Brokers on the Payroll: The Patronage Consequences of Becoming a Political Broker

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Abstract

Theories of clientelism hold that patronage employment is what pays political brokers for mobilizing voters, yet causal evidence that brokers actually collect these rewards is scarce. I study close internal elections for local party branch leaderships in Paraguay across three election cycles in the country's two largest parties: the long-ruling Colorado Party (2015 and 2022 cycles) and the opposition Liberal Party (2021). Linking more than 4,000 marginal candidacies to monthly administrative payroll records covering the universe of public employees, matched by national ID, I compare candidates who barely won a branch office to those who barely lost. Winning a branch presidency, the senior broker position, raises the maximum monthly budgeted public salary by about 2.85 million guaranies (roughly $390, or one minimum monthly wage; robust $p = 0.022$), and the estimate is positive in each of the three cycles taken separately. Employment shows no detectable response: more than half of marginal presidential candidates already hold public jobs before the party election, and neither entry into the payroll nor tenure status moves at the threshold. For lower-ranking branch members, every estimate is close to zero or negative. In these long-lived party machines, patronage rewards concentrate on the senior local broker and arrive as better pay within the state rather than as new jobs.

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"How are you goin' to interest our young men in their country if you have no offices to give them when they work for their party?"

– George W. Plunkitt, quoted in Riordon (1905)

1 Introduction

Clientelism (the exchange of targeted material benefits for political support) is pervasive in the developing world and is widely regarded as a key obstacle to democratic accountability and public goods provision (Stokes et al., 2013; Robinson and Verdier, 2013; Graham, Larreguy and Querubín, 2026). A large literature emphasizes that this exchange depends on political brokers, the local intermediaries who find recipients for targeted benefits and monitor that support is delivered in return (Stokes et al., 2013; Nichter, 2008; Finan and Schechter, 2012; Duarte et al., 2026). Broker effort is costly and hard to observe, which raises the question of why anyone takes on the work. The leading theoretical answer is patronage: a public sector job is a valuable reward, it can be targeted to a specific person, and it can be withdrawn if the broker stops performing (Levitsky, 2003; Robinson and Verdier, 2013; Oliveros, 2021; Brierley and Nathan, 2022). Despite the prominence of this claim, there is no causal evidence on whether brokers actually receive patronage rewards upon attaining their positions, or whether they would have received them anyway.

This paper asks: *does becoming a political broker causally improve public sector employment outcomes?* To answer this question, I exploit close internal elections for local party branch leaders in Paraguay's two largest parties: the Colorado Party (Asociación Nacional Republicana, ANR), which has governed the country for most of the past eight decades, and its main opponent, the Liberal Party (Partido Liberal Radical Auténtico, PLRA). The analysis covers three election cycles: the Colorado *seccional* elections of 2015 and 2022 and the Liberal *comité local* elections of 2021. In each party I study two positions, the branch president and the branch members. The former is a single position per branch; the latter form a council of several members elected alongside the president. These are formally vol-

untary and unpaid positions, so any compensation for the broker work they entail must flow through informal channels (see Section 2). The positions are filled in the parties' mandatory internal elections, which lets each party uncover its effective brokers through competition. The Colorado Party operates more than 400 branches spread across the country, at the neighborhood level in urban areas and the village level in rural ones, and the Liberal Party organizes its base through hundreds of *comités locales* in the same territories. These branch leaders are well-documented as brokers who coordinate vote buying, electoral mobilization, and the distribution of targeted benefits in their territories (Došek, 2023; Bandiera, Larreguy and Mangonnet, 2025; Duarte et al., 2026). Because the two parties sit on opposite sides of the ruling-opposition divide, the design also speaks to whether broker rewards require control of the national executive.

I link candidate registration records and election results from the three cycles to administrative public payroll data covering the universe of Paraguayan public employees from 2015 through 2026, matching individuals by their national ID number. Using a regression discontinuity design (RDD) that compares candidates who barely won to those who barely lost their branch elections, I estimate the causal effect of winning a broker position on subsequent public employment, entry into the public sector, salaries, tenure status, and placement at high-value government agencies. The pooled samples contain around 2,000 presidential and member candidacies, with effective samples within the RD bandwidths of roughly 1,000 to 1,400.

The headline result is that winning a branch presidency raises pay but not employment. Pooling the three cycles, barely elected presidents see their maximum monthly budgeted salary rise by 2.85 million Paraguayan guaranies relative to barely defeated rivals (about \$390, or roughly one minimum monthly wage with robust $p = 0.022$), with mean monthly salary moving in the same direction (1.18 million, $p = 0.088$).[1] The estimate is positive in

[1] Throughout, I convert at approximately 7,300 guaranies per U.S. dollar and benchmark against a minimum monthly wage of roughly 2.68 million guaranies. Salaries are in nominal guaranies, the headline salary outcomes are winsorized at the 1st and 99th percentiles within each cycle, and cycle fixed effects absorb level differences across election years.

every cycle taken separately: 4.4 million guaranies in the 2022 Colorado cycle, 2.4 million in the 2015 Colorado cycle, and 1.9 million among Liberal presidents.[2] Employment itself does not respond. The probability of holding any public job rises by a statistically insignificant 4.0 percentage points ($p = 0.33$), first-time entry into the public payroll rises 3.5 points ($p = 0.23$), and tenure status is essentially flat ($p = 0.39$).[3] The reason is visible in the baseline: within the RD bandwidth, more than half of barely losing presidential candidates are already on the public payroll before the party election takes place, against roughly 5% of the Paraguayan population.[4] For lower-ranking branch members I find no rewards at all. Member estimates are close to zero or negative on every outcome, including employment (0.2 points, $p = 0.95$) and winsorized maximum salary (-0.85 million, $p = 0.063$).

This paper complements a rich theoretical (Robinson and Verdier, 2013; Graham, Larreguy and Querubín, 2026), ethnographic (Auyero, 2000, 2012), and survey-based (Oliveros, 2021; Brierley and Nathan, 2022) literature on the exchange of patronage for brokerage by isolating the causal effect of broker selection on public employment outcomes. It also connects to a growing body of empirical work studying patronage employees' selection and performance (Colonnelli, Prem and Teso, 2020; Akhtari, Moreira and Trucco, 2022; Duarte, 2026; Brierley, 2026). By distinguishing *entry* from *upgrading*, it identifies the relevant margin of patronage in long-standing party machines, extending the self-enforcing contract framework of Robinson and Verdier (2013) and Oliveros (2021) to settings where the baseline patronage equilibrium has been operating for decades and, as in Brierley (2026), political influence operates through career progression beyond just initial hiring. Because the design covers

[2] In the Liberal Party the branch president is, by party rule, the branch's most-voted elected member, in practice the top candidate of the most-voted list, so the presidential running variable is the vote gap between the top two lists and both marginal candidates are elected branch members. The Liberal presidential contrast therefore identifies the premium to the presidency conditional on holding a branch membership, whereas the Colorado contrast compares a president to a candidate who holds no branch office. Section 4 discusses the resulting pooled estimand.

[3] With tenure, employees cannot be dismissed without "just cause" and, in the event of dismissal, are entitled to compensation based on seniority.

[4] Computed from the same payroll: distinct national IDs appearing on the public payroll in a calendar year, divided by Paraguay's population at the 2022 census (6,109,903; Instituto Nacional de Estadística (INE), 2024). The rate runs from 4.4% to 5.2% across the years spanned by the three cycles' pre-election windows, and is 4.8% in 2022 (295,243 people).

both the ruling and the main opposition party, it can also separate rewards that require control of the national executive from rewards a party can deliver through the subnational governments it holds. Finally, the paper contributes to the study of how party machines sustain themselves organizationally (Levitsky, 2003; Brierley, 2021; Brierley and Nathan, 2022; Graham, Larreguy and Querubín, 2026).

The remainder of the paper is organized as follows. Section 2 describes the Paraguayan political context and the role of branch leaders as brokers. Section 3 presents the data sources, samples, and outcome variables. Section 4 outlines the empirical strategy and identification assumptions. Section 5 presents the results. Section 6 discusses them and concludes.

2 Context: Paraguay, Patronage, and Party Brokers

Paraguay has been governed by the Colorado Party almost continuously since 1947, with the sole interruption of a center-left coalition government involving the Liberal party from 2008 to 2013 (Abente Brun, 2007; Lachi and Rojas-Schaffer, 2018; Mandl, 2021). Much of this dominance traces to the Stroessner dictatorship (1954–1989), which fused the party apparatus with the state bureaucracy and built an extensive patronage network to reward supporters and punish defectors (Abente Brun, 2007; Painter, 1983; Bandiera, Larreguy and Mangonnet, 2025). The democratic transition did not dismantle these structures. Rather, clientelism and vote-buying became the dominant mode of electoral competition (Došek, 2023; Setrini, 2025; Duarte et al., 2026). Paraguay consistently ranks among the most corrupt countries in the Western Hemisphere (Transparency International, 2025), and recent empirical work documents that patronage appointments at the customs authority reduce tax collection by approximately 11% (Duarte, 2026).

Paraguay's public sector lacks entry examinations for the vast majority of positions, making discretionary appointment the default hiring mechanism (Schuster, 2021; Mandl, 2021). Public posts are widely characterized as "prizes for political connections", with nu-

merous cases highlighted in newspapers of record (Última Hora, 2021, 2024; ABC Color, 2024). Substantial bureaucratic turnover also occurs at the start of each new administration, including the replacement of human resources directors who control hiring decisions. This institutional environment makes public employment a relevant medium for patronage exchange. Public jobs are valuable, since public sector wages typically exceed private sector alternatives (Abdallah, Coady and Jirasavetakul, 2023), and appointments can be targeted to specific individuals. They are also reversible: employees, especially those without tenure, can be dismissed when the political winds shift. These are precisely the properties that make patronage employment self-enforcing in the model of Robinson and Verdier (2013) and the empirical framework of Oliveros (2021).

Paraguay's major parties are organized into hundreds of local branches, each corresponding to a defined geographic territory; the Colorado Party calls them *seccionales* and the Liberal Party *comités locales*. The Colorado and Liberal parties operate more than 400 party branches nationwide, each headed by an elected president and governing board. These are formally voluntary and unpaid positions: the party itself does not directly compensate *seccional* leaders for the brokerage work the role entails (e.g., voter mobilization, benefit distribution, monitoring compliance). Standard accounts of patronage (Robinson and Verdier, 2013; Oliveros, 2021; Brierley and Nathan, 2022) hold that compensation for this work flows indirectly, through preferential access to public sector employment granted by copartisan politicians and bureaucrats. Public payroll outcomes are therefore the natural empirical setting in which to test whether brokers are rewarded.

Previous research and extensive media coverage identify branch leaders as political brokers: intermediaries who facilitate the exchange of targeted benefits for political support between patron politicians and voter-clients (Došek, 2023; Bandiera, Larreguy and Mangonnet, 2025). These leaders run turnout and vote-buying operations, and serve as the party's main point of contact with its base. Branch leadership positions are contested in mandatory internal party elections (*internas*), and many races are closely contested, with

winning margins of only a handful of votes. This competitive selection from below was not designed by the party but inherited from a 1990 democratic transition reform. The parties have since adapted to it as a screening mechanism for effective brokers, in line with recent accounts of bottom-up broker selection (Auerbach and Thachil, 2018, 2023). This generates the natural experiment that I exploit.

The direct election of branch presidents and members was imposed on all Paraguayan parties by the 1990 Electoral Code and subsequently constitutionalized in 1992 (Rizova, 2007). During the Stroessner dictatorship, local *seccional* committees served as instruments of territorial control, and contested leadership lists at the local level were actively suppressed by the central party apparatus (Hanratty and Meditz, 1988). After Stroessner's 1989 ouster, President Andrés Rodríguez (a member of the "traditionalist" faction of the Colorado Party) pushed a reform mandating direct vote primaries for all party candidates and all party leadership positions, including *seccional* presidents and members. The reform passed the lower chamber by a single tie-breaking vote and the Senate by 20–15 (Rizova, 2007). The *seccionales* themselves opposed the reform, correctly anticipating that direct member voting would erode the gatekeeping authority they had previously exercised over candidate selection.

The Liberal Party mirrors this structure from the opposition. It held the presidency in a coalition only during the 2008–2013 interlude, but it retains departmental and municipal strongholds, and its *comités locales* perform the same territorial work of mobilization and benefit distribution that *seccionales* perform for the Colorados. Because the 1990 Electoral Code applied to all parties, Liberal branch leaderships are also filled through mandatory internal elections; the cycle I study was held in 2021. One institutional difference matters for the empirical design: Colorado branches elect their president in a separate direct race, while the Liberal *comité* presidency goes by party rule to the branch's most-voted elected member. Section 3 describes how this shapes the running variables.

This formal, electoral mode of broker selection coexists with the informal brokerage emphasized in ethnographic accounts of Latin American clientelism, in which brokers such as the

Peronist *punteros* of greater Buenos Aires emerge through years of community-level problem-solving rather than through any discrete selection event (Auyero, 2000, 2012). Lower-level brokers in both parties operate informally as well, but branch presidents and members are formally registered candidates selected through a competitive event with publicly observable vote totals, which makes causal identification of patronage rewards feasible for this subset of higher-level brokers.

3 Data

3.1 Party Branch Elections

The election data cover three cycles of internal branch elections in Paraguay's two largest parties. For the Colorado Party, I use the *seccional* elections of July 26, 2015 (whose winners took office on April 28, 2016) and December 18, 2022 (winners took office on January 10, 2023). For the Liberal Party, I use the *comité local* elections of June 20, 2021. In each cycle, the candidate lists registered with each party's electoral tribunal provide names, national identification numbers (*cédula de identidad*), party list affiliations, and roles, and the corresponding tally records provide vote totals from which I construct winning margins.

Branch members are elected by D'Hondt proportional representation in all three cycles, which is what generates close races at the last allocated seat. The cycles differ in one respect: the 2022 Colorado and 2021 Liberal elections let voters cast preference votes within lists, so a list's seats go to its most-voted candidates, whereas in the 2015 Colorado election seats follow the printed ballot order within each list. This changes which individuals sit at the seat boundary, not the location of the boundary itself. Branch presidents are chosen differently in the two parties. Colorado branches elect their president in a separate direct race, so the natural contrast is the winner against the runner-up. In the Liberal Party the presidency goes by rule to the party's most-voted elected member in each branch, which in practice is the top preference-vote candidate of the most-voted list. The presidential contrast therefore

compares the top candidates of the two leading lists, both of whom win council seats. I return to the resulting difference in estimands in Section 4.

3.2 Public Payroll

The outcome data come from the public payroll panel of Paraguay's Civil Service Secretariat (*Secretaría de la Función Pública*, SFP), which contains monthly records for the universe of public sector employees from January 2015 to 2026, including budgeted and earned salaries, government agency identifiers, tenure/permanent employment status, and demographic covariates such as age, gender, and profession.[5]

Matching candidates to the payroll requires a national ID for every candidate. The Colorado registries include IDs directly. The Liberal candidate documents omit them for a subset of candidates, so I recover IDs by matching names against national electoral registers and the recovery rate is 99.5%.[6] The Liberal tally documents also omit candidates who received zero preference votes, which removes the record of the marginal winner in seven member *comités*. I reconstruct those winners from the party's roster of elected members and flag them, and the results are robust to dropping these branches (Section 5).

3.3 Estimation Samples and Running Variables

I construct two parallel RD estimation samples in each cycle. For *branch presidents*, I compare the two leading candidates in each branch, with the running variable defined as the vote margin between them divided by total valid votes. For *branch members*, I define the running variable as the normalized quotient gap at the boundary of the last allocated D'Hondt seat, comparing the marginal winner (the candidate whose quotient secured the

[5]The payroll data include employees at all levels of government (federal, departmental, and municipal). One caveat is that members of the police, military, and national security agencies are anonymized for safety reasons, so they appear in the dataset with artificial IDs. In any case, these are not relevant positions for patronage, as the Constitution explicitly prohibits national security members from political engagement and partisan affiliation.

[6]This statistic is for all the Liberal candidates without IDs, for my estimation sample focused on the marginal winning and losing candidates I have 100% coverage of ID numbers.

final seat) to the marginal loser (the highest-ranked candidate who did not receive a seat).[7] Pooling the three cycles yields 2,067 presidential candidacies (740 from Colorado 2015, 793 from Colorado 2022, and 534 from Liberal 2021) and 1,981 member candidacies (739, 786, and 456, respectively).[8] Races decided by an exact tie carry a margin of zero and are resolved by lot or by a statutory tie-break rather than by the voters, so they do not identify a winner-versus-loser contrast at the threshold; I exclude them under a single rule applied to every cycle, which removes two presidential and eleven member rows.

3.4 Outcomes

I organize outcomes into three categories that capture different dimensions of potential patronage rewards:

- **Entry (extensive margin):** Whether the candidate is employed in the public sector in the year after taking office, and, separately, whether the candidate enters the public payroll for the first time in that year, having never appeared in any monthly payroll since the data begin in January 2015. The latter measure is constructed identically in all three cycles.[9]

- **Upgrading (intensive margin):** Change in mean and maximum monthly budgeted salary across months of employment, as well as change in tenured employment status. The headline salary outcomes are winsorized at the 1^{st} and 99^{th} percentiles within each cycle. The distribution of salary changes is heavily tailed: the median change is zero, the 1^{st} and 99^{th} percentiles sit tens of millions of guaranies below and above it, and the largest individual changes run into the hundreds of millions. As a local

[7] The normalization is done by dividing the quotient gap by the total number of valid votes (that is, all votes minus blank and null votes).

[8] The Liberal party sample is smaller as only around 60% of the Liberal party have contested races with two or more party lists, in contrast to more than 95% in both Colorado party internal elections.

[9] As the payroll begins in January 2015, the window over which prior absence can be verified is only 4 months for the 2015 Colorado cycle, against 66 for the 2021 Liberal cycle and 84 for the 2022 Colorado cycle. A shallower lookback is a weaker requirement, so it mechanically produces more apparent entrants. A variant imposing the common four-month depth on all three cycles gives the same results.

linear estimator weights the candidates nearest the cutoff most heavily, a handful of such cases can carry outsized leverage in an unwinsorized estimate, winsorizing bounds that leverage without discarding the observation. Unwinsorized estimates are reported by cycle in Appendix Table A5. I do not report $\ln(Y+1)$ salary outcomes: salary is exactly zero for candidates who are not on the payroll, and log-like transformations of outcomes with mass at zero have no unit-invariant interpretation (Chen and Roth, 2024).

- **Valuable agencies:** Whether the candidate is employed at a "high-value" government agency, classified using an AKM-style wage premium measure (Abowd, Kramarz and Margolis, 1999). For this I estimate a fixed effects regression of monthly salary on agency, worker, and month-year fixed effects (plus time-varying controls for public sector experience, tenure status, and age), and define agencies in the top quartile of estimated agency fixed effects as "high-value."[10]

Outcomes are measured over twelve-month windows around the date winners take office: the pre window is the year before taking office and the post window the year after. For the 2015 Colorado cycle this means May 2015 through April 2016 against May 2016 through April 2017; for the Liberal cycle, July 2020 through June 2021 against July 2021 through June 2022; for the 2022 Colorado cycle, calendar 2022 against calendar 2023. All outcomes enter as first differences ($\Delta Y_i = Y_{i,\text{post}} - Y_{i,\text{pre}}$) to remove pre-existing individual-level heterogeneity in employment status and salary.

4 Empirical Strategy

The unit of analysis is candidate i in branch s of cycle c. The running variable m_i is the vote margin (for presidents) or normalized D'Hondt quotient gap (for members), centered

[10] A caveat of this measure is that it identifies high-value agencies in terms of their formal wage premium, which does not include informal rent-seeking opportunities that could be equally or more important. I am exploring other potential measures I could use to identify high-value agencies.

at zero so that $m_i > 0$ corresponds to winning and $m_i < 0$ to losing. Treatment is defined as $D_i = \mathbf{1}(m_i > 0)$. The estimand is the local average treatment effect at the threshold:

$$\tau = \lim_{m \downarrow 0} \mathbb{E}[Y_i \mid m_i = m] \; - \; \lim_{m \uparrow 0} \mathbb{E}[Y_i \mid m_i = m], \tag{1}$$

estimated via local linear regression (Gelman and Imbens, 2019) with bias-corrected robust confidence intervals (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2018). Bandwidth selection follows the Mean Squared Error (MSE)-optimal procedure of Calonico, Cattaneo and Farrell (2020), and the default kernel is triangular. The running variables are built identically across cycles, so the pooled estimates stack the three cycles in one regression, adding cycle fixed effects as covariates (Calonico et al., 2019) and clustering standard errors on party-branch units (the same Colorado *seccional* appearing in both of its cycles forms a single cluster). Per-cycle estimates use the same local-linear specification within each cycle, without covariates and without clustering, since each branch contributes a single winner–loser pair. Throughout I report the bias-corrected point estimate together with the robust bias-corrected standard error of Calonico, Cattaneo and Titiunik (2014), which accounts for the sampling variability of the bias correction itself.

Estimands. The member RD identifies the same object in every cycle: the effect of winning the last council seat relative to just missing it. The president RDs differ across parties, as Section 3 describes. The Colorado contrast compares the president to a runner-up who holds no branch office, while the Liberal contrast compares the top candidates of the two leading lists, both of whom become council members, and so identifies the premium to the presidency over and above membership. The pooled president estimate blends the two contrasts; per-cycle estimates keep them separate, and any table reporting the pooled president row carries this caveat.

Identification and validity. The identifying assumption is that candidates cannot precisely manipulate their vote margin at the threshold (Lee, 2008). Appendix Tables A1 and A2 report RD estimates on ten predetermined characteristics (pre-period employment, tenure, salaries, and high-value agency placement, plus gender, age, and incumbency) for the pooled sample and for each cycle separately. Incumbency is defined against the previous cycle's roster of elected branch officers, which exists only for the Colorado party data, so that row is estimated for the Colorado samples and left blank in the pooled and Liberal columns.[11] In the pooled samples no covariate jumps at the cutoff. Joint tests that regress the treatment indicator on gender and age (controlling for the running variable and its interaction with treatment, within the bandwidth) do not reject balance ($p = 0.50$ for presidents and $p = 0.68$ for members), and neither does a joint test over the full set of pre-period covariates ($p = 0.56$ and $p = 0.77$).

The per-cycle columns reveal one imbalance worth reporting. In the Liberal member sample, marginal winners look weaker on paper before the election: their pre-period maximum salary is lower by about 4.4 million guaranies ($p = 0.028$) and their pre-period mean salary by about 2.3 million ($p = 0.038$). No comparable salary imbalance appears in either Colorado cycle or in the pooled member sample, and the Liberal joint test does not reject ($p = 0.55$). As the member estimates reported below are the negative ones, I probe them directly: re-estimating the member RDs with the pre-period covariates added to the specification leaves the estimates essentially unchanged (Appendix Table A3), so the negatives are not an artifact that covariate adjustment removes.

Two further falsification exercises support this. Placebo RDs on five pre-election outcomes are all null. The estimates are the pre-period rows of Appendix Tables A1 and A2, which report them for every cycle. Placebo RDs at artificial cutoffs away from zero (Catta-

[11] The standard density discontinuity test (McCrary, 2008; Cattaneo, Jansson and Ma, 2020) is mechanically uninformative in this setting as the estimation sample includes both the winner and the runner-up (for presidents) or the marginal winner and marginal loser (for members) from each branch election. This paired structure guarantees that the density of the running variable is symmetric around the cutoff by construction, in each cycle and hence also in the pool. The tables report the resulting p-values for completeness.

neo, Idrobo and Titiunik, 2020), at the median of each side of the threshold, run in all three cycles and produce one significant estimate out of 59 (member employment on the winners' side of the 2022 Colorado cycle, $p = 0.034$, Appendix Table A4), fewer than the three that chance alone would deliver at the 5% level.

I use first differences ($\Delta Y_i = Y_{i,\text{post}} - Y_{i,\text{pre}}$) rather than post-election levels because baseline public employment among candidates is high: within the pooled bandwidth, over half of barely losing presidential candidates are already public employees. Using post-election levels would conflate the treatment effect with pre-existing variation in employment status. First-differencing conditions on each individual's pre-election trajectory and isolates the change attributable to winning.

5 Results

I present results in three parts: pooled estimates for branch presidents (the senior broker position), pooled estimates for branch members (the lower-level position), and the pattern across cycles and parties. Throughout, I distinguish patronage as *entry* (access to a public sector job) from patronage as *upgrading* (better compensation, tenure, or agency assignment for people already embedded in the state). Table 1 collects the estimates.

5.1 Branch Presidents

Winning a branch presidency pays. Pooling the three cycles, the maximum monthly budgeted salary of barely elected presidents rises by 2.85 million guaranies relative to barely defeated rivals (about $390, or roughly one minimum monthly wage; $p = 0.022$), and mean monthly salary rises by 1.18 million ($p = 0.088$). Figure 1 shows the corresponding RD plots.[12] These salary increases are consistent with winners being promoted to higher-paying positions,

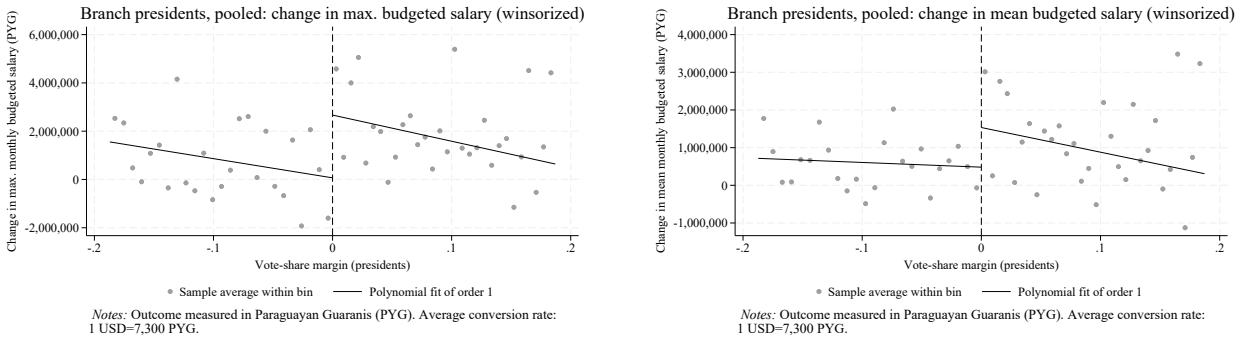
[12]For the 2022 Colorado cycle, Appendix Figure A1 expresses the salary effects as percentages of the predicted post-election salary of barely losing candidates at the cutoff, following Chen and Roth (2024): 36.9% of the control mean for the maximum ($p = 0.024$) and 25.8% for the mean ($p = 0.077$).

Table 1: RD estimates: the effect of winning a branch office

Outcome	Pooled	ANR	PLRA	2015	2022
Panel A: Branch presidents					
Employed (pp)	3.96	2.46	5.86	5.29	3.53
	(4.06)	(4.59)	(6.68)	(8.16)	(6.47)
	[1281]	[1069]	[310]	[459]	[418]
Entered public sector (pp)	3.48	4.05	4.52	8.12	2.03
	(2.91)	(3.59)	(4.74)	(7.00)	(2.98)
	[1311]	[987]	[342]	[417]	[544]
Tenured status (pp)	3.33	1.70	4.37	4.07	3.35
	(3.89)	(4.51)	(5.00)	(8.00)	(5.93)
	[1139]	[987]	[262]	[391]	[442]
High-value agency (pp)	0.98	0.72	3.16	6.74	−3.31
	(2.11)	(2.63)	(3.07)	(4.22)	(3.36)
	[1185]	[889]	[312]	[365]	[466]
Mean salary, wins. (Million Gs.)	1.18*	1.20	0.61	0.88	2.12*
	(0.69)	(0.85)	(0.52)	(1.35)	(1.16)
	[1219]	[1011]	[262]	[513]	[390]
Max. salary, wins. (Million Gs.)	2.85**	3.00**	1.85*	2.44	4.36**
	(1.24)	(1.51)	(1.12)	(2.36)	(2.06)
	[1219]	[1005]	[306]	[529]	[396]
Panel B: Branch members					
Employed (pp)	0.22	−0.54	−1.79	−2.65	2.02
	(3.22)	(4.20)	(1.99)	(6.13)	(6.34)
	[1260]	[883]	[134]	[390]	[445]
Entered public sector (pp)	1.43	1.78	−1.69	0.93	3.50
	(2.49)	(3.01)	(1.20)	(4.45)	(4.53)
	[1208]	[959]	[142]	[500]	[420]
Tenured status (pp)	0.58	0.02	−0.52	5.40	−6.99
	(2.91)	(3.67)	(0.80)	(4.73)	(5.39)
	[1045]	[747]	[140]	[446]	[383]
High-value agency (pp)	−2.37**	−2.43	−0.38	−2.41	−3.16
	(1.21)	(1.57)	(0.26)	(1.91)	(2.87)
	[1137]	[739]	[64]	[404]	[379]
Mean salary, wins. (Million Gs.)	−0.29	−0.31	−0.22*	−0.41	0.14
	(0.26)	(0.36)	(0.13)	(0.55)	(0.38)
	[1280]	[905]	[130]	[502]	[326]
Max. salary, wins. (Million Gs.)	−0.85*	−0.95	−0.09	−1.06	−0.38
	(0.46)	(0.64)	(0.38)	(1.06)	(0.75)
	[1260]	[887]	[140]	[438]	[360]

Notes: Each cell reports the bias-corrected RD estimate of winning the office on the pre-to-post change in the row outcome, with robust standard errors in parentheses and effective sample sizes in brackets, using local linear specifications with triangular kernels and MSE-optimal bandwidths (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2020). Binary outcomes are in percentage points; salary outcomes are in millions of guaranies, winsorized at the 1st and 99th percentiles within cycle. The Pooled column stacks the three cycles with cycle fixed effects as covariates and standard errors clustered on party branches; the ANR column does the same for the two Colorado cycles; the 2015 and 2022 columns use each Colorado cycle's own sample without covariates, and the PLRA column the Liberal 2021 cycle's. In the Liberal Party both marginal presidential candidates are elected members, so the Liberal presidential rows identify the presidency premium conditional on membership (Section 4). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

receiving within-agency raises, or being transferred to positions with larger salary budgets, channels I plan to disentangle in future work. The dip in the fitted salary change just below the threshold also suggests that narrow losers may lose access to better-paying positions.



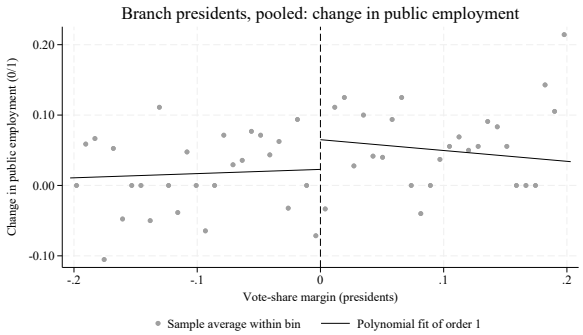
(a) Maximum salary: 2.85M Gs ($p = 0.022$)

(b) Mean salary: 1.18M Gs ($p = 0.088$)

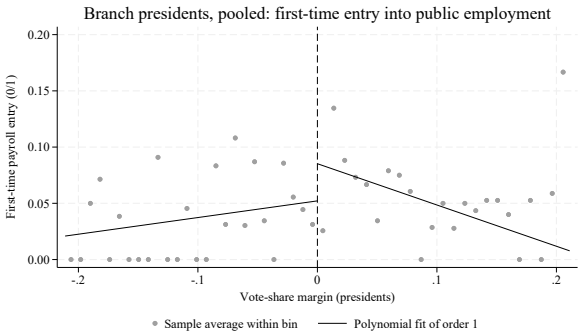
Figure 1: RD plots: Change in winsorized monthly budgeted salary, branch presidents, pooled
Notes: Pooled sample for three election cycles. Estimates use local linear RD specifications with triangular kernels, MSE-optimal bandwidths, and robust bias-corrected inference (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2020), with cycle fixed effects as covariates and standard errors clustered on party branches. Salaries winsorized at the 1st and 99th percentiles within cycle.

Employment does not respond. Figure 2 shows the RD plots for holding any public job and for first-time entry into the payroll: the pooled employment estimate is 4.0 percentage points ($p = 0.33$) and the entry estimate 3.5 points ($p = 0.23$). Tenured status and placement at high-value agencies are similarly flat (3.3 points, $p = 0.39$, and 1.0 point, $p = 0.64$; Figure 3). These nulls are informative rather than merely imprecise. Within the pooled bandwidth, 53.6% of barely losing presidential candidates are already employed in the public sector before the branch election (against 4.4% to 5.2% of the Paraguayan population over the same years, computed from the same payroll), and 42.1% already hold tenure. In the 2022 Colorado cycle the employment baseline across both sides of the cutoff reaches 63.4%. Most people who contest branch presidencies have already been absorbed into the state, presumably through earlier rounds of patronage, so there is less room for the election to move employment itself. The flat high-value agency estimate in turn suggests the salary gains happen within candidates' existing agencies rather than through transfers to the best-paying parts of the state (such as the binational hydroelectric entities Itaipu and Yacyreta),

though the AKM-based classification may not capture the agencies parties actually prize for patronage. Section 6 returns to this.



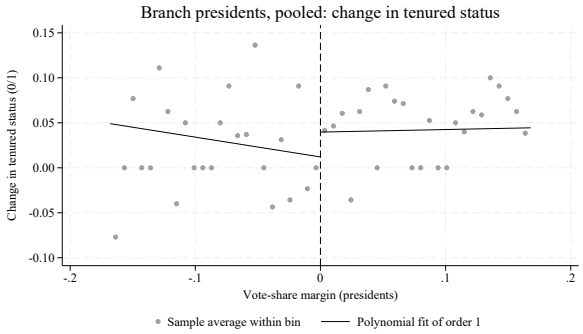
(a) Employment: 4.0pp ($p = 0.33$)



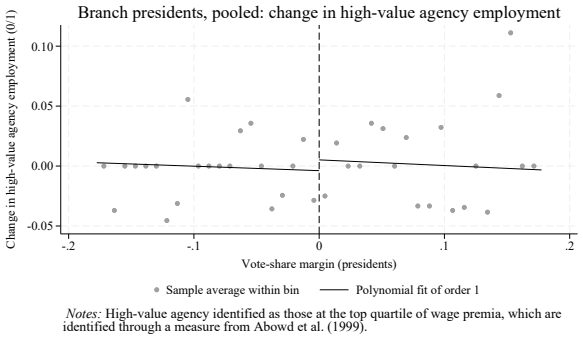
(b) First-time entry: 3.5pp ($p = 0.23$)

Figure 2: RD plots: Change in public employment and first-time payroll entry, branch presidents, pooled

Notes: Pooled sample for three election cycles. Estimates use local linear RD specifications with triangular kernels, MSE-optimal bandwidths, and robust bias-corrected inference (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2020), with cycle fixed effects as covariates and standard errors clustered on party branches. First-time entry means appearing on the public payroll in the year after taking office with no appearance at any point since the payroll data begin in January 2015.



(a) Tenured status: 3.3pp ($p = 0.39$)



(b) High-value agency: 1.0pp ($p = 0.64$)

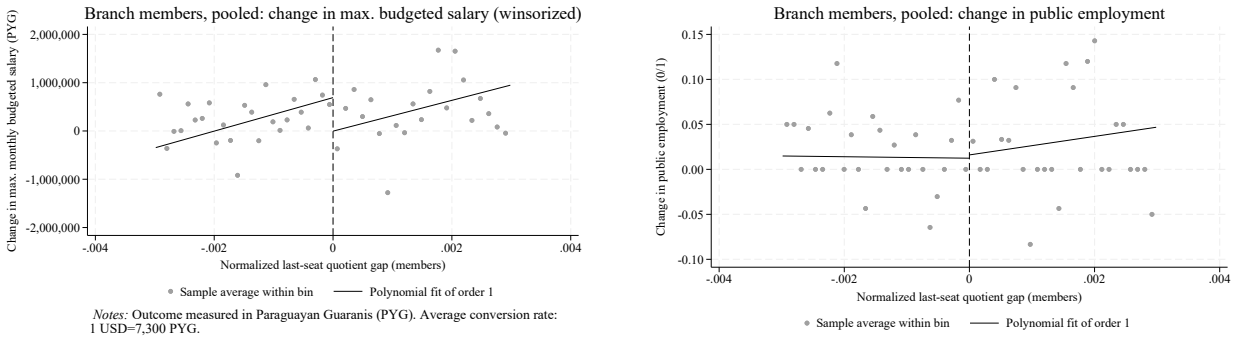
Figure 3: RD plots: Change in tenured status and high-value agency employment, branch presidents, pooled

Notes: Pooled sample for three election cycles. Estimates use local linear RD specifications with triangular kernels, MSE-optimal bandwidths, and robust bias-corrected inference (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2020), with cycle fixed effects as covariates and standard errors clustered on party branches. High-value agencies are those in the top quartile of AKM-estimated agency wage premia using a methodology akin to Abowd, Kramarz and Margolis (1999).

5.2 Branch Members

Branch members, which are one rung down the party hierarchy below party branch presidents, have no measurable rewards. Every pooled member estimate in Table 1 is close to

zero or negative: employment moves 0.2 percentage points ($p = 0.95$), first-time entry 1.4 points ($p = 0.56$), tenure 0.6 points ($p = 0.84$), and winsorized mean salary -0.29 million guaranies ($p = 0.27$). Figure 4 plots the salary and employment results.[13] Two estimates lean negative: winsorized maximum salary falls by 0.85 million ($p = 0.063$) and high-value agency employment by 2.4 points ($p = 0.0499$; Figure 5). Both call for caution rather than a causal story. Neither is significant within any single cycle, the high-value agency estimate sits against a control mean of essentially zero change, and the Liberal member sample carries the pre-period imbalance documented in Section 4, with marginal winners weaker on pre-election salary. Adjusting for the full set of pre-period covariates moves neither estimate materially (-0.76 million, $p = 0.097$, and -2.6 points, $p = 0.034$; Appendix Table A3), so the negatives are not a simple artifact of that imbalance, but I read them as evidence of no measurable rewards, and possibly of mild costs, rather than as a confirmed penalty.



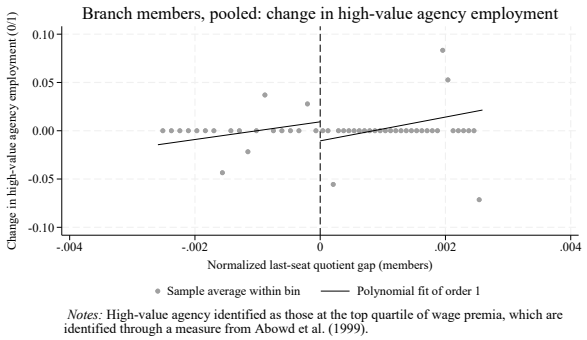
(a) Maximum salary: -0.85M Gs ($p = 0.063$)

(b) Employment: 0.2pp ($p = 0.95$)

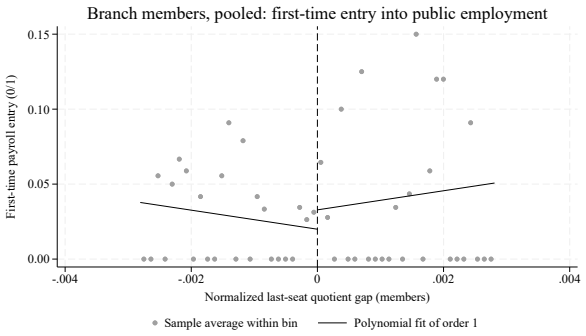
Figure 4: RD plots: Change in winsorized maximum salary and public employment, branch members, pooled

Notes: Pooled sample for three election cycles. Estimates use local linear RD specifications with triangular kernels, MSE-optimal bandwidths, and robust bias-corrected inference (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2020), with cycle fixed effects as covariates and standard errors clustered on party branches.

[13] For the 2022 Colorado cycle, Appendix Figure A2 shows the corresponding percent-of-control-mean member salary results.



(a) High-value agency: -2.4pp ($p = 0.0499$)



(b) First-time entry: 1.4pp ($p = 0.56$)

Figure 5: RD plots: Change in high-value agency employment and first-time payroll entry, branch members, pooled

Notes: Pooled sample for three election cycles. Estimates use local linear RD specifications with triangular kernels, MSE-optimal bandwidths, and robust bias-corrected inference (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2020), with cycle fixed effects as covariates and standard errors clustered on party branches.

The contrast with presidents admits a few interpretations, which the pooled cycles narrow but do not settle. The pooled member sample is as large as the presidential one and its confidence intervals now exclude president-sized salary effects, so a lack of statistical power does not seem to explain these results. The reading most consistent with the evidence is that rewards are stratified by rank, in line with Brierley and Nathan's (2022) finding that broker payment depends on position in the party hierarchy. Members may also be compensated with rewards the formal payroll cannot show, such as informal payments, community goods, social recognition, or positioning for future candidacies. Within the pooled bandwidth, 32.8% of barely losing member candidates hold public jobs, roughly three-fifths of the presidential rate, so members are less embedded in the patronage network to begin with.

5.3 Patterns Across Cycles and Parties

The president salary premium is not the product of any single election or party. The winsorized maximum-salary estimate is positive in all five columns of Table 1: 3.0 million guaranies in the pooled Colorado sample ($p = 0.048$), 4.4 million in the 2022 Colorado cycle alone ($p = 0.034$), 2.4 million in the 2015 Colorado cycle (imprecise, $p = 0.30$), and 1.9 million among Liberal presidents ($p = 0.097$). In the 2022 cycle, where the raw data are richest, the

unwinsorized estimate reaches 5.1 million guaranies ($p = 0.024$), 36.9% of the control mean at the cutoff (Appendix Figure A1). The Liberal estimate deserves emphasis for two reasons. It comes from the opposition party, so the premium does not require control of the national executive. Liberal brokers are plausibly rewarded through the departmental and municipal governments their party holds. And it identifies the presidency premium conditional on membership, since both marginal Liberal candidates win council seats, which makes it a within-branch comparison of the top job against an ordinary seat. Members, meanwhile, are flat to negative in every cycle and both parties.

6 Discussion and Conclusion

Entry versus upgrading. The finding that patronage for senior brokers operates through salary increases rather than job provision has implications for how we model patronage in settings with entrenched parties. In the theoretical framework of Robinson and Verdier (2013), public employment is attractive as a redistribution mechanism precisely because it is *selective and reversible*, and the standard interpretation is that the relevant "job" is the initial appointment. Where a party machine has operated for decades and has already placed most of its operatives in public posts, the margin of selectivity and reversibility appears to have shifted from hiring to upgrading: the reward is no longer getting a job but getting a better job. Under this interpretation the patronage contract resembles a performance bonus more than a hiring decision, and the self-enforcing contract framework of Robinson and Verdier (2013) and Oliveros (2021) extends to mature machines with the relevant reversibility operating at the promotion and raise margin. That the same pattern shows up in the opposition Liberal Party suggests the mechanism is a feature of branch leadership in machine parties, not a perk of holding the national executive.

The employment baseline for presidents. The high rate of public employment among presidential candidates, more than half at the margin and roughly ten times the 5% rate

in the general population, raises a chicken-and-egg question. One interpretation is *selection into candidacy*: individuals who already hold public jobs are more likely to run for branch president because the position complements their state employment, or because their patrons encourage them to run. The alternative is *dynamic patronage*: these individuals received their jobs at an earlier stage of their brokerage career, and the branch election is one step in an ongoing patronage trajectory. I plan to exploit the panel structure of the payroll data, which extends back to 2015, to test whether candidates' employment trajectories diverge from the general population well before they appear on a branch ballot.

External validity. Paraguay is a case of party-state fusion, and the effects documented here may not generalize to settings with stronger bureaucratic insulation, more competitive party systems, or weaker patronage traditions. Within Paraguay, however, the design spans the ruling and the main opposition party, three election cycles, and outcome windows that fall under three national administrations. The presidency premium appears on both sides of the ruling-opposition divide, at lower magnitude and precision for the Liberals, whose access runs through subnational governments rather than the federal bureaucracy. The remaining external-validity caveats are the standard RD ones: the estimates are local to competitive branches and marginal candidates, and how that subsample compares to the full candidate population remains an open question.

Identification and robustness. A residual identification concern is that party elites could manipulate close outcomes to favor preferred candidates. Interviews with local political scientists and party activists cut against this: broker-level elections are how the party learns which brokers actually deliver votes in their territory, a function elite manipulation would destroy. The balance evidence in Section 4, including the Liberal member imbalance reported there, and the covariate-adjusted estimates in Appendix Table A3 bound what manipulation could explain. Measurement is the other open front: the AKM-based agency classification may misidentify which agencies parties prize, so alternative definitions built from known

patronage hotspots (such as the binational dams, customs, and municipal governments) are a planned robustness check, alongside an analysis of spillovers within branches if a loser is dismissed to make room for the winner.

Extensions. Several analyses are planned on top of the pooled results. The monthly frequency of the payroll allows for dynamic treatment effects that trace when rewards arrive after each election. Linking brokers to subsequent general and municipal election returns would test whether rewards are contingent on delivered votes. Characterizing the jobs winners move into, by agency, title, and location, would separate promotions from transfers. In addition, a fourth cycle is on the horizon: the parties are holding new branch elections in 2026, whose results I intend to incorporate once the party finishes consolidating them.

Conclusion. Across two parties and three election cycles, winning a senior broker position in a Paraguayan party branch raises a winner's public salary by about one minimum monthly wage, while employment, tenure, and agency placement show no detectable change, while winning an ordinary council seat brings no measurable reward. The evidence supports theories that treat patronage as the credible, reversible payment sustaining broker effort, with one amendment: in long-lived machines the payment may arrive as a raise instead of a job. What sits behind the raises, whether promotions, transfers, or new titles, and when exactly they arrive are the questions my next draft will take up.

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Appendix

Pooled balance and robustness

Table A1: Covariate balance at the cutoff: branch presidents

Covariate	Ctrl. mean	Pooled	ANR	PLRA	2015	2022
Employed, pre (pp)	53.59	2.19	−1.91	14.66	3.20	−9.11
		(6.39)	(7.45)	(14.46)	(10.23)	(10.99)
Tenured, pre (pp)	42.09	0.09	−4.68	9.85	−0.70	−10.76
		(6.03)	(8.51)	(12.18)	(11.60)	(11.94)
Mean salary, pre (Million Gs.)	4.33	−0.86	−1.07	−0.06	0.05	−2.28
		(0.89)	(1.04)	(1.44)	(1.41)	(1.64)
Max. salary, pre (Million Gs.)	7.10	−2.01	−2.19	−1.44	0.07	−5.51
		(1.77)	(2.14)	(2.48)	(2.68)	(3.43)
Mean salary, pre, wins. (Million Gs.)	4.32	−0.97	−1.14	−0.33	−0.06	−2.26
		(0.87)	(1.03)	(1.35)	(1.37)	(1.64)
Max. salary, pre, wins. (Million Gs.)	7.03	−2.14	−2.33	−1.40	−0.76	−4.21
		(1.52)	(1.81)	(2.18)	(2.33)	(3.10)
High-value agency, pre (pp)	5.50	3.20	3.65	0.61	2.42	4.71
		(3.26)	(3.88)	(7.15)	(4.99)	(5.81)
Male (pp)	87.08	−3.42	−0.93	−4.45	−0.04	−0.45
		(3.97)	(4.72)	(8.57)	(5.88)	(7.11)
Age at election (years)	44.9	−1.4	−1.8	0.0	−2.0	−2.2
		(1.3)	(1.5)	(2.2)	(2.5)	(2.4)
Incumbent (pp)	11.66	–	1.50	–	7.57	−2.79
			(5.02)		(9.15)	(7.42)
Joint F p (male, age)		0.502	–	0.841	0.742	0.394
Joint F p (full pre-period set)		0.561	–	–	–	–
Density test p		0.998	–	1.000	0.972	0.992

Notes: Each cell reports the bias-corrected RD estimate on the predetermined row covariate, with robust standard errors in parentheses (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2020). The control mean is the covariate's mean among barely losing candidates within the pooled bandwidth. Column definitions follow Table 1. The two joint F rows differ only in which covariates they test. Each regresses the treatment indicator on the tested covariates, the running variable, and its interaction with treatment, within the MSE-optimal bandwidth for the change in employment, and reports the p-value on the covariates alone. The trend terms belong in the regression because treatment is a deterministic function of the running variable inside the window, so a covariate that merely trends in the margin would otherwise predict the side of the cutoff and reject without any discontinuity. The first row tests gender and age; the second adds the five pre-period payroll covariates (employment, tenure, winsorized mean and maximum salary, and high-value agency placement) and so asks whether any predetermined characteristic predicts winning, with the unwinsorized salary pair left out as nearly collinear with its winsorized counterpart. Blank cells are combinations not estimated: the full-bundle test is run on the pooled sample only. Incumbency is defined against the previous cycle's roster of elected branch officers, which exists only in the Colorado party data, so the pooled and Liberal columns are blank for that row and its control mean is the Colorado one. The density test rows report robust p-values from Cattaneo, Jansson and Ma (2020); the paired winner-loser construction makes the running variable's density symmetric by construction, so these are reported for completeness only. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A2: Covariate balance at the cutoff: branch members

Covariate	Ctrl. mean	Pooled	ANR	PLRA	2015	2022
Employed, pre (pp)	32.76	−3.26	−1.86	−25.72	−4.69	−2.09
		(7.39)	(7.55)	(19.54)	(11.12)	(12.12)
Tenured, pre (pp)	22.00	−9.56	−9.39	−14.58	−19.24*	0.60
		(6.32)	(6.63)	(17.79)	(10.74)	(9.63)
Mean salary, pre (Million Gs.)	1.71	1.12	1.94	−2.34**	3.99	−0.66
		(1.35)	(2.26)	(1.13)	(4.31)	(0.75)
Max. salary, pre (Million Gs.)	2.87	1.02	1.90	−4.45**	4.12	−0.99
		(1.48)	(2.39)	(2.03)	(4.43)	(1.46)
Mean salary, pre, wins. (Million Gs.)	1.74	−0.12	0.02	−2.34**	0.23	−0.54
		(0.51)	(0.52)	(1.13)	(0.80)	(0.73)
Max. salary, pre, wins. (Million Gs.)	2.86	−0.09	0.16	−4.35**	0.69	−0.43
		(0.89)	(0.97)	(2.02)	(1.31)	(1.36)
High-value agency, pre (pp)	1.77	2.84	4.80	−10.17	7.06*	2.41
		(2.37)	(3.00)	(6.28)	(4.01)	(3.87)
Male (pp)	68.39	−1.14	2.84	−13.71	−7.23	13.56
		(6.45)	(7.35)	(19.86)	(9.72)	(11.70)
Age at election (years)	41.9	0.5	0.4	3.0	−2.0	1.5
		(1.5)	(2.1)	(4.1)	(2.5)	(3.1)
Incumbent (pp)	16.89	–	−6.83	–	−8.96	−6.24
			(5.87)		(9.11)	(8.34)
Joint F p (male, age)		0.680	–	0.550	0.459	0.329
Joint F p (full pre-period set)		0.768	–	–	–	–
Density test p		0.807	–	0.998	1.000	0.563

Notes: See the notes to Table A1. The Liberal 2021 column shows the pre-period salary imbalance discussed in Section 4: marginal member winners in that cycle look weaker on pre-election salary. The pooled member sample is balanced on every covariate and the joint tests do not reject the null of balance. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A3: Robustness of the pooled RD estimates

Outcome	Pooled	Below median	Excl. recov.	+Male, age	+Pre-period
Panel A: Branch presidents					
Employed (pp)	3.96	1.96	3.96	4.13	3.37
	(4.06)	(5.19)	(4.06)	(4.07)	(3.96)
	[1281]	[579]	[1281]	[1273]	[1289]
Tenured status (pp)	3.33	−2.79	3.33	3.38	2.76
	(3.89)	(4.40)	(3.89)	(3.88)	(3.83)
	[1139]	[591]	[1139]	[1137]	[1147]
High-value agency (pp)	0.98	−0.51	0.98	1.02	1.04
	(2.11)	(2.32)	(2.11)	(2.13)	(2.11)
	[1185]	[729]	[1185]	[1175]	[1185]
Mean salary, wins. (Million Gs.)	1.18*	0.05	1.18*	1.20*	1.08
	(0.69)	(0.49)	(0.69)	(0.69)	(0.69)
	[1219]	[545]	[1219]	[1213]	[1227]
Max. salary, wins. (Million Gs.)	2.85**	0.09	2.85**	2.85**	2.21*
	(1.24)	(0.91)	(1.24)	(1.24)	(1.14)
	[1219]	[549]	[1219]	[1215]	[1225]
Entered public sector (pp)	3.48	4.27	3.48	3.35	2.91
	(2.91)	(3.62)	(2.91)	(2.88)	(2.90)
	[1311]	[699]	[1311]	[1315]	[1309]
Panel B: Branch members					
Employed (pp)	0.22	−4.07	0.24	0.17	−0.58
	(3.22)	(4.76)	(3.21)	(3.23)	(3.27)
	[1260]	[601]	[1258]	[1242]	[1166]
Tenured status (pp)	0.58	−4.30	0.57	0.57	0.33
	(2.91)	(3.70)	(2.92)	(2.91)	(2.90)
	[1045]	[380]	[1041]	[1043]	[1043]
High-value agency (pp)	−2.37**	−5.07	−2.39**	−2.39**	−2.59**
	(1.21)	(3.12)	(1.21)	(1.21)	(1.22)
	[1137]	[459]	[1127]	[1133]	[1071]
Mean salary, wins. (Million Gs.)	−0.29	−0.48*	−0.29	−0.28	−0.25
	(0.26)	(0.28)	(0.27)	(0.26)	(0.25)
	[1280]	[467]	[1266]	[1282]	[1238]
Max. salary, wins. (Million Gs.)	−0.85*	−0.80	−0.85*	−0.85*	−0.76*
	(0.46)	(0.57)	(0.46)	(0.45)	(0.46)
	[1260]	[469]	[1258]	[1246]	[1159]
Entered public sector (pp)	1.43	−1.66	1.44	1.43	1.11
	(2.49)	(4.36)	(2.49)	(2.50)	(2.52)
	[1208]	[491]	[1202]	[1196]	[1176]

Notes: Each cell reports the bias-corrected pooled RD estimate, with robust standard errors in parentheses and effective sample sizes in brackets; units follow Table 1. The Pooled column repeats the baseline. Below-median branch restricts to branches below the median electorate size within their cycle. Excl. recovered drops the seven Liberal member *comités* whose winner row is reconstructed from the roster of elected members (Section 3). The last two columns add covariates to the baseline specification: gender and age, then additionally pre-period employment, tenure, and winsorized mean and maximum salary. Presidential rows pool the two estimands described in Section 4. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Per-cycle falsification and salary diagnostics

The remaining appendix material reports two diagnostics, each estimated separately within every cycle. Table A4 is a grid of placebo RDs at artificial cutoffs, and Table A5 expresses the salary effects as percentages of the control mean at the cutoff. Incumbency balance belongs with the other predetermined covariates and so appears as a row of Tables A1 and A2. It is estimated for the Colorado samples alone, for the reason given in Section 4. Per-cycle RD plots are omitted, since they would only redraw estimates that Tables A1–A3 already carry for all three cycles.

Placebo RDs with artificial cutoffs

Table A4: Artificial cutoff placebo RD estimates, by cycle

Outcome	PLRA		2015		2022	
	Left	Right	Left	Right	Left	Right
Panel A: Branch presidents						
Employed (pp)	−6.10	11.08	5.15	−28.51	−31.22*	−32.42*
	(5.11)	(10.32)	(16.70)	(20.23)	(17.69)	(17.94)
Tenured status (pp)	16.17	−10.70	9.71	−15.22	−12.86	−0.53
	(14.45)	(14.78)	(10.44)	(12.17)	(14.89)	(12.18)
High-value agency (pp)	−1.65	11.91	8.49	−9.54	0.96	−2.38
	(1.23)	(12.23)	(8.83)	(9.36)	(0.85)	(1.84)
Mean salary (Million Gs.)	0.85	−3.58	−3.24	−2.87	−1.58	0.99
	(0.96)	(2.79)	(3.46)	(4.37)	(2.07)	(2.15)
Max. salary (Million Gs.)	−0.81	2.25	4.52	1.42	1.96	0.20
	(0.77)	(2.20)	(3.84)	(4.92)	(3.74)	(5.09)
Panel B: Branch members						
Employed (pp)	−3.08	−14.42	−10.43	2.81	10.70	26.14**
	(3.21)	(33.35)	(14.70)	(9.81)	(19.93)	(12.33)
Tenured status (pp)	0.11	−17.85	−8.33	−2.79	−1.88	−5.75
	(0.20)	(26.81)	(7.99)	(2.51)	(1.81)	(7.88)
High-value agency (pp)	–	1.96	13.06	−0.46	1.54	4.18
		(1.24)	(12.80)	(0.44)	(1.79)	(6.36)
Mean salary (Million Gs.)	0.05	0.78	0.03	0.05	−0.83	2.35
	(0.12)	(0.63)	(0.21)	(0.89)	(0.63)	(2.01)
Max. salary (Million Gs.)	0.16	0.66	−1.26*	−0.32	−1.67	−1.23
	(0.38)	(0.52)	(0.75)	(2.09)	(1.12)	(1.34)

Notes: Each row reports bias-corrected placebo RD estimates at artificial cutoffs, one on each side of the true threshold, placed at the median of the running variable among observations inside the true cutoff's MSE-optimal bandwidth on that side. Estimation is restricted to that side so the true cutoff cannot contaminate it (Cattaneo, Idrobo and Titiunik, 2020), and each placebo selects its own MSE-optimal bandwidth around its artificial cutoff. Robust standard errors in parentheses; units follow Table 1, with unwinsorized salaries. The cutoffs themselves are mechanical and cycle-specific, so they are not printed. One of the 59 estimates is significant at the 5% level (member employment on the winners' side of the 2022 Colorado cycle), fewer than the three chance alone would deliver. The sixtieth cell, member high-value agency on the losers' side of the Liberal cycle, is blank: too few observations survive on that side of the shifted cutoff for the estimator to run. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Salary effects as percentages of the control mean

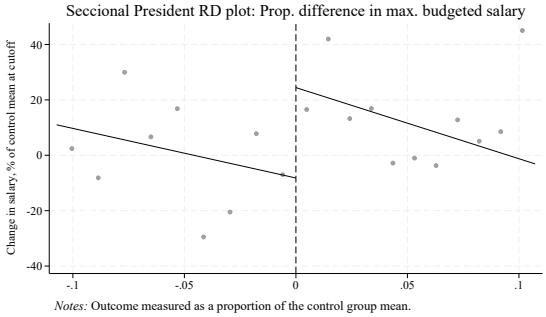
Table A5: Salary effects as a percentage of the control mean at the cutoff, by cycle

Outcome	PLRA	2015	2022
Panel A: Branch presidents			
Mean salary (Million Gs.)	0.62	1.29	2.15*
	(0.53)	(1.68)	(1.21)
	[18.4%]	[25.6%]	[25.8%]
Max. salary (Million Gs.)	2.43*	2.04	5.10**
	(1.36)	(2.97)	(2.26)
	[43.7%]	[27.4%]	[36.9%]
Panel B: Branch members			
Mean salary (Million Gs.)	−0.22*	−4.19	0.16
	(0.13)	(4.24)	(0.43)
	[−7.1%]	[−194.6%]	[7.7%]
Max. salary (Million Gs.)	−0.11	−4.42	−0.25
	(0.38)	(4.36)	(0.86)
	[−2.1%]	[−141.2%]	[−5.8%]

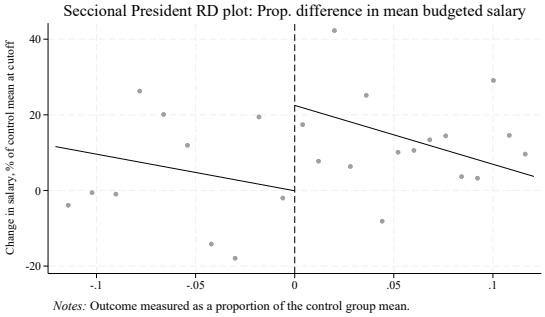
Notes: Each cell reports the bias-corrected RD estimate on the pre-to-post change in unwinsorized monthly budgeted salary, in millions of guaranies, with robust standard errors in parentheses and, in brackets, the estimate as a percentage of the predicted post-election salary of barely losing candidates at the cutoff. Following (Chen and Roth, 2024), this percentage replaces the $\ln(Y+1)$ transformation, which has no unit-invariant reading when salary is exactly zero for candidates off the payroll. Estimated separately within each cycle: the routine takes no covariates, so a pooled row could not carry the cycle fixed effects and branch clustering the rest of the paper uses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure A1: Salary Effects as Percentages of Barely-Losing Candidates' Salaries for Presidents

(a) Maximum



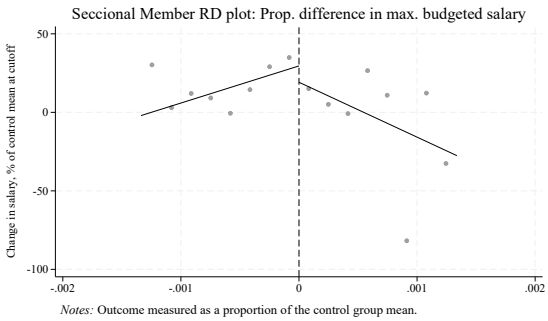
(b) Mean



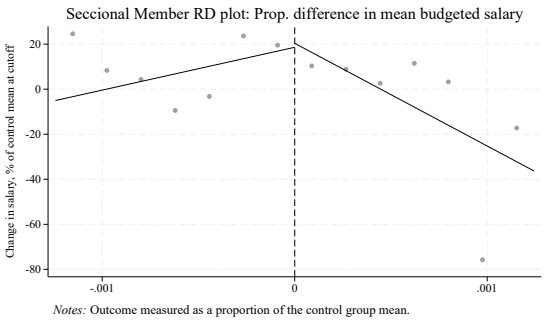
Notes: The panels show RD plots for changes in maximum and mean monthly budgeted salary. Following Chen and Roth (2024), I express the salary effects as percentages of the predicted post-election salary of barely losing candidates at the cutoff, avoiding the use of log-like transformations such as $\ln(Y+1)$ when salary differences can equal zero. Estimates use local linear RD specifications with triangular kernels, MSE-optimal bandwidths, and robust bias-corrected inference following Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2020). For maximum salary, the RD estimate is around 36.9% of the control mean at the cutoff ($p = 0.024$). For mean salary, the RD estimate is around 25.8% of the control mean at the cutoff ($p = 0.077$).

Figure A2: Salary Effects as Percentages of Barely-Losing Candidates' Salaries for Members

(a) Maximum



(b) Mean



Notes: The panels show RD plots for changes in maximum and mean monthly budgeted salary. Following Chen and Roth (2024), I express the salary effects as percentages of the predicted post-election salary of barely losing candidates at the cutoff, avoiding the use of log-like transformations such as $\ln(Y+1)$ when salary differences can equal zero. Estimates use local linear RD specifications with triangular kernels, MSE-optimal bandwidths, and robust bias-corrected inference following Calonico, Cattaneo and Titiunik (2014) and Calonico, Cattaneo and Farrell (2020). For maximum salary, the RD estimate is around -5.8% of the control mean at the cutoff ($p = 0.768$). For mean salary, the RD estimate is around 7.7% of the control mean at the cutoff ($p = 0.703$).