination cycle. The demand side operates episodically: in 2007, 190 of 210 incumbents re-ran but only 71 won—a 62.6% defeat rate driven by anti-incumbency sentiment, with 79% of voters reporting that their MPs did not keep promises.

Kenya is also the clearest case of deliberate institution-building, and the one where the link between reform and re-running is most visible. Barkan and Matiangi (2009) date the transformation of the National Assembly to 1998, when a group of roughly thirty to forty-five reform-minded MPs—younger, better-educated, and disproportionately from the opposition—began a decade of steps to wrest budgetary and administrative autonomy from the presidency (pp. 44–45). The central achievement, the 2000 amendment creating the Parliamentary Service Commission, required the votes of 65 percent of all MPs and was secured “through the coming together of a coalition of change composed of opposition MPs and most KANU backbenchers” (p. 46). That a reform strengthening members as a class had to be carried by members who intended to keep standing is consistent with reading re-running as the binding constraint: where legislators expect to return, they invest in the institution rather than exit it. Kenya is the natural test of whether legislatures strengthen where the re-running penalty on incumbents has eased, and our own estimates give a first look. The digitized 1997 results, matched backward to 1992 and forward to 2002, complete Kenya’s competitive multiparty series, and the timing matches the executive-control reading. Reporting each estimate at its outcome election (the convention used throughout the paper), the effect on re-running is close to zero through the transitions that span Moi’s rule—+0.03 into 1997 (+0.13 on winning; both insignificant) and essentially zero into 2002 ( $\hat{\tau}_{\text{run}} = -0.01$ ,  $\hat{\tau}_{\text{win}} = +0.10$ )—with winner re-running rates of 45% and 59%. The point estimates turn positive once Moi leaves office: +0.21 into 2007 (+0.35 on winning,  $p < 0.05$ ) and +0.15 into 2017 (+0.17 on winning; the re-running estimates are not individually significant), with winner re-running rates of 82% and 78%. The transition into 2013 is the exception, negative at both gates ( $\hat{\tau}_{\text{run}} = -0.11$ ,  $\hat{\tau}_{\text{win}} = -0.12$ ), tracking the anti-incumbency of the 2007 election feeding forward. That the re-running advantage is absent while Moi controls candidacy and

appears only as the Parliamentary Service Commission reforms take hold is consistent with reading re-running as the binding constraint that eased with institution-building.

**Lesotho.** Our reading of the literature is that the first gate is partly open: under the mixed-member proportional (MMP) system adopted in 2002, candidates run on both constituency and list tiers, incumbents ordinarily stand again, and funding is mixed; what drives turnover is chronic institutional disruption—ruling-party splits, prorogations, and snap elections in 1997, 2006, 2012, 2015, and 2016 (Weisfelder, 2015; Letsie, 2015). The 1993 election is a clear demand-side episode: the BCP swept all 65 seats. The quantitative sort narrowly classifies the first gate as closed: the re-running rate is high (66%) and the effect on re-running is positive and sizable, but it just misses significance once the anomalous 1993 BCP landslide enters the pooled estimate ( $\hat{\tau}_{\text{run}} = +0.22$ ,  $p = 0.11$ ;  $+0.27$  restricting to post-1995, and significant at the 5% level under constituency-clustered standard errors). The effect on winning is essentially zero. Reading the two columns together: the party system fragments and reconstitutes between elections, yet an individual incumbent who wishes to stand again is not blocked. Lesotho’s stand-alone 1965→1970 cycle—excluded from the pooled Table 1 estimate for lack of continuity with the post-1993 series but retained in Figures 3–4—is in keeping with this modern pattern: barely winning raised both re-running and winning ( $\hat{\tau}_{\text{run}} = +0.30$ ,  $\hat{\tau}_{\text{win}} = +0.62$ ). The first gate is open at the level of the individual legislator even when the system breaks down around the incumbent.

**Liberia.** Our reading of the literature is that the first gate is partly open: post-conflict candidate selection rests with local and community elites, incumbents may stand again, campaigns are self-funded, and party labels are not carried over: in 2017, 31 of 38 incumbents ran for different parties than those they had stood for in prior elections (Pailey and Harris, 2020). The 2005 election (the first after the civil war) had 503 candidates contesting 64 seats, with no incumbents from the pre-war period (Harris, 2006). The quantitative sort says partly open, in line with the literature reading:  $\hat{\tau}_{\text{run}} = +0.43$  ( $p < 0.05$ ) and  $\hat{\tau}_{\text{win}} = +0.30$  ( $p < 0.05$ )—the largest re-running effect in the sample,

though estimated on only two transitions—while the re-running rate stays below half (41%): the incumbent who stands is not blocked, but most incumbents do not return. We located no press reporting on Liberian legislative turnover, so the classification rests on the literature only.

**Madagascar.** Our reading of the literature is that the first gate is partly open: parties are leader-centric rather than durable organizations (Marcus and Ratsimbaharison, 2005), candidacies are self-funded, and the political elite is concentrated and small (Razafindrakoto et al., 2024). Each presidential transition or crisis produces near-total party turnover: the 2009 coup and the transitional period that followed reset the party system for the 2013 elections, and in October 2025 the president dissolved the National Assembly under Article 60 of the Constitution. The quantitative sort says the gate is open with an advantage at both gates:  $\hat{\tau}_{\text{run}} = +0.30^{***}$ , the highest re-running rate in the sample (77.3%), and  $\hat{\tau}_{\text{win}} = +0.18$ . The two readings are reconciled by where candidacy resides: parties dissolve and reform around leaders, but individual candidates persist. Re-running seems to be a decision of the individual politician, not the party.

**Malawi.** Our reading of the literature is that Malawi’s first gate is open, and unlike Zambia the gate is not held by a gatekeeper: candidate selection is open, independents are prominent, and campaigns are self-funded. Rakner et al. (2007) traces a party system that has fragmented every electoral cycle since 1994, with new parties forming around each presidential transition (DPP in 2005, PP in 2011, UTM in 2018). The fragmentation is voter-driven: every Malawian president since 1994 has lost popularity sharply within a single term, triggering floor-crossing and party realignment. Patel and Wahman (2015) document independent candidates emerging as the largest single bloc in 2014 (52 of 193 seats); Wahman and Seeberg (2022) report that 83% of campaign resources came from candidates’ personal funds, indicating that party machines provide neither protection nor screening. The quantitative sort says closed ( $\hat{\tau}_{\text{run}} = +0.11$ , indistinguishable from zero). Incumbents are not blocked at the nomination stage, but where candidacy is open to all and self-funded, the margin of victory counts for little at the re-nomination stage, and

incumbents cannot rely on incumbency to retain seats at the polls ( $\hat{\tau}_{\text{win}} = +0.01$ ). Our individual-level data extend the prior work on the Malawi chamber: linking the 2009–2014 Hansard to the following election, we find that a member’s share of speeches carries no association with re-running or winning (Online Appendix Table A.18).

**Mauritius.** The scholarly record on Mauritian candidate selection is thin relative to the rest of the sample, and we also found no newspaper sources online. The institutional descriptions establish that candidates are selected centrally by parties and pre-electoral alliances under the block vote: alliance slates are negotiated by party leaderships, the party funds campaigns, the label carries over, and sitting members are ordinarily re-adopted (Table 2)—a partly open first gate in the literature reading. The quantitative sort says open with an advantage at both gates:  $\hat{\tau}_{\text{run}} = +0.19^{**}$ , a re-running rate of 69.9%, and  $\hat{\tau}_{\text{win}} = +0.07$ . Mauritius, the strongest parliament in our sample by the Parliamentary Powers Index (0.66), looks most like the established democracies in which incumbency is studied at the second gate.

**Nigeria.** Our reading of the literature is that Nigeria’s first gate is closed, held by local patrons. Nigeria exhibits executive control through “godfatherism”—patron-client networks in which wealthy political brokers fund and secure nominations for chosen candidates, then extract post-election rents (Albert, 2005). Critically, godfatherism operates at the candidacy stage, not the voting stage: godfathers “take over the control of political parties and impose candidates on them” (p. 82). Albert (2005) documents how godfathers determine “who participates in politics and under what conditions,” making the party nomination process—not the general election—the binding constraint on political survival. The 2003 party conventions were described as a “Naira for Naira fight” in which policy qualification was irrelevant and only godfather strength mattered. Moreover, zoning and rotation agreements mechanically prevent re-running: communities within a constituency rotate access to the seat by informal agreement (Akinola and Mosunmola, 2023). Nigeria’s average legislative tenure of 3.6 years—the shortest in the 12-country sample of Bowles and Marx (2023)—reflects a system where legislative seats rotate among elites

rather than being won through voter evaluation. The executive’s willingness to purchase legislative outcomes was on display in 2006, when President Obasanjo’s bid to amend the constitution for a third presidential term reportedly mobilized cash inducements to legislators—bidding for votes “reached 50 million naira each (about US\$400,000)”—alongside “private threats about legislators’ political futures” (Lewis, 2009, p. 177). Lewis (2009) reads the episode as emblematic: in the Fourth Republic the National Assembly “often appears as little more than a trading floor in which political elites bid on the distribution of spoils” (p. 179). The quantitative sort says partly open:  $\hat{\tau}_{\text{run}} = +0.23^{***}$  and  $\hat{\tau}_{\text{win}} = +0.14^{**}$ , but the re-running rate stays below half (46%)—the gate opens at the margin while remaining a binding constraint overall. The demand side appears episodically: in 2015, the anti-Jonathan wave drove mass defections from the PDP to the APC, with mass party defection as a *symptom* of shifting voter demand rather than an independent cause.

**Seychelles.** Our reading of the literature is that the first gate is partly open: the ruling party (SPPF, later Parti Lepep) selects candidates in a small 25-seat chamber, renomination is contested, and the party funds campaigns (Baker, 2008). The quantitative sort says closed ( $\hat{\tau}_{\text{run}} = +0.34$ , not significant—a large point estimate but the most imprecise in the sample, estimated on the smallest sample). The 2011 election is best read as institutional disruption rather than as either gate binding: the opposition SNP boycotted the legislative elections after alleging vote-buying—a protest directed at the electoral system and the ruling party, not at the performance of specific legislators. “A total of 16,447 voters—almost 32 percent—spoilt their ballots” (Bakken, 2014), producing near-total opposition turnover for reasons unrelated to individual performance. We located no news reporting on Seychellois legislative turnover, so the classification rests on the literature.

**Sierra Leone.** Our reading of the literature is that the first gate is closed, held by the central party leaderships, and the quantitative sort agrees ( $\hat{\tau}_{\text{run}} = +0.28$ , positive but not significant on a two-transition panel, with a re-running rate of 26%). Casey et al. (2021) provide the most rigorous evidence that candidate selection is disconnected from

voter preferences. In Sierra Leone, where they partnered with both major parties (SLPP and APC) to experimentally reform candidate selection, they find that “the two political parties on average selected the aspirant who ranked first among local voters 38 percent of the time, which constitutes a clear and sizeable distortion away from voter preferences” (p. 17). The consequences for incumbents are stark: “there were 25 incumbents seeking re-nomination in our sample . . . Roughly half of them secured re-nomination from their party” (p. 27). If all countries in our sample selected candidates the way Sierra Leone does—with the party choosing independently of voter preferences—one would expect exactly the null incumbency effects we estimate: an incumbent’s margin of victory is irrelevant if the party does not consider electoral strength when deciding whom to re-nominate. As Casey et al. (2021) conclude: “If party officials select candidates with little input from voters, citizens may well be perfectly enfranchised on paper—entitled to participate in free and fair general elections—but wholly irrelevant in practice, at least for partisan strongholds” (p. 35).

**Tanzania.** Our reading of the literature is that Tanzania’s first gate is closed, held by the CCM Central Committee, and the quantitative sort agrees ( $\hat{\tau}_{\text{run}} = +0.09$ , not significant, with a re-running rate of 33%). Centralized nomination control under the ruling party long predates the personalized rule of the current period. Describing the 1975 elections, Martin (1978) documents a multi-stage screening in which each district’s two official candidates were selected by the party: aspiring candidates were “summoned before the annual district conference, where they are heard speak and are questioned,” after which ranked lists “progress upwards to subcommittees of the Central Committee and finally to TANU’s National Executive Committee, which makes the final decision and announces the names of the party’s two official candidates in each constituency” (Martin, 1978, p. 109). Our digitized single-party elections give a historical illustration. Tanzania’s first competitive cycle (1965→1970) shows a sizable but imprecise incumbency effect on re-running ( $\hat{\tau}_{\text{run}} = +0.20$ , not significant), suggestive of an early advantage at the nomination stage; the effect on winning is essentially zero ( $\hat{\tau}_{\text{win}} = -0.07$ , not significant). The 1970→1975 cycle shows a likewise insignificant effect on winning ( $\hat{\tau}_{\text{win}} = +0.21$ ;

re-running is not estimable, as we were unable to locate the 1975 candidate list), and turnover exceeds 78% in both cycles. Because these single-party contests do not connect continuously to the modern multiparty series, they are excluded from the pooled Table 1 estimate but retained in Figures 3–4.

Executive gatekeeping intensified under personalized rule. Under President Magufuli (2015–2021), nomination control was centralized to an unprecedented degree. Collord (2021) documents that the CCM Central Committee “selected the second, third and even fourth runners up in at least 19 constituencies” (p. 27). In one case, “Petrobas Katambi—formerly of CHADEMA—came fourth in the Shinyanga Town primary with only 12 votes but was selected over the incumbent CCM MP, Stephen Masele, who won the primary with 152 votes” (p. 27). Magufuli was explicit: “The aim was to make sure that we get *obedient* and competent candidates” (p. 27). In total, “at least 21 CCM candidates lost their primaries but were nevertheless selected by Magufuli and the Central Committee of CCM to contest the election” (p. 36). Collord (2022) traces how post-liberalization private wealth in Tanzania created “businessmen-cum-political financiers” who challenge incumbents in party primaries; by 2015, five sitting cabinet ministers lost at the CCM nomination stage, and press reports counted “about 59 CCM MPs from Mainland Tanzania” losing nomination to new entrants in 2020.<sup>9</sup> High parliamentary activity does not protect incumbents: Tanzania averages over 1,250 written questions per year yet has one of the lowest incumbent re-running rates in the sample. Official Tanzanian parliamentary records (*Wabunge wa Tanzania na Nafasi Zao 1965–2023*, Ofisi ya Bunge) allow a turnover comparison across all 12 parliaments since 1965: aggregate turnover from the full Bunge membership list averages 66–80% per transition (91% at the 1990–1995 multiparty transition), and constituency-level turnover is higher still, consistent with the executive rotating both elected and appointed legislators as an instrument of control.<sup>10</sup>

<sup>9</sup>*allAfrica*, July 2020; *The Citizen* (Tanzania), August 2025.

<sup>10</sup>The full Bunge series (12 parliaments, 1965–2023) is plotted as the hollow diamonds of Figure 1; the membership list distinguishes constituency, special-seat, and appointed members, and turnover is high in every category.

**Uganda.** Our reading of the literature is that the first gate in Uganda is open, but open to everyone with no support to incumbents, not even to carry the party label of incumbency. With re-running rates among the highest in our sample (71–75%), Uganda appears exceptional—but the outcome is the same. Under the NRM’s “no-party” movement system (1986–2005), candidates ran on individual merit without party labels, making the field crowded and disadvantaging incumbents who could not distinguish themselves (Kasfir and Twebaze, 2009). The same study reports how the no-party system left individual legislators financially exposed and dependent on patronage: “Ninety percent of all MPs are needy,” one former minister observed, “and that is why their decisions are swayed by financial handouts” (Kasfir and Twebaze, 2009, p. 103). Reformers used the 1995 Constitution’s committee and censure powers to check the executive, until Museveni “singled out some of the reformers and campaigned against them in the 2001 elections,” after which the Seventh Parliament “was less vigorous in its opposition” (Kasfir and Twebaze, 2009, p. 105). The press quoted in the chapter captured the resulting caution: debate stayed poor “with the majority of the MPs reluctant to talk for fear of being whipped” (*New Vision*, quoted in Kasfir and Twebaze, 2009, p. 104), a pattern relevant to the speech-and-question results of Section 6. Even after the return to multiparty politics in 2005, NRM primaries are highly permeable: losing candidates routinely stand as independents. Wilkins (2021) argues that high subnational turnover serves a *regime maintenance* function: “local competition in electoral authoritarian regimes can provide an outlet for accountability politics by redirecting widespread voter frustrations away from a regime and towards expendable local politicians.”<sup>11</sup> This is a familiar mechanism in the literature on electoral authoritarianism, where managed competition and the rotation of expendable elites sustain incumbent regimes rather than constrain them (Schedler, ed, 2006; Magaloni, 2006; Levitsky and Way, 2010). The roots of executive dominance in Uganda run deep: the colonial transformation of Buganda’s historically consultative kingship into an autocratic structure, and the post-independence maintenance of the chieftaincy system through the 1960s–80s, meant that the executive controlled appointments of chiefs,

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<sup>11</sup>Afrobarometer data show 85% of Ugandans report their MPs “never” or “only sometimes” listen to constituents: Afrobarometer Dispatch AD417.

the military, and land allocation, while parliament remained peripheral to the exercise of state power (Mamdani, 1996).

The quantitative sort says closed:  $\hat{\tau}_{\text{run}} = +0.13$  is not significant and  $\hat{\tau}_{\text{win}} = -0.07$ . The by-year path adds a recent turn: the effect on re-running is zero into 2016 ( $-0.04$ ) and  $+0.32$  ( $p < 0.01$ ) into 2021 (Figure 3), so Table 2 marks Uganda Closed→Open—a first gate that was closed over most of the sample and shows signs of opening in the latest cycle. The executive led electoral design ensures that no individual legislator accumulates enough support to challenge the regime—the same logic as elsewhere, achieved through openness and lack of any institutional support for incumbents rather than restriction.

**Zambia.** Our reading of the literature is that Zambia’s first gate is closed, held by the executive, and the quantitative sort agrees: the RDD effect on re-running is essentially zero ( $\hat{\tau}_{\text{run}} = -0.02$ , insignificant). Opalo (2019) traces the institutional roots: under one-party UNIP rule (1973–1991), Kaunda “banned party primaries altogether and granted UNIP the power to nominate candidates to the legislature.” The UNIP Central Committee disqualified 25 candidates in 1973 and 26 in 1978, predominantly sitting MPs perceived as allied with the opposition or challenging presidential authority; no formal reasons were required or given for these disqualifications. Among 1973 winners, only 34.4% were incumbent MPs—a turnover rate of 65.6%, consistent with the party’s strategy of removing potential challengers through the nomination process (Baylies and Szeftel, 1984). The demand side surfaced in 1991, when the creation of the MMD became the vehicle for overwhelming popular demand for democratic change. The supply-side pattern persists into the multiparty era: Sishuwa (2024) documents Zambia’s 2016 Article 72 constitutional amendment barring MPs who defect from running in the resulting by-election restricted one important re-running pathway. Re-running rates have risen gradually as control has loosened, from 23% in 1991 to 40% in 2016, and the RDD effect on re-running in the 2016–2021 cycle is the largest positive estimate in the Zambian multiparty series ( $\hat{\tau}_{\text{run}} = +0.23$ , compared with a pre-2016 mean of  $-0.05$  across five prior cycles), though the difference is not statistically significant ( $z = 0.93$ ,  $p = 0.35$ ). The most recent cycle shows signs of a renewed tightening: Sishuwa (2025) documents administrative levers—an adoption-

certificate requirement enforced through the state-controlled Registrar of Societies, and a delimitation adding seventy new constituencies—used to reassert executive control over which candidates reach the ballot, which the framework here reads as a force that should hold re-running down.

**Zimbabwe.** Our reading of the literature is that Zimbabwe’s first gate is closed, held by the ZANU–PF Central Committee through intense intra-party deselection. Hove (2019) documents how “ZANU PF dismissed its cadres for belonging to the wrong faction.” The scale of deselection is evident: in the 2023 ZANU-PF primaries, 12 of 18 sitting MPs lost in Mashonaland Central. Safe seats become battlegrounds for nominations rather than general elections, so even comfortable past winners are routinely screened out at the candidacy stage.<sup>12</sup> The quantitative sort says partly open:  $\hat{\tau}_{\text{run}} = +0.21^{**}$ , but the re-running rate stays below half (46%)—barely-winning incumbents are more likely to stand again, yet most incumbents still do not rerun.

**Disrupting the voter-punishment equilibrium.** Where the first gate is open, the second can still bind through voters. The demand side operates through two distinct channels: low information and low expectations. Gottlieb (2016) shows experimentally that voters in Mali systematically underestimate what local governments can deliver; raising expectations through civic education increases sanctioning of poor performers. This expectations channel complements the information channel emphasized by Bowles and Marx (2023), whose model of self-fulfilling electoral pessimism—in which high turnover, low-quality candidacies, and voter punishment of incumbents reinforce one another—we introduced in Section 1. Critically, Bowles and Marx (2023) show that this equilibrium can be disrupted. Using variation in CDF allocations driven by electoral malapportionment, they find that “a doubling of the CDF allocation per voter increases reelection rates by approximately 30%” (p. 3). Constituency Development Funds give legislators direct, attributable control over local spending. Nine of our countries have adopted one:

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<sup>12</sup>Individual cases of opposition persistence under executive pressure exist—Margaret Dongo’s success as an independent in Harare South in 1995 after being expelled from ZANU-PF, and Roy Bennett’s repeated election in Chimanimani for the MDC despite government harassment, are notable examples—but these are exceptions whose prominence derives precisely from the rarity of successful resistance.

Zambia (1995), Ghana (1996), Kenya (2003), Malawi (2006), Uganda (2006), Liberia (2009), Tanzania (2009), and Zimbabwe (2009), together with Nigeria; the other eight have none. In our cross-country comparison, CDF and non-CDF countries do not differ significantly in their RDD re-running estimates (mean  $\hat{\tau}_{\text{run}} = 0.15$  for CDF countries vs. 0.22 for non-CDF countries,  $p = 0.23$ ), though with only 17 countries the comparison has very low power. The before-and-after comparison within CDF countries is similarly inconclusive (Online Appendix Figure A.18).

### 4.3 Closed-List PR and Non-Partisan Countries

The 13 closed-list PR or non-partisan countries in our extended sample hold the individual-level demand side fixed: under closed lists voters choose parties, not named individuals, so a voter cannot punish a specific incumbent, and turnover reflects the party’s decisions about list composition. This does not by itself rule out a demand-side reading. A party leadership that controls the list may still be responding to voters—dropping or demoting candidates who would drag down the party’s vote share and promoting those who appeal to the electorate—so party control of the list is not automatically executive suppression. Two features of the data push against the pure demand reading, however. First, turnover is high: Martin et al. (2023) report turnover of 35–50% in European closed-list PR systems (the Netherlands, Belgium, Spain, and Sweden), whereas African closed-list PR turnover runs from 38% to 98%, above that benchmark, and only Namibia sits consistently below 50%. Second, when we read the country-specific literature and press—the same evidence we code for the RDD sample—we find support for executive control of re-nominations rather than vote-share-maximizing selection by the party.

A few cases are illustrative. Rwanda is the clearest: the RPF won 40 of 53 directly elected seats in 2018 and 37 in 2024, yet replaced 62% of its legislators between the two parliaments—turnover that reflects the president’s choice to reshuffle the list rather than voter dissatisfaction (Reyntjens, 2011, 2013). South Africa shows the same gatekeeping in an institutionalized chamber: Barkan (2009b) reports that ANC leaders “control the rank order of their candidates on both the national and provincial lists,” so that

failure to toe the line risks “denial of renomination” or “redeployment” (pp. 208–209). Senegal’s 2007–2012 transition (89% turnover) followed Macky Sall’s defeat of Wade and the collapse of the PDS coalition; Benin’s 2015–2019 transition returned only one of 81 deputies (99%), reflecting the 2018 *Charte des partis politiques* that barred major opposition parties from the 2019 election, even as Adamolekun and Laleye (2009) document a demand-side counterpart in earlier Beninese parliaments, where re-candidacy turned on constituency service. Cape Verde, with peaceful alternation between MpD and PAICV since 1991, sits at the lower end (60%) but still above the European benchmark, and is the one clear demand-side exception. We classify all 13 extended-sample countries on the same Literature and News evidence used for the RDD sample, splitting closed-list PR (where demand is ruled out by construction) from partial-PR systems (where it is not); the press quotes underlying the classification are in Online Appendix Section A.6.

## 5 Cross-Country Correlates of the First Gate

In most of our countries the first gate binds, and there the RDD cannot say what mechanisms or who controls it. Both the supply-side re-running-suppression account (Opalo, 2019) or the demand-side self-fulfilling voter-pessimism account (Bowles and Marx, 2023) predict the low or null incumbency effects of Section 3, and a near-zero  $\hat{\tau}_{\text{run}}$  looks the same under either. The two accounts differ, however, in what they imply about cross-country variation in legislative institutions, and we use that variation to ask what governs the first gate where it binds. We emphasize at the outset that with sixteen countries these correlations are suggestive rather than a test.

Consider first formal legislative powers. Formal powers can help an incumbent both re-run and win: a legislator in a chamber with real authority over legislation, budgets, and oversight can deliver benefits to constituents and point to them at the next election. Whether this translates into votes depends on attribution. Harding (2015) shows that Ghanaian voters reward the president for road improvements—an attributable public good—but not for outcomes that cannot be traced to a specific actor, and Bowles

and Marx (2023) show that Constituency Development Funds, which give an individual legislator attributable control over local spending, raise incumbent reelection. If formal powers operate this way, more powerful legislatures should show larger positive effects of incumbency on re-running and on winning. Where that correlation is absent—as it is in Table 3—two readings remain open: a voter-punishment equilibrium in which voters sanction incumbents regardless of the chamber’s formal powers (Bowles and Marx, 2023), or executive suppression of re-running that operates whatever the powers on paper.

Consider next *de facto* oversight—giving speeches, asking formal questions, holding the executive to account. Oversight is usually attributable: a legislator who questions a minister or investigates the executive does something visible that voters could reward at the ballot, or the executive punish at the re-nomination stage. Active oversight may also be a marker of political ambition and independence. If voters hold the gate, active oversight should be uncorrelated with re-running, or positive where voters reward accountability. If instead the executive holds it, oversight is costly—legislators who challenge the executive risk being denied renomination or having their resources withdrawn—so a negative correlation between oversight and re-running would point toward executive suppression.

## 5.1 Formal Powers and De Facto Oversight

We examine cross-country variation in legislative institutional strength. We distinguish two conceptually distinct types of measures. The first captures *formal institutional powers*: the Fish and Kroenig (2009) Parliamentary Powers Index (PPI), Polity V executive constraints, plenary sitting days per year (IPU Parline), and OBS budget oversight scores. The second captures *de facto oversight behavior*: V-Dem indicators measuring whether the legislature constrains the executive (`v2xlg_legcon`), questions the executive (`v2lgqstexp`), investigates the executive (`v2lginvstp`), and grants opposition autonomy (`v2lgoppart`) (Coppedge et al., 2025), plus written parliamentary questions per year (IPU Parline).<sup>13</sup> Because the four V-Dem oversight items are noisy and overlapping, we

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<sup>13</sup>IPU written-question counts are available for only 13 countries, and a few cells are unreliable—Lesotho reports a constant placeholder, and Côte d’Ivoire and Sierra Leone appear to report on a different

also form a pre-specified oversight composite (the mean of the four z-scored items) and a variant that adds z-scored IPU written questions; the composite is our preferred summary of de facto oversight. All time-varying measures are averaged over the post-1995 period, and country-level RDD estimates use only post-1995 elections.

Table 3 reports the correlations across 16 of the 17 RDD-sample countries.<sup>14</sup> The table reports unweighted Pearson correlations as primary; the population-weighted version is reported as a robustness check in Online Appendix Table A.15. Inference is by permutation (10,000).<sup>15</sup> Two findings emerge: the formal-power correlations are fragile to the weighting choice, whereas the negative association between de facto oversight and re-running is the one pattern stable across both weightings.

**Formal powers and re-running.** The PPI—which measures constitutional authority over legislation, budgets, and oversight—is not robustly related to the RDD re-running effect. Population-weighted, the correlation is large and positive ( $r_w = +0.68$ ,  $p < 0.10$ ); unweighted, it falls to a small and insignificant value ( $r = +0.17$ ). The weighted estimate is driven by a few large countries (Nigeria, Tanzania, and Kenya), suggestive of parliamentary strength being correlated with country size—intuitive if large countries require many regions to be represented and to bargain in the chamber—rather than of a general relationship between formal powers and re-running. The reweighting of the same 32 PPI powers proposed by Chernykh et al. (2017) shows the same weighting sensitivity at a higher level (weighted  $r_w = +0.78$ ; unweighted  $r = +0.57$ ). The remaining formal-power measures (Polity V executive constraints, plenary sitting days, and the Open Budget Survey score) are individually insignificant and change sign across weightings. The most one can say is that formal constitutional authority does not robustly predict the re-running effect, which is itself informative: it is hard to reconcile with a strong version

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definition or unit—so we apply a single ex-ante rule that drops the three defective written-question cells (Lesotho, Côte d’Ivoire, Sierra Leone) in the “excl. defective cells” row of Table 3; the negative correlation with the re-running effect is unchanged ( $r = -0.54$ ,  $p < 0.10$ , unweighted).

<sup>14</sup>Mauritius is excluded because its incumbency effect is estimated with a multi-member block-vote design (three winners per mainland constituency) rather than the single-winner winner-versus-runner-up margin used for the other 16 countries, and so is not part of the post-1995 correlation set (Online Appendix Table A.1).

<sup>15</sup>No individual correlation survives a Bonferroni correction at the 5% family-wise level.

Table 3: Legislative Power, Re-Running, and Turnover: Cross-Country Correlations (Post-1995, Unweighted)

| Measure                                                | RDD Effects |         | Raw Rates  |          |                       |
|--------------------------------------------------------|-------------|---------|------------|----------|-----------------------|
|                                                        | Re-running  | Winning | Rerun Rate | Turnover | Turnover <sup>‡</sup> |
| <i>Formal Institutional Powers</i>                     |             |         |            |          |                       |
| Parl. Powers Index (PPI)                               | +0.17       | −0.41   | +0.29      | −0.33    | —                     |
| Rewighted PPI (Chernykh)                               | +0.57**     | −0.19   | +0.37      | −0.46*   | —                     |
| Polity Exec. Constraints                               | +0.03       | −0.26   | +0.15      | −0.06    | −0.42**               |
| Plenary Sitting Days/Year                              | −0.15       | −0.31   | −0.34      | +0.32    | —                     |
| excl. defective cells                                  | −0.22       | −0.25   | +0.05      | −0.06    | —                     |
| OBS Budget Oversight                                   | −0.20       | −0.05   | −0.07      | +0.20    | —                     |
| <i>De Facto Oversight</i>                              |             |         |            |          |                       |
| Oversight Composite (V-Dem)                            | −0.16       | −0.15   | +0.07      | −0.09    | −0.11                 |
| Oversight Composite (V-Dem + IPU)                      | −0.73***    | −0.30   | −0.18      | +0.21    | —                     |
| V-Dem Constrains Exec.                                 | −0.19       | −0.26   | +0.05      | −0.10    | −0.13                 |
| V-Dem Questions Exec.                                  | −0.11       | +0.06   | −0.06      | +0.06    | −0.13                 |
| V-Dem Investigates Exec.                               | −0.30       | −0.23   | +0.11      | −0.11    | −0.05                 |
| V-Dem Opp. Autonomy                                    | +0.02       | −0.15   | +0.18      | −0.19    | −0.08                 |
| Written Questions/Year                                 | −0.57**     | −0.26   | −0.13      | +0.14    | —                     |
| excl. defective cells                                  | −0.54*      | −0.14   | −0.29      | +0.35    | —                     |
| <i>Clientelism, Corruption, and Elite Contestation</i> |             |         |            |          |                       |
| V-Dem Clientelism Index                                | −0.10       | +0.41   | −0.04      | +0.18    | +0.43**               |
| V-Dem Party Linkages (G-N)                             | −0.23       | −0.36   | +0.02      | −0.10    | −0.41**               |
| V-Dem Political Corruption                             | +0.06       | +0.29   | +0.12      | +0.03    | +0.25                 |

Pearson correlations across 16 of the 17 RDD-sample countries (Mauritius, estimated with a multi-member design, is not in the post-1995 correlation file; see the sample-construction table in the Online Appendix). Unweighted Pearson correlations. Inference: permutation p-values (10,000 permutations of the outcome vector, fixed seed); \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . Of the 69 correlations, 0 survive a Bonferroni correction at the 5% family-wise level. The Oversight Composite is the pre-specified mean of z-scored V-Dem oversight items (constrains, questions, investigates, opposition autonomy); the V-Dem + IPU variant adds z-scored written questions ( $N = 13$ ). The ‘excl. defective cells’ rows apply a single ex-ante rule: drop IPU country cells with documented reporting defects (Côte d’Ivoire and Sierra Leone unit/definition mismatch; Lesotho constant placeholder; Madagascar zero-plenary years). The Turnover<sup>‡</sup> column extends the turnover sample with 13 closed-list PR / non-partisan countries; a dash (—) indicates the measure is not observed for the added countries. The population-weighted counterpart is in the Online Appendix. Post-1995 elections only (V-Dem and Polity averaged over the post-1995 period).

of the demand-side prediction that attributable formal powers should raise incumbent retention, as Bowles and Marx (2023) document for the CDF case.

**Oversight activity and re-running.** De facto oversight tells a more stable story, and it is the one cross-country pattern that survives the weighting choice. The oversight composite that adds IPU written questions correlates negatively with the RDD re-running effect under both weightings ( $r = -0.73$ ,  $p < 0.01$  unweighted,  $N = 13$ ;  $r_w = -0.85$ ,  $p < 0.01$  weighted), as does IPU written questions on its own ( $r = -0.57$ ,  $p < 0.05$  unweighted;  $r_w = -0.67$ ,  $p < 0.10$  weighted).<sup>16</sup> The V-Dem-only composite is small unweighted ( $r = -0.16$ ) and negative weighted ( $r_w = -0.46$ ), so the result is carried by the behavioural written-questions measure rather than by the expert-coded V-Dem items. Where legislatures more actively question the executive, barely winning is associated with a *lower* probability of re-running. Note the shift in level: this is a relationship between a *chamber's* question output and its country's re-running effect; whether the same relationship holds for the individual legislator—whether the member who asks the questions is the member who does not return—is taken up with the Hansard data in Section 6. Barkan (2009a) offers a qualitative reading: legislators who challenge the executive “are also targeted for political extinction” through denial of party nominations or financing of opponents (p. 19).

The runner-up RDD provides a placebo for this pattern. If oversight correlates with the re-running effect because it operates on the office-holding margin—active office-holders are screened out at re-nomination—then it should *not* correlate with the runner-up effect at the rank-2-versus-rank-3 margin, where no office is held. That is what we find: across the same countries, written questions correlate with the runner-up re-running effect at  $r = +0.15$  (95% CI  $[-0.44, +0.65]$ ) and the oversight composite at  $r = +0.06$ , both indistinguishable from zero (Online Appendix Table A.13). The placebo is reassuring but underpowered.

We cannot, from cross-country correlations, identify why oversight and re-running

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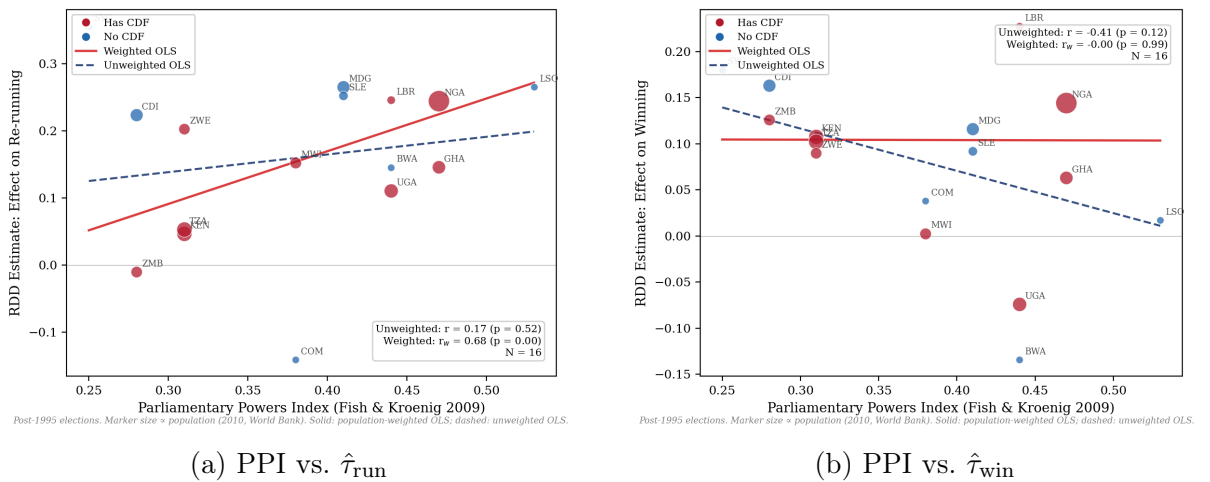
<sup>16</sup>The negative sign is unchanged when the IPU cells with documented reporting defects are dropped (the “excl. defective cells” row of Table 3).

move together. Under re-running suppression by the executive (supply-side), executives deny renomination to legislators who challenge them. But we cannot rule out able legislators who invest in oversight growing disillusioned with a powerless chamber and exiting voluntarily; or legislators who already expect not to return asking questions because it is the only tool left. Only the first is squarely supply-side; the latter two are compatible with the demand-side account.

A separate literature attributes high turnover to clientelism. Golden and Nazrul-laeva (2023) find that clientelism predicts lower legislative retention across democracies, whereas Wiebrecht (2025) finds no such relationship in 130 nondemocratic regimes. Our sample sits closer to the latter: the raw re-running rate is essentially uncorrelated with V-Dem clientelism, and although the RDD re-running effect correlates positively with clientelism and with the V-Dem political-corruption index under population weighting ( $r_w = +0.81$  and  $+0.77$ ), both fall to essentially zero when unweighted, so we treat these associations as weighting-dependent and not robust (Table 3; weighted values in On-line Appendix Table A.15). In any case, clientelism and corruption operate upstream of nomination control rather than as a third channel competing with supply and demand.

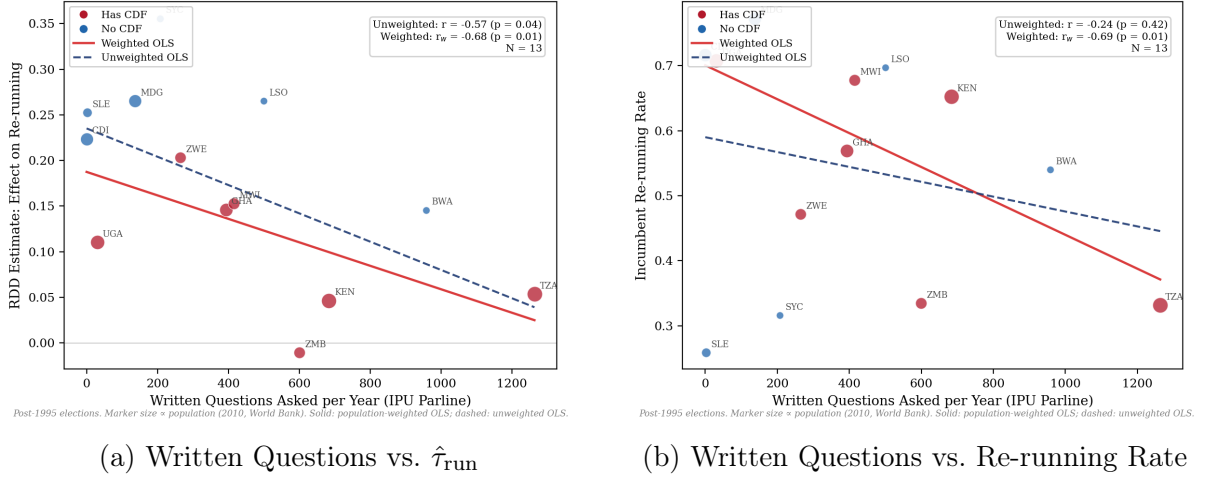
Figures 5–7 display these relationships.

Figure 5: Formal Legislative Authority (PPI) and RDD Incumbency Effects



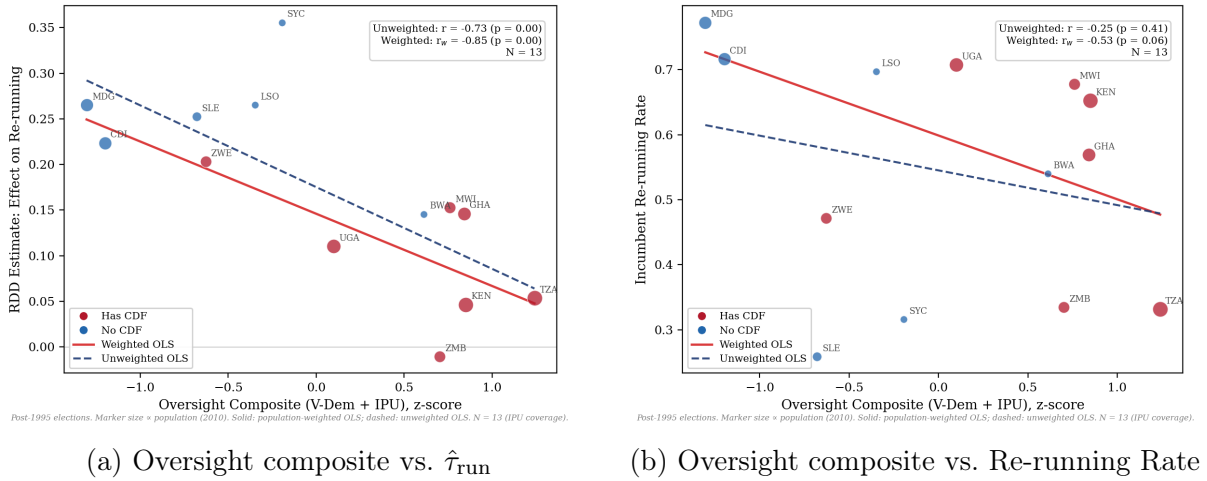
*Notes:* Parliamentary Powers Index (Fish and Kroenig, 2009) vs. RDD estimates for re-running (left) and winning (right). Marker size proportional to 2010 population. Red: countries with CDFs. Blue: without. Solid line: population-weighted OLS; dashed line: unweighted OLS. Post-1995 elections.

Figure 6: Legislative Oversight Intensity (IPU Written Questions) and Incumbency Effects



*Notes:* Written parliamentary questions per year (IPU Parline) vs. the RDD re-running effect (left) and the raw incumbent re-running rate (right). Marker size proportional to 2010 population. Red: countries with CDFs. Blue: without. Solid line: population-weighted OLS; dashed line: unweighted OLS. Post-1995 elections.  $N = 13$  countries with IPU data.

Figure 7: De Facto Legislative Oversight (V-Dem + IPU Composite) and Incumbency Effects



*Notes:* The horizontal axis is the *oversight composite*—the mean of five z-scored measures of how actively the legislature scrutinizes the executive: the four V-Dem oversight items (constrains the executive, questions the executive, investigates the executive, and opposition-party autonomy) together with IPU written parliamentary questions per year. Higher values denote more active oversight. The composite is defined for the 13 countries with IPU written-question coverage. The vertical axis is the RDD effect on re-running,  $\hat{\tau}_{run}$  (left panel), and the raw incumbent re-running rate (right panel). Marker size is proportional to 2010 population; red markers are countries with constituency development funds (CDFs), blue without; the solid line is the population-weighted OLS fit and the dashed line the unweighted fit. Post-1995 elections.

The contrast between formal powers and de facto oversight echoes a recurring pattern in African constitutionalism: post-1990 reforms formally empowered parliaments, but the executives who oversaw these transitions retained de facto control over nominations and resources (Fombad, ed, 2016, ch. 1). Next we explore how individual parliamentary activity is linked to re-running and winning re-election.

## 6 Individual Activity in Parliament

The evidence in Section 5.1 is measured at the level of the parliament: assemblies that file more written questions have lower re-running rates. We now turn to the individual legislator. The question is whether a member’s own activity in the chamber helps clear that gate, i.e., re-run and, ultimately, win re-election. Being active in parliament is associated with re-running, but the effect is weak in Africa when compared to the UK. Moreover, what is rewarded differs: visibility (speech) in Africa, oversight (questions) in the UK. For eight countries (Ghana, Kenya, Mauritius, Nigeria, Tanzania, Uganda, Zambia, and Zimbabwe) we build, from the verbatim record of debates (Hansard), two measures of each member’s activity over the parliament: the member’s share of all floor speeches, and the member’s share of all formal questions to government; for Malawi we add a speech measure from the debate data of Wahman et al. (2021). Our construction follows the now-standard approach in the legislative-debate literature: speakers are matched to the certified roster of winners by name, constituency, and title (Wahman et al., 2021; Kronborg, 2025), activity is counted from the verbatim record (Bäck et al., eds, 2021), and shares are expressed in percentage points of the chamber total so that they are comparable across the two activities and across parliaments of different size and length.<sup>17</sup> We relate each measure to the member’s probability of re-running and of winning at the next election, with election-year fixed effects. There is no estimation model beyond this: the exercise matches names to speeches and to questions, counts them, and reports

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<sup>17</sup>Construction details, match rates, and the treatment of silent winners are in Online Appendix Section A.8. For the United Kingdom we use the per-member counts published by the UK Parliament. The estimation sample is the full set of constituency winners, including members who never spoke or never asked a question.

OLS regressions of the two outcomes on the resulting activity shares. Two caveats apply throughout. First, the African ‘questions’ are measured from speeches in parliament (in Ghana, both oral and written questions are recorded), whereas the United Kingdom measure counts questions formally put forward in written form. Second, members are matched to the Hansard record by name, with match quality that varies across countries, and members differ in ambition and ability in ways we do not observe. All estimates in this section are descriptive associations, not causal effects, and—as we stress below—the direction of any association can run either way.

**Previous electoral results and speech.** Kronborg (2025) shows, for the National Assembly of Zambia, that members who won by a larger margin speak *more*, reading floor time as an audition for ministerial office rather than as a vote-seeking activity. We reproduce his design across our Hansard countries (Online Appendix Table A.19). Our Zambia speech panel is itself drawn from Kronborg (2025), so this is a replication on his data: the Poisson coefficient of speeches on the win margin is  $+0.008$  ( $p < 0.01$ ), matching his estimate. The positive sign also appears in Ghana and Uganda. In Tanzania the association is *negative* ( $-0.005$ ,  $p < 0.01$ ), the pattern Kronborg (2025) hypothesizes but rejects for Zambia. In the remaining countries, several with small samples, it is not distinguishable from zero. An alternative reading of the positive association is about security rather than audition: members who won by larger margins may feel more secure, perceive less risk of executive retaliation, and therefore speak more—a reading consistent with the suppression account of Section 4.

**Speeches and subsequent re-running and winning.** Speaking is the most basic form of parliamentary participation, and a plausible marker of a legislator’s effort, competence, and willingness to remain in politics. The share of speeches is positively associated with both re-running and winning in most countries (Online Appendix Table A.18). The coefficient on re-running is 0.09 in Uganda ( $p < 0.01$ ), 0.073 in Tanzania ( $p < 0.01$ ), 0.052 in Ghana ( $p < 0.05$ ), 0.043 in Kenya ( $p < 0.01$ ), and 0.082 in Zambia ( $p < 0.10$ ), with comparable magnitudes for winning. The reading is straightforward: more active legisla-

tors are more likely to seek, and to secure, re-election. This mirrors evidence from established democracies that visible legislators are rewarded (Kellermann, 2013; Marcinkiewicz and Stegmaier, 2019; Louwerse and van Vonna, 2022) and that legislative service builds careers (Cirone and Van Coppenolle, 2018). Malawi is the exception: there the share of speeches is unrelated to either outcome (Online Appendix Table A.18). Wahman et al. (2021) construct and analyze the Malawi Hansard to study which members participate in debate and on what topics—a descriptive account of who speaks—and Frantzeskakis et al. (2021) likewise use the Malawi debate record to describe patterns of participation. Neither links that activity to electoral survival. Our contribution is to make that link: we relate a member’s speech to the member’s probability of re-running and of winning, and in Malawi we find no association.

**Questions versus speeches.** The contrast between the two activities is our distinctive measure: neither Kronborg (2025) nor Frantzeskakis (2023) separates parliamentary questions from speeches, yet questions are the key instrument of oversight in the UK Parliament. Separating the two is possible for only a few parliaments, because it requires a Hansard that tags or structurally separates formal questions to government from ordinary debate speeches. The Hansards of Ghana, Kenya, Mauritius, Tanzania, and Zambia print formal questions under their own heading (in Ghana, oral and written questions on a numbered order paper), so the two activities can be counted apart; the records we hold for Nigeria, Uganda, Zimbabwe, and Malawi do not separate questions from debate, so for those four we measure speech only. The two text types differ in form. A formal question to government is a short, addressed request for information or action—it names the minister or department responsible, states the matter at issue, and asks the executive to confirm, explain, or supply figures, often with a request to lay documents before the house. A floor speech is a longer, discursive intervention in debate—a member arguing a position on a bill, motion, or constituency matter, addressed to the chamber rather than to a single minister, and not structured around a request for an answer. Table 4 places the share of questions next to the share of speeches. In our African parliaments the share of questions predicts re-running in Ghana (0.07,  $p < 0.01$ ), Zambia (0.04,  $p < 0.01$ ),

and Mauritius (0.04,  $p < 0.05$ ), but not in Tanzania or Kenya; and it predicts winning at the five-percent level in no African country.<sup>18</sup> In the United Kingdom the weight of the two activities is the opposite: questions matter more than speeches. The share of questions predicts both re-running (0.10,  $p < 0.01$ ) and, especially, winning (0.20,  $p < 0.01$ ), and more strongly than speeches, whose share is not statistically significant; in our African parliaments it is speech that carries the association and questions that carry none.<sup>19</sup> Table 4 also reports the four corpus countries with speeches but no separately coded question record (Nigeria, Uganda, Zimbabwe, and Malawi). Their speech returns are positive everywhere except Nigeria, where both estimates are negative.

The association between a member’s share of speeches and later re-running and winning is open to two readings. It is compatible with voters rewarding active members of parliament; it is equally compatible with the executive rewarding members who are active in supporting it. Separating speeches from formal questions is a check on which reading fits: the two activities should behave alike if voters reward visible effort, but can come apart if the executive rewards loyal participation. They do come apart. Asking questions does not track winning: the share of questions carries no association with winning in any African parliament, with a marginal exception in Ghana; in Tanzania the sign is negative; and in Kenya, Mauritius, and Zambia it is indistinguishable from zero—even though in Zambia and Mauritius questions are associated with higher re-running. Note that Mauritius’s first gate is relatively open (Section 4), so a re-running return to questions there is consistent with a chamber where oversight is not screened out at nomination. A formal question to government is an act of public oversight over the executive, and the United Kingdom estimates are consistent with voters rewarding that oversight at the ballot; in our African parliaments the activity associated with winning is general speech, not oversight—consistent with Ghanaian citizens’ stated priorities, which

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<sup>18</sup>In Ghana—the largest panel, covering three parliaments—the association with winning is marginal (0.04,  $p < 0.10$ ) and smaller than the speech coefficient; it disappears once the share of speeches enters the same regression, whereas the speech coefficient is unchanged.

<sup>19</sup>Only about a third of members table any formal question (Ghana 33%, Zambia 34%; Kenya 64%, Tanzania 73%). This reflects genuinely low question-asking, not missing data: speaker coverage of the speech record is high (e.g., Ghana 83%, Zambia  $\approx 100\%$  of winners matched), and members who never ask a question enter the estimation sample as true zeros.

Table 4: Share of Questions versus Share of Speeches, and Subsequent Re-Running and Winning

| Country            | Ran $t+1$            |                      | Won $t+1$            |                      | $N$  |
|--------------------|----------------------|----------------------|----------------------|----------------------|------|
|                    | Questions            | Speeches             | Questions            | Speeches             |      |
| Ghana              | +0.070***<br>(0.017) | +0.052**<br>(0.021)  | +0.044*<br>(0.025)   | +0.071***<br>(0.021) | 703  |
| Kenya <sup>†</sup> | -0.035<br>(0.053)    | +0.043***<br>(0.012) | +0.079<br>(0.058)    | +0.088***<br>(0.021) | 290  |
| Malawi             | n.a.                 | +0.032<br>(0.037)    | n.a.                 | +0.027<br>(0.045)    | 193  |
| Mauritius          | +0.036**<br>(0.016)  | +0.055<br>(0.034)    | +0.023<br>(0.017)    | +0.052<br>(0.037)    | 123  |
| Nigeria            | n.a.                 | -0.017<br>(0.023)    | n.a.                 | -0.001<br>(0.022)    | 663  |
| Tanzania           | -0.045<br>(0.036)    | +0.073***<br>(0.027) | -0.065*<br>(0.035)   | +0.037<br>(0.030)    | 1031 |
| Uganda             | n.a.                 | +0.092***<br>(0.020) | n.a.                 | +0.134***<br>(0.025) | 1493 |
| Zambia             | +0.041***<br>(0.014) | +0.080*<br>(0.042)   | +0.024<br>(0.017)    | +0.082**<br>(0.040)  | 296  |
| Zimbabwe           | n.a.                 | +0.076<br>(0.048)    | n.a.                 | +0.086*<br>(0.048)   | 208  |
| United Kingdom     | +0.095***<br>(0.025) | +0.023<br>(0.064)    | +0.204***<br>(0.036) | +0.102<br>(0.073)    | 1949 |

*Notes.* Each cell is the coefficient from a separate OLS of the outcome on the legislator's *share* of the chamber's questions (resp. speeches) that parliament, with parliament fixed effects; robust standard errors in parentheses. The share is in percentage points. The estimation sample is all constituency winners, including those who never spoke or asked a question. Speaker resolution follows Kronborg (2025) and Wahman et al. (2021) (name + constituency + title, validated against the constituency record); Tanzania and Kenya use the upgraded within-corpus match. The United Kingdom rows use House of Commons winners of the 2015, 2017, and 2019 general elections. "n.a." marks the four corpus countries with no separately coded question record (speeches only); Malawi uses the Wahman et al. (2021) debate data. <sup>†</sup> Kenya's question measure is thin: floor question time was curtailed after the 2013 constitution. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

place constituency-related activity above parliamentary work (Ofosu, 2024). Kenya is the clearest example: a member’s speech is associated with re-running and winning, but questions are not; Tanzania shows the same split, the two countries studied by Opalo (2017, 2019).<sup>20</sup>

## 7 A Proposal for Electoral Monitoring: Re-Running Rates as an Early-Warning Indicator

The analysis above shows that incumbent re-running rates encode information about the political environment facing legislators. Rates reflect the balance between executive control of nominations, institutional autonomy, and electoral competitiveness—forces that operate before a single ballot is cast. This makes re-running rates a natural early-warning indicator for election monitoring.

First, we establish a historical baseline for each country: the average incumbent re-running rate, the RDD effect of winning on re-running ( $\hat{\tau}_{\text{run}}$ ), and the distribution of these quantities across electoral cycles. When a new election approaches and the candidate list is certified, we can compute the current re-running rate and compare it to the baseline. A sharp decline—fewer incumbents seeking re-election than historical patterns predict—warrants investigation. Is it driven by a single party (suggesting executive pressure on nominations)? By a single region (suggesting localized political pressure)? By both winners and losers equally (suggesting a broader institutional shift)?

The monitoring approach developed here puts into practice for all countries in our sample the suggestion for Malawi present in De Magalhães and Spigel-Sinclair (2021). They

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<sup>20</sup>We also scored tone. Applying the Lexicoder Sentiment Dictionary (Young and Soroka, 2012) to every speech in the English-language corpora (Nigeria, Kenya, Mauritius), a member’s mean sentiment—and the tone of executive-directed passages—carries no association with re-running or winning in any of the three countries. We did not pursue tone further, for three reasons. Formal questions are institutionally templated (“asked the Minister whether...”), so dictionary valence largely reflects the subject matter of the question (shortages, disease, insecurity) rather than its aggressiveness toward the government. The construct relevant to the suppression reading is executive-directed criticism, a matter of target and stance that valence dictionaries do not measure. And the Tanzanian record—the central case for the questions—speeches split—is in Swahili; multilingual sentiment approaches exist (Proksch et al., 2019) but require language-specific validation beyond our scope. On the evidence we have, the margin that matters is the volume of oversight, not its tone.

show that the absence of change in RDD estimates across electoral cycles is consistent with the 2019 election in Malawi being free and fair. Because we now have cross-country baselines, deviations can be assessed not only relative to a country’s own history but also relative to the broader African distribution.

**Validating the indicator.** We validate the indicator against an external benchmark of election quality: the V-Dem clean-elections index and its components—a free and fair contest run by the election management body, freedom from opposition intimidation, and genuinely multiparty competition (Coppedge et al., 2025).<sup>21</sup> The rule of thumb is deliberately simple: for each election we compute the incumbent (winner) re-running rate—the share of the previous election’s winners who stand again—and flag any election whose rate falls below the country’s baseline (its previous election, or the mean of its prior elections).

Two correlations make the case. First, the *level* of incumbent re-running tracks election quality: across the 88 country-elections for which both are measured, the re-running rate is positively correlated with the clean-elections index ( $r = +0.31$ ,  $p < 0.01$ ), and the correlation is the same within countries ( $r = +0.30$ , estimated with country fixed effects); Appendix Figure A1 plots the relationship for each country separately. Second, a *drop* in re-running below the country’s own baseline is positively but imprecisely associated with deterioration in the nomination-relevant components—free-and-fair, opposition-intimidation, and multiparty ( $r \approx +0.18$ , not statistically significant)—though not with the overall composite, a pattern consistent with the indicator capturing manipulation at the candidacy stage rather than on polling day. Figure 8 shows both. As a flag the drop is specific but not sensitive: at a 10–15 percentage-point threshold it rarely fires in clean elections but misses elections whose manipulation is concentrated at the count. The RDD *effect* on re-running, by contrast, carries no association with election quality; the deployable and validated signal is the re-running *rate*, readable from

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<sup>21</sup>Full construction, the threshold and baseline grid, and component-by-component results are in the replication package. Two further benchmarks—hand-coded international observer reports and the NELDA and Perceptions of Electoral Integrity datasets (Hyde and Marinov, 2012; Norris et al., 2013)—are left for a companion robustness exercise.

the certified candidate list before polling, not the RDD estimate.<sup>22</sup>

The country cases make the pattern concrete. Tanzania 2020 is the clearest: re-running into the 2020 election fell to 10%, a 41-percentage-point drop from 2015 and the lowest in the Tanzanian series, while the V-Dem free-and-fair score for that election collapsed; roughly thirty opposition parliamentary candidates were kept off the ballot at nomination, and the ruling party imposed candidates over primary winners (Section 4; Paget, 2021; Collord, 2021). Zimbabwe 2023 fires on the level rather than a sharp drop: persistently low re-running (40%) coincided with an election the EU observation mission judged “marked by... the lack of a level playing field” (European Union Election Observation Mission, 2023). Ghana 2016 is the complementary case—stable, high re-running (59%) alongside an election observers called “credible, transparent and inclusive” (Commonwealth Observer Group, 2017), and the indicator does not fire. Uganda 2021 is the instructive failure: high re-running (70%) coincided with a violent, low-quality election, because executive control there operates through an open but unsupported field rather than re-nomination suppression (Section 4). The indicator therefore flags one specific mechanism, must be read as a deviation from a country’s own baseline, and requires corroborating evidence on who exited and why.

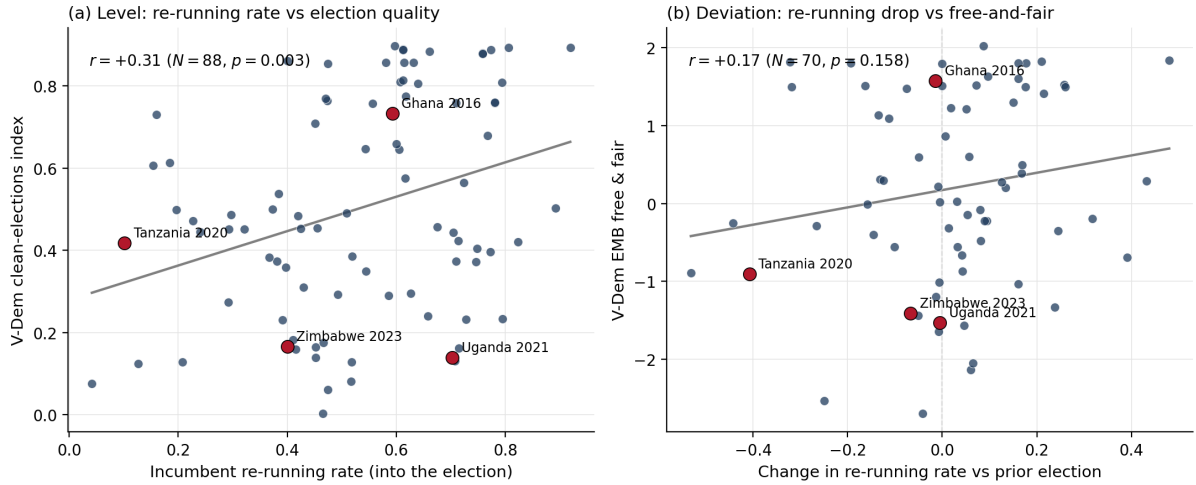
The approach has three attractions: it is *timely* (re-running rates can be computed from the certified candidate list, before observers typically arrive, by which point manipulation of candidacy has already occurred); *data-driven* (the baseline comes from the country’s own prior elections); and *transparent* (a comparison of current to historical rates is easy to communicate to observers, journalists, and civil society).

Two limitations qualify the proposal. The by-year estimates that would form each baseline are noisy (Section 3), so distinguishing a genuine deviation from sampling variation requires more electoral cycles than most countries provide; and the indicator is not

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<sup>22</sup>A rule of thumb based on the re-running rate alone is sufficient: adding the RDD effect does not help. We tested a hierarchical rule that consults  $\hat{\tau}_{\text{run}}$  only where the re-running rate is already low—flagging an election only if the rate is low *and*  $\hat{\tau}_{\text{run}}$  is also low—and it does not improve on the rate alone. Within the low-re-running subsample  $\hat{\tau}_{\text{run}}$  is uncorrelated with election quality, the two-stage flag trades away sensitivity for little precision, and adding  $\hat{\tau}_{\text{run}}$  and its interaction with the rate to a logit for a lower-quality election does not improve fit (likelihood-ratio  $p = 0.82$ ). The RDD effect, estimated on a few hundred close races per election, is too noisy to add information beyond the rate.

Figure 8: Incumbent Re-Running and Election Quality



*Notes:* Each point is a country-election in the RDD sample. Panel (a): the incumbent (winner) re-running rate into the election against the V-Dem clean-elections index (*v2xel\_frefair*). Panel (b): the change in the re-running rate relative to the country's previous election against the V-Dem election-management-body free-and-fair score (*v2elfrfair*); the vertical dashed line marks no change. Gray line: OLS fit. Red markers label the cases discussed in the text.

one-sided—a sharp *drop* can signal executive pressure on nominations, but Uganda shows *high* re-running coexisting with executive control through an open but unsupported field (Section 4), so deviation from a country's own baseline, not the level, is the informative object, and corroborating evidence on who exited and why remains essential. We propose a public online tool that, as each certified candidate list is published ahead of polling, computes re-running rates and flags unusual deviations from the country's historical baseline and the broader African distribution.

Zambia illustrates how the indicator applies out of sample. The country's winner re-running rate rose gradually from 23% in 1991 to 40% in 2016, which our framework reads as the post-2001 loosening of executive control over candidate adoption once the ruling party lost its commanding majority (Section 4). Contemporary scholarship argues that the executive has since moved to reassert that control through administrative gatekeeping over which candidates are cleared to stand (Sishuwa, 2024, 2025); where such tightening succeeds, the indicator predicts re-running should fall back below the rising trend. Computing the re-running rate from a certified candidate list is exactly the pre-polling test the tool prescribes, and it is the natural way to evaluate whether the tightening has

bitten. A single case is a proof of concept, not a validation of the approach.

## 8 Conclusion

This paper provides the first systematic estimates of the effect of winning a legislative seat on re-running across African legislatures, together with the first runner-up RDD estimates for the continent, using newly digitized election results and individual-level candidate tracking across 17 countries and 86 elections.

Barely winning a legislative election has a modest effect on re-running and little effect on winning the next election—in sharp contrast with the US and the UK, where incumbents clear the first gate, re-nomination, almost automatically and the incumbency advantage is felt at the second gate, the vote. In African legislatures the first gate often binds. Two theories account for this: Opalo (2019)’s supply-side account, in which the executive denies re-nomination or withholds the resources to run again, and Bowles and Marx (2023)’s demand-side equilibrium, in which voter pessimism about politicians is self-fulfilling. We expand Opalo’s analysis of Kenya and Zambia and Bowles and Marx’s 12-country study to 17 countries with RDD, and compare with the US and the UK.

To say what governs the first gate, we use cross-country variation in legislative institutions, individual-level parliamentary activity, and country-case evidence. Exercising oversight is associated with lower re-running at the level of the chamber—countries whose parliaments file more written questions have smaller re-running effects—consistent with executive retaliation against active legislators; the runner-up placebo, though underpowered, supports reading the institutional correlations as operating on the office margin rather than the informational margin. At the individual level, the descriptive associations are broadly consistent with this reading: a legislator’s share of speeches is associated with re-running and re-election in most countries, while formal questions to government carry no association with winning in any African country in our panel.

The most actionable implication of these findings concerns where election monitoring effort is directed. In most of our countries the binding constraint on legislative careers

operates at the nomination stage, not on polling day: whoever holds the first gate—a gatekeeper or the cost of standing again—has done the work before observers arrive. Election-day observation works where it is applied: Ofosu (2019) finds that Ghanaian legislators elected from intensively monitored constituencies were substantially more responsive over the following term. Note that observation also moves what it does not remove: Asunka et al. (2019) show that Ghanaian parties relocated fraud and violence to polling stations without observers. Neither effect reaches a nomination decision taken months earlier. International election observers and domestic monitors typically concentrate resources on the vote count; they should reallocate effort upstream, toward party primaries and candidate selection.

Constitutional and party-level reforms are longer-term complements. Where parliaments have formal authority, the cross-country patterns in Section 5 suggest incumbents invest more in seeking reelection, though only in the population-weighted model. Reforms that protect incumbents’ right to re-run, mandate competitive primaries, assign committees by seniority, or grant constituency-level resource control that grows with tenure would all raise the value of holding office and reduce executive discretion over re-candidacy. Without such reforms, the cycle persists: high turnover sustains voter pessimism, and pessimism in turn raises turnover.

These reforms speak to a longer time horizon than a cross-sectional design can capture. Barkan (2009a) argues that African legislatures are gradually “becoming institutions ‘that matter’ ... still weak, but increasingly significant” (p. 1), and that this institutional development “does not occur at the earliest stages of democratization, but later—five to ten years into the process” (p. 16), as reform coalitions accumulate the experience and capacity to check the executive. Kenya, whose National Assembly built genuine budgetary and oversight autonomy after the ruling party’s near-loss of its majority in 1997, is the leading case (Barkan and Matiangi, 2009). That accumulation runs through individual careers, and careers run through re-running: where re-running is held low, no reform coalition can establish itself, and the slow strengthening Barkan describes never begins. Whether legislatures strengthen over time precisely where the re-running penalty

on incumbents has eased is the most direct test of the mechanism, and the natural next step.

Future research should pursue several directions. First, the mechanisms we identify—executive gatekeeping and demand-side voter punishment—deserve direct empirical investigation using within-country variation in institutional reforms. The policy instruments available to individual legislators are a natural place to start: Bowles and Marx (2023) show that Constituency Development Funds, which give members attributable control over local spending, raise incumbent reelection, so the staggered adoption and expansion of such funds offers within-country variation for identifying whether stronger legislator-level tools open the first gate.

A second direction concerns what the vote can reward at all. Müller-Crepon and Bormann (2026) estimate, from dyadic comparisons of Afrobarometer respondents across 28 countries, that coethnicity raises the probability that two voters support the same party by 17 percentage points, at least five times the effect of any other cleavage they measure, and that the pattern holds across institutionally diverse countries. Their design speaks to party choice rather than to individual records, so the implication for incumbency is indirect. Where the vote is organized around group alignment, however, the return to an individual legislator’s performance at the second gate is correspondingly limited, and how much of the cross-country variation in re-running that accounts for is an open question.

A final direction for future work concerns structural origins. Mamdani (1996) traces African executive dominance to the colonial bifurcated state, and the European historical literature links the rise of parliaments to the executive’s need for legislative consent to tax (North and Weingast, 1989; Tilly, 1990; Dincecco et al., 2011; De Magalhães and Giovannoni, 2022). Foreign aid and resource revenues that enter the executive budget without legislative appropriation may remove the drive for legislative empowerment (Bräutigam and Knack, 2004; Moore, 2004); whether these revenue sources explain cross-country differences in re-running is a question we leave to future research.

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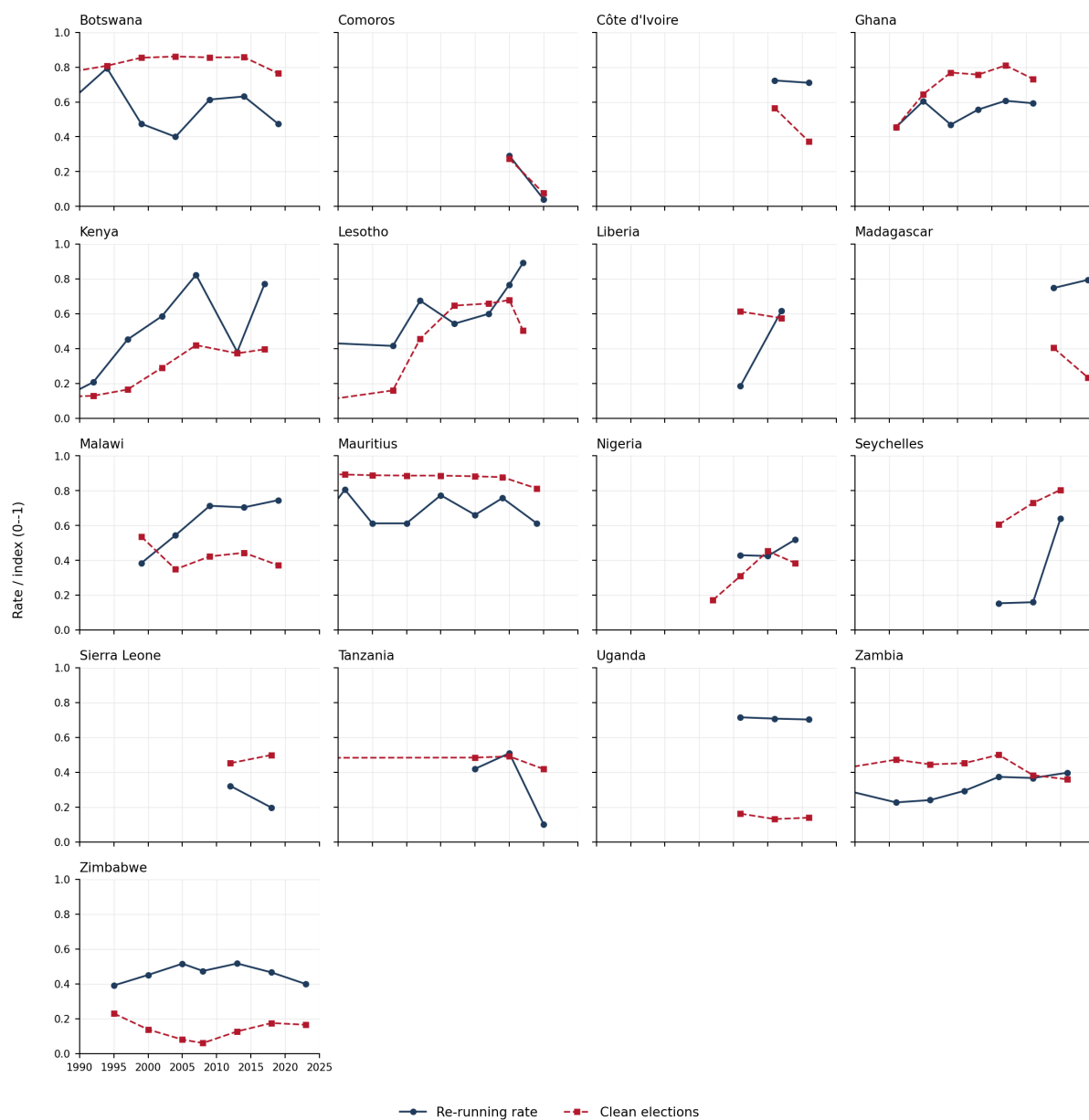
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# Appendix

Figure A1: Incumbent Re-Running and Election Quality, Within Countries



*Notes:* Each panel plots, for one country, the incumbent (winner) re-running rate into each election against the V-Dem clean-elections index for that election. The within-country correlation, estimated with country fixed effects on the pooled panel, is +0.30; Section 7 reports the pooled correlation ( $r = +0.31$ ,  $N = 88$ ,  $p < 0.01$ ).