

Information and voting: Evidence from Peru’s 2026 general elections §

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This version: October 31, 2026

Abstract

Logistical failures during Peru’s 2026 general elections postponed voting at thirteen locations in Lima Sur, so that roughly 55,000 registered electors cast ballots after election-night estimates and partial official returns had become public. We exploit this natural experiment to study whether public signals about candidate viability reshape choices on simultaneous presidential and legislative ballots. In our model of joint electoral choice, voters update beliefs about runoff qualification and legislative prospects and trade off candidate affinity, office-specific preferences, and the value of aligned representation. Upward revisions to perceived viability raise a candidate’s presidential support and, through aligned representation, that of its legislative list, while office-specific preferences can generate divergent responses across ballots. Using cardinality matching, we compare delayed locations to election-day locations balanced on prior electoral preferences, demographics, and socioeconomic conditions. Estimates, as shares of the registered electorate, reveal substantial reallocation of presidential support: Nieto, López Aliaga, and Sánchez gain 11.2, 3.9, and 1.2 percentage points; Álvarez, López Chau, and Belmont lose 4.4, 2.9, and 2.8; the remaining parties jointly lose 9.9; blank and invalid ballots fall by 2.9; and Fujimori’s estimate is indistinguishable from zero. Legislative support moves in the same direction for every party, but by smaller magnitudes. The results are consistent with perceived viability shaping choice across offices, with distinct presidential and legislative responses, and underscore the stakes of rules governing the release of election-night estimates and partial results while some electors have yet to vote.

JEL Classifications: D72, D82, D83, C21, C93

Keywords: electoral information, Bayesian updating, strategic voting, exit polls, natural experiment, cardinality matching.

AI disclosure: Earlier versions used Anthropic’s Claude for prose editing, code checks, and L^AT_EX formatting, and Refine (refine.ink) for referee-style feedback. This revision used OpenAI Codex for language editing, mathematical review, and document preparation. The authors remain responsible for the content and for verification of the analysis and references. All errors are our own.

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§We thank César Martinelli and José Gallardo for valuable feedback on this paper, as well as the participants in the XIV Coloquio de Estudiantes de Economía at PUCP (2026) for valuable comments, in particular Alejandro Granda, Janneth Leyva, and Aldo Ramírez.

1 Introduction

Public information can change an electoral choice without changing a voter’s preferences. A voter may favor one candidate but support another when new information changes which acceptable alternatives appear capable of reaching a runoff. This distinction between affinity and perceived viability is particularly relevant in fragmented elections with weak party attachments. We examine it using an unexpected interruption of Peru’s 2026 presidential and legislative elections.

On Sunday, 12 April, Peru held the first round of its 2026 presidential and legislative election. Yet, for 55,261 registered electors, election day did not unfold as planned. At 13 voting centers in Lima Metropolitana, comprising 187 polling stations, electoral materials failed to arrive on time. The *Jurado Nacional de Elecciones* (JNE) responded by extending voting at the affected centers to Monday, 13 April ([Jurado Nacional de Elecciones, 2026e](#); [Oficina Nacional de Procesos Electorales, 2026a](#)).

Meanwhile, on election night, the two leading polling firms, Ipsos and Datum Internacional, released their *flash electoral* estimates within minutes of one another. Both placed Keiko Fujimori in the lead, but they disagreed on who ranked second. Datum named Rafael López Aliaga, while Ipsos named Roberto Sánchez. Ricardo Belmont and Jorge Nieto remained competitive and within the margin of error in both estimates, leaving the contest for second place uncertain.¹ Ipsos also released legislative estimates ([Ipsos Perú, 2026](#)). By the time voting resumed on Monday, partial official returns and further news had also become available, giving Monday voters access to information unavailable to electors who had voted before these releases.

The unexpected delay created an opportunity to examine whether public signals about candidate viability reshape electoral choices. To do so, we compare the thirteen delayed locations with Sunday locations in the districts of Lima Sur,² selected through cardinality matching on pre-treatment electoral, demographic, and socioeconomic characteristics. Under conditional comparability and no interference, this comparison identifies the average effect across treated locations of a delayed-voting regime that combines access to additional information with changed participation conditions.

We measure each outcome as a share of registered electors, dividing candidate and party votes, blank and invalid ballots, and abstention by the location’s registered electorate. To summarize legislative support, we use the minimum of a party’s shares across the three legislative contests. This index measures aggregate support, and its difference from presidential support seeks to identify the gap between support for the presidential

¹We now know that Roberto Sánchez ultimately qualified to face Keiko Fujimori in the runoff ([El Peruano, 2026](#)).

²The control pool comprises the Sunday polling locations in the eleven districts of Lima Sur: Chorrillos, Lurín, Pachacámac, Pucusana, Punta Hermosa, Punta Negra, San Bartolo, San Juan de Miraflores, Santa María del Mar, Villa El Salvador, and Villa María del Triunfo.

candidate and the party’s core vote. Using these measures, we find substantial changes in presidential support. Nieto gains 11.2 points, López Aliaga 3.9, and Sánchez 1.2. Fujimori gains 0.8 points, although this estimate is not statistically distinguishable from zero. By contrast, Álvarez, López Chau, and Belmont lose 4.4, 2.9, and 2.8 points, respectively, while the remaining parties jointly lose 9.9 points. The blank-and-invalid share falls by 2.9 points, and abstention rises by 5.8 points. The corresponding party-level legislative index moves in the same direction as each of the eight presidential estimates, but by smaller amounts. These legislative responses may reflect the direct news contained in Ipsos’s estimates, complementarity with presidential choices, or both.

We then develop a model of joint presidential and legislative choice in which voters update their beliefs about runoff qualification and legislative prospects in response to public signals. Voters weigh these beliefs alongside their affinity for candidates, legislative preferences, and the expected benefit of aligned representation. Depending on their prior preferences and interpretation of the news, they may switch their presidential vote while retaining their legislative choice or increase their support for the same party across offices.

The paper contributes evidence on actual electoral returns following an unusual difference in access to late information, complementing survey and laboratory studies (Agranov et al., 2018; Dahlgaard et al., 2017) and natural experiments involving exit polls or partial counts (Araújo & Gatto, 2022; Morton et al., 2015). It extends the comparison across presidential and legislative contests and provides a common framework for interpreting their shared movement and divergence.

Section 2 describes the election and the voting delay. Section 3 develops the political and literature background. Sections 4 and 5 present the model and its empirical implications. Section 6 describes the matching design and estimation; Sections 7 and 8 present the results and discuss the evidence and its limits.

2 Peru’s 2026 general elections

2.1 Electoral framework

Peru holds general elections every five years. Presidential elections follow a two-round system. A candidate wins in the first round by obtaining more than 50% of valid votes, excluding blank and invalid ballots. Otherwise, the two leading candidates compete in a runoff. Legislative elections are decided in a single round, held simultaneously with the first presidential round. Beginning in 2026, voters elect a bicameral Congress comprising 60 senators and 130 deputies.

The ballot contains separate sections for the presidential ticket, senators elected in a single nationwide district, senators elected through multiple electoral districts, deputies,

and representatives to the Andean Parliament. Voters can support a different party in each section.

Access to congressional seats is subject to an electoral threshold (*valla electoral*), applied separately to each chamber. A party must simultaneously obtain at least 5% of valid votes nationwide for the corresponding chamber and qualify for at least 5% of its seats, equivalent to three senators and seven deputies. For the Senate, the vote threshold is calculated by combining votes from both electoral modalities ([Jurado Nacional de Elecciones, 2026a](#)).

2.2 Peruvian political crisis (2016–present)

Peru entered the 2026 elections amid a prolonged political, institutional, and social crisis. Since 2016, confrontations between the executive and Congress, corruption scandals, and rapid presidential turnover had deepened institutional instability. The broad constitutional provision permitting presidential removal on grounds of “permanent moral incapacity” facilitated the recurrent use of vacancy motions as instruments of political competition ([Eguiguren Praeli, 2017](#)). Castillo’s attempted self-coup in December 2022 and subsequent mass protests further marked this deterioration ([Barrenechea & Vergara, 2023](#)). By the eve of the election, nine individuals had held the presidency since 2016, while congressional measures had weakened judicial independence and restricted investigations into corruption and organized crime ([Human Rights Watch, 2026](#)).

Political polarization had also intensified during these years. Examining the period 2000–2023, [Mejía Navarrete \(2024\)](#) identifies a marked increase since 2016, encompassing ideological divisions, hostility toward political opponents, and broader social fractures. These divisions made dialogue and political agreement increasingly difficult, adding to an already strained institutional environment.

Meanwhile, economic growth had slowed considerably. Average annual GDP growth declined from 5.34% during 2001–2015 to 2.46% during 2016–2025, a period that also included a recession in 2023. Poverty increased sharply during the COVID-19 pandemic and had yet to return to its pre-pandemic level. Peru thus approached the election amid institutional turmoil and heightened polarization, accompanied by slower growth and persistent social hardship.

2.3 Candidates and pre-electoral polls

The 2026 presidential field was highly fragmented, creating acute uncertainty over which candidate would secure the second runoff spot. While the leading contenders spanned distinct programmatic positions, candidates occupying adjacent ideological spaces often competed through separate electoral vehicles. The descriptive classification in [Table 9](#) ([Appendix A](#)) summarizes an interpretation of government plans. Furthermore, Support

was fragmented across candidates, with Fujimori polling at roughly 14% to 19% during the final month (Figure 3, Appendix A). Among her challengers, rankings shifted substantially between polling waves, as shown by the regular Ipsos and Datum series (Figures 1 and 2, Appendix A), leaving no clear second contender for the runoff.³ Earlier April waves showed considerable dispersion, with several candidates below 7% and Belmont above 13% on 8 April. By the 11 April Ipsos *simulacro* and the 12 April flash, six leading challengers were within a 7% to 13% band. The publication of voting-intention polls in domestic media was prohibited from 6 April until the election ([Jurado Nacional de Elecciones, 2026b](#)). Against this backdrop, the electoral flash, released after a week without published polls and based on information collected on election day, provided a more timely and potentially more reliable signal of candidates' relative chances of reaching the runoff than pre-election polls.

2.4 The administrative failure

On Sunday 12 April, election day proceeded normally throughout Peru except in Lima, where many polling stations opened late. The most serious incident involved thirteen *locales de votación* in Lima Metropolitana, comprising 187 polling tables and 55,261 registered electors, where electoral materials did not arrive in time and the tables were never installed. ONPE documented the affected locations, tables, and registered electorate ([Oficina Nacional de Procesos Electorales, 2026a](#)).

The affected locations were concentrated in three southern Lima districts: San Juan de Miraflores (three locations), Lurín (seven), and Pachacámac (three). The JNE agreement lists their addresses, polling tables, and registered electorates ([Jurado Nacional de Elecciones, 2026c, annex](#)). A subsequent official audit traced the disruption to delayed equipment redeployment and delivery-route rescheduling as well as transport failures ([Contraloría General de la República, 2026](#)).

While these voters remained unable to vote, the election-night information arrived on schedule. At 18:00 on 12 April, Ipsos and Datum released their *flash electoral* estimates (Table 1). Both placed Fujimori first but disagreed on second: Datum put López Aliaga second (12.8%) with Nieto third (11.6%); Ipsos put Sánchez second (12.1%) with Belmont third (11.8%).

The presidential estimates placed Fujimori ahead while leaving several candidates close to second place. A right-versus-right runoff was in view, but Sánchez appeared as a viable left contender in the Ipsos estimate, while the centrist candidates Nieto and Belmont remained within the margin of error. Voters' assessments of Belmont's candidacy may

³The series are compiled from the *La Encerrona* tracker ([La Encerrona, 2026](#)). Some Ipsos *simulacros* correspond to survey waves conducted during the period in which the publication of voting-intention polls in Peruvian media was prohibited. These polls collected information during that period but were not published in Peru before the election.

Table 1: Election-night exit-poll estimates, 12 April 2026, 18:00

Candidate	Datum (%)	Ipsos (%)
Keiko Fujimori	16.5	16.6
Rafael López Aliaga	12.8	11.0
Jorge Nieto	11.6	10.7
Ricardo Belmont	10.5	11.8
Roberto Sánchez	10.0	12.1
Alfonso López Chau	8.6	7.1
Carlos Álvarez	7.1	7.0
Others	22.9	23.7

Source: Authors’ compilation from [Agencia Andina \(2026\)](#) and [Ipsos Perú \(2026\)](#).

also have been affected by reports about his absence from the polls and questions about his health ([Espinoza, 2026](#)).⁴ Ipsos also published estimates for the national Senate and, in Lima, the district Senate and deputies (Table 2)⁵([Ipsos Perú, 2026](#)).

That night, the JNE extended voting to Monday, 13 April, at locations where materials had not arrived. The ruling also covered specified overseas locations, which are outside our sample ([Jurado Nacional de Elecciones, 2026e](#)). The agreement preserved ONPE’s processing and publication of results, maintained electoral-propaganda restrictions in the affected locations, and urged that polls and quick counts not be disseminated ([Jurado Nacional de Elecciones, 2026c, points 3–4](#)). The information already released nevertheless remained available to voters at the delayed locations, whereas those at comparison locations had voted on Sunday. Monday voters thus faced new information about multiple contests. Table 3 records the verified announcement times.

Table 2: Ipsos legislative exit-poll estimates released at 18:00 on 12 April

Party	National Senate	Lima Senate	Lima deputies
Fuerza Popular	14.6	16.4	15.8
Renovación Popular	10.5	20.2	19.2
Partido del Buen Gobierno	10.6	16.7	15.3
Juntos por el Perú	11.5	—	—
Partido Cívico Obras	10.5	10.1	9.8
Ahora Nación	7.8	8.6	8.7
País para Todos	6.0	6.7	6.4
Primero la Gente	—	4.6	4.1
Others (as published)	28.5	16.7	20.7

Source: [Ipsos Perú \(2026, pp. 14, 16–17\)](#). Entries are published party-share percentages. A dash means the party is not separately reported and is included in “Others”. National and Lima columns cover different electorates.

⁴At 80, Belmont was not legally required to vote. On 9 April he had stated publicly that his doctors had found an unspecified condition and that he was awaiting test results ([Espinoza, 2026](#)).

⁵The flash tables report candidate and party shares without a separate blank-and-invalid category, whereas our outcomes use registered electors as their denominator.

Table 3: Documented timing of public information and voting arrangements

Time (Peru)	Documented event or announcement
12 April, 13:29	JNE notice announces that Sunday voting is extended through 18:00 (Jurado Nacional de Elecciones, 2026d).
12 April, 17:02	Press report that Belmont has not appeared at his assigned polling location, recalling his 9 April statements about health complications; the version updated at 19:10 states that he did not vote before the 18:00 close (Espinoza, 2026).
12 April, 18:00	Presidential flash estimates are released. Ipsos also publishes the national Senate, Lima Senate, and Lima deputy estimates in Table 2 (Agencia Andina, 2026 ; Ipsos Perú, 2026).
12 April, 20:33	ONPE notice confirms that its results portal was enabled at 18:00 and that overseas returns are already available. Records enter the count as they are processed; this is not a representative nationwide snapshot (Oficina Nacional de Procesos Electorales, 2026b).
12 April, 21:59	ONPE announces Monday voting at the thirteen Lima locations. The JNE agreement preserves official counting and publication while extending voting (Jurado Nacional de Elecciones, 2026c ; Oficina Nacional de Procesos Electorales, 2026a).
13 April, 07:00–18:00	Scheduled Monday voting window at the affected locations (Oficina Nacional de Procesos Electorales, 2026a).

Notes. Times 13:29, 20:33, and 21:59 are the publication times of the respective official notices. Time 17:02 is the first publication time of the press report.

3 Background and literature

3.1 Political parties and the electorate in Peru

Peru combines weak positive party identification with a fragmented party system in which electoral organizations often lack continuity ([Barrenechea & Vergara, 2023](#)). This instability complicates representation and accountability, which depend in part on identifiable competitors that persist across elections ([Mainwaring, 2018](#)). Weak party attachments do not, however, imply an absence of political identities, as rejection of a movement can organize preferences even without loyalty to a competing party ([Meléndez, 2022](#)).

Comparative research links partisan detachment to difficulties in public-service provision ([Morales Quiroga, 2011](#)) and to economic vulnerability that limits parties’ capacity to represent voters’ interests ([Altamirano, 2019](#)). Blurred party brands can further weaken identification by reducing the distinctions voters perceive between organizations ([Lupu, 2014](#)). These findings help explain why electoral support need not translate into stable party loyalty.

Weak partisan attachment also does not preclude substantive voting. In Peru’s 2016 presidential election, voters’ choices were related to their positions on economic intervention and the discretionary exercise of political power ([Sulmont, 2018](#)). Programmatic affinities can therefore coexist with political aversions. In a Peruvian survey experiment, [de la](#)

Cerda (2026) finds that cues associated with a disliked political group reduce policy support, without a detectable positive effect from ingroup cues. These findings motivate distinguishing substantive agreement from opposition to an alternative.

Electoral viability adds a separate consideration. In a two-round system, a voter may abandon a preferred but uncompetitive candidate for an acceptable alternative with better prospects of advancing. Evidence from Brazil’s 2018 presidential election links this type of strategic voting to ideological preferences and voters’ confidence in their own predictions of the electoral outcome (Plutowski et al., 2021). Rejection can also make viability more valuable: German voters crossed party lines to support viable opponents of the AfD in competitive districts (Hillen & Kuhlmann, 2026), while US primary-election experiments show that negative partisanship can increase willingness to trade policy proximity for electability (Albert & Costa, 2025).

Separate legislative ballots add office-specific preferences, a possible benefit from supporting the same party across offices, and the potential for presidential coattails to influence legislative voting (Gomberg et al., 2019). The model below derives the resulting choice between aligned and split tickets.

3.2 Related literature

Our paper draws on four literatures. The first studies information and Bayesian updating. Information design formalizes how signals map states into posterior beliefs (Bergemann & Morris, 2019; Kamenica & Gentzkow, 2011). We use the language of signals and likelihoods, without modeling pollsters as strategic persuaders.

To take this structure to behavior, we turn to a second literature on choice modeling. Expected utility over complete ballot combinations provides a decision-theoretic foundation for the tradeoff between legislative affinity and aligned representation. A joint random-utility logit (Train, 2009) maps those utilities into smooth choice probabilities. We use this framework to interpret aggregate returns without estimating individual preference or belief parameters.

The methodological core of the paper draws on the literature on matching for causal inference. We use cardinality matching to maximize sample size subject to balance constraints on pre-treatment covariates, followed by minimization of Mahalanobis distances within the selected sample (Visconti & Zubizarreta, 2018; Zubizarreta et al., 2014). We allow the number of controls to vary across treated locations (Ming & Rosenbaum, 2000). Under selection on observables and overlap, this design supports identification of the ATT for retained treated locations. We discuss design-consistent sensitivity analysis using Rosenbaum bounds (Rosenbaum, 1987, 1988) and inference with few treated locations under explicit assignment or symmetry assumptions (Canay et al., 2017; Ferman, 2021).

Finally, our paper relates to the literature on how polling information can shape voting behavior. In Europe, survey experiments have studied the effects of fictional polling news and real polling figures on stated voting intentions (Dahlgaard et al., 2017; van der Meer et al., 2016). In Latin America, research in Mexico has used survey experiments and observational data to examine polling information, electoral expectations, and strategic voting (Cantú & Márquez, 2021; Castro Cornejo, 2022). Laboratory elections in the United States have examined how polls and beliefs about electoral outcomes affect costly participation (Agranov et al., 2018).

Related research has studied information released during or after elections, distinguishing polling estimates from actual vote counts. In Europe, a natural experiment in France has examined how early access to exit-poll information affects observed turnout and voting (Morton et al., 2015), while an event-study design in Germany has examined how state election results affect subsequent voting intentions elsewhere in the country (Gerling & Kellermann, 2022). In Latin America, a natural experiment arising from unexpected voting delays in Brazil has examined how exposure to partial election results affects votes subsequently cast (Araújo & Gatto, 2022). Therefore, our contribution rests on a rare event and a new dataset: linking 2026 presidential and legislative returns to 2021 returns, the electoral roll, and census data for 364 polling locations in the districts of Lima Sur, we estimate how support shifted when thirteen of them voted the day after exit polls and partial results became public.

4 Information, Bayesian updating, and choices across ballots

This section provides a microfoundation for individual presidential and legislative choices in response to electoral news. We derive how new information can change beliefs about electoral viability and the expected benefit of aligned representation, and how these changes affect joint presidential and legislative choices. Aggregating those individual decisions connects the model to the voting patterns studied empirically: shifts in presidential support, changes in legislative support, and differences between them. The framework supplies a behavioral mechanism for interpreting the estimated responses to delayed voting. We do not estimate its individual-level parameters or identify its channels separately. With secret ballots, the available returns report aggregate counts rather than linked individual choices across offices; voters' beliefs and exposure to specific information sources are unobserved.

4.1 States, signals, and posterior beliefs

Let $\mathcal{C}$ be the presidential candidates and $\tilde{\mathcal{C}} = \mathcal{C} \cup \{\emptyset\}$ include the observed blank-and-invalid category. Let $\mathcal{P}$ be the set of all parties in the election, $\mathcal{P}_r \subseteq \mathcal{P}$ the parties available in legislative contest $r \in \{1, 2, 3\}$, and $\tilde{\mathcal{P}}_r = \mathcal{P}_r \cup \{\emptyset\}$. The mapping $g : \tilde{\mathcal{C}} \rightarrow \mathcal{P} \cup \{\emptyset\}$ assigns each valid presidential candidate c to her party, with $g(\emptyset) = \emptyset$. A candidate's party need not be available in every legislative contest. Conditional on a runoff being held, the finite state space Ω specifies both entrants and the prospective legislative environment. Write $I_c(\omega) = 1$ if c reaches the runoff in state ω and 0 otherwise, with $I_\emptyset = 0$. Keeping Ω fixed before and after the news avoids eliminating candidates solely because a flash estimate placed them below a cutoff. Fujimori's qualification can be highly likely without being certain.

A belief is a probability distribution over Ω ; the set of beliefs is the unit simplex

$$\Delta(\Omega) = \left\{ \mu \in \mathbb{R}_+^{|\Omega|} : \sum_{\omega \in \Omega} \mu(\omega) = 1 \right\}.$$

Voter j has prior belief $\mu_j^0 \in \Delta(\Omega)$, where $\mu_j^0(\omega)$ is the probability assigned to electoral state ω before the news. The voter observes news s , including the flash estimates and related announcements. With perceived joint likelihood $L_j(s \mid \omega)$ and positive prior predictive probability, Bayes' rule gives the posterior probability of each state:

$$\mu_j^1(\omega) = \frac{L_j(s \mid \omega) \mu_j^0(\omega)}{\sum_{\nu \in \Omega} L_j(s \mid \nu) \mu_j^0(\nu)}. \quad (1)$$

For any belief distribution μ over Ω , define candidate c 's perceived probability of reaching the runoff as

$$q_c(\mu) = \sum_{\omega \in \Omega} I_c(\omega) \mu(\omega) = \mathbb{P}_\mu(I_c = 1). \quad (2)$$

Thus $\mu_j^t(\omega)$ describes belief about a complete electoral state, whereas $q_c(\mu_j^t)$ sums the probabilities of all states in which c qualifies, for $t \in \{0, 1\}$. For any two states with positive prior and posterior probabilities,

$$\frac{\mu_j^1(\omega)}{\mu_j^1(\nu)} = \frac{\mu_j^0(\omega)}{\mu_j^0(\nu)} \frac{L_j(s \mid \omega)}{L_j(s \mid \nu)}. \quad (3)$$

News raises the relative probability of states under which it is more likely. This is how the flash estimates can change a candidate's perceived chance of qualifying.

Disagreement between Datum and Ipsos does not imply that every nearby contender gains viability. The full margins and perceived reliability matter. We do not assume independent errors: source likelihoods can be multiplied only under conditional independence. Candidate-specific news can change likelihoods without assigning a zero probability of

qualification, so Belmont remains in the choice set. Legislative beliefs can also change, since Ipsos released legislative estimates (Section 2.4), so legislative responses need not run only through presidential choices.

4.2 Expected utility and the reason for splitting a ticket

For an attending voter, $\mathbf{k} = (k_1, k_2, k_3)$ records the parties chosen for senators elected nationwide (k_1), senators elected by electoral district (k_2), and deputies (k_3), respectively. Each $k_r \in \tilde{\mathcal{P}}_r$ is an available party or $\emptyset$ (blank or invalid); parties may differ across contests. The state-dependent utility of $(c, \mathbf{k})$ is

$$u_j(c, \mathbf{k}; \omega) = a_j(c) + w_j(c)I_c(\omega) + \sum_{r=1}^3 h_{jr}(k_r; \omega) + \sum_{r=1}^3 \mathbf{1}\{c \neq \emptyset, k_r = g(c)\} [\kappa_{jr} + \gamma_{jr}I_c(\omega)\eta_{cr}(\omega)]. \quad (4)$$

Each term has the following interpretation:

- $a_j(c)$ is voter j 's presidential affinity for c .
- $w_j(c)I_c(\omega)$, with $w_j(c) \geq 0$, is the value of supporting a candidate who qualifies; it is obtained only in states where c reaches the runoff.
- $h_{jr}(k_r; \omega)$ is legislative utility in contest r . It combines preferences for a party or its candidates with the value of its representation.
- $\mathbf{1}\{c \neq \emptyset, k_r = g(c)\}$ indicates an aligned choice: a valid presidential vote and a vote for the same party in contest r .
- $\kappa_{jr} \geq 0$ is the consistency benefit of an aligned choice, whether or not c qualifies.
- $\gamma_{jr}I_c(\omega)\eta_{cr}(\omega)$ is the policy benefit of an aligned choice if c qualifies, with weight $\gamma_{jr} \geq 0$. The term $\eta_{cr}(\omega) \in [0, 1]$ measures how useful that party's legislative representation would be for c 's agenda, including prospects of eventual executive power.

Set $w_j(\emptyset) = \eta_{\emptyset r}(\omega) = 0$.

These are preferences over the political outcomes a voter supports. They provide an expected-utility foundation for viability-sensitive choice. Blank and invalid ballots share an observed category; the model does not imply that all are intentional.

Taking expectations under beliefs μ gives

$$W_j(c, \mathbf{k}; \mu) = v_j(c; \mu) + \sum_r b_{jr}(k_r; \mu) + \sum_r \Lambda_{jcr}(\mu) \mathbf{1}\{c \neq \emptyset, k_r = g(c)\}, \quad (5)$$

$$v_j(c; \mu) = a_j(c) + w_j(c)q_c(\mu), \quad b_{jr}(k; \mu) = \mathbb{E}_\mu[h_{jr}(k; \omega)], \quad (6)$$

$$\Lambda_{jcr}(\mu) = \kappa_{jr} + \gamma_{jr} \int_{\Omega} I_c(\omega) \eta_{cr}(\omega) d\mu(\omega), \quad c \in \mathcal{C}. \quad (7)$$

An affinity-scaled viability motive is a special case. For valid presidential candidates $c \in \mathcal{C}$, let $m_j(c) \in [0, 1]$ measure affinity and set $a_j(c) = \rho_j m_j(c)$ and $w_j(c) = \beta_j m_j(c)$, with $\rho_j > 0$ and $\beta_j \geq 0$. Then

$$v_j(c; \mu) = m_j(c) [\rho_j + \beta_j q_c(\mu)].$$

Under this restriction, greater viability contributes little directly to the utility of supporting a candidate the voter finds unacceptable. Changes in choice probabilities still depend on competing candidates and legislative prospects.

For the presidential blank option, set $\Lambda_{j\emptyset r} = 0$. The expectation in (7) allows qualification and legislative effectiveness to be correlated. When $q_c(\mu) > 0$,

$$\int_{\Omega} I_c(\omega) \eta_{cr}(\omega) d\mu(\omega) = q_c(\mu) \mathbb{E}_\mu[\eta_{cr} \mid I_c = 1].$$

Thus qualification news can change alignment incentives even with fixed preferences. If $\eta_{cr} = \bar{\eta}_{cr}$ is known,

$$\Lambda_{jcr}(\mu) = \kappa_{jr} + \gamma_{jr} \bar{\eta}_{cr} q_c(\mu).$$

The illustrative example below uses this form.

Proposition 1 (A threshold for split-ticket voting). *Fix a valid presidential choice c and a legislative contest in which $g(c)$ is available. In the absence of taste shocks, conditional on a valid legislative vote and apart from ties, the voter splits her ticket if and only if*

$$\max_{k \in \mathcal{P}_r \setminus \{g(c)\}} b_{jr}(k; \mu) - b_{jr}(g(c); \mu) > \Lambda_{jcr}(\mu). \quad (8)$$

Proof. Conditional on c , an aligned legislative choice has utility $b_{jr}(g(c); \mu) + \Lambda_{jcr}(\mu)$; every alternative valid party has utility $b_{jr}(k; \mu)$. Comparing these quantities gives the result. If the blank option is allowed, the winning valid alternative must also beat $b_{jr}(\emptyset; \mu)$. $\square$

A preferred legislative party can therefore retain support when another party's presidential candidate becomes more viable.

An illustrative Bayesian switch. Consider two candidates, A and B , competing for the remaining runoff place behind a fixed qualifier. Restrict the illustrative ballot choice

to these two candidates and two legislative parties, with all other options dominated. Set $a_A = a_B = 0$, $w_A = w_B = 1$, $b_A = 0$, $b_B = 0.6$, and $\Lambda_c = 0.2q_c$ in one legislative contest. A prior $q_A = 0.25$ and likelihoods $L(s \mid A \text{ qualifies}) = 0.9$ and $L(s \mid B \text{ qualifies}) = 0.1$ imply posterior $q_A = 0.75$. The expected utilities are

(president, legislature)	(A, A)	(A, B)	(B, A)	(B, B)
Before news	0.30	0.85	0.75	1.50
After news	0.90	1.35	0.25	0.90

(9)

The optimal combination changes from (B, B) to (A, B) : the legislative affinity advantage 0.6 exceeds the new alignment premium 0.15, so only the presidential choice changes. This example illustrates the mechanism without calibrating it to Peruvian voters.

4.3 Joint choice probabilities and actual ticket splitting

Attach an unobserved taste shock to each complete ballot combination:

$$U_j(c, \mathbf{k}; \mu) = W_j(c, \mathbf{k}; \mu) + \varepsilon_{j,c,\mathbf{k}}.$$

The voter maximizes U_j . Independent standard type-I extreme-value shocks across combinations, independent of the signal conditional on voter type, yield the joint logit probability (Train, 2009, Ch. 3):

$$\mathbb{P}_j(c, \mathbf{k}; \mu) = \frac{\exp(W_j(c, \mathbf{k}; \mu))}{\sum_{h \in \tilde{\mathcal{C}}} \left[\sum_{\mathbf{m} \in \prod_{r=1}^3 \tilde{\mathcal{P}}_r} \exp(W_j(h, \mathbf{m}; \mu)) \right]}$$

These probabilities average over taste shocks at given beliefs, so news changes them only through beliefs. Define

$$\begin{aligned} Z_{jcr}(\mu) &= \sum_{k \in \tilde{\mathcal{P}}_r} \exp(b_{jr}(k; \mu) + \Lambda_{jcr}(\mu) \mathbf{1}\{c \neq \emptyset, k = g(c)\}), \\ A_j(c; \mu) &= \sum_r \log(Z_{jcr}(\mu)), \\ Q_j^r(k \mid c; \mu) &= \frac{\exp(b_{jr}(k; \mu) + \Lambda_{jcr}(\mu) \mathbf{1}\{c \neq \emptyset, k = g(c)\})}{Z_{jcr}(\mu)}. \end{aligned} \tag{10}$$

The legislative inclusive value $A_j(c; \mu)$ summarizes the appeal of legislative options associated with presidential choice c ; $Q_j^r(k \mid c; \mu)$ is the conditional probability of legislative option k . Additivity and the logit assumption imply that, conditional on c , the probability

of a legislative ticket is the product of its three Q_j^r probabilities. The marginal probabilities are

$$p_{jc}^P(\mu) = \frac{\exp(v_j(c; \mu) + A_j(c; \mu))}{\sum_{h \in \tilde{\mathcal{C}}} \exp(v_j(h; \mu) + A_j(h; \mu))}, \quad (11)$$

$$p_{jp}^{L,r}(\mu) = \sum_c p_{jc}^P(\mu) Q_j^r(p | c; \mu). \quad (12)$$

Here $p \in \tilde{\mathcal{P}}_r$ and the sum includes the presidential blank-and-invalid option. This conditional representation describes simultaneous choice; it does not assume that the presidential ballot is chosen first. For a valid c whose party is available in contest r , conditional on casting a valid legislative ballot, the split probability is

$$s_j^r(c; \mu) = \frac{\sum_{k \in \mathcal{P}_r \setminus \{g(c)\}} e^{b_{jr}(k; \mu)}}{e^{b_{jr}(g(c); \mu) + \Lambda_{jcr}(\mu)} + \sum_{k \in \mathcal{P}_r \setminus \{g(c)\}} e^{b_{jr}(k; \mu)}}. \quad (13)$$

If $g(c) \notin \mathcal{P}_r$, every valid legislative choice is split and $s_j^r(c; \mu) = 1$. Holding c and legislative indices fixed when alignment is available,

$$\frac{\partial s_j^r(c; \mu)}{\partial \Lambda_{jcr}} = -s_j^r(c; \mu) [1 - s_j^r(c; \mu)] \leq 0.$$

Thus a larger alignment premium lowers splitting conditional on the same presidential choice. Information can nevertheless raise total splitting by shifting presidential support toward a party different from the voter's preferred legislative party, as (9) shows. The joint probability of two valid ballots for different parties is

$$M_j^r(\mu) = \sum_{c \in \mathcal{C}} p_{jc}^P(\mu) \left[\sum_{k \in \mathcal{P}_r \setminus \{g(c)\}} Q_j^r(k | c; \mu) \right]. \quad (14)$$

To condition on casting two valid ballots, define

$$B_j^r = \sum_{c \in \mathcal{C}} p_{jc}^P \left[\sum_{k \in \mathcal{P}_r} Q_j^r(k | c) \right], \quad S_j^r = \frac{M_j^r}{B_j^r}.$$

Here B_j^r is the probability that both ballots are valid, and S_j^r is the split rate conditional on that event when $B_j^r > 0$. Suppressing belief arguments, the population rate for equally weighted attending voters is

$$\frac{\sum_j M_j^r}{\sum_j B_j^r}.$$

This weights individual rates by B_j^r ; it generally differs from their simple average. Measuring it would require joint ballot data. For a population of registered voters with attendance

probabilities α_j , defined in Section 4.5, the corresponding rate among those casting two valid ballots is $\sum_j \alpha_j M_j^r / \sum_j \alpha_j B_j^r$, when its denominator is positive.

4.4 Information and electoral responses

Superscripts 0 and 1 indicate evaluation at voter j 's prior and observed posterior, respectively, and $\Delta f = f(\mu_j^1) - f(\mu_j^0)$ for any belief-dependent quantity f . Define the change in candidate c 's total choice index and the overall inclusive value as

$$d_{jc} = w_j(c) \Delta q_c + \Delta A_j(c), \quad (15)$$

$$V_j(\mu) = \log \left(\sum_{h \in \tilde{C}} \exp(v_j(h; \mu) + A_j(h; \mu)) \right). \quad (16)$$

Fixed presidential affinity cancels from d_{jc} . Taking ratios in (11) shows that voter j becomes more likely to choose c exactly when $d_{jc} > \Delta V_j$. Greater viability alone is insufficient: competitors and legislative prospects also matter.

For a legislative option p , the change in support decomposes exactly as

$$\Delta p_{jp}^{L,r} = \underbrace{\sum_c Q_j^r(p | c; \mu_j^1) \Delta p_{jc}^P}_{\text{presidential reallocation}} + \underbrace{\sum_c \Delta Q_j^r(p | c) p_{jc}^P(\mu_j^0)}_{\text{conditional legislative response}}. \quad (17)$$

Both sums include the presidential blank option. The conditional legislative response can reflect revised alignment benefits or direct news about legislative parties.

The presidential blank option has $w_j(\emptyset) = 0$, so its probability falls exactly when $\Delta V_j > \Delta A_j(\emptyset)$, which reduces to $\Delta V_j > 0$ with fixed legislative indices. Neither condition follows solely from greater informativeness, so the model does not predict an unconditional decline in blank or invalid votes.

4.5 From individual choices to polling-location outcomes

ONPE records votes in polling-table returns (*actas*), which we sum within each polling location before constructing outcome shares. This connects individual choices to the location-level unit of treatment, matching, and estimation.

Let ℓ index voting locations and $t \in \{0, 1\}$ denote the condition: $t = 0$ uses prior beliefs and $t = 1$ uses beliefs after the news, allowing attendance to differ between conditions. Let G_ℓ be the distribution of registered voter types j at location ℓ , fixed before election day. Types include preferences, priors, and perceived signal reliability. Write $\alpha_j^t \in [0, 1]$ for the probability of attendance. Conditional on attendance and type, taste shocks retain the

distribution specified above, so $p_{jc}^P(\mu_j^t)$ is the choice probability among attending voters of that type. The presidential share of registered electors has population counterpart

$$\pi_{\ell c}^{P,t} = \int \alpha_j^t p_{jc}^P(\mu_j^t) dG_\ell(j). \quad (18)$$

The abstention share is $a_\ell^t = 1 - \int \alpha_j^t dG_\ell(j)$, giving $\sum_{c \in \tilde{C}} \pi_{\ell c}^{P,t} + a_\ell^t = 1$. Legislative shares use the same attendance weights and registered-elector denominator, replacing p_{jc}^P with $p_{jp}^{L,r}$. To make the aggregation from tables explicit, let $\mathcal{A}_\ell$ be the polling tables at location ℓ , N_a the number of registered electors at table a , and $N_\ell = \sum_{a \in \mathcal{A}_\ell} N_a$. If $\pi_{ac}^{P,t}$ is the corresponding table-level share, then

$$\pi_{\ell c}^{P,t} = \sum_{a \in \mathcal{A}_\ell} \frac{N_a}{N_\ell} \pi_{ac}^{P,t}.$$

These within-location weights aggregate votes over registered electors; the treatment effect defined below gives equal weight to each treated location, regardless of its number of polling tables or registered electors. If attendance is fixed for every type, $\alpha_j^1 = \alpha_j^0 = \alpha_j$, information changes expected support by

$$\Delta \pi_{\ell c}^P = \int \alpha_j \left[p_{jc}^P(\mu_j^1) - p_{jc}^P(\mu_j^0) \right] dG_\ell(j). \quad (19)$$

Individual responses can offset one another, so a small aggregate change can hide large individual switches. When attendance changes, the exact decomposition is

$$\Delta \pi_{\ell c}^P = \int \alpha_j^1 \Delta p_{jc}^P dG_\ell(j) + \int (\alpha_j^1 - \alpha_j^0) p_{jc}^P(\mu_j^0) dG_\ell(j), \quad (20)$$

where $\Delta p_{jc}^P = p_{jc}^P(\mu_j^1) - p_{jc}^P(\mu_j^0)$. The first term captures choice updating at the new attendance weights; the second captures attendance changes at prior choice probabilities. This is an accounting decomposition, and our estimates combine both terms. Attendance is not endogenized in the choice model and can be affected by the fine for not voting.

A presidential–legislative gap nets out opposite flows, whereas splitting adds them. Consider two parties, A and B , and a voter who casts valid presidential and legislative ballots. Let $\mathbb{P}(x, y)$ be the probability of voting for party x for president and party y for the legislature. The order matters: $\mathbb{P}(A, B)$ is the probability of voting for A for president and B for the legislature, and $\mathbb{P}(B, A)$ is the probability of the opposite combination. Let p_A^P and p_A^L be the probabilities of voting for A for president and for the legislature,

and let S be the probability of splitting the ticket. Since $p_A^P = \mathbb{P}(A, A) + \mathbb{P}(A, B)$ and $p_A^L = \mathbb{P}(A, A) + \mathbb{P}(B, A)$,

$$\begin{aligned} p_A^P - p_A^L &= \mathbb{P}(A, B) - \mathbb{P}(B, A), \\ S &= \mathbb{P}(A, B) + \mathbb{P}(B, A). \end{aligned} \tag{21}$$

The gaps estimated below therefore do not measure how many voters split their tickets.

Our legislative outcome is the minimum of a party's shares across the three contests. The contest attaining this minimum can vary by location, and the index is an upper bound on the share voting for the party in all three contests. Let $Y_{\ell c}^P$ be the presidential outcome and $Y_{\ell, g(c)}^L$ the legislative index of the candidate's party. The gap and its treatment effect are

$$D_{\ell c} = Y_{\ell c}^P - Y_{\ell, g(c)}^L, \quad \tau_c^D = \tau_c^P - \tau_{g(c)}^L.$$

The second identity requires identical samples, weights, and denominators, which hold for the estimates in Table 7.

Let $Y_{\ell c}(d)$ be the potential outcome under delayed voting ($d = 1$) or regular Sunday voting ($d = 0$). The target is the average effect across the thirteen treated locations in $\mathcal{T}$, each with equal weight:

$$\tau_c = \frac{1}{13} \sum_{\ell \in \mathcal{T}} \mathbb{E}[Y_{\ell c}(1) - Y_{\ell c}(0)]. \tag{22}$$

Let $T_\ell = 1$ indicate delayed voting and $T_\ell = 0$ regular voting, and let H_ℓ collect pre-treatment covariates. Identification of the population average treatment effect on the treated (ATT), τ_c^{pop} , rests on four assumptions:

- *Conditional exchangeability*: mean regular-voting outcomes are equal across treatment groups conditional on H_ℓ .
- *Overlap*: treated locations have comparable controls.
- *Consistency*: observed outcomes match the realized regime.
- *No interference*: there is no relevant interference between locations.

These assumptions imply

$$\tau_c^{\text{pop}} = \mathbb{E}[Y_{\ell c} \mid T_\ell = 1] - \mathbb{E}[m_{0c}(H_\ell) \mid T_\ell = 1], \quad m_{0c}(h) = \mathbb{E}[Y_{\ell c} \mid H_\ell = h, T_\ell = 0]. \tag{23}$$

The first term is observed, and matching approximates the second. Balance alone does not establish the identifying assumptions.

Delayed voting creates an opportunity to receive overnight news; exposure and posteriors are unobserved. It can also change voting costs, other news, and electorate composition.

The estimates therefore concern the delayed-voting regime. A pure information interpretation requires additional exclusions, and an interpretation as individual ticket splitting requires joint ballot information absent from the aggregate returns.

5 Conditional implications

The following implications organize interpretation: beliefs, preferences, and individual ballot combinations are unobserved.

Conditional implication 1 (Reallocation among presidential candidates). For voter j , the probability of choosing candidate c rises if and only if $d_{jc} > \Delta V_j$.

For López Aliaga, Sánchez, and Nieto, the signs and relative sizes remain empirical questions. Neither proximity to second place nor presidential affinity alone determines the ranking of effects.

Conditional implication 2 (The leading candidate). If Fujimori’s perceived qualification probability is unchanged, her direct viability term is unchanged. Her vote probability can still change through competitors’ appeal and legislative prospects.

Conditional implication 3 (Legislative transmission and ticket splitting). News can change legislative support through presidential reallocation, alignment benefits, or legislative beliefs. Without taste shocks, conditional on a fixed valid presidential choice whose party is available and on a valid legislative vote, a voter splits when another party’s legislative advantage exceeds the alignment premium, as in (8). With logit shocks, a higher premium weakly lowers the conditional probability of splitting, holding legislative indices fixed.

A presidential switch can coexist with an unchanged legislative choice. Partial transmission of the same sign is not a general prediction of the model, so the shared signs in Table 7 are an empirical finding.

Conditional implication 4 (Blank and invalid presidential ballots). For voter j , the blank-and-invalid probability falls if and only if $\Delta V_j > \Delta A_j(\emptyset)$.

With stable legislative indices, the individual condition reduces to $\Delta V_j > 0$. Greater informativeness does not establish this condition for the realized signal; observed shares can also reflect turnout composition and ballot errors.

6 Empirical strategy

We estimate the average effect of delayed voting across the thirteen treated locations under the assumptions in Section 4.5. Cardinality matching selects controls subject to

pre-treatment balance constraints; a second stage holds those controls fixed and minimizes within-set Mahalanobis distances by reassigning them across treated locations. Each treated location receives equal weight, regardless of its number of controls.

6.1 Data and measurement

The study covers 364 polling locations and 4,930 polling-table returns in southern Lima. Thirteen locations voted on Monday, 13 April 2026; the remaining 351, which form the control pool, voted on Sunday, 12 April, and are located in the eleven districts of Lima Sur listed in Section 1. Electoral returns and registered-voter characteristics come from the National Office of Electoral Processes (ONPE). We link them to 2021 presidential returns and to socioeconomic stratum and Indigenous-language measures from the 2017 Population and Housing Census conducted by the National Institute of Statistics and Informatics (INEI).

Treatment, matching, and estimation operate at the location level. For each contest, we first sum votes across the location’s polling tables. We measure presidential and legislative outcomes as vote shares, including the presidential blank-and-invalid share, and abstention as the share of electors who did not vote. All outcomes use registered electors as their denominator, so the presidential candidate shares, the blank-and-invalid share, and abstention sum to one. Because the registry is fixed before election day, the denominator does not respond to the delay.

The party’s legislative-support index is the minimum of its shares in the national Senate, district Senate, and deputy contests. This minimum is an upper bound on the share supporting that party in all three contests. The gap between presidential and legislative support is the presidential share minus the corresponding legislative index; both share the registered-electors denominator. The *other parties* outcome groups all candidates and parties other than the seven leading ones. Its presidential share is their combined votes over registered electors, its legislative index is the minimum across the three contests of their combined shares, and its gap is the difference between the two.

We geocode locations using the Google Maps Platform Geocoding API and construct population-weighted census measures from blocks within one kilometer. These describe the surrounding population, so they are proxies for the registered electorate. ONPE’s *Elige tu local* program, through which voters rank up to three preferred locations subject to capacity, motivates this local measure. We link 2021 returns to 2026 polling locations directly whenever possible and otherwise rely on geographic proximity, subject to location availability.⁶ These approximate links account for 23% of locations overall and include one of the 13 treated locations and nine of the 39 controls in the final matched sample.

⁶This approach follows ONPE’s methodology for reassigning voters based on geographic proximity and polling-location availability.

6.2 Cardinality matching

Let $\mathcal{T}$ denote the thirteen Monday locations and $\mathcal{C}_0$ the pool of 351 Sunday controls, restricted to the districts of Lima Sur described in Section 6.1. We select the largest control sample satisfying balance constraints (Visconti & Zubizarreta, 2018; Zubizarreta et al., 2014), allowing the number of controls per treated location to vary (Ming & Rosenbaum, 2000). Figure 6 in Appendix C summarizes the construction of the control group, the balance covariates, and the estimation weights.

Matching is based on nine pre-treatment covariates measured at the polling-location level, chosen to reflect the electoral cleavages identified by Barrenechea and Encinas (2025) and the voter profiles reported by the Institute of Peruvian Studies (IEP), summarized in Table 10 (Appendix A). The first seven covariates come from ONPE’s 2021 electoral returns and electoral-roll records; the last two are census-based measures of the surrounding population, constructed with the spatial procedure described above. The covariate vector of polling location ℓ is

$$H_\ell = (\text{Right}_\ell^{2021}, \text{Bloc}_\ell^{2021}, \text{Fuji}_\ell^{2021}, \text{Age}_\ell, \text{NoSecondary}_\ell, \text{HigherEd}_\ell, \text{Female}_\ell, \text{SES}_\ell, \text{Indig}_\ell),$$

with $K = 9$ components indexed by a , so $H_{\ell a}$ denotes covariate a at location ℓ . The abbreviations are defined in Table 4.

Table 4: Definitions of the balance covariates.

Component	Definition	Unit
Right_ℓ^{2021}	Right-wing vote share: Renovación Popular and Avanza País, the two right-wing parties with the most votes in Lima in 2021.	Proportion
Bloc_ℓ^{2021}	Four-party vote share: Juntos por el Perú, Perú Libre, Acción Popular, and Partido Morado; an operational aggregate.	Proportion
Fuji_ℓ^{2021}	Fujimorista vote share: Fuerza Popular.	Proportion
Age_ℓ	Average age of registered electors.	Years
NoSecondary_ℓ	Registered electors without completed secondary education.	Percent
HigherEd_ℓ	Registered electors with higher education.	Percent
Female_ℓ	Female registered electors.	Percent
SES_ℓ	Socioeconomic status (SES), measured by the socioeconomic stratum of the surrounding population.	Index, 1–5
Indig_ℓ	Residents of the surrounding population whose mother tongue is an Indigenous language.	Proportion

Notes. The three 2021 vote shares use ballots cast as their denominator, unlike the 2026 outcomes, which use registered electors. Proportions lie in $[0, 1]$, percentages in $[0, 100]$, and the socioeconomic-stratum index ranges from 1 (lowest) to 5 (highest).

Descriptive statistics before matching are reported in Table 5. The 26 outcomes comprise eight presidential shares (the seven leading candidates and other parties), eight legislative-support indices, eight presidential–legislative gaps, blank and invalid presidential

ballots, and abstention, all measured as shares of registered electors. Descriptive statistics are presented in Table 11 (Appendix B).

Table 5: Descriptive statistics of the balance covariates, before matching.

Covariate	Group	N	Mean	SD	Min	Max
2021 right vote share	Control	351	0.261	0.067	0.117	0.711
	Treated	13	0.241	0.032	0.181	0.292
2021 four-party vote share	Control	351	0.206	0.032	0.096	0.365
	Treated	13	0.215	0.042	0.136	0.281
2021 Fujimori vote share	Control	351	0.134	0.033	0.041	0.224
	Treated	13	0.138	0.022	0.095	0.174
Average age	Control	351	42.543	5.819	24.650	55.620
	Treated	13	40.495	6.188	30.950	50.720
Registry: below-secondary education (%)	Control	351	30.729	8.570	11.260	74.196
	Treated	13	42.332	10.703	22.074	58.669
Registry: higher education (%)	Control	351	5.329	4.383	0.386	33.278
	Treated	13	2.834	1.688	1.027	6.589
Female registered voters (%)	Control	351	49.941	2.325	44.340	58.080
	Treated	13	49.192	1.911	46.880	53.880
Socioeconomic stratum	Control	351	2.594	0.646	1.000	4.325
	Treated	13	2.231	0.472	1.835	3.675
Indigenous-language share	Control	351	0.092	0.041	0.019	0.250
	Treated	13	0.113	0.050	0.049	0.228

Notes. N denotes the number of polling locations; Control includes the full pre-matching control pool. SD denotes the standard deviation. The 2021 right vote share combines Renovación Popular and Avanza País; the 2021 four-party vote share combines Juntos por el Perú, Perú Libre, Acción Popular, and Partido Morado; the 2021 Fujimori vote share is that of Fuerza Popular. These three shares are computed over ballots cast and measured as proportions in $[0, 1]$, as is the Indigenous-language share. Registry education categories and female registered voters are expressed as percentages in $[0, 100]$. Socioeconomic stratum is the INEI index ranging from 1 (lowest) to 5 (highest).

First stage: largest balanced control sample. Let $m_{i\ell} \in \{0, 1\}$ indicate that control $\ell \in \mathcal{C}_0$ is assigned to treated location $i \in \mathcal{T}$. Define the number of controls assigned to i and the set of selected controls, respectively, as

$$k_i = \sum_{\ell} m_{i\ell}, \quad \mathcal{C}(m) = \left\{ \ell \in \mathcal{C}_0 : \sum_i m_{i\ell} = 1 \right\}.$$

The first stage maximizes the number of selected controls subject to balance and structural constraints:

$$\begin{aligned}
& \max_{\{m_{i\ell}\}} \sum_{i \in \mathcal{T}} \left[\sum_{\ell \in \mathcal{C}_0} m_{i\ell} \right] \\
& \text{s.t. } \frac{|\bar{H}_{a,\mathcal{T}} - \bar{H}_{a,\mathcal{C}(m)}^w|}{s_a} \leq \delta_a, \quad a = 1, \dots, K, \\
& 1 \leq k_i \leq 7 \quad \forall i \in \mathcal{T}; \quad \sum_{i \in \mathcal{T}} m_{i\ell} \leq 1 \quad \forall \ell \in \mathcal{C}_0; \quad \sum_{i \in \mathcal{T}} k_i \leq 39,
\end{aligned} \tag{24}$$

where $\bar{H}_{a,\mathcal{T}}$ is the treated mean of covariate a , s_a is its standard deviation among treated locations, and

$$\bar{H}_{a,\mathcal{C}(m)}^w = \frac{1}{13} \sum_{i \in \mathcal{T}} \left[\frac{1}{k_i} \sum_{\ell \in \mathcal{C}_0} m_{i\ell} H_{\ell a} \right] \tag{25}$$

is the control mean under the estimation weights. Each selected control in set i receives weight $1/(13k_i)$. We set $\delta_a = 0.05$ for the seven ONPE covariates and $\delta_a = 0.10$ for the two census-based proxies. The structural constraints retain all treated locations, assign each between one and seven controls without replacement, and cap the total at 39 controls, or three per treated location on average. Let $m^{(1)}$ denote the optimal assignment returned by the first stage and define

$$x_\ell^\star = \sum_{i \in \mathcal{T}} m_{i\ell}^{(1)}, \quad \mathcal{C}^\star = \{\ell \in \mathcal{C}_0 : x_\ell^\star = 1\}, \quad M^\star = |\mathcal{C}^\star|.$$

The first stage selects $M^\star = 39$ controls.

Second stage: closest assignment within the selected sample. The second stage holds the first-stage control sample $\mathcal{C}^\star$ fixed and reassigns those controls across the thirteen treated locations. It retains the balance and structural constraints in (24) and imposes

$$\sum_{i \in \mathcal{T}} m_{i\ell} = x_\ell^\star \quad \text{for every } \ell \in \mathcal{C}_0. \tag{26}$$

Thus every selected control is used exactly once. The total remains $M^\star = 39$, while assignments and set sizes k_i may change. The weighted balance constraints remain necessary because reassignment can change control weights even when sample membership is fixed. Subject to these constraints, we minimize the sum of average within-set Mahalanobis distances:

$$\min_{\{m_{i\ell}\}} \sum_{i \in \mathcal{T}} \left[\frac{1}{k_i} \sum_{\ell \in \mathcal{C}_0} m_{i\ell} d(i, \ell) \right], \quad d(i, \ell) = \left[(R_i - R_\ell)^\top \Sigma_R^+ (R_i - R_\ell) \right]^{1/2}, \tag{27}$$

where R_ℓ is the vector of standardized ranks of the covariates of location ℓ among all treated and control locations, with average ranks for ties, Σ_R is the empirical covariance of these ranks, and Σ_R^+ is its Moore–Penrose inverse. The $1/k_i$ weighting prevents treated locations with more controls from dominating the objective. The solution partitions $\mathcal{T} \cup \mathcal{C}^*$ into thirteen matched sets $G_i = \{i\} \cup \mathcal{C}_i$, one per treated location, where $\mathcal{C}_i = \{\ell : m_{i\ell} = 1\}$ and $|\mathcal{C}_i| = k_i$ may differ across sets. Because k_i is itself chosen by the program, both the weighted mean in (25) and the objective in (27) contain reciprocals of decision variables. We linearize them by indexing assignments by set size, with binary variables z_{ik} indicating that treated location i receives exactly $k \in \{1, \dots, 7\}$ controls and $x_{i\ell k}$ indicating that control ℓ joins set i when that set has k controls. The factor $1/k$ is then a constant, so both stages are mixed-integer linear programs. They are implemented in R with the `ompr` interface and solved with HiGHS.

Estimation. We estimate the ATT as the mean of the thirteen matched-set contrasts. Standard errors equal their sample standard deviation divided by $\sqrt{13}$, and inference uses a t distribution with 12 degrees of freedom. This treats the thirteen matched-set contrasts as independent draws with a common mean; we refer to it below as the independent-contrast approximation.

For outcome c , the contrast in matched set i is

$$\hat{\Delta}_{i,c} = Y_{i,c} - \frac{1}{k_i} \left[\sum_{\ell \in \mathcal{C}_i} Y_{\ell,c} \right], \quad i \in \mathcal{T}, \quad (28)$$

and the ATT estimator is their equally weighted average,

$$\hat{\tau}_c = \frac{1}{13} \sum_{i \in \mathcal{T}} \hat{\Delta}_{i,c}. \quad (29)$$

Table 7 reports the estimates, standard errors, and t -test results.

As a complementary design-based check, we use a sign-score randomization test of the sharp null of no effect at any location. Under this null, the outcomes of the $n_i = k_i + 1$ locations in set i are fixed. For each possible treated location $h \in G_i$, let d_{ih} be its outcome minus the mean outcome of the other k_i locations, and let a_i count the locations with $d_{ih} > 0$. Under a working assignment model with one uniformly assigned treated location per set and independent assignments across sets, $B_i = \mathbf{1}\{\hat{\Delta}_{i,c} > 0\}$ is Bernoulli with probability a_i/n_i . Hence $T_c = \sum_{i \in \mathcal{T}} B_i$ has a Poisson–binomial distribution. The two-sided p -value is twice the smaller exact tail probability, capped at one. Without ties, matched pairs give the usual sign test; with multiple controls, a_i/n_i need not equal one half. Rejection concerns the sharp null. Table 13 (Appendix D.1) reports the sign-score statistics and p -values.

Balance diagnostics. Balance diagnoses comparability on observed covariates. The post-match standardized mean difference is

$$\text{SMD}_a = \frac{\bar{H}_{a,\mathcal{T}} - \bar{H}_{a,\mathcal{C}^*}^w}{s_a}, \quad (30)$$

using the same weights and tolerances δ_a as the matching program. Table 6 reports SMDs before and after matching; Appendix C adds weighted Kolmogorov–Smirnov statistics, density plots, and quantile–quantile plots. These pooled diagnostics do not reveal individual set membership or establish homogeneous assignment propensities within sets.

Sensitivity and robustness. We assess robustness to the influence of individual treated locations, hidden bias, and multiple testing. To assess influence, we omit each treated location together with its controls, retain the other matched sets, and recompute $\hat{\tau}_{c,(-i)}$. Table 14 (Appendix D.2) summarizes the thirteen iterations, showing whether the estimates depend heavily on a particular matched set.

Balance in observed covariates does not establish the absence of unobserved confounding. We therefore assess sensitivity using Rosenbaum’s parameter Γ , which bounds the relative odds of treatment within a matched set (Rosenbaum, 1987, 1988). With identical observed covariates, departures from equal treatment odds are conventionally attributed to hidden bias. Because some observed differences remain within our matched sets, Γ can capture departures associated with both observed and unobserved covariates. At $\Gamma = 1$, assignment within each set is uniform.

The sensitivity analysis tests the sharp null of no treatment effect at any location. The test statistic is the mean of the matched-set contrasts in (29) and thus coincides numerically with $\hat{\tau}_c$. Its reference distribution is induced by treatment assignments within matched sets under Rosenbaum’s sensitivity model (Rosenbaum, 2007). For each Γ , we use the `senmv` function in the R package `sensitivitymv`; this mean-contrast statistic under the sharp null corresponds to `method="t"` and `tau=0` (Rosenbaum, n.d.). Applying the function to the outcomes and their negatives yields large-sample normal approximations to the upper bounds on the two one-sided p -values. We construct an approximate two-sided upper bound by doubling the smaller of these bounds and capping the result at one. With only thirteen matched sets, this procedure does not provide finite-sample exact sensitivity bounds, and the resulting sensitivity thresholds should be interpreted with this limitation in mind. Table 16 (Appendix D.4) reports the largest Γ on a grid from 1 to 10 in increments of 0.1 for which the approximate two-sided upper bound remains below 0.05.

The main results report individual p -values without adjustment for the 26 outcomes, some of which are mechanically related through the presidential and legislative gaps. To

address the probability of at least one false rejection across this family, Appendix D.3 reports Holm-adjusted p -values for the t -tests.

Considerations. Two considerations qualify the design. First, matching balances only the observed covariates H_ℓ and does not by itself establish conditional exchangeability, which remains our identifying assumption. The sensitivity bounds of Section 6.2 (Appendix D.4) quantify how large a departure from this assignment model would be needed to overturn our conclusions.

Second, we do not measure voters’ exposure to the newly available information and therefore cannot isolate its effect from those of other developments between Sunday and Monday. Monday voting changed participation conditions through workday costs and announcements about exemption from abstention fines for affected electors ([Organization of American States, 2026, p. 7](#)). Although measuring outcomes over registered electors removes the mechanical effect of turnout on the denominator, it does not eliminate changes in who votes. The estimated increase in abstention is 5.8 percentage points, but this result is sensitive to the inference procedure (Section 6.2). Our estimates also capture any participation responses to changed costs and perceived penalties. To the extent that these conditions discouraged attendance without otherwise changing vote choices, positive effects could understate informational gains, while negative effects could overstate informational losses, including reductions in blank and invalid ballots. However, abstention may itself have responded to the additional information. Voters who revised downward their preferred candidate’s chances of advancing and found no acceptable alternative, as well as those intending to cast a blank or invalid ballot, may have chosen not to attend after updating their information. In this sense, participation constitutes another dimension of electoral choice through which information may operate. We acknowledge the inability to disentangle informational responses from changes in participation costs and incentives and other overnight developments as a limitation of our analysis.

7 Results

Matching selects 39 of the 351 control locations, reaching the cap of three controls per treated location on average. All nine covariates meet their designated balance thresholds, set at 0.05 for electoral and demographic characteristics, and 0.10 for socioeconomic stratum and indigenous language share (Table 6).

Table 7 reports the main estimates for presidential shares of the seven leading candidates and other parties, the corresponding legislative-support indices, their gaps, the blank-and-invalid presidential share, and abstention. All outcomes are shares of registered electors. Standard errors and stars refer to the t -tests of zero average effects. Appendix D collects the complementary checks. Table 13 reports the sign-score statistics and p -values for

Table 6: Covariate balance before and after matching.

Covariate	Target SMD	Treated mean	Control (all)	Control (matched)	SMD before	SMD after
2021 right vote share	0.05	0.2410	0.2614	0.2398	-0.6433	0.0382
2021 four-party vote share	0.05	0.2152	0.2056	0.2140	0.2304	0.0293
2021 Fujimori vote share	0.05	0.1382	0.1336	0.1372	0.2042	0.0445
Average age	0.05	40.4954	42.5435	40.2171	-0.3310	0.0450
Registry: below-secondary education (%)	0.05	42.3325	30.7293	41.8025	1.0841	0.0495
Registry: higher education (%)	0.05	2.8340	5.3293	2.8710	-1.4782	-0.0219
Female registered voters (%)	0.05	49.1923	49.9411	49.2842	-0.3918	-0.0481
Socioeconomic stratum	0.10	2.2305	2.5938	2.1852	-0.7702	0.0961
Indigenous-language share	0.10	0.1125	0.0917	0.1172	0.4121	-0.0933

Notes. Vote shares and the Indigenous-language share are expressed as proportions in $[0, 1]$; registry education categories and female registered voters are expressed as percentages in $[0, 100]$. “Control (all)” denotes the full pre-matching control pool; “Control (matched)” denotes the matched control mean under the estimation weights. SMD is computed as in (30), using the standard deviation among treated locations as the scale. Target |SMD| denotes the maximum permitted absolute standardized mean difference after matching.

the sharp null, Table 14 presents the leave-one-out exercise, Table 15 details the Holm adjustments, and Table 16 provides the Rosenbaum sensitivity thresholds.

7.1 Presidential support

Nieto’s gain of 11.2 percentage points of the registered electorate is the largest presidential estimate, followed by López Aliaga’s 3.9 points and Sánchez’s 1.2 points. Fujimori’s estimate of 0.8 points is small relative to its standard error and not statistically distinguishable from zero. The negative estimates are 4.4 points for Álvarez, 2.9 for López Chau, 2.8 for Belmont, and 9.9 for the remaining parties combined. This pattern is consistent with information changing contenders’ relative appeal through perceived viability.

Because presidential shares, the blank-and-invalid share, and abstention sum to one over registered electors, the estimates satisfy an adding-up restriction. The four positive candidate estimates (17.1 points) and the rise in abstention (5.8 points) offset the losses of the remaining candidates and parties (20.0 points) and the decline in the blank-and-invalid share (2.9 points), up to rounding.

Belmont remained competitive in both exit-poll estimates, so those figures did not mechanically exclude him from contention. His decline, however could be explained by the news about his candidacy that circulated on election day (Section 2.4), which could lower his perceived viability without any change in the published vote estimates. Fujimori’s small and imprecise estimate is compatible with Implication 2, which suggests that news leaving her perceived qualification probability largely unchanged need not move her support, although the appeal of competing candidates could still affect it.

Table 7: Average treatment effects on the treated: presidential support, the legislative-support index, their aggregate gap, and participation.

Party (presidential candidate)	Presidential	Legislative index	Aggregate gap
Fuerza Popular (Keiko Fujimori)	0.008 (0.010)	0.006 (0.007)	0.003 (0.005)
Juntos por el Perú (Roberto Sánchez)	0.012*** (0.003)	0.008** (0.003)	0.004*** (0.001)
Renovación Popular (Rafael López Aliaga)	0.039*** (0.009)	0.024*** (0.006)	0.015*** (0.004)
Partido del Buen Gobierno (Jorge Nieto)	0.112*** (0.009)	0.078*** (0.007)	0.033*** (0.004)
Partido Cívico Obras (Ricardo Belmont)	-0.028*** (0.002)	-0.021*** (0.002)	-0.007*** (0.001)
País para Todos (Carlos Álvarez)	-0.044*** (0.003)	-0.026*** (0.002)	-0.018*** (0.002)
Ahora Nación (Alfonso López Chau)	-0.029*** (0.002)	-0.020*** (0.002)	-0.009*** (0.001)
Other parties (combined)	-0.099*** (0.005)	-0.065*** (0.005)	-0.035*** (0.005)
Blank-and-invalid share	-0.029*** (0.005)	—	—
Abstention	0.058** (0.024)	—	—

Notes. Effects are expressed in share units of the registered electorate; multiplying by 100 gives percentage points. All outcomes are computed over registered electors: presidential shares and the blank-and-invalid share are votes divided by registered electors; legislative-support indices are the minimum across the three legislative contests of party votes divided by registered electors; gaps are presidential shares minus legislative indices; abstention is the share of registered electors who did not vote. “Other parties” combines all candidates and parties other than the seven listed. Standard errors in parentheses equal the standard deviation of the thirteen matched-set contrasts divided by $\sqrt{13}$. Stars report unadjusted two-sided p -values of the t -test of $H_0 : \tau_c = 0$ with 12 degrees of freedom, with $*p < 0.10$, $**p < 0.05$, and $***p < 0.01$. Because presidential shares, the blank-and-invalid share, and abstention sum to one, the presidential column sums to zero up to rounding. Sign-score statistics and p -values for the sharp null appear in Table 13, Holm adjustments in Table 15, and Rosenbaum sensitivity thresholds in Table 16, all in Appendix D.

The remaining parties together lose 9.9 points, the largest presidential loss. Their support was dispersed across candidates with little prospect of reaching the runoff, so the decline is consistent with voters abandoning alternatives that the news marked as uncompetitive.

The blank-and-invalid presidential share falls by 2.9 percentage points. This is consistent with valid ballot combinations becoming more attractive, although the model does not predict an unconditional decline. The outcome also combines protest, uncertainty, marking

errors, and changes in participation; with a registered-electors denominator, nonattendance among would-be blank or invalid voters could also lower it.

7.2 Responses across ballots

For all eight party outcomes, the legislative-index estimate has the same sign as the presidential estimate and between 58% and 77% of its absolute magnitude. This comparison describes effects on the minimum index. The gaps therefore move in the direction of the presidential estimates: Nieto’s gap rises by 3.3 points, López Aliaga’s by 1.5, and Sánchez’s by 0.4, while the gaps of other parties, Álvarez, López Chau, and Belmont fall by 3.5, 1.8, 0.9, and 0.7 points. Fujimori’s gap estimate is not distinguishable from zero. With common samples, denominators, and estimation weights, each gap effect equals the difference between the two effects before rounding.

For comparison with proportional scaling across offices, let $\bar{P}_{0c}^w$ and $\bar{L}_{0c}^w$ be the matched-control means for a candidate’s presidential share and her party’s legislative index. Define $\beta_c = \bar{L}_{0c}^w / \bar{P}_{0c}^w$. A descriptive benchmark preserving that baseline ratio is $\tau_{g(c)}^{L, \text{prop}} = \beta_c \hat{\tau}_c^P$. Because all thirteen treated locations have equal weight, the control means equal the treated means in Table 11 (Appendix B) minus the effects in Table 7. Table 8 computes them from the unrounded values; its residuals are descriptive and have no accompanying test.

Table 8: Legislative-index changes relative to a baseline-proportional benchmark.

Candidate	Matched-control means (%)		β_c	$\hat{\tau}_{g(c)}^L / \hat{\tau}_c^P$	Residual (pp)
	Presidential	Legislative			
Fujimori	14.2	10.2	0.714	0.678	-0.03
Sánchez	2.8	2.5	0.879	0.635	-0.29
López Aliaga	8.7	6.5	0.745	0.623	-0.47
Nieto	9.7	8.1	0.828	0.702	-1.42
Belmont	8.0	6.1	0.764	0.767	-0.01
Álvarez	7.1	4.3	0.605	0.584	+0.09
López Chau	3.8	3.9	1.024	0.702	+0.92
Other parties	14.2	12.4	0.878	0.652	+2.24

Notes. Presidential shares and legislative indices are percentages of registered electors. Matched-control means use the estimation weights and equal the treated means minus the estimated effects. β_c is the control legislative-index mean divided by the control presidential mean. The last column is $100(\hat{\tau}_{g(c)}^L - \beta_c \hat{\tau}_c^P)$, in percentage points. All entries are computed from unrounded estimates. No sampling uncertainty or inference is reported for these nonlinear calculations.

The smaller legislative effects do not by themselves establish incomplete cross-office transmission. For Fujimori, Belmont, and Álvarez, the observed changes are within 0.1 percentage points of baseline-proportional scaling, and for Sánchez and López Aliaga within 0.5 points. Nieto’s legislative-index gain is approximately 1.4 points below that

benchmark. Among the losses, López Chau’s legislative decline is approximately 0.9 points smaller than proportional scaling implies, and that of other parties 2.2 points smaller. These descriptive departures do not identify a transmission parameter, particularly because the legislative outcome is a minimum across contests.

The model supplies a mechanism for both shared movement and divergence: voters may change their presidential choice while retaining a preferred legislative party, or support the same party when the expected alignment benefit is sufficiently large. Neither response requires a common transmission coefficient across parties. Ipsos also released legislative estimates (Section 2.4), so direct legislative updating cannot be separated from complementarity between offices. The minimum-based index adds a measurement limitation: the contest attaining the minimum can change, and its voters need not be the same across contests. The aggregate gap consequently does not measure the proportion of voters splitting their tickets. Effects for each legislative contest, their blank-and-invalid shares, and the frequency with which each contest attains the minimum are needed for a direct comparison with the model; they cannot be recovered from this index alone.

7.3 Inference, sensitivity, and participation

The t -tests reject a zero average effect at 1% for seven of the eight presidential outcomes, six of the eight legislative indices, and seven of the eight gaps; Sánchez’s legislative index rejects at 5%. Fujimori’s three outcomes are the exceptions, with p -values above 0.4. Holm adjustments preserve rejection at 5% for every presidential and legislative outcome except Fujimori’s when the two eight-outcome families are adjusted separately. With all 26 outcomes in one family, rejection at 5% holds for seven presidential shares, six legislative indices, seven gaps, and the blank-and-invalid share; Sánchez’s legislative index and abstention no longer reject (Table 15, Appendix D.3).

The sign-score test of the sharp null of no effect at any location (Table 13, Appendix D.1) yields the same qualitative pattern. All thirteen contrasts are positive for Nieto’s three outcomes, and all thirteen are negative for the presidential, legislative, and gap outcomes of Álvarez, López Chau, Belmont, and other parties and for the blank-and-invalid share; each of these p -values is below 0.001. López Aliaga’s presidential share has eleven positive contrasts ($p = 0.005$) and Sánchez’s twelve ($p = 0.004$). Sánchez’s legislative index ($p = 0.056$) and Fujimori’s three outcomes ($p \geq 0.07$) do not reject at 5%.

Under the approximate sensitivity analysis of the permutation test based on the mean matched-set contrast (Table 16, Appendix D.4), rejection at 5% holds through $\Gamma = 6.8$ for Nieto’s presidential share, 7.0 for his legislative index, and 5.2 for his gap. For the losses of Álvarez, López Chau, Belmont, and other parties, the presidential thresholds range from 6.7 to 7.8, the legislative thresholds from 5.7 to 7.6, and the gap thresholds from 2.7 to 5.0; the blank-and-invalid share holds through $\Gamma = 4.1$. López Aliaga’s estimates

hold through $\Gamma = 3.9, 3.7,$ and 2.8 , and Sánchez’s through $2.2, 1.3,$ and 2.2 , for the presidential share, legislative index, and gap, respectively; Sánchez’s legislative index is thus the most sensitive of the significant party outcomes. The abstention effect holds only through $\Gamma = 1.2$. Removing one treated location and its matched controls changes the sign of only one estimate, Fujimori’s gap, in a single iteration; that estimate is not significant in the full sample (Table 14, Appendix D.2). Significance at 5% under the t -test changes only for abstention, which loses it in four of the thirteen iterations. Apart from Fujimori’s gap, all signs remain unchanged; apart from abstention, all significance decisions remain unchanged. This exercise assesses individual sets’ influence on signs and rejection decisions. The estimated abstention effect is an increase of 5.8 percentage points. The t -test rejects a zero average effect at 5% ($p = 0.033$), whereas the sign-score test, with nine positive contrasts, does not reject its sharp null ($p = 0.194$). The abstention effect is therefore fragile.

8 Conclusion

This paper examines whether public signals about candidate viability reshape choices across presidential and legislative ballots. Peru’s 2026 general elections provide an unusual comparison, as logistical failures postponed voting at thirteen locations in Lima Sur, allowing approximately 55,000 registered electors to vote after election-night estimates, partial official returns, and other news became public. A model links perceived viability, affinity, and the benefits of aligned representation. Cardinality matching balances previous electoral preferences, electoral-roll demographics, and surrounding socioeconomic conditions.

The estimates indicate substantial presidential reallocation. As percentage points of the registered electorate, Nieto, López Aliaga, and Sánchez gained 11.2, 3.9, and 1.2 points, respectively. Álvarez, López Chau, and Belmont lost 4.4, 2.9, and 2.8 points; remaining parties jointly lost 9.9 points. Blank and invalid ballots declined by 2.9 points. The estimated increase in abstention is 5.8 points, but it is not robust across inference procedures. Fujimori’s estimated gain of 0.8 points is statistically indistinguishable from zero. The seven other presidential candidate-share effects survive Holm adjustment across 26 outcomes and remain significant under the approximate Rosenbaum sensitivity analysis, although robustness varies across candidates. These findings are consistent with viability influencing electoral choices, but aggregate returns cannot identify belief updating separately.

Legislative support moves in the same direction for all eight party outcomes, with effects between 58% and 77% of their presidential counterparts. Nieto’s party gains 7.8 points, López Aliaga’s 2.4, and Sánchez’s 0.8. Smaller legislative effects, however, do not necessarily imply incomplete transmission across offices: several closely approximate

proportional changes based on baseline legislative-to-presidential support ratios. Nieto, López Chau, and the remaining parties show the clearest departures from that benchmark. Sánchez’s legislative estimate is particularly fragile and loses significance under alternative inference and joint multiplicity adjustment. The model accommodates both retaining a preferred legislative party after switching presidential support and choosing aligned representation, but aggregate gaps cannot measure individual ticket splitting. The electoral threshold and Peru’s experience of presidential removals by Congress provide plausible reasons for these contrasting responses, without establishing which mechanism accounts for the estimated changes in support.

Despite our efforts to obtain credible estimates through careful design and robustness checks, several limitations qualify their interpretation. Treatment combines information exposure with workday voting, announced fine-relief measures, and other overnight developments; estimates therefore capture the delayed-voting regime. We do not separately assess the influence of social media or interpersonal communication, which may transmit or amplify electoral news. Identification requires selection on observables and no interference, while matching leaves some distributional imbalances. Our strategy cannot address changes in voter composition, and the abstention estimate is fragile. Approximate links between polling locations across elections, contextual census measures, and inference based on thirteen treated locations introduce further uncertainty. Effects are local to Lima Sur and need not generalize to electorates with different preferences.

These findings carry a practical implication for electoral administration. Peruvian law prohibits the publication of voting intention polls during the week before the election precisely to limit their influence, yet when voting is extended, election-night estimates and partial counts reach part of the electorate before it votes. The estimated reallocations under delayed voting are consistent with an informational response, although the design cannot separate information from changes in participation conditions. Rules governing the release of flash estimates and partial results when voting is delayed deserve the same attention as the pre-electoral polling ban.

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A Political context, polls, and voter profiles

This appendix supplements Sections 2.3 and 6.2 with government-plan profiles (Table 9), IEP voter profiles motivating the balance covariates (Table 10), and polling series (Figures 1–3). These descriptive materials do not enter the estimation.

Table 9: Programmatic profiles of government plans (*Planes de Gobierno*)

Party	Government Plan Summary
Juntos por el Perú	Consistently left-wing across all dimensions: interventionist economic policy, emphasis on social reinsertion over punitive security, expanded public spending, nationalist foreign policy, and progressive social values.
Primero la Gente	Centre-left: moderately interventionist, leans toward reinsertion-based security, slight preference for public spending, centrist on foreign policy, and moderately progressive on social values.
Ahora Nación	Centre-left with a strong progressive stance on social values; moderately interventionist, centrist on security and foreign policy, and leans toward public spending.
Buen Gobierno	Similar to Ahora Nación: moderately interventionist, centrist on security and state size, neutral on foreign policy, and markedly progressive on social values.
Partido Cívico Obras	Mixed economic profile: supports private investment, free trade, and a smaller state apparatus, alongside state participation in strategic enterprises and public infrastructure investment. These positions do not imply an unambiguous left–right classification.
País para Todos	Centre-right: centrist on economic policy, leans toward <i>mano dura</i> security, centrist on state size, moderately globalist, and centrist on social values.
Renovación Popular	Right-wing: favors free-market economics, <i>mano dura</i> security, bureaucratic reduction, but leans nationalist on foreign policy and conservative on social values.
Fuerza Popular	Right-wing on most dimensions: free-market orientation, clearly <i>mano dura</i> on security, favors state reduction, centrist on foreign policy, and conservative on social values.

Source: Authors’ interpretation of TuVoto Perú’s AI-assisted and editorial government-plan classification (TuVoto Perú, 2026), except Obras, summarized from its official plan (Partido Cívico Obras, 2025, pp. 4–6). The five dimensions cover economic intervention, security policy, public spending, international integration, and social values. Classification involves inference where plans are silent and does not measure campaign discourse or voter preferences.

Table 10: Profile of the seven leading candidates' voters, IEP April II 2026 (column %).

	KF	RS	RLA	JN	RB	CA	LCH
<i>Sex</i>							
Men	48.3	58.9	49.2	40.0	51.4	47.6	62.0
Women	51.7	41.1	50.8	60.0	48.6	52.4	38.0
<i>Age</i>							
18–29	24.7	13.5	14.8	50.0	29.9	23.8	14.1
30–49	40.4	56.2	36.7	35.7	39.3	54.8	43.7
50+	34.8	30.3	48.4	14.3	30.8	21.4	42.3
<i>Education</i>							
Basic	68.5	70.3	21.1	20.7	49.5	47.6	29.6
Higher education	31.5	29.7	78.9	79.3	50.5	52.4	70.4
<i>When decided</i>							
Election day	20.2	17.8	11.7	14.3	13.1	31.0	16.9
Final week	16.9	24.9	21.1	33.6	38.3	31.0	16.9
During March	4.5	13.5	15.6	34.3	29.9	21.4	16.9
Long ago	57.9	41.1	51.6	17.9	18.7	16.7	49.3

Notes. Bold marks each row's maximum; columns follow the IEP report's photo sequence. KF = Keiko Fujimori, RS = Roberto Sánchez, RLA = Rafael López Aliaga, JN = Jorge Nieto, RB = Ricardo Belmont, CA = Carlos Álvarez, LCH = Alfonso López Chau. Source: [Instituto de Estudios Peruanos \(2026, p. 7\)](#). These retrospective profiles come from a post-first-round survey and provide descriptive context. Survey education categories and candidate-conditioned electorates differ from the registry proxies used in matching. Timing nonresponses are omitted; totals may differ from 100 because of nonresponse or rounding.

Figure 1: Regular vote-intention polls, Ipsos Perú (July 2025–April 2026). Percentages of all respondents, including blank, invalid, and undecided responses in the denominator. Dates mark the last day of fieldwork. Source: [La Encerrona \(2026\)](#).

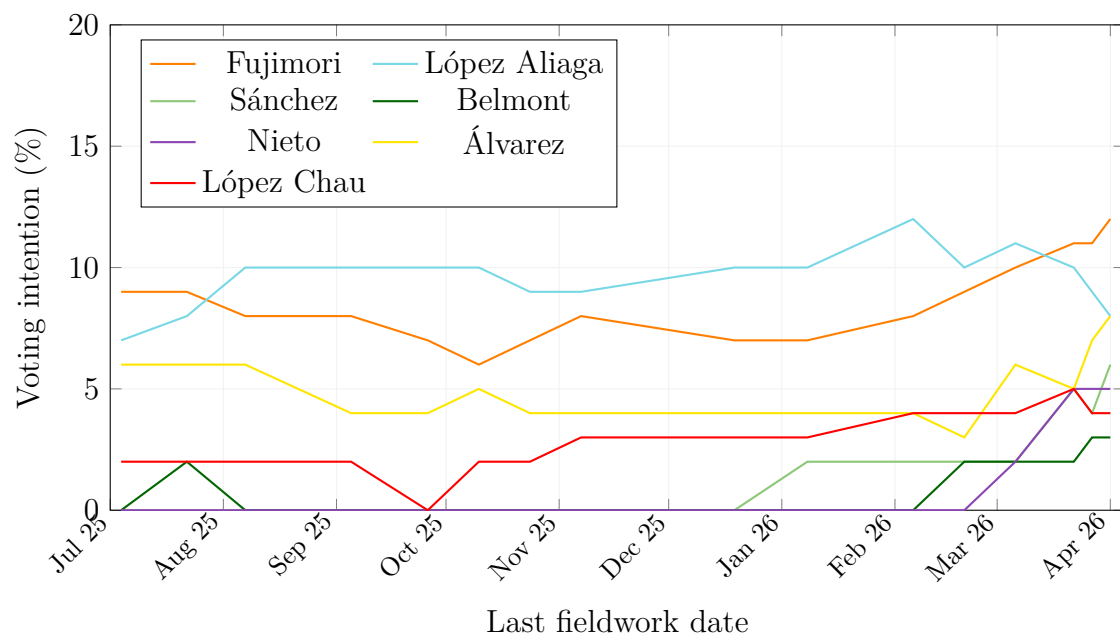


Figure 2: Regular vote-intention polls, Datum Internacional (December 2025–April 2026). Percentages of all respondents, including blank, invalid, and undecided responses in the denominator. Dates mark the last day of fieldwork. Source: [La Encerrona \(2026\)](#).

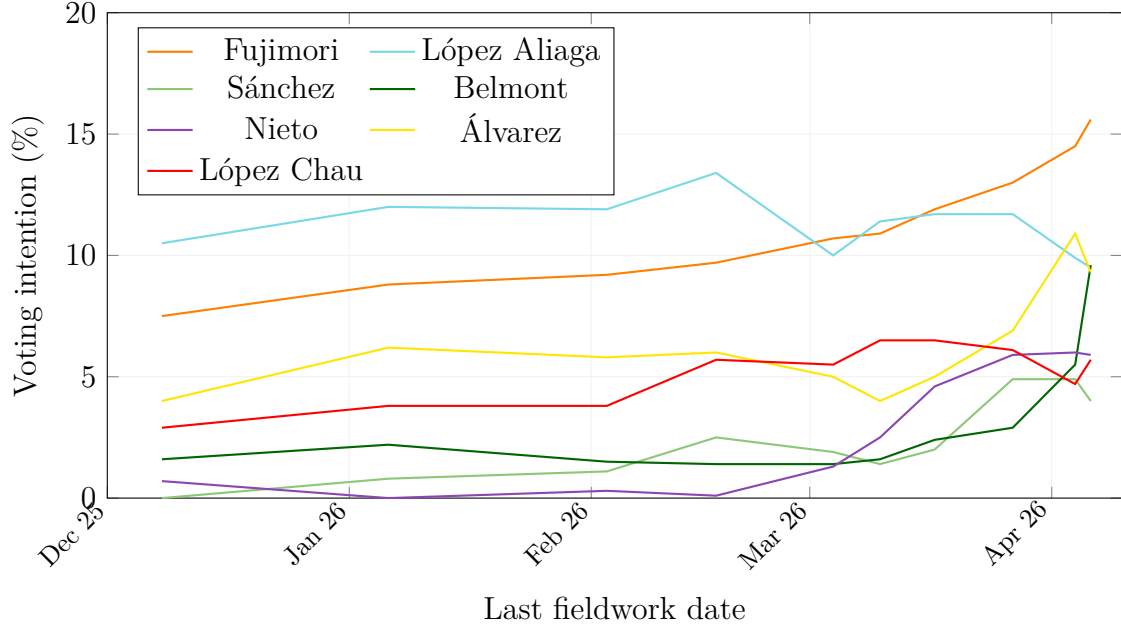
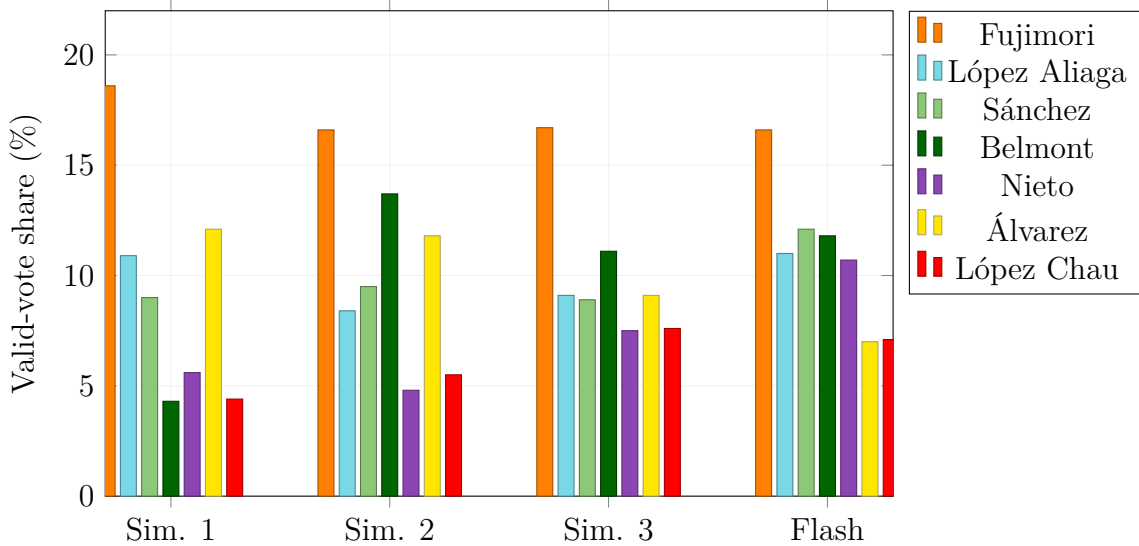


Figure 3: Ipsos *simulacros* and flash estimate, Peru 2026. Valid-vote basis. Simulacros 1–3 correspond to survey waves dated 2, 8, and 11 April, as reported in the 12 April election-night release. The flash was released on 12 April. Source: [Ipsos Perú \(2026, pp. 5–6\)](#).



B Descriptive statistics of the outcomes

Table 11 reports, before matching, the distribution of the 26 outcomes of Section 6.1 across the thirteen treated locations and the 351 locations in the control pool. All outcomes use registered electors as the denominator; aggregate gaps are signed differences of shares.

Table 11: Descriptive statistics of the outcomes, before matching.

Outcome	Group	<i>N</i>	Mean	SD	Min	Max
<i>Panel A. Presidential vote shares</i>						
Rafael López Aliaga (RP)	Control	351	0.118	0.052	0.034	0.598
	Treated	13	0.126	0.043	0.075	0.216
Roberto Sánchez (JP)	Control	351	0.022	0.011	0.000	0.078
	Treated	13	0.040	0.014	0.024	0.068
Jorge Nieto (BG)	Control	351	0.107	0.031	0.046	0.231
	Treated	13	0.209	0.043	0.126	0.310
Keiko Fujimori (FP)	Control	351	0.140	0.025	0.055	0.211
	Treated	13	0.151	0.033	0.095	0.200
Ricardo Belmont (O)	Control	351	0.069	0.017	0.011	0.136
	Treated	13	0.052	0.008	0.043	0.066
Carlos Álvarez (PT)	Control	351	0.074	0.014	0.012	0.112
	Treated	13	0.028	0.006	0.020	0.042
Alfonso López Chau (AN)	Control	351	0.043	0.010	0.019	0.082
	Treated	13	0.009	0.003	0.005	0.015
Other parties	Control	351	0.137	0.018	0.069	0.236
	Treated	13	0.042	0.006	0.030	0.050
Blank-and-invalid share	Control	351	0.105	0.026	0.018	0.278
	Treated	13	0.087	0.012	0.061	0.104
<i>Panel B. Legislative-support indices</i>						
Renovación Popular	Control	351	0.087	0.041	0.035	0.465
	Treated	13	0.089	0.028	0.053	0.151
Juntos por el Perú	Control	351	0.019	0.010	0.001	0.074
	Treated	13	0.032	0.012	0.017	0.058
Buen Gobierno	Control	351	0.086	0.027	0.030	0.202
	Treated	13	0.159	0.034	0.090	0.240
Fuerza Popular	Control	351	0.101	0.021	0.035	0.164
	Treated	13	0.107	0.023	0.069	0.135
Obras	Control	351	0.051	0.014	0.006	0.112
	Treated	13	0.040	0.008	0.028	0.055
País para Todos	Control	351	0.045	0.010	0.011	0.075
	Treated	13	0.018	0.003	0.012	0.024
Ahora Nación	Control	351	0.044	0.011	0.018	0.087
	Treated	13	0.019	0.005	0.011	0.027

Continued on next page

Table 11 (continued)

Outcome	Group	<i>N</i>	Mean	SD	Min	Max
Other parties	Control	351	0.125	0.016	0.065	0.194
	Treated	13	0.059	0.006	0.050	0.069
<i>Panel C. Presidential-legislative gaps</i>						
Renovación Popular	Control	351	0.030	0.016	-0.001	0.133
	Treated	13	0.037	0.018	0.016	0.065
Juntos por el Perú	Control	351	0.003	0.003	-0.008	0.018
	Treated	13	0.008	0.004	0.002	0.015
Buen Gobierno	Control	351	0.020	0.009	-0.005	0.065
	Treated	13	0.050	0.013	0.036	0.070
Fuerza Popular	Control	351	0.039	0.012	-0.003	0.098
	Treated	13	0.043	0.012	0.025	0.067
Obras	Control	351	0.017	0.007	0.001	0.055
	Treated	13	0.012	0.004	0.005	0.019
País para Todos	Control	351	0.029	0.008	0.002	0.064
	Treated	13	0.010	0.004	0.004	0.018
Ahora Nación	Control	351	-0.002	0.005	-0.014	0.025
	Treated	13	-0.009	0.004	-0.016	-0.004
Other parties	Control	351	0.012	0.016	-0.050	0.107
	Treated	13	-0.017	0.006	-0.027	-0.009
<i>Panel D. Abstention</i>						
Abstention rate	Control	351	0.186	0.053	0.046	0.369
	Treated	13	0.255	0.091	0.103	0.440

Notes. *N* denotes the number of polling locations; Control includes the full pre-matching control pool. SD denotes the standard deviation. All outcomes use registered electors as the denominator: vote shares and the blank-and-invalid share are votes divided by registered electors, legislative-support indices are minima of such shares across the three legislative contests, and abstention is the share of registered electors who did not vote. “Other parties” combines all candidates and parties other than the seven listed. Within each group, the mean presidential shares, the mean blank-and-invalid share, and the mean abstention rate sum to one. These shares and the legislative-support indices lie in $[0, 1]$. Aggregate gaps are signed differences between the presidential vote share and the corresponding legislative-support index, lie in $[-1, 1]$, and do not measure individual ticket splitting.

C Balance diagnostics

This appendix supplements the mean-balance results in Table 6 with Kolmogorov–Smirnov statistics (Table 12), density plots (Figure 4), and QQ-plots (Figure 5). Figure 6 summarizes the matching design. Before matching, all thirteen treated locations and 351 controls

receive equal weight within their groups. After matching, the thirteen treated locations retain equal weight and each of the 39 selected controls receives the estimation weight $1/(13k_i)$ of Section 6.2, so that the controls in set i jointly carry the same weight as their treated location. The treated distribution therefore remains fixed while the control sample and its weights change.

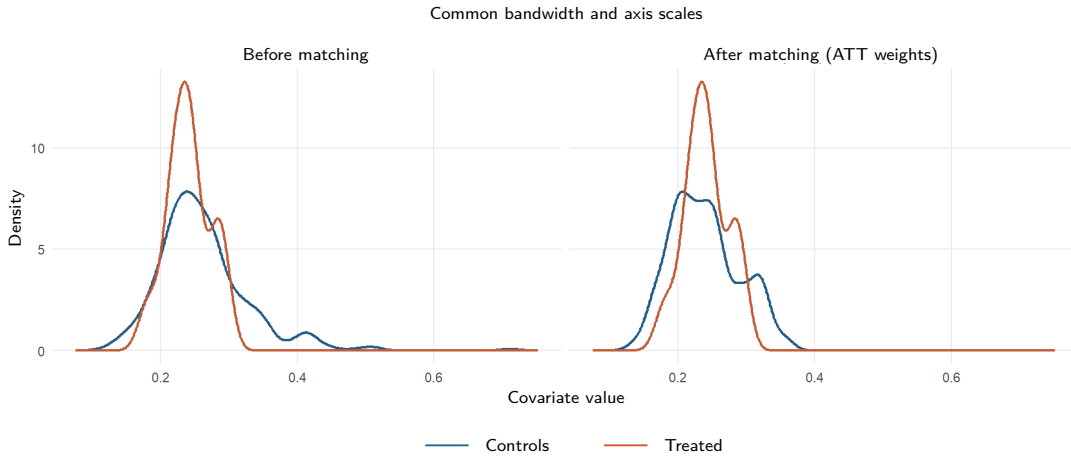
Mean balance does not guarantee distributional balance: the weighted KS statistic rises from 0.228 to 0.244 for the 2021 right vote share and from 0.174 to 0.213 for Fujimori's 2021 share. The education QQ plot also shows selected controls above the treated range for the below-secondary registry share. These marginal plots do not identify which controls were assigned to which treated location. Set-level covariate contrasts and distances are needed to assess the remaining discrepancies; the KS changes alone do not establish that matching or identification has failed.

Table 12: Weighted Kolmogorov–Smirnov statistics by covariate, before and after matching.

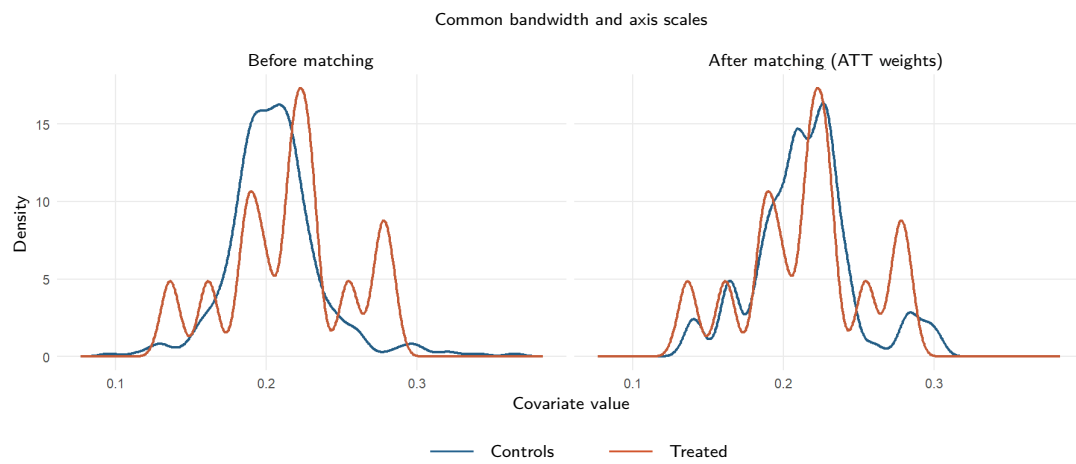
Covariate	Before matching	After matching
2021 right vote share	0.228	0.244
2021 four-party vote share	0.325	0.141
2021 Fujimori vote share	0.174	0.213
Average age	0.231	0.187
Registry: below-secondary education (%)	0.541	0.251
Registry: higher education (%)	0.373	0.119
Female registered voters (%)	0.305	0.167
Socioeconomic stratum	0.433	0.202
Indigenous-language share	0.345	0.192

Notes. Entries report the weighted two-sample Kolmogorov–Smirnov statistic for the location-level distribution of each covariate. Before matching, all polling locations receive equal weight. After matching, the statistic uses the estimation weights. Lower values indicate closer agreement between the treated and control distributions.

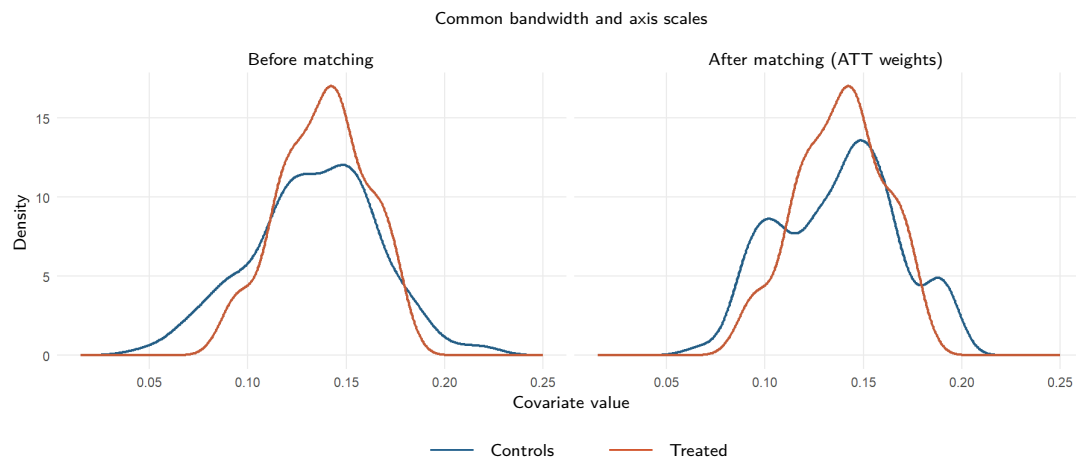
Figure 4: Covariate densities at treated and control polling locations, before and after matching.



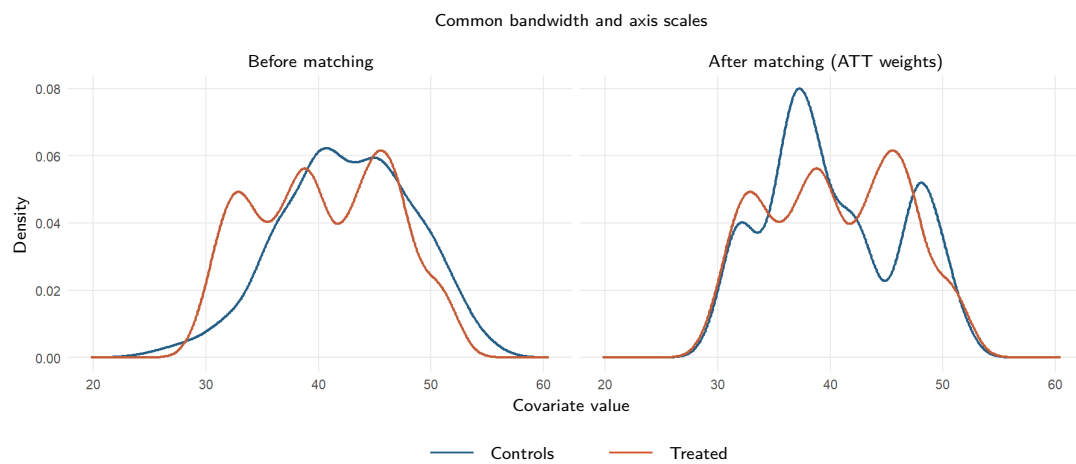
(a) 2021 right vote share



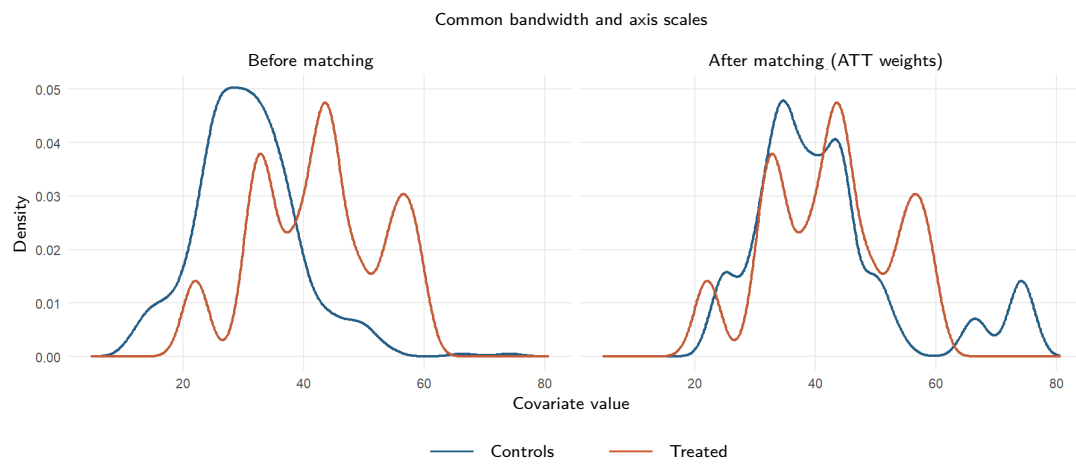
(b) 2021 four-party vote share



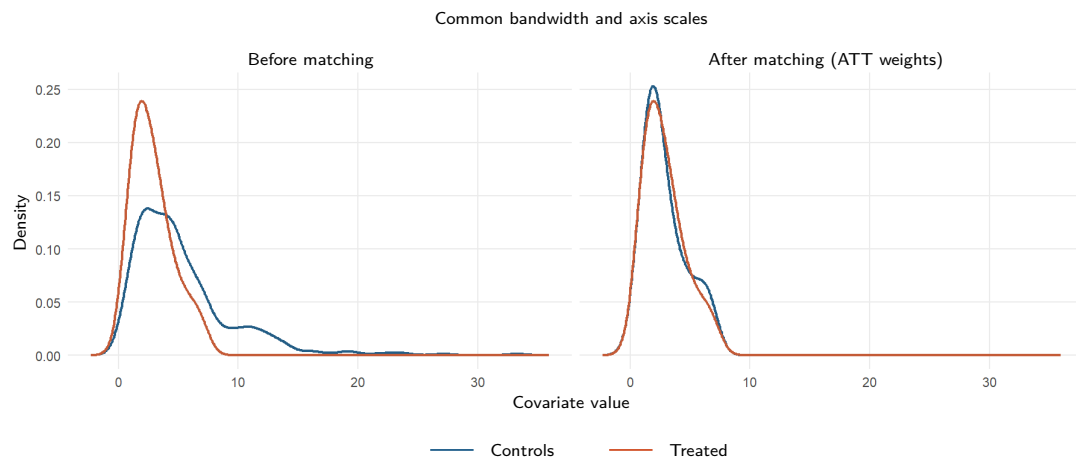
(c) 2021 Fujimori vote share



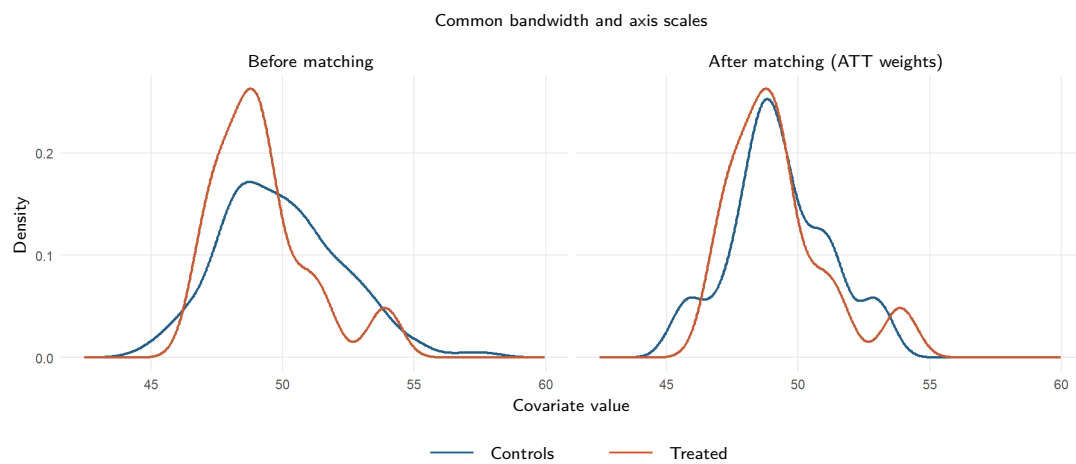
(d) Average age



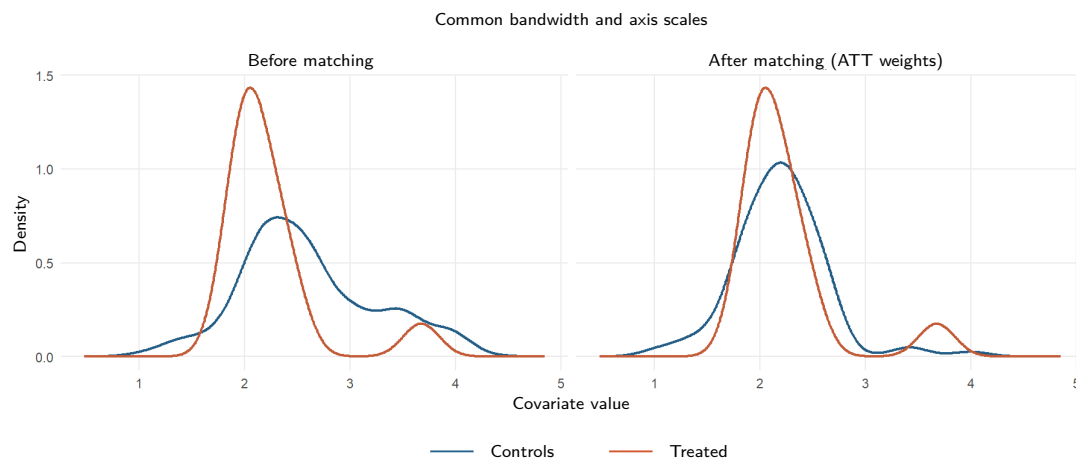
(e) Below-secondary education share



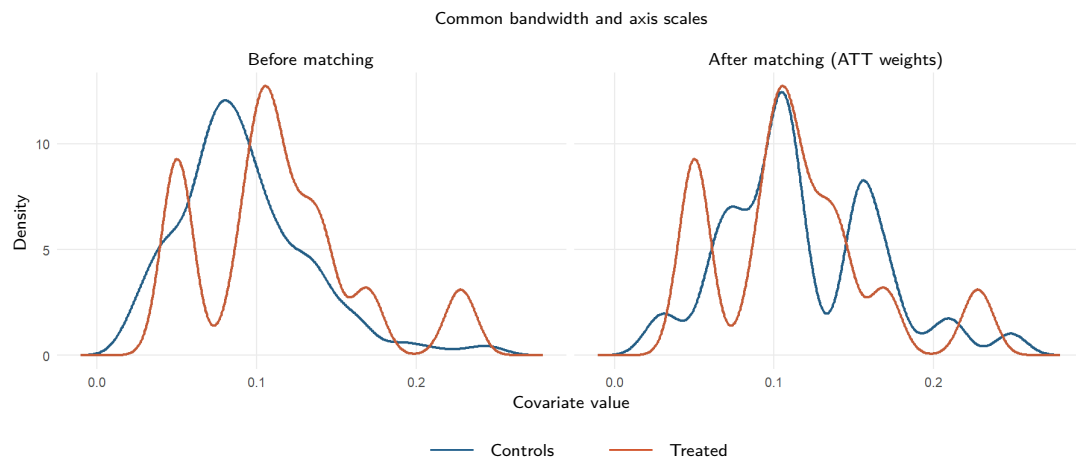
(f) Higher-education share



(g) Female-voter share

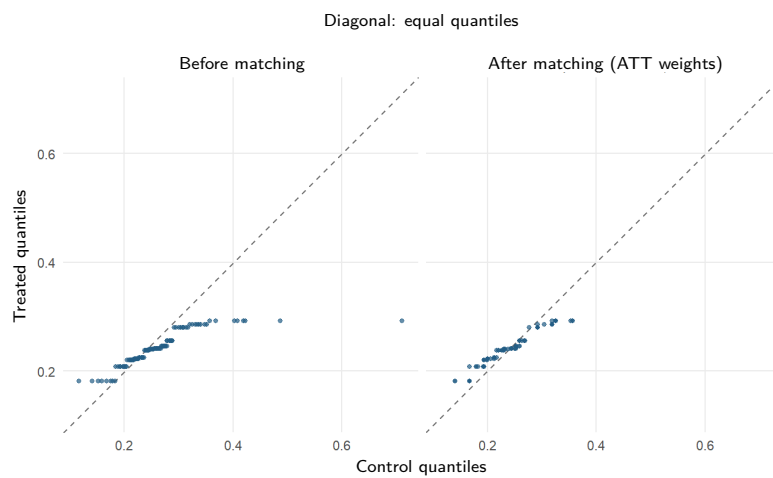


(h) Socioeconomic stratum

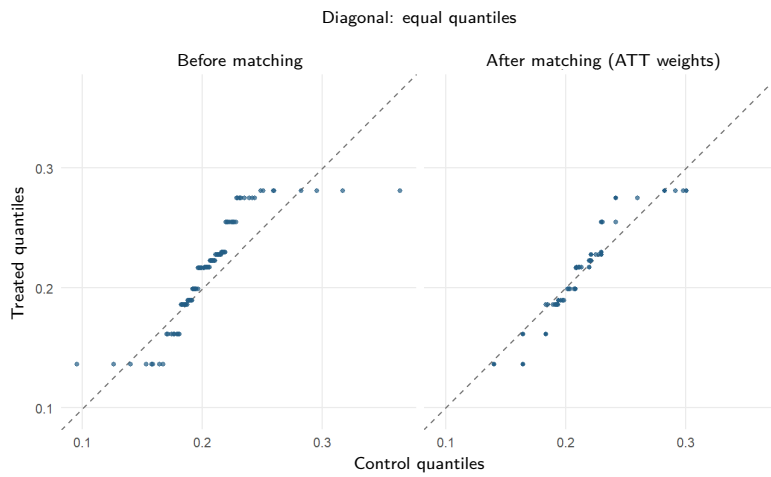


(i) Indigenous-language share

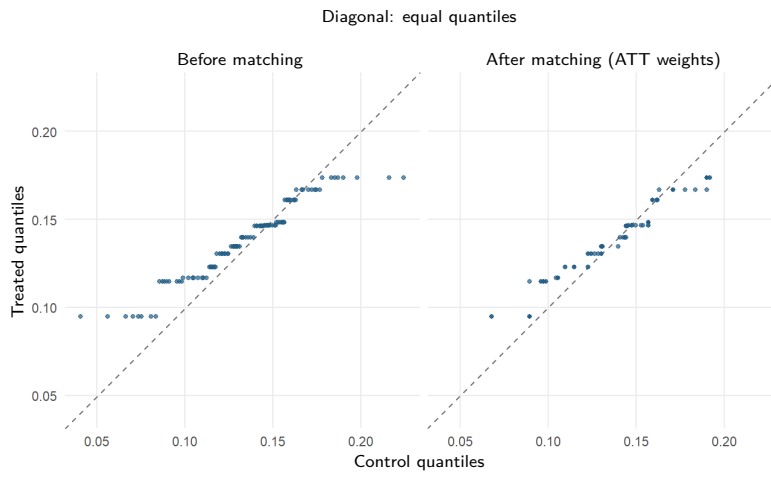
Figure 5: QQ-plots of treated and control covariates, before and after matching.



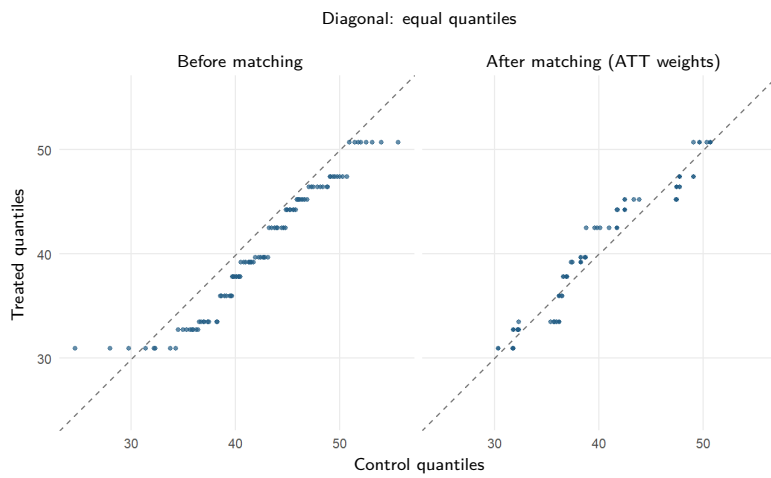
(a) 2021 right vote share



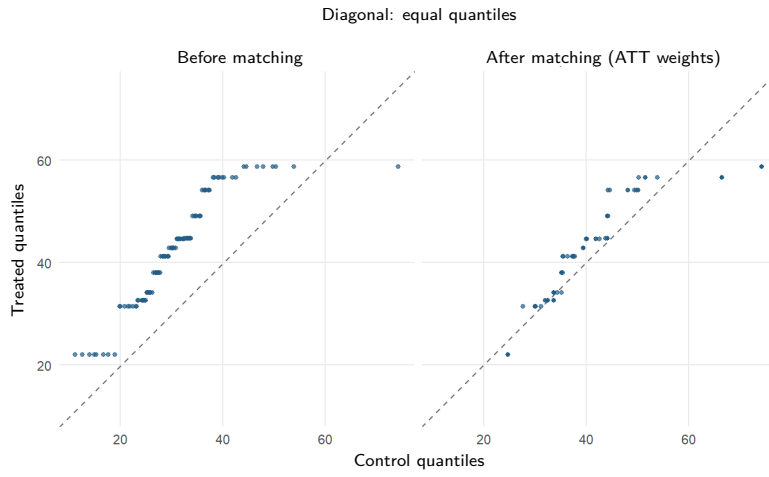
(b) 2021 four-party vote share



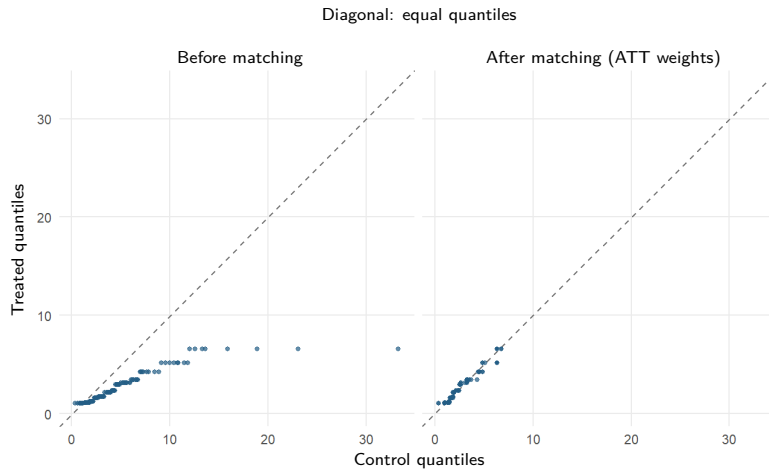
(c) 2021 Fujimori vote share



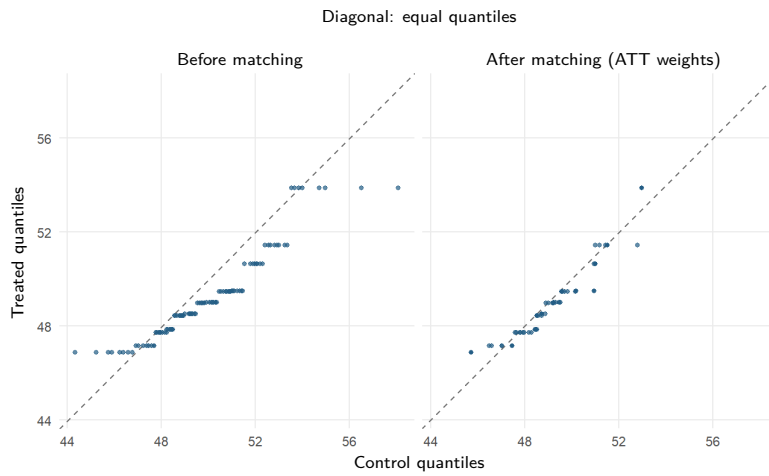
(d) Average age



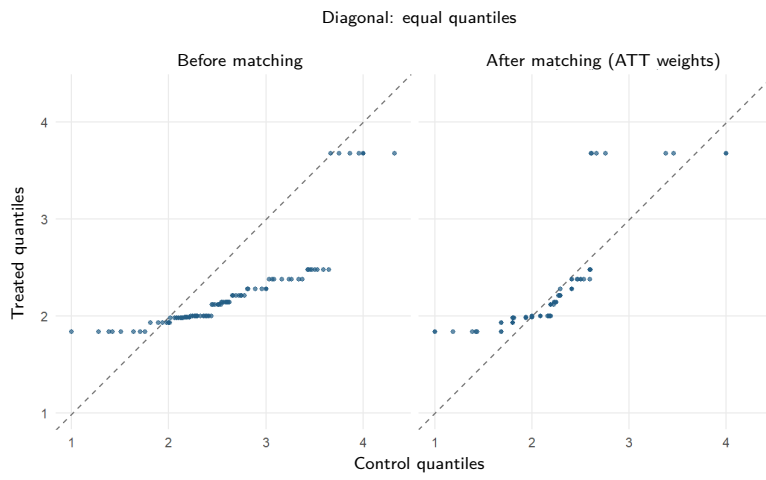
(e) Below-secondary education share



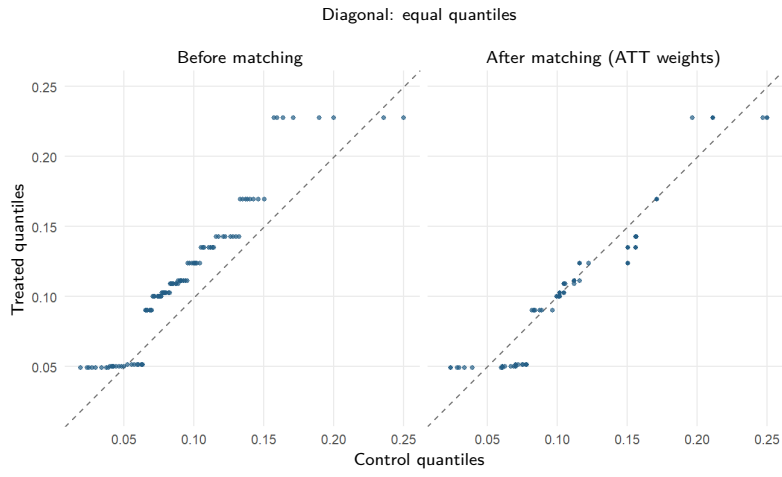
(f) Higher-education share



(g) Female-voter share

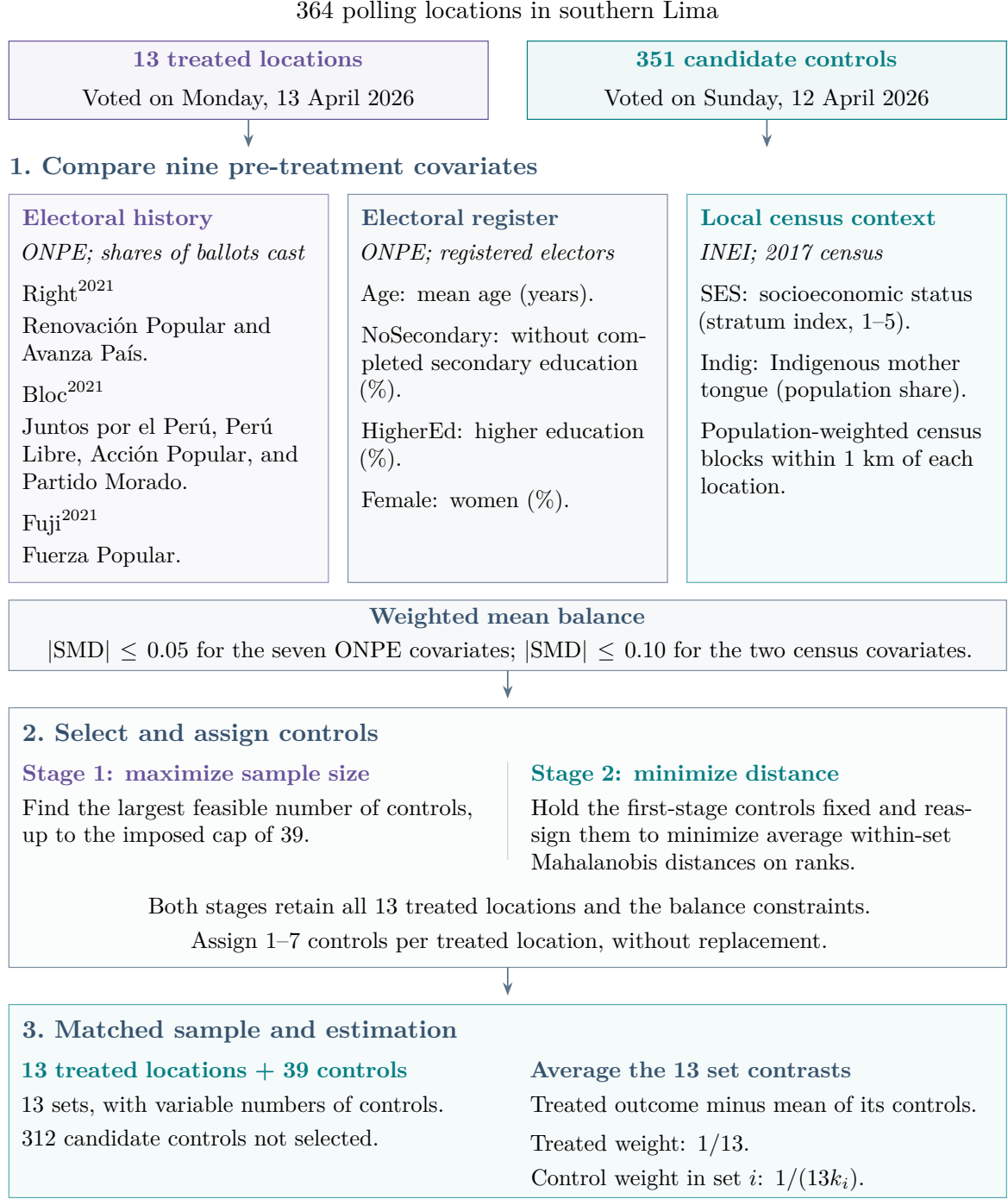


(h) Socioeconomic stratum



(i) Indigenous-language share

Figure 6: Construction of the matched comparison group.



Notes. The diagram summarizes the design in Section 6.2. ONPE denotes the National Office of Electoral Processes; INEI, the National Institute of Statistics and Informatics; SMD, standardized mean difference. Each matched set contains one treated location and k_i controls; all thirteen treated locations receive equal estimation weight.

D Design-based inference and robustness checks

This appendix reports the checks that complement the t -tests of Table 7: the sign-score test of the sharp null (Section D.1), the leave-one-out exercise (Section D.2), Holm adjustments for the 26 outcomes (Section D.3), and Rosenbaum sensitivity thresholds (Section D.4). Each addresses a different question and none replaces the identifying assumptions of Section 4.5.

D.1 Sign-score tests

Table 13 reports, for each outcome, the number of positive matched-set contrasts and the exact two-sided p -value of the sign-score test described in Section 6.2.

Table 13: Sign-score statistics and p -values for the sharp null of no effect at any location.

Party (presidential candidate)	Presidential		Legislative index		Aggregate gap	
	Positive	p -value	Positive	p -value	Positive	p -value
Fuerza Popular (Keiko Fujimori)	9/13	0.232	10/13	0.071	8/13	0.766
Juntos por el Perú (Roberto Sánchez)	12/13	0.004	10/13	0.056	11/13	0.024
Renovación Popular (Rafael López Aliaga)	11/13	0.005	11/13	0.011	11/13	0.012
Partido del Buen Gobierno (Jorge Nieto)	13/13	< 0.001	13/13	< 0.001	13/13	< 0.001
Partido Cívico Obras (Ricardo Belmont)	0/13	< 0.001	0/13	< 0.001	0/13	< 0.001
País para Todos (Carlos Álvarez)	0/13	< 0.001	0/13	< 0.001	0/13	< 0.001
Ahora Nación (Alfonso López Chau)	0/13	< 0.001	0/13	< 0.001	0/13	< 0.001
Other parties (combined)	0/13	< 0.001	0/13	< 0.001	0/13	< 0.001
Blank-and-invalid share	0/13	< 0.001	—	—	—	—
Abstention	9/13	0.194	—	—	—	—

Notes. Outcomes are defined as in Table 7, over registered electors. “Positive” reports the number of positive set-level contrasts out of thirteen, which is the sign-score statistic. The p -values are two-sided exact p -values for the sharp null of no effect at any location under the working assignment model of Section 6.2, without adjustment for multiple outcomes. Because the null probability of a positive contrast depends on the size and outcomes of each matched set, equal counts can yield different p -values. Values below 0.001 are indicated explicitly.

D.2 Leave-one-out robustness

Table 14 summarizes the thirteen iterations in which one treated location and its matched controls are removed and the estimates of Table 7 are recomputed on the remaining twelve matched sets.

D.3 Multiplicity adjustments

The p -values in Tables 7 and 13 are unadjusted. For the t -tests of the average effects, Table 15 reports Holm-adjusted p -values (Holm, 1979) computed from the unrounded

Table 14: Leave-one-out robustness across thirteen iterations.

Party (presidential candidate)	Presidential		Legislative index		Aggregate gap	
	Sign	Signif.	Sign	Signif.	Sign	Signif.
Fuerza Popular (Keiko Fujimori)	0	0	0	0	1	0
Juntos por el Perú (Roberto Sánchez)	0	0	0	0	0	0
Renovación Popular (Rafael López Aliaga)	0	0	0	0	0	0
Partido del Buen Gobierno (Jorge Nieto)	0	0	0	0	0	0
Partido Cívico Obras (Ricardo Belmont)	0	0	0	0	0	0
País para Todos (Carlos Álvarez)	0	0	0	0	0	0
Ahora Nación (Alfonso López Chau)	0	0	0	0	0	0
Other parties (combined)	0	0	0	0	0	0
Blank-and-invalid share	0	0	—	—	—	—
Abstention	0	4	—	—	—	—

Notes. Outcomes are defined as in Table 7, over registered electors. Each iteration excludes one of the thirteen treated locations together with its matched controls, keeping the remaining matched sets fixed. “Sign” reports the number of iterations in which the estimated effect changes sign relative to the full-sample estimate. “Signif.” reports the number of iterations in which significance at the 5% level under the t -test of Table 7 changes relative to the full sample. The only changes in significance occur for abstention, which is significant in the full sample. Dashes indicate outcomes not reported.

estimates and standard errors. We report separate eight-outcome families for the presidential shares and legislative indices, as well as a joint family containing all 26 outcomes. These are supplementary checks. Holm’s procedure allows arbitrary dependence across outcomes; the validity of the underlying t -tests still requires the independent-contrast approximation described in Section 6.2.

Table 15: Holm-adjusted p -values for the t -tests of average effects.

Candidate	Separate families of eight		Joint family of 26		
	Presidential	Legislative	Presidential	Legislative	Gap
Fujimori	0.422	0.414	1.000	1.000	1.000
Sánchez	0.002	0.025	0.006	0.062	0.004
López Aliaga	0.002	0.003	0.007	0.008	0.012
Nieto	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Belmont	< 0.001	< 0.001	< 0.001	< 0.001	0.001
Álvarez	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
López Chau	< 0.001	< 0.001	< 0.001	< 0.001	0.001
Other parties	< 0.001	< 0.001	< 0.001	< 0.001	< 0.001
Blank-and-invalid share (joint family of 26)				< 0.001	
Abstention (joint family of 26)				0.130	

Notes. Adjusted p -values use the two-sided t_{12} reference distribution and the unrounded estimates and standard errors underlying Table 7. Values below 0.001 are indicated explicitly. The separate families contain the eight presidential shares and the eight legislative indices, respectively. The joint family comprises eight presidential shares, eight legislative indices, eight gaps, the blank-and-invalid share, and abstention.

Within the separate families, every presidential and legislative effect except Fujimori’s retains an adjusted p -value below 0.05. With all 26 outcomes grouped together, this holds for seven presidential shares, six legislative indices, seven gaps, and the blank-and-invalid share; it does not hold for Fujimori’s three outcomes, Sánchez’s legislative index, or abstention. These calculations address multiplicity only; they do not resolve confounding or dependence across matched sets.

D.4 Sensitivity to hidden bias

Table 16 reports, for each outcome in Table 7, the largest Rosenbaum sensitivity parameter Γ at which the permutation test based on the mean matched-set contrast rejects the sharp null at 5%, computed as described in Section 6.2.

Table 16: Rosenbaum sensitivity thresholds Γ .

Party (presidential candidate)	Presidential	Legislative index	Aggregate gap
Fuerza Popular (Keiko Fujimori)	None	None	None
Juntos por el Perú (Roberto Sánchez)	2.2	1.3	2.2
Renovación Popular (Rafael López Aliaga)	3.9	3.7	2.8
Partido del Buen Gobierno (Jorge Nieto)	6.8	7.0	5.2
Partido Cívico Obras (Ricardo Belmont)	6.7	6.1	2.7
País para Todos (Carlos Álvarez)	6.9	6.6	5.0
Ahora Nación (Alfonso López Chau)	6.7	5.7	3.6
Other parties (combined)	7.8	7.6	5.0
Blank-and-invalid share	4.1	—	—
Abstention	1.2	—	—

Notes. Outcomes are defined as in Table 7, over registered electors. Each entry is the largest Γ , on a grid from 1 to 10 in steps of 0.1, at which the upper bound of the two-sided p -value of the permutation test of the sharp null of no effect at any location remains below 0.05. The test statistic is the mean matched-set contrast in (29); bounds are large-sample normal approximations computed with `sensitivitymv::senmv`, and the two-sided bound is twice the smaller one-sided bound, capped at one (Section 6.2). The bound at $\Gamma = 1$ need not equal the t -test p -value underlying the stars in Table 7. “None” indicates no rejection at 5% even at $\Gamma = 1$. Dashes indicate outcomes not reported. Thresholds depend on the stated assignment model and test statistic and do not address dependence across matched sets.