

Missing Voters? Evidence from Polling Station Delays in the 2026 Peruvian Elections*

TECHNICAL WORKING PAPER

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Abstract

To what extent do delayed opening times of polling booths influence levels of turnout? We exploit administrative and logistical disruptions across voting sites in Lima during the first round of the 2026 Peruvian presidential election, leveraging idiosyncratic variation in polling-station opening times generated by these disruptions to estimate the effect of delays on voter participation. Although only affecting a small share of voting sites, our estimates suggest sizeable impacts of the delays on voter participation, ranging from approximately a 2 to 5 percentage point decline in turnout. Nonetheless, back-of-the-envelope estimates of total foregone votes are not enough to change electoral outcomes, including the margin between the second- and third-place candidates. These results suggest that even localized administrative failures can impose meaningful participation costs, while also underscoring the distinction between electorally consequential disruption and outcome-changing disruption.

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“Ya no pude ejercer [mi derecho al voto]... He estado a las 6 de la mañana y me he retirado a las 11:30.”

“I was not able to exercise [my right to vote] anymore... I was there at 6 in the morning and left at 11:30.”

– Jose Reyes Flores, 85 year-old voter in Lima, Peru¹

1 Introduction

Competent election administration is a prerequisite of democracy: elections not only translate the polity’s preferences into outcomes following the rules set by the constitution, but they also renew the legitimacy of the democratic system itself (Lipset 1960). In turn, when election administration is put into question, confidence in the entire democratic project can falter. Despite high levels of sustained economic growth, Peru is a well-documented setting of declining satisfaction with democracy, with confidence in electoral institutions ranking among the lowest in the world (Mainwaring 2006; Mainwaring and Pérez-Liñán 2023).²

On Sunday April 12th 2026, Peruvians went to the polls for the first round of general elections.³ However, extraordinary and unprecedented logistical errors in the delivery of election materials, including ballots themselves, delayed the opening of several polling stations (*mesas de sufragio*), some by more than *seven hours* across stations in Lima. These delays were so severe that the original legal deadline for polling site installation of 12pm (Article 363 of the *Ley Orgánica de Elecciones*) had to be extended to 2pm. One month later, and at the time of this paper’s writing, the final results of the first round of the election have only just been announced. Amidst the backdrop of these irregularities, Peruvian democracy now finds itself at an inflection point: the projected margin between second and third place candidates, with only one advancing to the runoff, is approximately 21,000 votes (0.1%), the narrowest in any election since Peru’s return to democracy in 2000.

The presidential candidate who narrowly missed the ballotage, Rafael López Aliaga, has taken to the streets the last several weeks to contest the results of the election, promoting what we term the *missing voters theory*: hundreds of thousands of would-be voters showed up at their assigned polling stations on April 12th, waited for hours as *mesas* failed to open on time, and ultimately left without voting. According to this theory, these missing voters, not those who chose to abstain, are the decisive bloc whose disenfranchisement cost López Aliaga a place in the runoff. In public appearances, López Aliaga claims the number of missing voters totals “roughly 600,000 people ... [who] were prevented from voting through deliberate malice by Mr. Corvetto”, subsequently calling for the annulment of the electoral process and the imprisonment of the head of the National

1. Interview with [América Noticias \(2026\)](#).

2. See Appendix A.1 for proportion of individuals from the Gallup World Poll that report confidence in the honesty of elections for Peru, rest of Latin America, and rest of the World.

3. Just over 30 days prior to the publication of this report.

Office of Electoral Processes (ONPE).⁴

This paper rigorously assesses the impact of the delayed opening times of polling stations across Lima in the first round of the general election, held one month ago, on voter turnout. Given the lack of an official elections database, given ballots were still being counted at the time of this paper’s writing, we scrape the near-universe of available “statements of votes” (*actas*) across *mesas* within voting sites in Lima to construct our own dataset covering the 2026 election. We then employ state-of-the-art large language models (LLM) over 29,229 PDFs of *actas* to extract the installation times of polling stations, either in digital or hand-written format, thus allowing us to directly measure opening delays across Lima on both the intensive and extensive margin.

Our empirical strategy leverages highly granular cross-sectional variation at the *mesa*-level covering approximately 29,000 *mesas*. We use standard ordinary least squares and employ a fractional logit model to estimate the impact of opening times on turnout (Papke and Wooldridge 1996). We complement this by aggregating *mesas* up to the voting site level (e.g. schools), where we are then able to exploit within-voting site variation across four general elections since 2011. Using a difference-in-differences strategy via static two-way fixed effects and a dynamic event study, we thus exploit an alternative source of variation to gauge differences in turnout among delayed voting sites in 2026 relative to historical baselines.

The core treatment variable of interest is based on whether a *mesa* opened after 10am, almost three hours later than the legal starting installation time of 7am (which was the modal opening hour for *mesas* in Lima in 2021), although we also vary this installation threshold to probe treatment effects on the intensive margin. Given the possibility for misreporting of opening times, we also rely on a report released four days after the election by the [Jurado Nacional de Elecciones \(2026\)](#) (JNE) which flagged over one-hundred *mesas* that were confirmed as having opened after the 2pm extended legal deadline, thus serving as a reasonable “ground truth” for delayed installations. As an additional measurement, we also focus on those *mesas* that never opened on Sunday but opened a full day later on Monday.

First, exploiting cross-sectional variation, our core estimate suggests that across 2,750 *mesas* that opened after 10am on Sunday, turnout rates declined by approximately 3 percentage points. This effect largely remains stable when shifting the threshold to later hours (e.g. 11am, 12pm, etc), with largest effect sizes among *mesas* that opened after 2pm, including those flagged by the [Jurado Nacional de Elecciones \(2026\)](#) report. We also estimate just more than a 5 percentage point decrease in turnout among those *mesas* that opened on Monday. Results are essentially identical when running fractional logit models. Binning opening times into hourly intervals to more flexibly (i.e. semi-parametrically) estimate the impact of *mesa*-level delays, we estimate a general decrease in turnout per additional hour relative to a 7-7.59am base, although the trend is non-monotonic, with a brief resurgence in turnout around 12pm, but then declining thereafter across

4. Interview with [CNN \(2026\)](#). Piero Corvetto was the President of the National Office of Electoral Processes until April 21st. He has since resigned and is under criminal investigation.

the afternoon.

Using our panel dataset at the voting site-level, our core TWFE model also estimates a 3 percentage point decrease in turnout among voting sites comprised of *mesas* that opened after 10am, with largest effect sizes again emerging for voting sites where all *mesas* opened after 2pm. Exploiting extensive margin variation for the 10am+ openers and JNE-flagged 2pm+ openers, an event study reveals that our TWFE estimates are not driven by pre-existing turnout trends from prior elections, thus providing confidence that the opening delays were a non-artificial and unanticipated “shock”. Using static TWFE, we also estimate a sizable 6.9 percentage point decrease in turnout among *mesas* that opened on Monday, although an event study (as reported in the Appendix) reveals major pre-trends that coincide with treatment onset. Such effects of a loss in turnout for Monday-opening *mesas* are thus interpreted with caution. Descriptively, we find that at least 817,765 eligible voters in Lima were assigned to *mesas* that opened more than three hours late, and 69,139 to *mesas* that opened more than eight hours late. Additionally, 54,362 were assigned to *mesas* that did not open on Sunday at all, but instead opened on Monday.

These results are robust to a battery of sensitivity checks. When using Keiko Fujimori’s vote share and blank votes as placebo outcomes, given neither outcome should be directly affected by reduced access to polling stations unless the delays are also proxying for broader political composition, we find precisely estimated null effects. A randomization inference exercise, where we sequentially reassign the “delayed” treatment to randomly chosen *mesas* and voting sites, reveals that our observed treatment effects are persistently in the tails of the distribution of placebo estimates. Moreover, our results remain robust when sequentially dropping ever-treated *distritos* via a quasi-jackknife procedure, refining our sample of control units, in addition to pursuing alternative cluster arrangements for standard errors, including wild cluster bootstrapping (Cameron et al. 2008). We also show that our estimates of the (semi-)elasticity of installation time on turnout is as large as the income-elasticity, approximating local socioeconomic development using satellite estimates of gross domestic product (GDP) at the *distrito* level Chen et al. (2022).

The key question emerging from this first round of Peru’s general election, in line with López Aliaga’s missing voters theory, concerns: exactly how many missing voters resulted from the opening time delays on election day? To interpret the quantitative impact of polling station delays on voter turnout, we perform back-of-the-envelope calculations using our core estimates to calculate the number of foregone votes cast among the registered voting populations of impacted *mesas* and voting sites. To provide bounds on our estimates, we also use the delta method and a voting site-cluster bootstrap procedure to estimate the uncertainty interval. Our core estimate which combines the effects of *mesas* that opened after 10am on Sunday, alongside decreased turnout on Monday, suggests the total foregone votes cast resulting from the delays is approximately 27,900 votes, approximately 3.2 percent of the aggregate 869,550 registered voting population across affected *mesas*.

To translate this loss in voter participation to vote margins between López Aliaga and Sánchez,

the second and third placed candidates, we require a counterfactual estimate of delayed polling stations' vote distributions, given observed vote shares would plausibly be endogenous to the delays. As such, we approximate the partisan composition of marginal voters using nearby untreated voting units (Alipour and Lindlacher 2025). Specifically, for each treated *mesa* or voting site, we approximate a counterfactual López Aliaga-Sánchez vote-share margin from untreated units in the same *distrito* (district), using a geographic nearest-*distrito* fallback when no suitable same-*distrito* donor is available. We then multiply this counterfactual margin by the estimated number of foregone votes, again using site-cluster bootstrapping to provide bounds on our calculations. Our core estimate suggests that López Aliaga lost approximately 5,700 votes relative to Sánchez as a result of the polling station delays; this figure is evidently nowhere near enough to close the 21,000 vote gap based on currently counted ballots.

As a final exercise, based on manually checking the PDFs of *actas* among those flagged by the JNE as having opened after the 2pm extended legal threshold, we document evidence of extreme “sorting” of opening times around the initial 12pm and then extended 2pm installation deadlines using McCrary (2008)'s density test. Leveraging the Jurado Nacional de Elecciones (2026)'s report, we also show that several of these flagged *mesas* misreported their actual installation times by illustrating the distribution; some *mesas* that actually opened after 2pm self-declared having been installed as early as 7am!

Our paper has both academic and practical relevance for the study and administration of elections. First, although there exists a vast literature on the drivers of turnout (Blais 2006), there remains more limited research on the “time elasticity” of polling station openings and voter participation. The overarching logic suggests that opening times can be a meaningful participation cost in elections, alongside geographic distance and economic opportunity costs from voting (Brady and McNulty 2011; Cantoni 2020; Alipour and Lindlacher 2025). Even when polling places are formally available, delays can impose uncertainty, waiting costs, and scheduling conflicts that make voting practically harder for citizens with limited time, work flexibility, or caregiving capacity. Some evidence from Austria shows that a 10% increase in opening times can decrease turnout between 0.5 to 0.9 percentage points (Potrafke and Roesel 2020).⁵ Our estimates suggest that polling station opening delays can have sizable impacts on turnout. To the best of our knowledge, these are even some of the largest estimates among drivers of voter turnout.

Second, we also contribute to the broader literature on electoral administration, albeit in the context of a developing democracy (Diamond 1999). Given most studies in this area focus on consolidated democracies, especially the United States (e.g. Wolfinger and Rosenstone 1980), our rich data collection effort, tracking the Peruvian first-round presidential count in real time over the course of a month, allows us to evaluate the effects of administrative failures in alternative contexts where state capacity is limited.

5. Other evidence shows disparities in voter wait times between racial groups in the United States, with African Americans having to wait at polling sites disproportionately longer (Chen et al. 2022b).

Finally, with respect to practical implications for electoral administration, there remain several lessons, especially going into the second round in June. Given Peru’s prior experience with electoral administration, the state undoubtedly has the capacity to install polling stations on time. In closely contested elections with polarized electorates, delays in polling station openings only deepen citizen distrust and fuel accusations of electoral malpractice. Electoral authorities cannot allow significant delays in the second round, and the electoral court should establish clear, pre-announced rules for handling delays rather than relying on ad hoc election-day decisions. If delays do occur, *mesa* officials should also not alter recorded installation times. The credibility of the process depends on accurately documenting opening times and maintaining transparent rules regarding when late-opening polling stations may legally be counted. Electoral monitors and tribunal officials should therefore place particular emphasis on the truthful reporting of installation timestamps.

Ultimately, although our analysis tries to rigorously and objectively evaluate the impact of polling site delays on voter turnout and the knock-on consequences for the second- and third-placed candidate vote margin, we fundamentally find no strong evidence that the delays, albeit unfortunate, would overturn the first round of the election based on ballots counted. Peruvians can thus be reassured that, going into the second round of the presidential election, the top two candidates are *legitimate*, and hence the integrity of the broader electoral outcome does not appear to have been fundamentally compromised by the logistical failures surrounding polling booth installations.

The rest of the paper is organized as follows. Section 2 provides further background to general elections and electoral administration in Peru. Section 3 discusses our data collection effort. Section 4 outlines our empirical strategies for estimating the impact of delayed openings on turnout. Section 5 outlines the core results. Section 6 provides our estimates of the total foregone votes as a result of delayed *mesa* and voting site openings, alongside estimates of the foregone votes lost to the current second and third-placed candidates. Section 7 provides further evidence of potential strategic misreporting of *mesa* opening times. Section 8 concludes the paper with key takeaways and lessons learned for going into the second round of the election.

2 Background

2.1 Presidential Elections

Peru elects its president through a two-round electoral system. Presidential elections are held every five years, and all citizens between the ages of 18 and 70 are legally required to vote under the country’s compulsory voting framework. If no candidate obtains more than 50 percent of valid votes in the first round, a runoff election between the top two candidates is held several weeks later. The electoral system is administered primarily by three institutions: the Oficina Nacional de Procesos Electorales (ONPE), responsible for organizing and executing elections; the Jurado Na-

cional de Elecciones (JNE), responsible for electoral oversight and adjudication; and the Registro Nacional de Identificación y Estado Civil (RENIEC), which maintains the civil registry and voter rolls.

Voting takes place at designated polling locations/sites (*locales de votación*) (e.g. schools), each of which contains multiple polling tables (*mesas de sufragio*). Individual voters are assigned to a specific *mesa* based on their place of residence and voter registration information. Each *mesa* is administered by randomly selected citizens who serve as poll workers (*miembros de mesa*), and voting can only begin once the required personnel and electoral materials are present. Consequently, delays in the arrival of materials or the absence of poll workers can postpone the opening time of voting tables and reduce the effective time available for citizens to cast ballots.

Historically, Peru has experienced relatively high levels of electoral participation compared to many Latin American democracies, in part due to compulsory voting and the associated fines for abstention. Lima Metropolitana, which contains roughly one-third of the national electorate, is particularly consequential for national election outcomes and places substantial administrative demands on electoral authorities.

2.2 2026 Election Day Delays

The 2026 presidential election occurred in a highly fragmented political environment characterized by weak party institutionalization and substantial voter volatility (Mainwaring 2018). No candidate secured an outright majority in the first round, making the race for second place and qualification into the runoff particularly competitive. Given the narrow margin separating candidates, even relatively small changes in turnout became politically salient.

Election day was marked by reports of delayed *mesa* openings across parts of Lima Metropolitana. According to statements from the ONPE, the delays were primarily caused by logistical failures in the transportation and delivery of electoral materials, particularly in southern Lima. Electoral authorities stated that the private contractor responsible for distributing voting materials failed to comply with contractual obligations and lacked sufficient vehicles to complete deliveries on schedule. In addition, some polling tables experienced delays due to the late arrival or absence of designated *miembros de mesa*. Because of these delays, the legal deadline for installation of *mesas* of 12pm (Article 363 of the *Ley Orgánica de Elecciones*) was extended to 2pm by the JNE, established under the directive *Acuerdo del 12.04.2026*. While most *mesas* eventually opened later the same day, a smaller subset of voting locations postponed voting until Monday under an exceptional extension authorized by the JNE.

Following the release of preliminary results, political actors and media commentators raised concerns that these delays may have depressed turnout in electorally important areas and potentially affected the ordering of candidates advancing to the runoff. As per López Aliaga's claim of missing voters, several raised suspicions of outright electoral fraud. At the same time, identifying the

causal effect of delayed openings is challenging, as turnout is shaped by many political, socio-economic, and geographic factors, and delayed polling locations were not randomly distributed across space.

These events provide the central motivation for this paper. Using newly collected data on polling table opening times during the 2026 Peruvian presidential election, we evaluate whether delayed openings reduced turnout and whether any resulting effects were large enough to plausibly alter the final ranking of presidential candidates.

3 Data

3.1 Mesa-Level Data

Given we were tracking the election in real-time, no public *mesa*-level (voting tables) dataset was available for the Peruvian 2026 first round elections. We thus built our own dataset by leveraging the ONPE’s API backend service, which serves their “Resultado Electoral” website, and systematically scraped the results at the *mesa*-level from April 12th until May 10th as they were made public. We were able to gather key variables of interest, namely the *mesa* ID, *ubicación geográfica* (ubigeo) ID, the *distrito* (district) of the *mesa*, its *provincia* (province), in addition to the number of votes cast and the registered eligible number of voters. Overall, we scraped the near-universe of all results as they were being counted and uploaded in real-time. See Table 1 for summary statistics.

The meta-data for the underlying “Resultado Electoral” website via the ONPE API does not provide ready-to-scrape information on the installation times of *mesas*. Thus, to determine whether a *mesa* opened late, we downloaded directly from the website a PDF scan of each individual *acta* and used a custom multistage optimal-character recognition (OCR) pipeline to extract its opening time, captured in the phrase “Siendo las [insert time] del 12 de abril de 2026 ...”. Although we are able to download all of the PDFs, thus totaling essentially 92,000 PDFs across all of Peru’s polling stations (see Table 1), we perform the OCR exercise only on the 29,000 *mesas* in Lima.⁶

We pre-process Lima’s *actas* by first distinguishing between digitally generated records with machine-readable text and photographic scans of handwritten forms (Appendix A.2 provides examples of the *actas* PDFs that we processed). We do so by attempting embedded text extraction and applying a character-count threshold: digital *actas* produce structured text, while scanned forms typically return empty extractions. For digital *actas*, we recover opening times using a multi-pattern regex parser and correct implausible entries.⁷

6. This choice is partly driven by the computational demands of the OCR, but also given our sample of interest is Lima itself, as only delayed *mesas* were located in the capital.

7. 102 digital *actas* report opening times before 6:00 AM, more than an hour before the ONPE’s designated opening time. We find this reflects a data-entry artifact: the STAE system hardcoded AM for all entries, so a poll worker opening a *mesa* at 2:30 PM would enter 2:30, which the system recorded as 2:30 AM.” We therefore add 12 hours to any recorded time before 6:00 AM.

For handwritten *actas* that were scanned, we use a multi-system consensus protocol in which Apple Vision OCR and Gemini 2.5 Pro independently extract the installation time from each scanned PDF. The PDF is rendered at 2.5× resolution, rotated if in portrait orientation, and cropped to the upper-left quadrant of the page (55% width × 35% height), isolating Section A (Acta de Instalación) while discarding the vote-tallying section. The resulting JPEG is then passed to Gemini with a structured prompt instructing it to return only a time in H:MM AM/PM format, or NO TIME / ILLEGIBLE if the field is blank or unreadable. All scanned *actas* are parsed at least twice by different models; times in which both systems agree are provisionally labeled ‘verified’, and disagreements, indicative of hard to parse *actas*, are re-submitted on 4x resolution to another Gemini SOTA model for a third pass through the Google AI Vertex platform.

Finally, results were verified manually at post-processing. Our 2026 dataset identified 87,938 *mesas* available through the ONPE API and processed 87,178 distinct PDF *actas*, covering 99.91% of all *mesas* nationwide. In sum, our dataset covers 94.06% of the *actas* universe, and 96.33% of all *mesas* in Lima (see Appendix A.4 for choropleth map visualizing the distribution of opening times and turnout rates across Lima).

Table 1: Coverage of the 2026 First-Round Presidential Election Dataset

Geography	Actas	Acta PDF	Eligible Voters	Ballots Cast	Turnout
Total (Peru + Extranjero)	92,624 / 92,765 (99.85%)	92,603 (99.83%)	27,284,989	20,138,880	73.81%
Peru (in-country)	87,938 / 88,062 (99.86%)	87,900 (99.82%)	26,179,203	19,305,002	73.74%
Lima (department)	29,229 / 29,247 (99.94%)	29,179 (99.77%)	8,645,716	6,904,568	79.86%

Notes: figures are official ONPE counts of *mesas* for the first-round 2026 presidential election (denominators in the “Actas” column and reference for the “Acta PDF” coverage). “Acta PDF” indicates the count and share of *actas* for which we successfully retrieved the scanned (i.e. handwritten) or digital *acta* record.

3.2 Voting Site-Level Data

To enable comparisons across past electoral cycles, we also collect data at the *mesa*-level from three preceding first-round general elections (2011, 2016, and 2021), the earliest cycles available in ONPE’s public historical archive. Because we cannot track *mesas* accurately over time, our second best effort is to aggregate *mesas* to their voting site based on site names. Of all voting sites observed in 2011, we are able to track almost 90% of them over time. See Appendix A.3 for summary statistics.

3.3 *Distrito-Level Data*

Albeit not core to the analysis, we also collect data on local gross domestic product (GDP) at the *distrito* level as a proxy for socioeconomic development. We gather this from the dataset developed by [Chen et al. \(2022\)](#), compiled via satellites. Given this dataset is only available up until 2019, we then use within-*distrito* compound annual growth rates to impute post-2019 values up until 2026. Including measures of GDP allows us to not only control substantively for the impact of local development which could co-determine the opening times of voting sites and levels of turnout, but also allows us to benchmark our “time elasticity” to an “income elasticity” of turnout. This also helps address the concern raised by a recent [Jurado Nacional de Elecciones \(2026\)](#) report (*N° 000073-2026-WTG-JEELIMASUR1-EG2026/JNE*) that absenteeism is difficult to separate from a multitude of other factors beyond the opening time of voting sites, especially socioeconomic characteristics.⁸

4 Empirical Strategy

We rely on two kinds of variation available in the data, albeit at different levels of analysis. Our core estimand of interest is the “time elasticity” with respect to turnout, i.e. how does a change in polling station opening time affect subsequent voter turnout. Intuitively, this captures the extent to which delays in the voting process impose participation costs on citizens, thereby depressing electoral participation.

4.1 Cross-Sectional Variation of *Mesas*

First, at the *mesa* level, we exploit cross-sectional variation in the time of opening and turnout rates. Our identifying variation thus compares differences in the opening times of *mesas* across Lima and whether this correlates with differential levels of turnout.

Ordinary Least Squares Our baseline specification is a standard linear model using OLS:

$$\tau_{msdp} = \alpha_d + \beta h_{msdp} + \gamma \mathbf{X}_{msdp} + \varepsilon_{msdp} \quad (1)$$

Here, τ_{msdp} refers to the turnout rate, based on the number of individuals who voted divided by total registered voters, for a given *mesa* m of voting site s in *distrito* d of *provincia* p . Given the lack of control variables available at the *mesa* level, we always include *distrito* fixed effects, α_d , which helps to absorb any time-invariant and/or *distrito*-level factors common to all *mesas*, and also permits sharper “apples-to-apples” comparisons by contrasting *mesas* within the same *distrito*. In

8. As quoted directly from the [Jurado Nacional de Elecciones \(2026\)](#) report: “Toda vez que el nivel de ausentismo electoral responde a una pluralidad de factores – entre ellos, las condiciones socioeconómicas de los electores, el grado de motivación o desafección política, las dificultades de transporte, la distancia hacia los locales de votación, así como otros elementos contextuales propios de cada circunscripción –, no resulta jurídicamente válido atribuirlo, de manera exclusiva o determinante, a la hora de instalación de las mesas de sufragio” (pg. 8).

Appendix C.11, we drop *distrito* fixed effects to include *distrito*-level GDP as a covariate in \mathbf{X}_{msdp} (although continue to include *provincia* fixed effects to go some way in absorbing unobservables albeit at a more aggregate geographic unit).

The estimand of interest is β which thus captures the treatment effect of a *mesa* opening up late. We define the core treatment variable of h_{msdp} in various ways. First, we denote a *mesa* as a “late-opener” based on the reported hour of opening from the *actas*, varying this from 10am+ to 2pm+. Next, we also define a *mesa* as having opened late based on a [Jurado Nacional de Elecciones \(2026\)](#) report (*N° 000073-2026-WTG-JEELIMASUR1-EG2026/JNE*), which outlines those *mesas* that reported having opened before 2pm but in actuality, according to election observers, opened after 2pm (we explore this further in section 7).⁹ We also then study only those *mesas* that had to vote on Monday, given they never opened on Sunday. Finally, we take the logarithm of the opening time among Sunday-openers to fully exploit intensive margin variation.

To semi-parametrically evaluate the time-elasticity over the range of opening hours, we also generate treatment “bins” per hour of opening, using *mesas* that opened between 7-7.59am as the base category:

$$\tau_{msdp} = \alpha_d + \sum_{k \in \{8, \dots, 14+\}} \beta_k \mathbf{1}[h_{msdp} \in k] + \gamma \mathbf{X}_{msdp} + \varepsilon_{msdp} \quad (2)$$

The last bin of interest, $k = 14+$, thus combines all *mesas* that opened between 2pm to 5pm, which would include all those identified by the [Jurado Nacional de Elecciones \(2026\)](#) report.¹⁰

Fractional Logit One concern with using OLS with turnout rate as a bounded outcome, $\tau_{msdp} \in [0, 1]$, concerns the functional form assumption of linearity. Given turnout is also a fraction, we thus also exploit the fractional response logit model developed by [Papke and Wooldridge \(1996\)](#):

$$\mathbb{E}[\tau_{msdp} \mid h_{msdp}, \mathbf{X}_{msdp}, \alpha_d] = \Lambda(\alpha_d + \beta h_{msdp} + \gamma \mathbf{X}_{msdp}) \quad (3)$$

The function $\Lambda(\cdot)$ represents the logistic cumulative distribution function, ensuring fitted values remain within the unit interval. Unlike OLS, the fractional response model does not impose constant marginal effects and explicitly accounts for the bounded nature of turnout rates. We estimate the model via quasi-maximum likelihood and report average marginal effects throughout for ease of interpretation. We similarly run a flexible specification as per equation (2) to estimate the time-elasticity semi-parametrically:

$$\mathbb{E}[\tau_{msdp} \mid h_{msdp}, \mathbf{X}_{msdp}, \alpha_d] = \Lambda\left(\alpha_d + \sum_{k \in \{8, \dots, 14+\}} \beta_k \mathbf{1}[h_{msdp} \in k] + \gamma \mathbf{X}_{msdp}\right) \quad (4)$$

9. Note that given it was confirmed that these *mesas* definitely opened after 2pm, we are able to include them in our 10am+ to 2pm+ late-opening definitions as well.

10. Throughout the analysis, for all delayed polling stations that still opened on Sunday, we cap the installation time at 5pm.

4.2 Within-Variation of Voting Sites

As an alternative source of variation, we further probe the impact of late openings by studying turnout rates over time. One caveat is that we can only pursue such temporal variation at the voting site level, and hence must aggregate *mesas* to this unit. Notwithstanding this constraint, we are able to gather data at the voting site level as far back as the 2011 election, giving us four time periods to evaluate turnout rates before the 2026 late-opening shock. Rather than comparing treated and untreated *mesas* within a single election, this panel approach estimates whether turnout at sites experiencing abnormal delays in 2026 declined relative to their own historical turnout patterns and relative to never-treated sites.

Two-Way Fixed Effects We first run a standard two-way fixed effects (TWFE) difference-in-differences specification:

$$\tau_{sdpt} = \alpha_s + \delta_{dt} + \beta h_{sdpt} + \gamma \mathbf{X}_{sdpt} + \varepsilon_{sdpt} \quad (5)$$

To calculate site-level turnout, we thus sum total votes cast across *mesas* and then divide by the sum of eligible voters.¹¹ α_s now refers to a site-level fixed effect, so we are thus capturing changes *within* voting sites pre-post the late-opening shock in the 2026 election.

The core identifying assumption for estimating β in equation (5) is *strict exogeneity*, which implies that ε_{sdpt} cannot be correlated with past, future or current regressors, and so there can be no feedback effects (i.e. previous turnout rates affecting late opening), anticipation effects (i.e. late opening in 2026 impacts prior turnout rates) or time-varying confounders. Strict exogeneity also implies the standard parallel trends assumption, such that the untreated potential outcomes of all voting sites have parallel trajectories in levels of turnout. We can comfortably rule out anticipation effects, as it is unlikely voters across hundreds to thousands of *mesas* expected a late-opening, especially given these delays did not occur in prior elections.

By subsetting our specifications to using just voting sites in Lima, which is where the disputed *mesas* are concentrated, we are also, by construction, alleviating any concerns related to potential urban-rural unobservables that could co-determine turnout and late-openings. Nonetheless, to further allay potential confounding factors attributable to unobservables, we also include *distrito* \times year fixed effects, δ_{dt} , which absorbs any time-varying characteristics of *distritos*.

\mathbf{X}_{sdpt} refers to any time-varying control variables at the site level. Across specifications, we always includes the number of *mesas* associated with sites to account for the “size” of voting sites, as this can also change over time. Finally, ε_{sdpt} refers to an idiosyncratic error term, where we cluster standard errors at the site-level as the unit of treatment assignment.

Event Study Albeit untestable, to more transparently probe the presence of any pre-trends as

11. Results are also robust to simply taking an average of turnout rates across *mesas* within voting sites. See Appendix C.1.

an immediate violation of the parallel trends assumption, we also run a dynamic difference-in-differences “event study”:

$$\tau_{sdpt} = \alpha_s + \delta_{dt} + \sum_{k \neq 2021} \beta_k \mathbf{1}[h_{sdpt} = k] + \gamma \mathbf{X}_{sdpt} + \varepsilon_{sdpt}, \quad (6)$$

for $k \in \{2011, 2016, 2021, 2026\}$. The specification above now permits treatment effect heterogeneity over time relative to the “static” estimate of β in equation (5). Given we do not have variation in treatment cohorts, with 2026 as the only year of treatment adoption, we use the 2021 election as the natural pre-treatment “base” for comparison.

5 Core Results

5.1 Cross-Sectional Variation of Mesas

Table 2 outlines the core results using OLS and fractional logit models. The core result is in column 1, which defines a *mesa* as having opened late if its installation time was reported after 10am on Sunday. Our point estimate in panel A suggests that, in expectation, these *mesas* saw decreases in turnout by approximately 3 percentage points relative to the control group of *mesas* that were opened before 10am. This result is almost identical when using a fractional logit (−2.9 percentage points), thus allaying concerns related to functional form assumptions.

Columns 2-5 then vary the threshold hour for defining a late opening, where column 2 denotes treatment if a *mesa* opened after 11am, column 3 after 12pm, and so on through column 5, which uses a 2pm threshold. We consistently estimate statistically significant negative effects across all definitions of opening late, with the magnitude of the estimates increasing as the threshold becomes more stringent, from -3 percentage points at the 10am threshold (column 1) to -3.4 percentage points at the 2pm threshold (column 5). Albeit a non-monotonic pattern across columns, this is consistent with the intuition that later openings impose greater costs on voters and therefore lead to larger reductions in turnout. In column 6, where we only use the JNE-flagged late opening *mesas* (after 2pm), we also estimate a 5 percentage point drop in turnout.

Column 7 then focuses on those *mesas* that never opened on Sunday and hence voting occurred on Monday. Interestingly, we also estimate a negative impact on turnout of approximately 5 percentage points across both panels A and B, which could suggest either voter disaffection from the disruptions the day before and/or an inability of voters to turnout on a work day. Nonetheless, these results are only marginally statistically significant.

Finally, column 8 provides a direct estimate of the time (semi-)elasticity with respect to turnout, exploiting full intensive margin variation over installation times. Using OLS, the point estimate suggests that the average difference in turnout between a *mesa* opening at 7am versus 12pm reflects a predicted 4 percentage point decrease in turnout. This effect is almost identical to the

average marginal effect estimated using the fractional logit model in panel B.

Table 2: Effect of Late Opening on Turnout (*Mesa*-Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: OLS</i>								
	Dependent variable is Turnout Rate (<i>Mesa</i> -Level)							
Late Opening	-0.030*** (0.007)	-0.029*** (0.008)	-0.016** (0.008)	-0.023** (0.009)	-0.034*** (0.011)	-0.044*** (0.016)	-0.053* (0.030)	
ln(Opening Hour)								-0.076*** (0.012)
Observations	28,796	28,796	28,796	28,796	28,796	28,796	26,217	28,661
<i>Panel B: Fractional Logit</i>								
Late Opening	-0.029*** (0.006)	-0.028*** (0.007)	-0.016** (0.007)	-0.022*** (0.009)	-0.032*** (0.010)	-0.041*** (0.014)	-0.052* (0.027)	
ln(Opening Hour)								-0.074*** (0.012)
Observations	28,800	28,800	28,800	28,800	28,800	28,800	26,221	28,665
<i>Distrito</i> Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday	Non-JNE
Treated <i>Mesas</i>	2,750	1,572	657	423	233	135	171	–

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by voting site in parentheses. Treated sample refers to the definition of the “delayed” treatment variable. JNE refers to *mesas* observed opening after 2pm by the [Jurado Nacional de Elecciones \(2026\)](#) report. Panel A reports OLS estimates. Panel B reports average marginal effects from fractional logit models. Columns 1-5 always include the JNE-flagged *mesas* that definitively opened after 2pm. Column 7 only uses *mesas* that opened before 10am on Sunday as the control group. Column 8 drops all *mesas* flagged in the [Jurado Nacional de Elecciones \(2026\)](#) report given installation times cannot be confirmed before 2pm.

The time (semi-)elasticity estimated in column 7 assumes a concave relationship between a *mesa*’s opening time and turnout. Figure 1 now plots estimates after employing the semi-parametric approach from equations (2) and (4) to go some way in relaxing this model assumption. Although the relationship is not monotonic across the range of opening hours, there is a clear decline in turnout relative to a *mesa* having opened up “on time” between 7-7.59am. *Mesas* that opened between 10-10.59am see a statistically significant drop in turnout by approximately 3.5 percentage points relative to the average turnout rate of *mesas* that opened between 7-7.59am.

Between 12-1.59pm, there is a clear resurgence in turnout, which potentially suggests an “*almuerzo* effect”; an opportunity in the day for individuals to return to voting sites. Clearly the point estimates show turnout rates increasing and the confidence intervals cross zero, thus suggesting no statistically significant difference with the average turnout rates of *mesas* that opened between 7-7.59am. However, when we look at those sites that opened after 1pm, the decline in turnout emerges. Relative to the 7-7.59am opening *mesas*, these very late openers see a decline in turnout by approximately 2.5-5 percentage points, respectively, and both the 1pm and 2pm+ point estimates are statistically significant to the 5% level.

A potential concern relates to the “size” of *mesas*: turnout may be mechanically lower in *mesas* with

a larger number of registered voters. In the Peruvian context, at least for the *mesas* in Lima that we focus on in 2026, almost all *mesas* have a constant number of registered voters around 300 individuals, with a standard deviation of 14 voters. This allows us to comfortably rule out potential size effects on declining turnout. Moreover, because voters cannot be reassigned to another *mesa* or voting site when delays occur, we can largely rule out spillover effects onto neighboring *mesas*, particularly those within the same voting site, that might otherwise affect turnout rates.

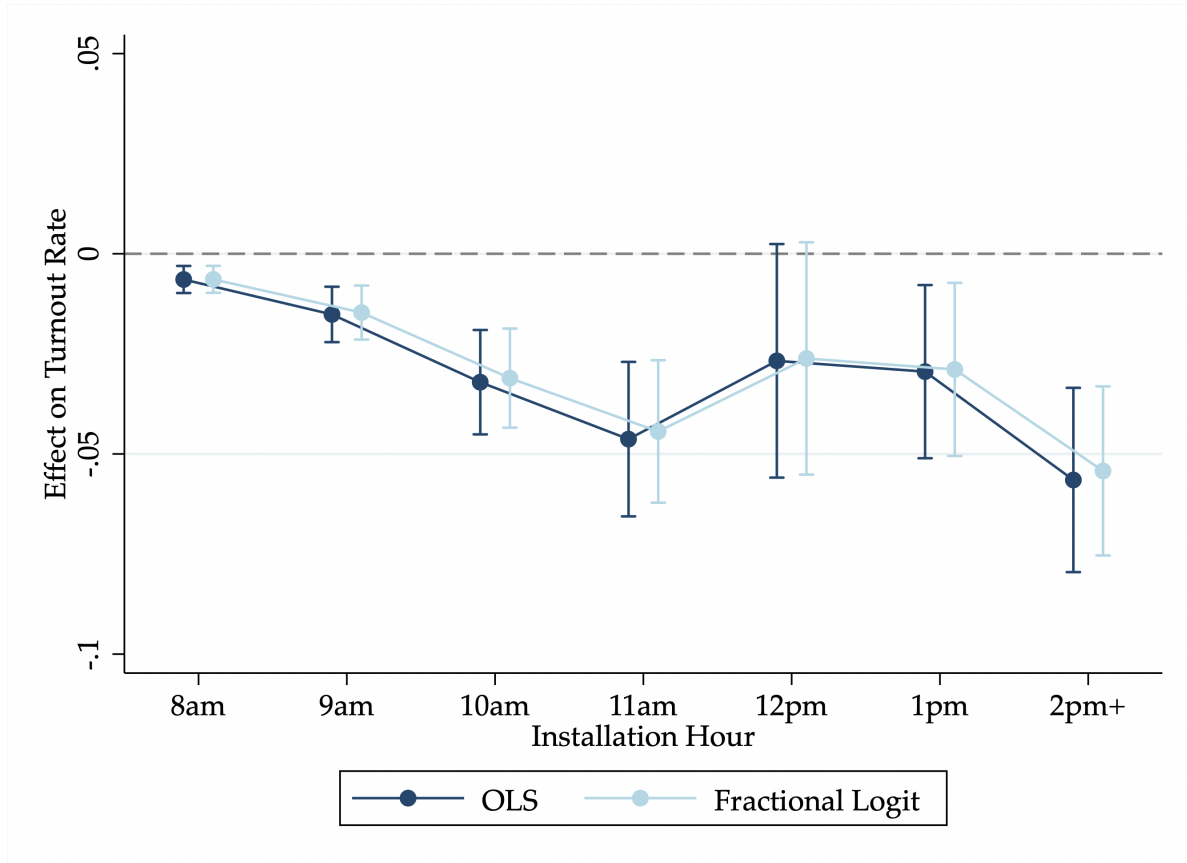


Figure 1: Binned Estimates of *Mesa* Opening Hour on Turnout

Note: errors bars represent 95% confidence intervals, with robust standard errors clustered at the voting site level. Dark blue dots refer to OLS estimates. Light blue dots refer to average marginal effects estimated using fractional logit. Both specifications include *distrito* fixed effects. Base category are all *mesas* that opened between 7-7.59am. The *x*-axis refers to an interval of time, e.g. 8am uses all *mesas* that opened between 8-8.59am, 9am uses all *mesas* that opened between 9-9.59am, etc. Only the 2pm+ bin includes those *mesas* that reported opening late from official *actas* in addition to the late-opening *mesas* flagged by the [Jurado Nacional de Elecciones \(2026\)](#). The upper bound on hours for this bucket is 5pm.

5.2 Within-Variation at the Voting Site Level

Table 3 outlines the core results now exploiting within-voting site variation. Panel A uses the fraction of *mesas* within a voting site that opened after given hours. Panel B focuses on extensive margin variation where we define a voting site as treated if *any mesa* opened late based on a

given hourly threshold. Finally, panel C defines treatment at the site-level if at least 50% of its *mesas* opened late according to a hourly threshold of treatment assignment. Using this alternative source of variation relative to the cross-sectional design in the previous section, we find consistent results with Table 2.

Table 3: Effect of Late Opening on Turnout (Site-Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable is Turnout Rate (Site-Level)							
<i>Panel A: Fraction of Mesas</i>							
Late Opening	-0.030*** (0.008)	-0.028*** (0.010)	-0.028** (0.012)	-0.046*** (0.012)	-0.065*** (0.015)	-0.065*** (0.017)	-0.069*** (0.020)
Observations	5,391	5,391	5,391	5,391	5,391	5,391	4,582
Treated Sites	270	177	87	52	33	11	12
<i>Panel B: Any Late Mesa</i>							
Late Opening	-0.030*** (0.005)	-0.024*** (0.006)	-0.018*** (0.007)	-0.023*** (0.008)	-0.037*** (0.010)	-0.057*** (0.016)	-0.069*** (0.020)
Observations	5,391	5,391	5,391	5,391	5,391	5,391	4,582
Treated Sites	270	177	87	52	33	11	12
<i>Panel C: At Least 50% of Mesas Late</i>							
Late Opening	-0.024*** (0.007)	-0.020** (0.008)	-0.027** (0.012)	-0.044*** (0.011)	-0.058*** (0.015)	-0.065*** (0.017)	-0.069*** (0.020)
Observations	5,391	5,391	5,391	5,391	5,391	5,391	4,582
Treated Sites	132	76	36	26	13	10	12
Voting Site FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Distrito</i> × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by voting site in parentheses. All specifications include site and *distrito* × year fixed effects and control for the number of *mesas* within each site. Panel A uses the continuous share of late-opening *mesas* within a site. Panel B defines treatment as any late-opening *mesa* within the site. Panel C defines treatment as at least 50% of *mesas* opening late. Note results across panels for column 7 are identical as there was no variation across *mesas* within voting sites that opened on Sunday (i.e. all *mesas* opened on Sunday for these sites, and so by definition at least 50% of *mesas* at these sites opened on Monday).

The core specification in column 1 continues to show a negative impact on turnout among those *mesas* that opened after 10am on Sunday. Panel A, which better reflects treatment “intensity”, suggests that, if all *mesas* within a voting site opened after 10am, then turnout declined by 3 percentage points in expectation. Across columns 2-5, we find consistent evidence of negative effects across all hourly thresholds, with the magnitudes varying as the threshold becomes more stringent, from -3 percentage points at the 10am threshold to -6.5 percentage points at the 2pm threshold. This pattern is again very consistent with our cross-sectional estimates in Table 2 and reinforces the intuition that later openings impose progressively greater costs on turnout, albeit non-monotonically. Columns 5 and 6 reveal the largest effect sizes among Sunday-openers: panel A suggests that voting sites opening after 2pm on Sunday experienced a very sizable 6.5 percentage point decline in turnout, a magnitude that coincides exactly with the estimate in column 6 using the definitive JNE-flagged *mesas*.

In column 7, which now focuses on those voting sites that opened on Monday, we continue to find statistically significant evidence of a drop in turnout. Again, while hard to determine the specific channel, whether this was driven by voter disaffection resulting from the Sunday disruptions or voters were more heavily impeded in participating on a work day, the estimated drop in turnout is again sizable at 6.9 percentage points.

To exploit extensive margin variation in running the event study specification in equation (6), this necessitates choosing the appropriate fraction of *mesas* that opened late within voting sites to assign treatment at the site-level. Rather than use arbitrary cut-offs, we pursue a grid search by iteratively re-running core specifications of interest, defining treatment iteratively by increasing the fraction of treated *mesas* within sites in increments of 0.05. Using the minimum residual sum of squares (RSS) across models, alongside the adjusted R^2 as an additional diagnostic, our results suggest that a threshold of 0 provides reasonable model fit, i.e. defining an entire voting site as treated if *any mesa* opened after a given hourly threshold (see Appendix B.1).¹²

Figure 2 outlines the results, focusing on all *mesas* that opened after 10am as our core treatment variable of interest, in addition to those on the JNE list which opened after 2pm. Panels (a) and (c) clearly show a relative decline in turnout levels, totaling approximately 2.5 to 5 percentage points. Although we are limited to having only two pre-treatment periods, we see no evidence of pre-trends such that late-opening *mesas* were differentially trending in levels of turnout in prior elections.

To better interpret the results, panels (b) and (d) plot average turnout rates across voting sites over time between the ever-treated group of late-opening *mesas* in 2026, based on respective definition, and the control group of those *mesas* that opened before the applied hourly threshold. An interesting pattern emerges: corresponding with the results in panels (a) and (c), turnout rates trended relatively closely before the 2026 election, especially in panel (b), but those that opened late saw a decline whereas earlier-opening *mesas* experienced an *increase* in turnout. The implied counterfactual thus suggests that, in the absence of the late-opening shock, turnout rates among impacted *mesas* may have been close to 80 percent as opposed to 75 percent.

In Appendix C.2, we also run the event study for those voting sites where all *mesas* opened on Monday. As shown, unlike those that were delayed but still opened on Sunday, we see strong pre-trends, such that turnout was already substantially lower in treated sites prior to the 2026 election. Relative to 2021, turnout in these sites was between roughly 4 and 8 percentage points lower in both 2011 and 2016, respectively, with confidence intervals excluding zero for 2011 and marginally overlapping zero for 2016. This suggests that sites whose *mesas* only opened on Monday were systematically different from control sites even before the logistical disruptions of 2026,

12. Our preference for a threshold of zero should not be interpreted as evidence that all delayed *mesas* generate identical causal effects within and across voting sites. Rather, the grid search suggests that extensive-margin disruption, i.e. whether a site experienced any meaningful delay at all, better captures the variation in site-level turnout versus stricter intensity-based definitions, which may discard substantial variation by only classifying heavily affected sites as treated.

likely reflecting persistent administrative or unobservable disadvantages rather than a plausibly exogenous delay. The estimated effects for Monday-opening *mesas* and voting sites in Tables 2 and 3 are thus, at best, suggestive evidence of a turnout decline as a result of logistical errors and delays.

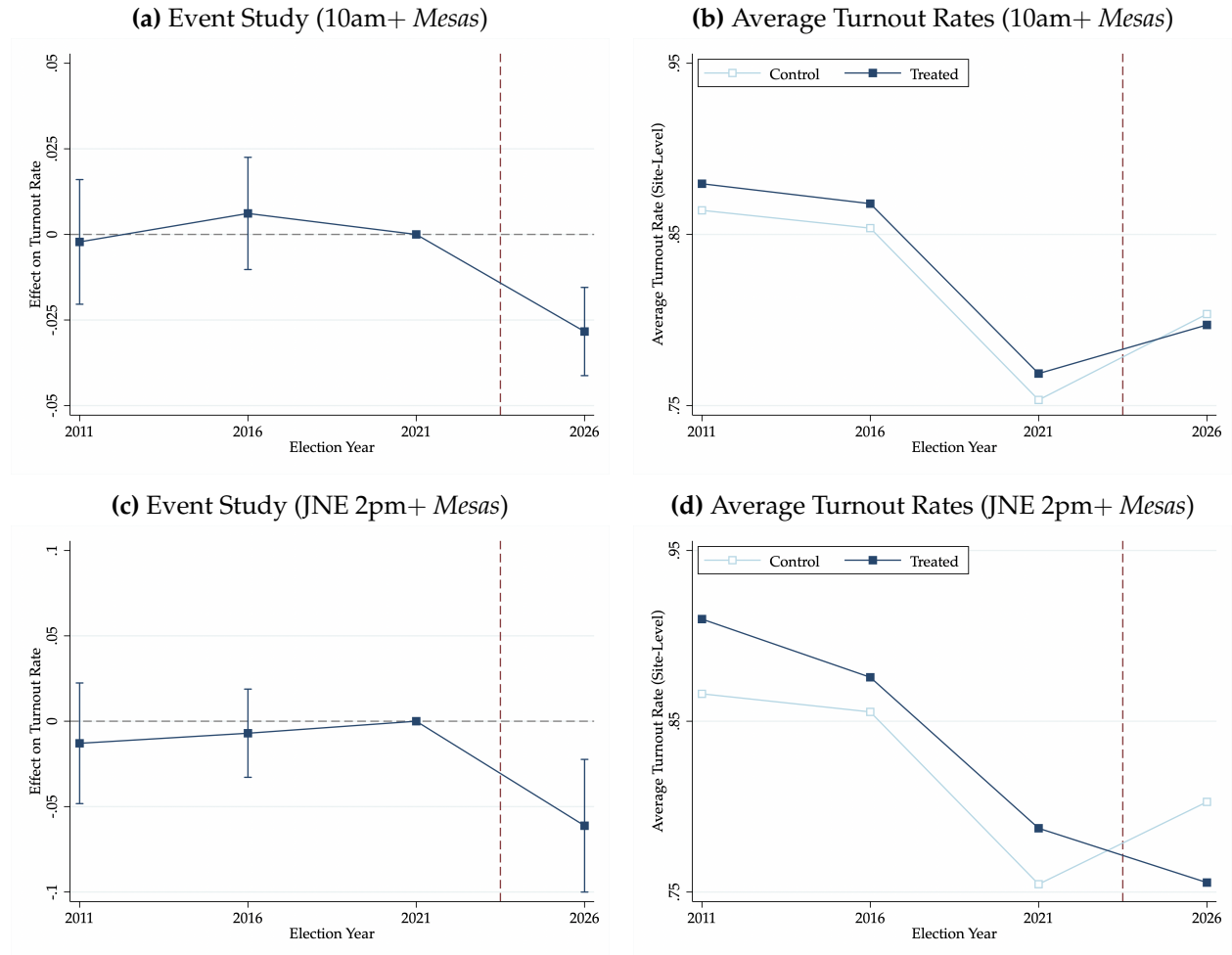


Figure 2: Event Study Results and Mean Turnout

Note: error bars represent 95% confidence intervals, with standard errors clustered at the voting site level. Panels (a) and (c) plot the estimates performing our event study from equation (6), including voting site and *distrito* × year fixed effects. Panels (b) and (d) plot the average turnout rates across sites per election based on treatment status. Panels (a) and (b) use *mesas* that opened after 10am (i.e. column 1 of panel B in Table 3). Panels (c) and (d) only use *mesas* that were listed by the [Jurado Nacional de Elecciones \(2026\)](#) report as having opened after 2pm. Dark blue line refers to the ever-treated group. Light blue line refers to control (i.e. “never-treated”) group. The base year for the event study is 2021. Vertical dashed red line conveys that 2026 is the year of treatment onset.

5.3 Robustness Checks and Extensions

In the Appendix, we document the robustness of the results against a battery of alternative specifications and sample splits. These are outlined in further detail below.

Fujimori Placebo Test One of the core arguments made by the López Aliaga camp suggests that voting sites affected by the logistical disruptions disproportionately impacted his core electoral base in Lima. Although we cannot directly evaluate this claim, it motivates a placebo exercise using Keiko Fujimori’s vote shares. Because Fujimori ran in every presidential election covered by our panel dataset, her vote share provides a stable benchmark for examining whether the areas exposed to delayed openings were systematically different even prior to 2026. If our estimates were primarily capturing persistent political or demographic characteristics of voting sites, rather than the effects of delayed openings themselves, we would expect to observe similar relationships using Fujimori’s historical vote shares. We do not find consistent evidence of such patterns. See Appendix C.3.

Blank Votes Placebo Test As an additional placebo, we test whether late openings affect the share of blank ballots among votes cast. The proposed mechanism of delays should operate through whether voters can cast a ballot, not necessarily through *how* ballots are marked. Thus, a null effect would help rule out that our main results reflect broader differences in local political composition. We acknowledge this is an imperfect placebo: frustrated voters could cast blank ballots in protest of the delay, and compositional shifts in who turns out could mechanically move the blank share. But in practice we find precisely estimated null effects, suggesting that any such channels are negligible. See Appendix C.4.

Provincia Fixed Effects We also experiment with *provincia* fixed effects as a more aggregate geographic unit. This helps to address any concerns related to over-fitting by running a less saturated specification, especially issues related to “incidental parameters” for the fractional logit models. For the site-level dynamic specifications, we also experiment with *provincia* × year fixed effects. See Appendix C.5.

Randomization Inference Given we have few treated units, both for our cross-sectional analysis at the *mesa* level and our panel dataset at the voting site level, we also pursue randomization inference to assess whether our estimated treatment effects could plausibly arise by chance alone. Fixing the proportion of treated units, we randomly assign treatment to *mesas* or voting sites, iteratively re-perform core specifications of interest, and then compare the distribution of placebo estimates to our observed coefficients. This allows us to evaluate the extent to which our findings are distinguishable from estimates generated under random assignment. See Appendix C.6.

Dropping Units To ensure the core results are not driven by specific ever-treated units, we sequentially drop all *mesas* within each *distrito* and iteratively repeat the core analysis. Cross-sectionally, we do this for our OLS and fractional logit models using our core treatment variable of *mesas* that opened after 10am on Sunday. For our panel analysis, we also pursue this exercise with our TWFE specification. See Appendix C.7.

Sample Robustness Although the results in Table 2 use all *mesas* in Lima for comparison, we also restrict the sample to only those *distritos* where voting sites were impacted by delays. This

effectively limits our control group to ever-treated *distritos*, where at least one voting site or *mesa* within a voting site was affected. As an additional check, given we have an unbalanced panel dataset at the voting site-level, we re-perform our panel analysis solely on voting sites we can observe at least three elections over time since 2011. See Appendix C.8.

Alternative Error Structures Given we have few clusters of treated units across both the cross-sectional *mesa* analysis and panel dataset exploiting within-voting site variation, we try alternative cluster arrangements for standard errors, clustering at the *distrito* level, and also pursue wild cluster bootstrapping at the voting site level (Cameron et al. 2008). Inference of core results remain essentially unchanged. See Appendix C.9.

de’Chaisemartin and D’Haultfœuille Estimator A growing literature has recognized the drawbacks of standard difference-in-differences estimators due to the presence of heterogeneous treatment effects (Goodman-Bacon 2021). Although we do not run into many of these issues given treatment only occurs in 2026 and so we only have a single treatment “cohort”, for additional robustness we also use the dynamic estimator proposed by Chaisemartin and D’Haultfœuille (2024). Results are identical. See Appendix C.10.

Income-Elasticity of Turnout as a Benchmark In the broader turnout literature, a 2.5-5 percentage point shift in turnout is considered substantively large, even if the share of affected *mesas* we study is relatively small. Yet, because evidence of the time-elasticity of turnout remains limited, it is difficult to benchmark our estimated effects against existing studies (with the exception of Potrafke and Roesel (2020)). We therefore compare our time-elasticity estimates to an “income elasticity” of turnout, since areas with higher local GDP may plausibly exhibit higher turnout rates due to being richer, more populous, denser, and plausibly closer to voting sites. We use district-level GDP estimates from Chen et al. (2022) and include logged GDP as an additional covariate alongside logged opening time and the treatment variables used in Table 2. The logarithm of GDP ultimately provides a benchmark for comparing the magnitude of the time-elasticity to local economic conditions. See Appendix C.11 for details.

6 How Many Missing Voters? And for Whom?

6.1 Estimating the Number of Missing Voters

Thus far we have interpreted the results with respect to percentage point changes in turnout rates across *mesas* that were delayed in opening. A key question thus concerns how many voters were effectively disenfranchised as a result of these delays across Lima? And to what extent would this impact the vote margins between the second and third-placed candidates, Roberto Sánchez and Rafael López Aliaga, respectively?

To interpret the magnitude of the results, we use estimates from our core specifications to perform back-of-the-envelope calculations of the number of fewer votes cast as a result of *mesas* opening

late. Using our OLS and TWFE estimates to provide a range of estimates, we compute

$$\widehat{\Delta \text{Votes}} = \widehat{\beta} \sum_{m=1}^M h_m N_m; \quad \widehat{\Delta \text{Votes}} = \widehat{\beta} \sum_{s=1}^S h_s N_s,$$

where the first expression corresponds to the *mesa*-level analysis (i.e. Table 2 results) and the second to the voting site-level analysis (i.e. Table 3 results).

At the *mesa* level, h_m denotes the late-opening treatment status of *mesa* m under a given treatment definition (suppressing the voting site, s , *distrito*, d , and *provincia*, p , notation from equation (1)), N_m is the number of registered voters assigned to that *mesa*, and $\widehat{\beta}$ is the estimated treatment effect from the corresponding cross-sectional specification. Intuitively, this calculation scales the estimated turnout effect by the number of registered voters directly exposed to delayed openings at the *mesa* level. Regarding h_m and corresponding effects on turnout, we focus on treatment effects for i) *mesas* that opened after 10am, ii) those that were confirmed as having opened after 2pm by the JNE, and iii) *mesas* that opened on Monday. To calculate an overall estimate of foregone votes following the logistical disruptions, we combine the estimates from the post-10am opening *mesas* on Sunday and the Monday-opening *mesas*.

For our voting site-level analysis, we instead aggregate estimates to the site-year level. Here, h_s denotes whether voting site s was exposed to delayed openings under a given treatment definition, N_s is the total number of registered voters assigned to the site (i.e. summing all registered voters across *mesas* within the same site), and $\widehat{\beta}$ is obtained from our TWFE specifications in Table 3. For consistency with our *mesa*-level estimates, we similarly focus on voting sites that contained *mesas* opening after 10am on Sunday, voting sites where the JNE-flagged *mesas* being installed after 2pm, and those voting sites that opened on Monday. We also combine the estimates of the post-10am opening *mesas* and the Monday-opening *mesas* to calculate an aggregate estimate of foregone votes. Given we aggregate from *mesas* to voting sites, we provide estimates of foregone votes primarily using intensive margin variation (i.e. panel A in Table 3, the proportion of *mesas* within a site that were delayed) to more accurately reflect the “intensity” of delays within voting sites.¹³

Finally, to obtain upper and lower bounds on these estimates, we construct uncertainty intervals using the delta method. For the *mesa*-level individual estimates, the associated standard error is:

$$\text{SE}(\widehat{\Delta \text{Votes}}) = \text{SE}(\widehat{\beta}) \left(\sum_{m=1}^M h_m N_m \right).$$

13. For completeness and transparency, we also document estimates using extensive margin variation (i.e. panel B in Table 3), where the whole site is considered delayed if even a single *mesa* opened late.

Analogously, for the voting site-level calculations, we estimate:

$$SE(\widehat{\Delta\text{Votes}}) = SE(\widehat{\beta}) \left(\sum_{s=1}^S h_s N_s \right).$$

These expressions thus propagate uncertainty from the estimated treatment coefficients from Tables 2 and 3 into the aggregate estimate of foregone votes. Under the assumption that the scaling term is fixed, the variance of the aggregate effect is simply the variance of the treatment estimate multiplied by the squared number of exposed registered voters.

For the combined post-10am on Sunday and Monday-opening *mesas* turnout estimates, we sum the two implied turnout effects and compute uncertainty using a site-cluster bootstrap. This reruns the constituent estimations within each bootstrap sample, thus preserving the covariance between the two effects when constructing confidence intervals for the combined effect. See Appendix D.1 for further explanation.

Table 4: Estimates of Missing Voters from Delayed Openings

	(1)	(2)	(3)	(4)
	$\widehat{\beta}$	Exposure-Weighted Registered Voters	$\widehat{\Delta\text{Votes}}$	95% Confidence Interval for $\widehat{\Delta\text{Votes}}$
<i>Panel A: OLS (Dummy)</i>				
10am+	-0.030	817,765	-24,161	[-34,609, -13,714]
2pm+ (JNE)	-0.044	40,010	-1,746	[-2,999, -493]
Voted Monday	-0.053	50,776	-2,710	[-5,689, 268]
10am+ and Voted Monday	-	868,541	-26,872	[-39,506, -16,774]
<i>Panel B: TWFE (Fraction)</i>				
10am+	-0.030	818,174	-24,329	[-37,724, -10,934]
2pm+ (JNE)	-0.065	40,608	-2,630	[-3,964, -1,297]
Voted Monday	-0.069	51,376	-3,566	[-5,591, -1,541]
10am+ and Voted Monday	-	869,550	-27,895	[-41,929, -12,855]

Note: exposure-weighted registered voters refers to $\sum_i h_i N_i$ in the estimation of missing voters, where h_i is treatment exposure and N_i is the number of registered voters. In Panel A, for *mesa*-level OLS estimates, i indexes *mesas*. In panel B, for site-level TWFE estimates, i indexes voting sites. Panel B reports TWFE estimates using the continuous fraction of *mesas* that opened late within a site (Voted Monday is included here as it is effectively a 100% fraction). Combined 10am+ and Voted Monday rows sum the predicted foregone votes cast from the two constituent estimates, with uncertainty computed using a site-cluster bootstrap. Uncertainty for individual-delay effects are calculated using the delta method.

Table 4 provides a range of predicted foregone votes as a result of the opening time delays at both the *mesa* level and the voting site level. We use core specifications from panel A of Table 2 and from Table 3. At the *mesa* level in panel A of Table 4, we estimate that, among those *mesas* that opened after 10am, impacting an estimated 817,765 voters, the delays decreased participation by 24,161 votes, with an upper bound of 34,609 and lower bound of 13,714 based on the estimated 95% confidence intervals. When we combine the effects of opening after 10am on Sunday and the

estimated turnout decline for those that voted on Monday, we only see a marginal change in the predicted turnout loss, increasing to 26,872 voters.

Panel B of Table 4 now uses our TWFE estimates from panel A of Table 3. Focusing on intensive margin variation by looking at the fraction of *mesas* within sites that opened after 10am+, thus capturing greater treatment heterogeneity and more accurately reflecting closed *mesas*, we estimate 24,329 foregone votes. Despite relying on a complementary and alternative source of identifying variation, this result is clearly consistent with our result in panel A of Table 4 at the *mesa*-level.

As a final exercise, to capture the impact of delays on turnout among late-opening Sunday *mesas* and voting sites alongside those that opened on Monday, we combine these two effects for an overall estimate. To estimate the general upper bound, we focus on *mesas* and voting sites that opened after 10am. In panel B, which uses TWFE at the voting site-level, we estimate that 27,895 total votes were foregone as a result of the overall delays across Sunday and Monday. When using our cross-sectional estimate in panel A where we analyze variation at the *mesa*-level, we estimate that 26,872 votes were foregone as a result of the delayed openings.

6.2 Estimating the Impact on the López Aliaga-Sánchez Margin

Additional to the estimates provided in Table 4, a major question resulting from the 2026 election concerns the *distribution* of missing voters: how did the delays impact the vote shares of candidates, especially the tightly contested second and third-placed candidates? To calculate the aggregate change in the vote margin between the third-placed López Aliaga and second-placed Sánchez attributable to late openings, we combine the estimated turnout effect with an estimate of the partisan composition of the marginal voters affected by the disruption. For the *mesa*-level specifications, we compute:

$$\widehat{\Delta\text{Margin}} = \widehat{\beta} \sum_{m=1}^M h_m N_m (\tilde{s}_{m,\text{López Aliaga}} - \tilde{s}_{m,\text{Sánchez}}),$$

where h_m denotes the late-opening treatment status of *mesa* m , N_m is the number of registered voters assigned to that *mesa*, and $\widehat{\beta}$ is the estimated treatment effect from the corresponding specification.

The sign of this estimate has a straightforward interpretation. Since the estimated turnout effect is negative, late openings reduce the number of votes cast in affected *mesas*. Whether this hurts López Aliaga or Sánchez depends on the partisan composition of the voters deterred from voting. If the affected *mesas* are located in areas where voters would have favored López Aliaga over Sánchez, then the missing voters are inferred to have been relatively more López Aliaga-leaning, and the disruption lowers López Aliaga's margin. Conversely, if affected *mesas* are located in relatively Sánchez-leaning areas, the same turnout loss would reduce Sánchez's margin and me-

chanically benefit López Aliaga. Thus, the aggregate estimate does not assume that all foregone voters supported one candidate; it weights each lost vote by the local López Aliaga-Sánchez margin.

Naturally, we cannot use the observed local López Aliaga-Sánchez margin in delayed *mesas*, as candidates' vote shares would plausibly be endogenous to the installation times. Thus, the term $\tilde{s}_{m,c}$ above represents a *counterfactual* estimate of the vote share for candidate c among voters who would have turned out absent the disruptions. Given limited observable characteristics at the *mesa* and voting site levels, matching-on-observables methods are difficult to pursue without imposing stronger modeling assumptions (Rosenbaum and Rubin 1983). We therefore use geography (i.e. “local matching”) as a transparent proxy for local political composition: nearby untreated *mesas*, especially within the same *distrito*, should provide a reasonably credible benchmark for the partisan preferences of voters at treated locations.¹⁴

To construct this benchmark, we use votes for López Aliaga and Sánchez across all untreated *mesas* in the same *distrito* as *mesa* m , excluding every *mesa* from m 's own voting site to avoid mechanically using outcomes from the same location and/or potential spillovers. Thus, $\tilde{s}_{m,c}$ is generally the candidate- c vote share among untreated *mesas* in the same *distrito*, excluding the treated site's voting location. If no such untreated *mesas* remain, we instead use untreated *mesas* from the nearest *distrito*, where distance is calculated using Euclidean distance between *distrito* centroids. This approach keeps the counterfactual benchmark local while ensuring that it is constructed only from voting units whose observed outcomes were not directly affected by the opening delays.

For site-level specifications, the analogous calculation is:

$$\widehat{\Delta\text{Margin}} = \hat{\beta} \sum_{s=1}^S h_s N_s (\tilde{s}_{s,\text{López Aliaga}} - \tilde{s}_{s,\text{Sánchez}}),$$

where h_s is the site-level treatment exposure, N_s is the number of registered voters assigned to site s , and $\tilde{s}_{s,c}$ is the leave-one-site-out local untreated vote share for candidate c . These calculations estimate how many votes the turnout effect would move in the López Aliaga-Sánchez margin by weighting the affected electorate by the local untreated partisan gap. Positive values of $\tilde{s}_{\text{López Aliaga}} - \tilde{s}_{\text{Sánchez}}$ indicate areas where the marginal lost turnout is expected to have favored López Aliaga relative to Sánchez, while negative values indicate the reverse.

We now construct uncertainty intervals using a site-cluster bootstrap. Unlike the individual turnout estimates in Table 4, where the exposure-weighted number of registered voters is kept fixed and uncertainty comes only from the estimated turnout effect, the López Aliaga-Sánchez margin estimates contain two sources of uncertainty: the estimated turnout effect *and* the counterfactual partisan composition of the marginal voters. In each bootstrap sample, b , we therefore

14. See Alipour and Lindlacher (2025) for a similar exercise on “local matching”.

re-estimate the relevant turnout effect and recompute the counterfactual vote-share benchmark, including the leave-one-site-out donor calculation and nearest-*distrito* fallback.¹⁵ For individual margin estimates, the confidence interval is thus based on the bootstrap distribution of

$$\widehat{\beta}^{(b)} \sum_i h_i^{(b)} N_i^{(b)} \left(\widehat{s}_{i,\text{López Aliaga}}^{(b)} - \widehat{s}_{i,\text{Sánchez}}^{(b)} \right),$$

where i denotes either *mesas*, m , or voting sites, s , depending on the specification of interest. For the combined estimates of ‘10am+ and Voted Monday’, we compute both component margin effects within the same bootstrap sample and use the bootstrap distribution of their sum. This approach allows the intervals to reflect uncertainty in both the treatment effects and the counterfactual partisan benchmark.

Table 5: Estimates of Missing López Aliaga-Sanchez Margin from Delayed Openings

	(1)	(2)	(3)
	$\widehat{\beta}$	$\widehat{\Delta\text{Margin}}$	95% Confidence Interval for $\widehat{\Delta\text{Margin}}$
<i>Panel A: OLS (Dummy)</i>			
10am+	-0.030	-5,055	[-8,042, -2,205]
2pm+ (JNE)	-0.044	-194	[-428, -24]
Voted Monday	-0.053	-413	[-956, 57]
10am+ and Voted Monday	-	-5,468	[-8,645, -2,866]
<i>Panel B: TWFE (Fraction)</i>			
10am+	-0.030	-5,088	[-9,468, -2,058]
2pm+ (JNE)	-0.065	-288	[-553, -57]
Voted Monday	-0.069	-603	[-1,194, -134]
10am+ and Voted Monday	-	-5,691	[-10,216, -2,399]

Note: $\widehat{\Delta\text{Margin}}$ estimates the change in the López Aliaga-Sanchez margin from delayed openings by combining the estimated turnout effect with the local counterfactual López Aliaga-Sánchez vote-share margin among untreated voting units. Panel B reports TWFE estimates using the continuous fraction of *mesas* that opened late within a site (Voted Monday is included here as it is effectively a 100% fraction). Combined 10am+ and Voted Monday rows sum the predicted margin effects from the two constituent estimates, with uncertainty computed using a site-cluster bootstrap.

Table 5 reports estimates of the foregone López Aliaga-Sánchez margin attributable to the opening time delays, computed at both the *mesa* and voting site levels. We draw on the same core specifications from panel A of Table 2 and from Table 3. In panel A of Table 5, applied at the *mesa* level, we find that for *mesas* opening after 10am, the disruption translates into a foregone margin of 5,055 votes, bounded by 95% confidence intervals of 2,205 and 8,042. Layering on the Voted Monday

15. Due to the computational demands of the bootstrapping, we repeat this exercise over 199 iterations.

channel alongside the 10am+ effect leaves this figure essentially unchanged.

Panel B of Table 5 draws on our TWFE estimates from Table 3. Allowing for intensive variation by exploiting the share of *mesas* within a site that opened after 10am, which again better captures heterogeneity in treatment intensity, we estimate a foregone margin of 5,088 votes.

Finally, we again combine the Sunday late-opening and Monday voting effects to gauge the overall implication of the delays for the López Aliaga-Sánchez vote margin. Concentrating on *mesas* and voting sites that opened after 10am, our preferred site-level specification in panel B, which exploits the share of *mesas* within a site that opened late, implies a combined foregone margin of 5,691 votes attributable to the disruptions across Sunday and Monday. Turning to the *mesa*-level cross-sectional estimate in panel A, the implied foregone margin from the delayed openings is 5,468 votes.

7 Misreporting of Late-Opening *Mesas*

As a final piece of analysis, we probe the possibility of irregular reporting of installation/opening times of *mesas* resulting from the logistical errors. According to the original 12pm deadline under Article 363 of the *Ley Orgánica de Elecciones*, *mesas* must be installed by noon in order for votes to be legally counted, otherwise they can be annulled. Yet due to the administrative errors that occurred on election day, the JNE extended this installation deadline to 2pm, established under the directive *Acuerdo del 12.04.2026*.

Although we do not run a regression discontinuity design, given we do not have a strict running threshold that defines treatment assignment, we are able to use the standard McCrary (2008) test to detect evidence of “sorting” or “bunching” around the 12pm and 2pm thresholds. If reported installation times were faithful, notwithstanding the logistical errors, this should imply no major discontinuity exists in the distribution of opening times at the 12pm and 2pm thresholds.

Figure 3 shows the results of the McCrary (2008) test, where we clearly see a sharp discontinuity in reporting times. Instead, both panels show pronounced discontinuities at the legal cutoffs, with large mass just before the initial 12pm installation deadline in panel (a), and another body of mass appearing just before or on the extended 2pm deadline in panel (b). As opposed to a smooth, continuous benchmark, this bunching just before the two deadlines is difficult to infer as genuine “just-in-time” installations of a non-trivial number of *mesas*. The fact this appears across *both* deadlines reinforces this interpretation, especially if *mesas* have incentives to misreport actual installation/opening times to avoid the annulment of votes.

That said, it is technically possible that several *mesas* for both the 12pm and 2pm thresholds opened just in time to avoid being annulled. No matter how implausible, the evidence in Figure 3 cannot definitely rule out this hypothesis. Confirming irregular reporting of installation times would require a “ground truth” assessment of when *mesas* *actually* opened relative to what

was recorded in the *actas* by *miembros de mesas*.

As final suggestive evidence, we thus leverage an internal [Jurado Nacional de Elecciones \(2026\)](#) report (N° 000073-2026-WTG-JEELIMASUR1-EG2026/JNE) that identifies, based on delivery way-bills, twelve voting sites where electoral materials were confirmed as having arrived after the 2pm extended legal cutoff for *mesa* installation. Since voting materials for all *mesas* at any voting site are supposed to be delivered simultaneously, all *mesas* across these sites can be confirmed as having opened after 2pm.

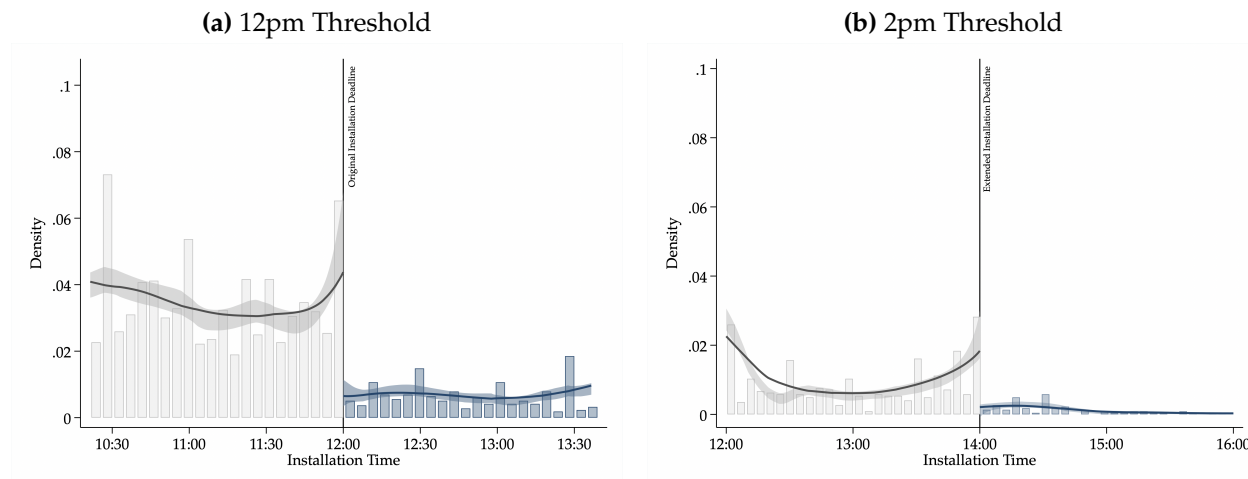


Figure 3: Results for [McCrary \(2008\)](#) Test on *Mesa* Installation Time

Note: panel (a) uses a threshold cut-off of 12pm. Panel (b) uses a threshold cut-off of 2pm. The unit of analysis is the *mesa*. Sample refers to all *mesas* in Lima where we have reported installation/opening times, including those *mesas* flagged by the [Jurado Nacional de Elecciones \(2026\)](#) that opened after 2pm. Sample excludes *mesas* that opened on Monday.

Figure 4 plots the distribution of reported installation times between the JNE-flagged *mesas* that were confirmed as having opened after the 2pm deadline, and compares this against all other non-JNE flagged *mesas*. Two clear patterns emerge from the distribution. First, several *mesas* lie to the left-side of the 2pm threshold, thus declaring they were installed before 2pm when in actuality they were not. Strikingly, some *mesas* go as far as to claim they opened as early as 7am. Second, we evidently observe sorting just before the 2pm extended deadline, with 52 *mesas* (34.2% of all JNE-flagged *mesas*) reporting an opening time between 1:30pm and 2pm. Only 30 *mesas* (19.7% of all JNE-flagged *mesas*) reported a faithful opening time after 2pm in their respective *acta*.

When manually checking the *actas* PDFs for some of these JNE-flagged *mesas*, we find that *miembros de mesa* (i.e. *mesa* volunteers) that had initially recorded an opening time after 2pm attempted to retroactively amend the record by handwriting an earlier time in the freeform “observations” section. Appendix D.3 provides two such examples of this. In one of the *actas*, the claimed installation time was 2pm, although a volunteer corrects this in the “*observaciones*” freeform section of the *acta* as having opened up at 3pm in actuality. As another example, one of the *actas* puts down

an installation time of 4.30pm. However, in the “*observaciones*” section, this is walked back on, with a *miembro de mesa* claiming the actual installation time was 1.30pm.

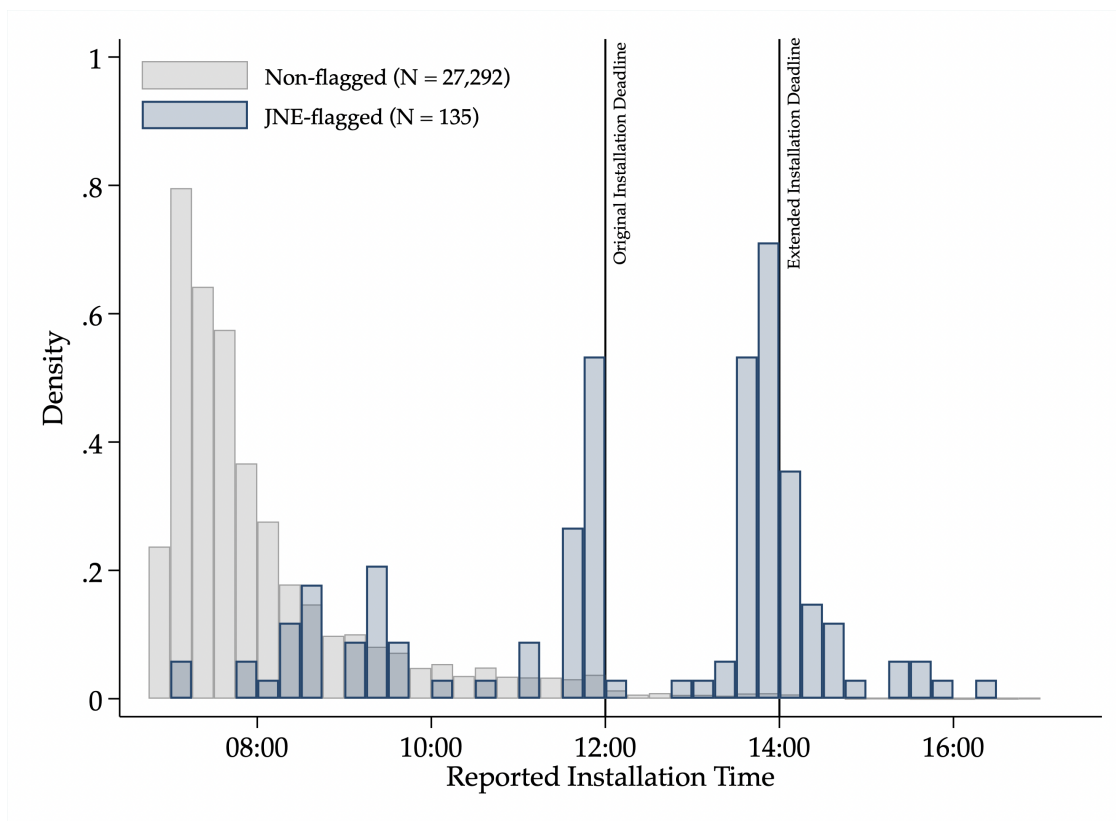


Figure 4: Distribution of Reported Installation Times

Note: blue-shaded histogram bars reflect the distribution of reported installation times among those *mesas* flagged by the [Jurado Nacional de Elecciones \(2026\)](#) as having opened after the 2pm extended legal deadline. Grey-shaded histogram bars reflect the distribution of all other *mesas* in Lima that were not flagged by the [Jurado Nacional de Elecciones \(2026\)](#). Sample excludes *mesas* that opened on Monday.

8 Discussion

This paper has provided a comprehensive and rigorous assessment of the impact of polling station delays on voter turnout during the first round of the 2026 Peruvian presidential election. Collecting rich *mesa*-level data in real-time, in tandem with the official counting of ballots over the course of a month, we document a large, negative effect on turnout as a result of logistical errors that precipitated installation delays. Using both cross-sectional variation at the *mesa*-level and within-voting site variation, our core estimates suggest that the delays led to a 2.5-5 percentage point decline in turnout levels.

Using these estimates, back-of-the-envelope calculations suggest that the “missing voters”, resulting from the delayed installations, totals approximately 24,000 voters. And with respect to the distribution of these foregone votes cast across the second and third-placed candidates, a local

matching exercise suggests that, relative to Sánchez, López-Aliaga lost out on approximately 5,700 votes.

Our core conclusion from the analysis is hence that, despite estimating a sizable impact on turnout, the logistical errors were not large enough to close the 21,000 vote gap in counted ballots between López-Aliaga and Sánchez. The top two candidates in this first round of the election, Keiko Fujimori and Roberto Sánchez, can thus be confirmed as being the legitimate winners going into the second round on June 7th.

Several scholars and commentators have referred to Peru as having undergone a sustained period of “democratic hollowing” – as opposed to an excessive concentration of political power, democratic decay has arisen due to extreme fragmentation ([Barrenechea and Vergara 2023](#)). While our analysis suggests that late-opening across select *mesas* is not necessarily enough to overturn current election results, there are still a multitude of lessons that can fundamentally be learned from this electoral experience, especially going into the second round in the coming weeks.

In polarized and closely contested elections, delays in polling station openings deepen citizen distrust and fuel accusations of electoral malpractice, while also undermining political equality by discouraging participation. In the short-run, electoral authorities should therefore establish clear, pre-announced protocols for handling delays rather than relying on ad hoc election-day decisions, while ensuring that *mesa* officials and electoral monitors accurately record installation times. The credibility of the electoral process depends not only on minimizing delays, but also on transparently documenting them and maintaining clear rules regarding when late-opening polling stations may legally be counted.

At the same time, the deeper structural challenges facing Peruvian democracy will not disappear in the immediate future. Meaningful reform requires institutional changes that reduce congressional fragmentation and create incentives for broader presidential coalitions. Voters would benefit from facing a more manageable set of candidates, rather than the cognitive burden of choosing among 35 presidential options. Under the current system, it is entirely possible that a candidate who received only 12 percent of first-round preferences could ultimately become president.

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Appendix

Appendix A Data Discussion

Appendix A.1 Confidence in the Honesty of Elections

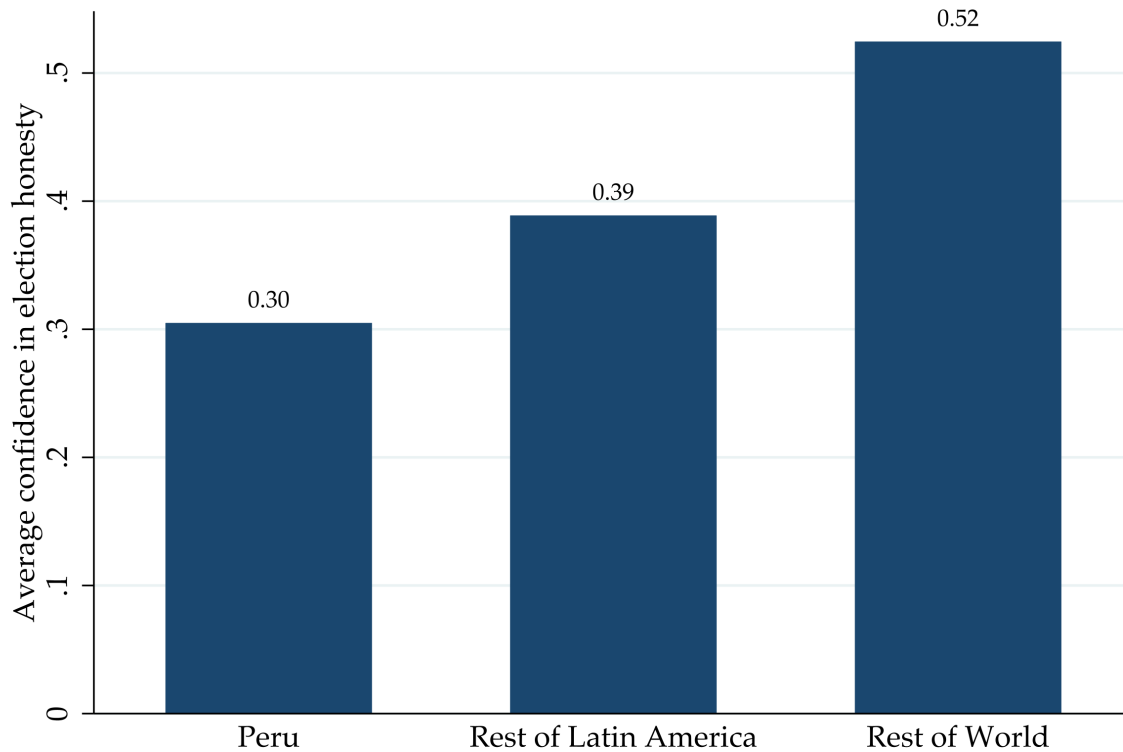


Figure A.1.1: Confidence in the Honesty of Elections (Peru, Latin America, and the World)

Note: data comes from the Gallup World Poll, based on the survey item: “Do you have confidence in each of the following, or not? How about the honesty of elections?” (WP144). We generate a dummy variable based on whether respondents report yes/no. We first take country-year averages, and then take a pooled average between 2005 to 2021.

Appendix A.2 Actas Processed via OCR

Figure A.2.1: Types of Actas Processed

MESA DE SUFRAGIO N.*
058981

DEPARTAMENTO: LIMA PROVINCIA: LIMA

ACTA ELI

A ACTA DE INSTALACIÓN
Siendo las 3:20 pm del 12 de abril de 2026, se conformó e instaló la Mesa de sufragio, continuando luego con la revisión del material electoral recibido. El material se recibió en buen estado: SI NO

CANTIDAD DE CÉDULAS DE SUFRAGIO RECIBIDAS
→ trescientos (en letras) 300 (en números)

OBSERVACIONES: Finalizó 219

(a) Late-Opening Scanned Acta Example

MESA DE SUFRAGIO N.*
055240

DEPARTAMENTO: LIMA PROVINCIA: LIMA DISTRITO: SAN JUAN DE MIRAFLORES

ACTA ELECTORAL TOTAL DE ELECTORES HABLES: 299

A ACTA DE INSTALACIÓN
Siendo las 11:07 a. m. del 12 de abril de 2026, se conformó e instaló la Mesa de Sufragio, continuando luego con la revisión del material electoral recibido. El material se recibió en buen estado: SI NO

CANTIDAD DE CÉDULAS DE SUFRAGIO RECIBIDAS
→ DOSCIENTOS NOVENTA Y NUEVE (en letras) 299 (en números)

OBSERVACIONES:
No hay observaciones.

(b) Late-Opening Digital Acta Example

Appendix A.3 Panel Coverage of Voting Sites

Table A.3.1: Lima Panel Coverage by Site across Elections

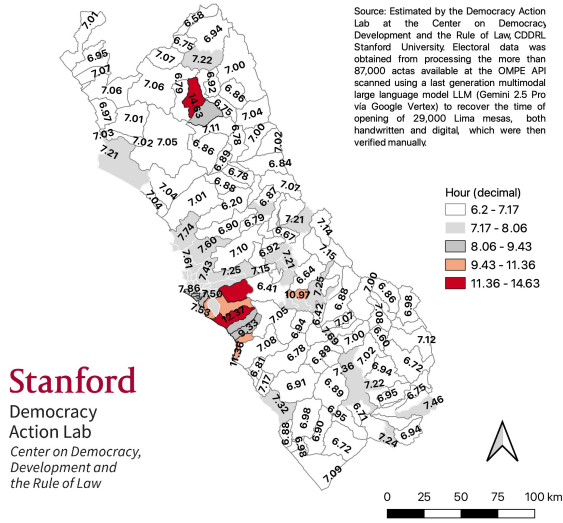
Elections Present	Sites	% sites	Mesa-Year Obs.	% Obs.
≥ 4 (balanced)	768	21.2%	66,021	56.0%
≥ 3	1,158	32.0%	83,388	70.8%
≥ 2	1,923	53.1%	99,807	84.7%
≥ 1	3,624	100.0%	117,562	99.8%

Notes: universe is the 117,810 mesa-year observations in Lima department across the 2011, 2016, 2021, and 2026 first-round presidential elections. A “site” is a unique voting location (*local de votación*) identified by our cross-election matching procedure. The balanced panel (≥ 4 years) contains 21.2% of distinct Lima sites but accounts for 56.0% of mesa-year observations because larger, more persistent sites concentrate disproportionate mesa volume.

Appendix A.4 Distribution of Installation Time and Turnout across Lima

(a) Installation Time

Average time of installation of mesas in each district in the Province of Lima in the first round of the 2026 Presidential elections



(b) Turnout

Average turnout in the voting sites in each district in the Province of Lima in the first round of the 2026 Presidential elections

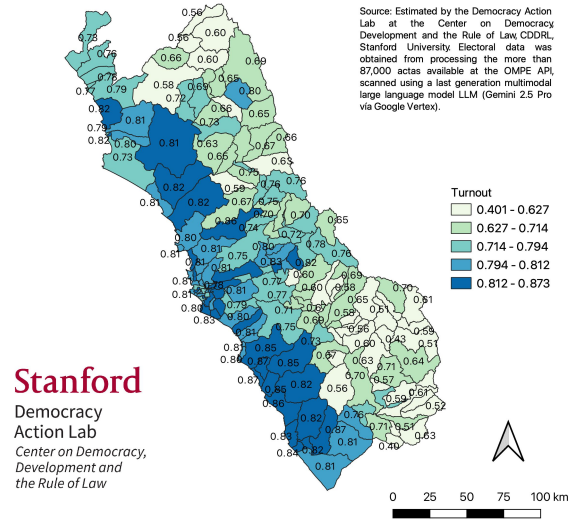


Figure A.4.1: Distribution of Installation Time and Turnout across Lima (*Distrito-Level*)

Note: panel (a) plots installation time. Panel (b) plots turnout level. Unit of analysis is the *distrito* (i.e. “ubigeo”).

Appendix B Additional Empirical Strategy Material

Appendix B.1 Grid Search

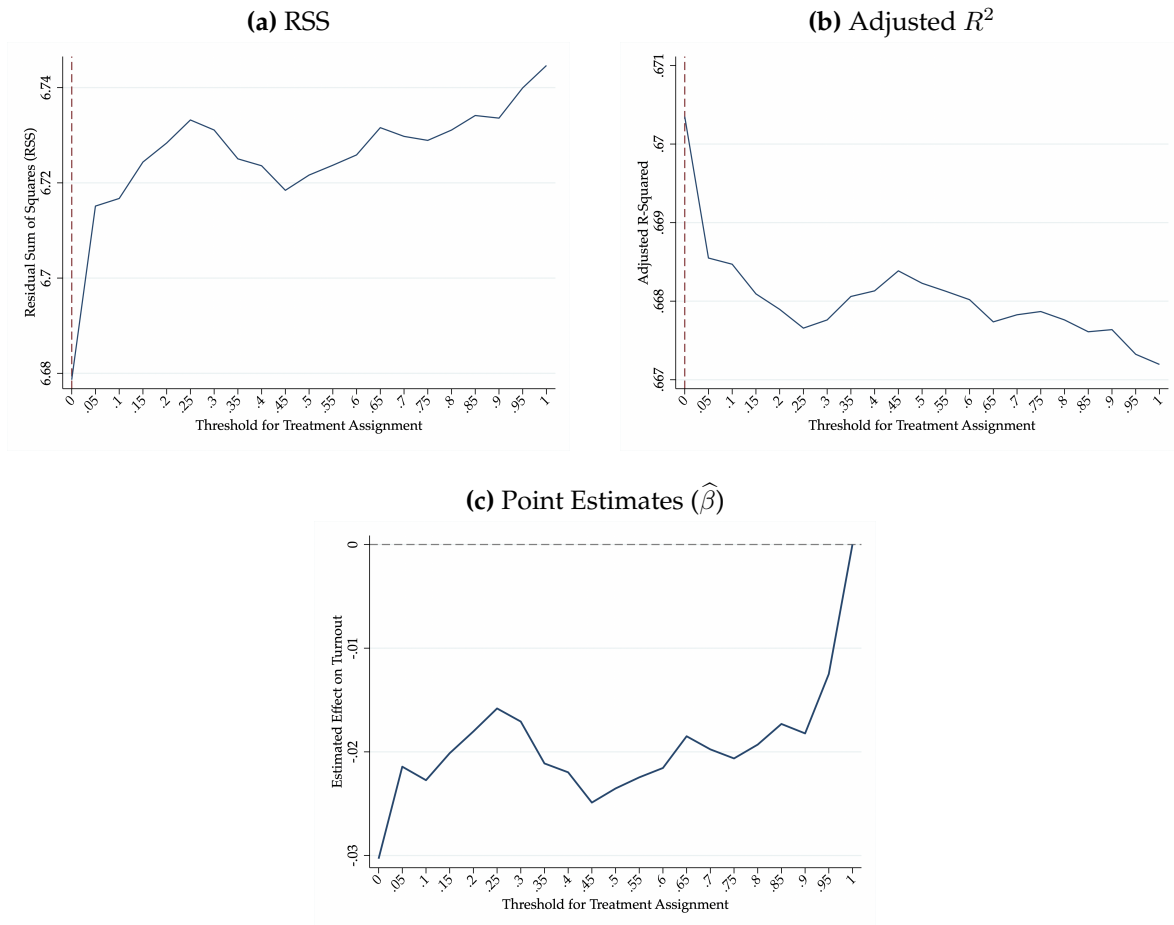


Figure B.1.1: Grid Search Results for 10am+ *Mesas*

Note: panel (a) plots the RSS across iterations. Panel (b) plots the adjusted R^2 across iterations. Red dashed vertical line outlines the threshold associated with the minimum RSS in panel (a) and the maximum adjusted R^2 in panel (b). Panel (c) plots the range of point estimates, $\hat{\beta}$, associated with each iteration of the grid search vis-à-vis the threshold for assigning treatment to the voting site.

Appendix C Robustness Checks and Extensions

Appendix C.1 Event Study Using Average *Mesa*-Level Turnout Rates

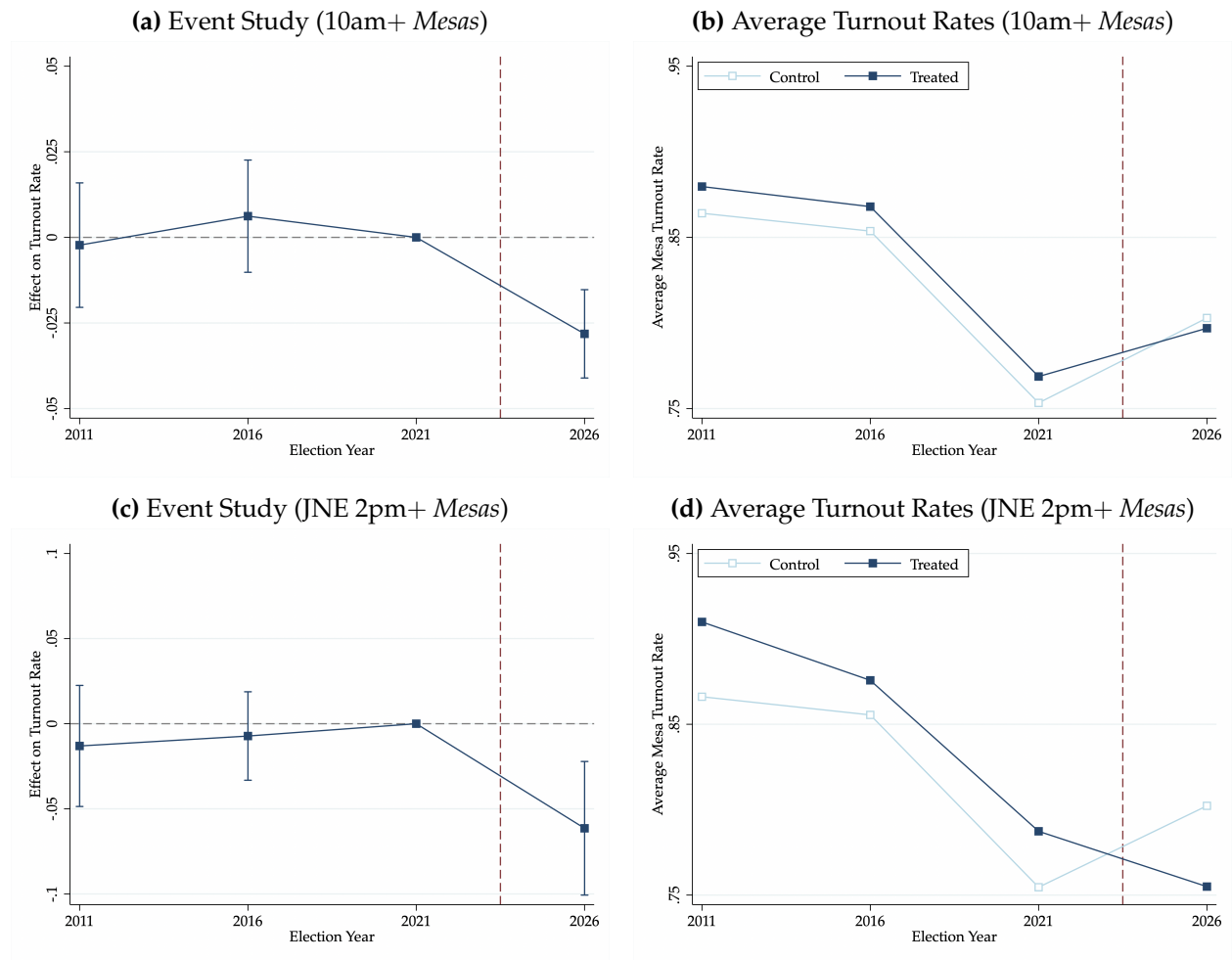


Figure C.1.1: Event Study Results and Mean Turnout using *Mesa* Average Turnout within Sites

Note: error bars represent 95% confidence intervals, with standard errors clustered at the voting site level. Panels (a) and (c) plot the estimates performing our event study from equation (6), but now using average *mesa*-level turnout rates within voting sites as opposed to measuring site-level turnout by summing over voters and registered voters across *mesas*. All specifications include voting site and *distrito* \times year fixed effects. Panels (b) and (d) plot the average *mesa*-level turnout rate across voting sites per election based on treatment status. Panels (a) and (b) use *mesas* that opened after 10am (i.e. column 1 of panel B in Table 3). Panels (c) and (d) use *mesas* that were listed by the JNE as having opened after 2pm. Dark blue line refers to the ever-treated group. Light blue line refers to control group. The base year for the event study is 2021. Vertical dashed red line conveys that 2026 is the year of treatment onset.

Appendix C.2 Voting Monday Event Study

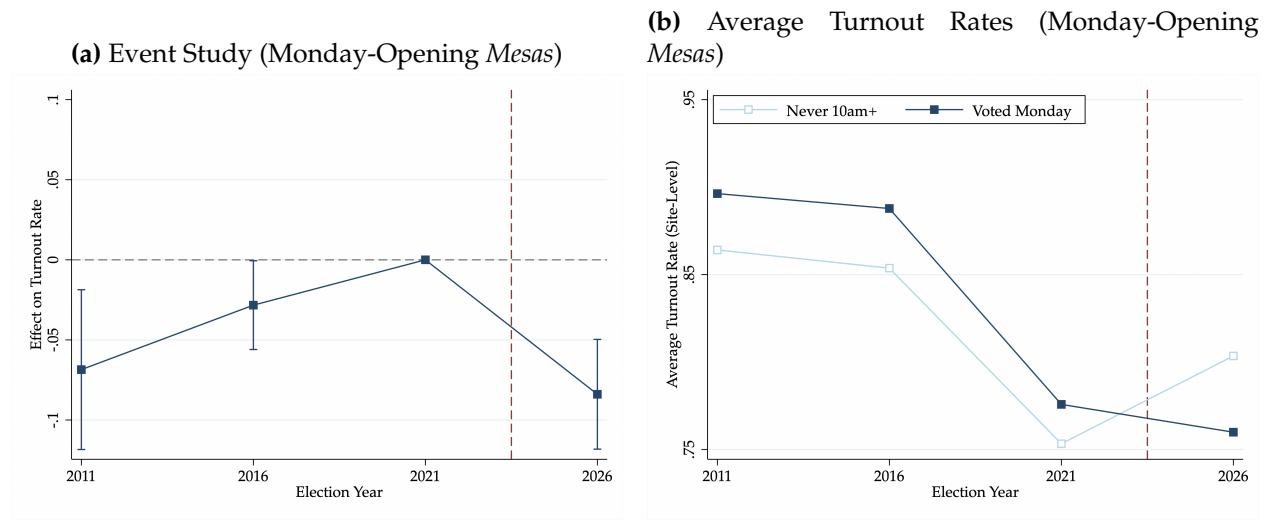


Figure C.2.1: Event Study Results for Monday-Opening Mesas

Note: error bars represent 95% confidence intervals, with standard errors clustered at the voting site level. Panel (a) plots the estimates performing our event study from equation (6), including voting site and *distrito* \times year fixed effects. Panel (b) plots the average turnout rate across sites per election based on treatment status. Dark blue line refers to the ever-treated group. Light blue line refers to control group. The base year for the event study is 2021. Vertical dashed red line conveys that 2026 is the year of treatment onset. Control group refers to sites that opened before 10am on Sunday (i.e. “on time” and hence a never-treated set of controls).

Appendix C.3 Fujimori Placebo Test

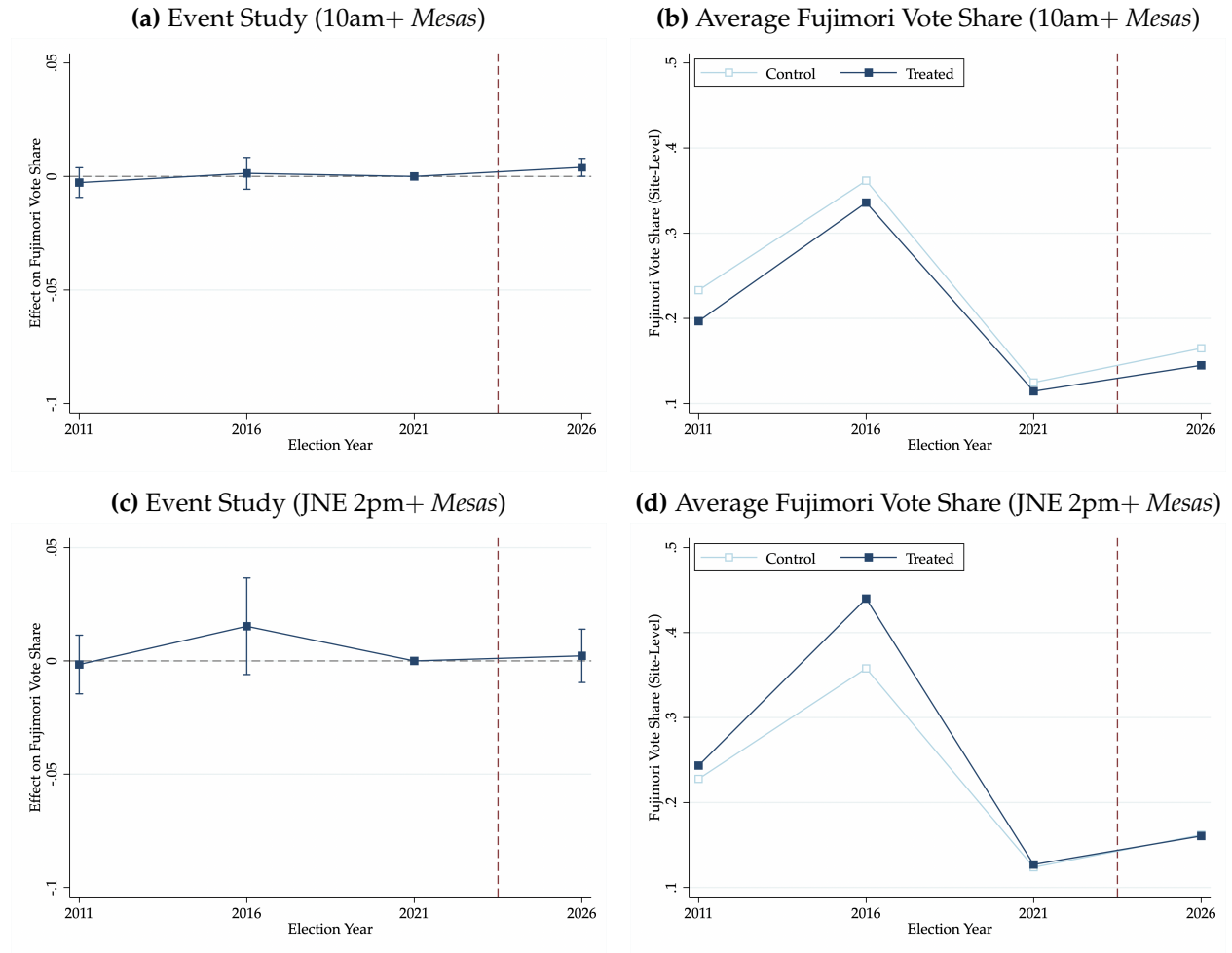


Figure C.3.1: Event Study Results and Mean Turnout

Note: error bars represent 95% confidence intervals, with standard errors clustered at the voting site level. Panels (a) and (c) plot the estimates performing our event study from equation (6), including voting site and *distrito* \times year fixed effects. Panels (b) and (d) plot the average Fujimori vote share across sites per election based on treatment status. Panels (a) and (b) use *mesas* that opened after 10am (i.e. column 2 of panel B in Table 3). Panels (c) and (d) use *mesas* that were listed by the JNE as having opened after 2pm. Dark blue line refers to the ever-treated group. Light blue line refers to control group. The base year for the event study is 2021. Vertical dashed red line conveys that 2026 is the year of treatment onset.

Appendix C.4 Blank Votes Placebo Test

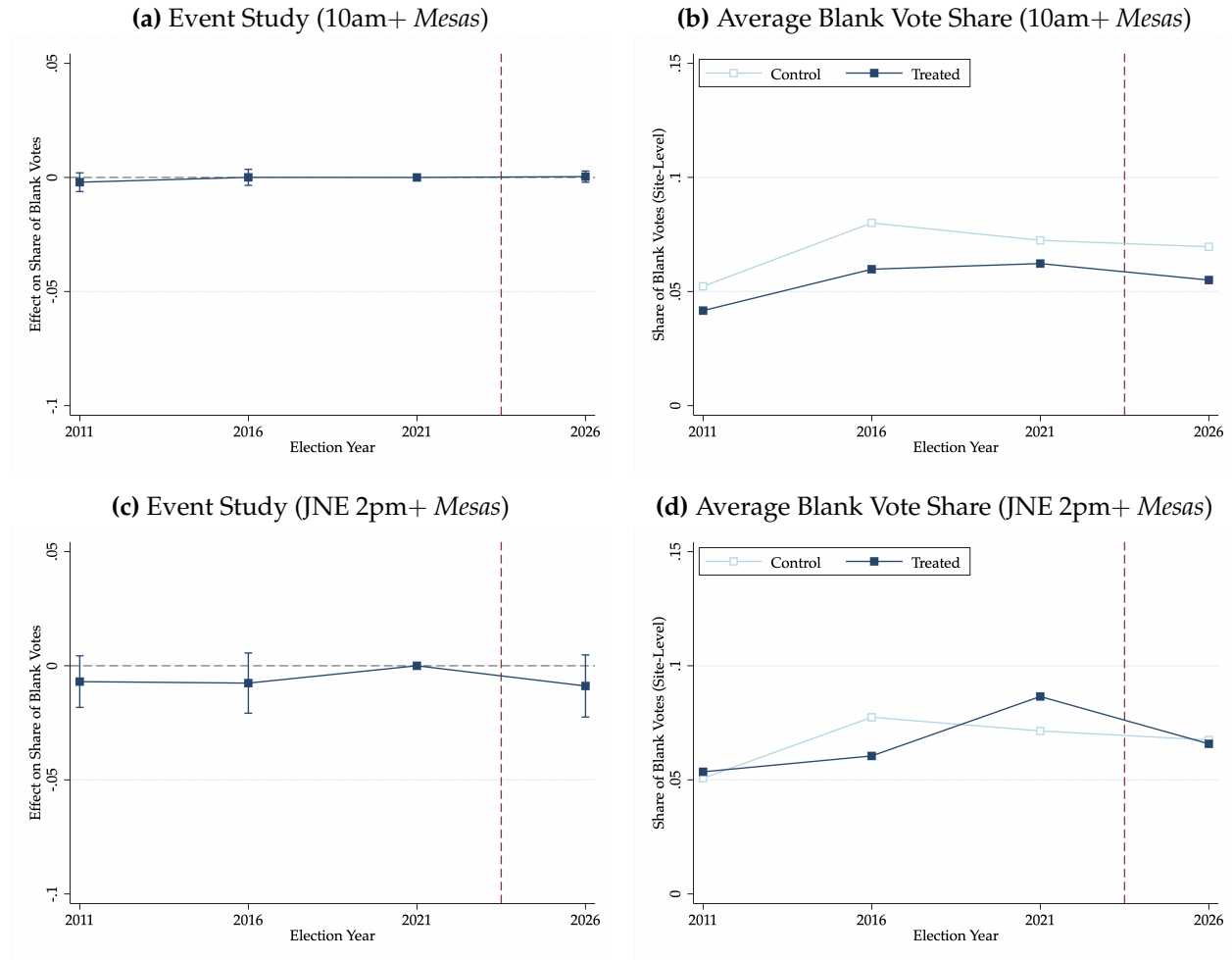


Figure C.4.1: Event Study Results and Mean Blank Votes Share

Note: error bars represent 95% confidence intervals, with standard errors clustered at the voting site level. Panels (a) and (c) plot the estimates performing our event study from equation (6), including voting site and *distrito* \times year fixed effects. Panels (b) and (d) plot the average blank votes share across sites per election based on treatment status. Panels (a) and (b) use *mesas* that opened after 10am (i.e. column 2 of panel B in Table 3). Panels (c) and (d) use *mesas* that were listed by the JNE as having opened after 2pm. Dark blue line refers to the ever-treated group. Light blue line refers to control group. The base year for the event study is 2021. Vertical dashed red line conveys that 2026 is the year of treatment onset.

Appendix C.5 Core Results with *Provincia* Fixed Effects

Table C.5.1: Effect of Late Opening on Turnout (*Mesa*-Level, *Provincia* FE)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: OLS</i>								
	Dependent variable is Turnout Rate							
Late Opening	-0.022*** (0.006)	-0.023*** (0.007)	-0.011 (0.008)	-0.017** (0.008)	-0.033*** (0.010)	-0.044*** (0.016)	-0.036* (0.021)	
ln(Opening Hour)								-0.052*** (0.011)
Observations	28,800	28,800	28,800	28,800	28,800	28,800	26,221	28,665
<i>Panel B: Fractional Logit</i>								
Late Opening	-0.022*** (0.006)	-0.022*** (0.007)	-0.011 (0.007)	-0.017** (0.008)	-0.032*** (0.009)	-0.041*** (0.014)	-0.034* (0.018)	
ln(Opening Hour)								-0.052*** (0.011)
Observations	28,800	28,800	28,800	28,800	28,800	28,800	26,221	28,665
<i>Provincia</i> FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday	Non-JNE

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by voting site in parentheses. Treated sample refers to the definition of the treatment variable. JNE refers to *mesas* observed opening after 2pm by the [Jurado Nacional de Elecciones \(2026\)](#) report. Panel A reports OLS estimates. Panel B reports average marginal effects from fractional logit models. Column 7 only uses *mesas* that opened before 10am on Sunday as the control group. Column 8 drops all *mesas* flagged in the [Jurado Nacional de Elecciones \(2026\)](#) report given installation times cannot be confirmed before 2pm.

Table C.5.2: Effect of Late Opening on Turnout (Site-Level, *Provincia* × Year FE)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Fraction of Mesas</i>							
	Dependent variable is Site Turnout Rate						
Late Opening	-0.026*** (0.006)	-0.036*** (0.008)	-0.045*** (0.011)	-0.059*** (0.013)	-0.085*** (0.013)	-0.088*** (0.016)	-0.057*** (0.012)
Observations	5,742	5,742	5,742	5,742	5,742	5,742	4,929
<i>Panel B: Any Late Mesa</i>							
Late Opening	-0.017*** (0.004)	-0.022*** (0.004)	-0.026*** (0.007)	-0.035*** (0.008)	-0.046*** (0.010)	-0.082*** (0.016)	-0.057*** (0.012)
Observations	5,742	5,742	5,742	5,742	5,742	5,742	4,929
<i>Panel C: At Least 50% of Mesas Late</i>							
Late Opening	-0.024*** (0.005)	-0.031*** (0.007)	-0.044*** (0.010)	-0.056*** (0.012)	-0.078*** (0.013)	-0.088*** (0.016)	-0.057*** (0.012)
Observations	5,742	5,742	5,742	5,742	5,742	5,742	4,929
Voting Site FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Provincia</i> × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by voting site in parentheses. All specifications include voting site and *provincia* × year fixed effects and control for the number of *mesas* within each site. Panel A uses the continuous share of late-opening *mesas* within a site. Panel B defines treatment as any late-opening *mesa* within the site. Panel C defines treatment as at least 50% of *mesas* opening late. Note results across panels for column 7 are identical as there was no variation across *mesas* within voting sites that opened on Sunday (i.e. all *mesas* opened on Sunday for these sites, and so by definition at least 50% of *mesas* at these sites opened on Monday).

Appendix C.6 Randomization Inference

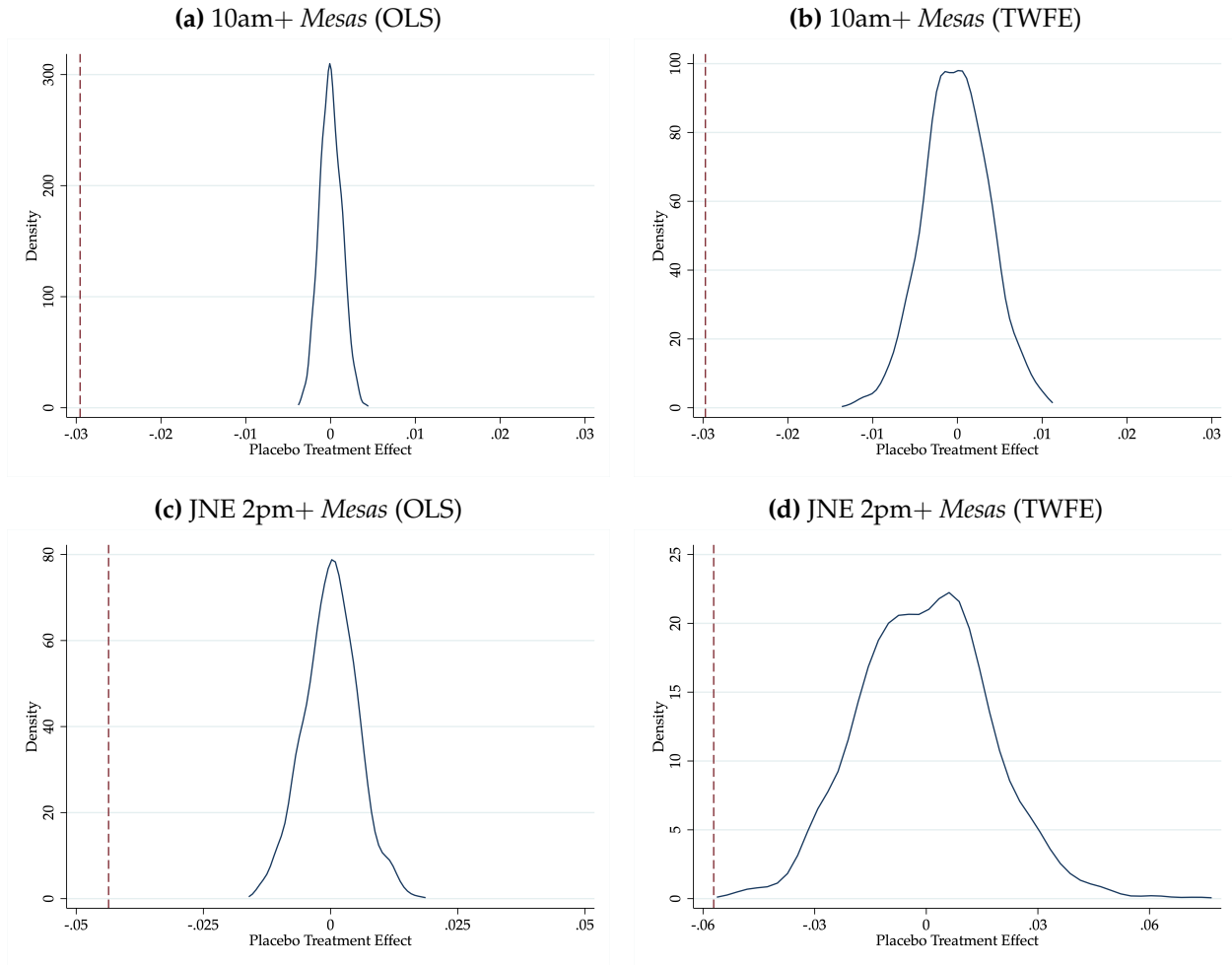


Figure C.6.1: Randomization Inference Results

Note: panels (a) and (c) plot the distribution of placebo estimates using OLS from Table 2. Panels (b) and (d) plot the distribution of placebo estimates using TWFE from panel B of Table 3. Red dashed vertical line outlines the observed estimate in Tables 2 and 3. We always preserve the proportion of treated units as those in the core results in Tables 2 and 3. We generate placebo estimates across 1,000 iterations.

Appendix C.7 Dropping Units

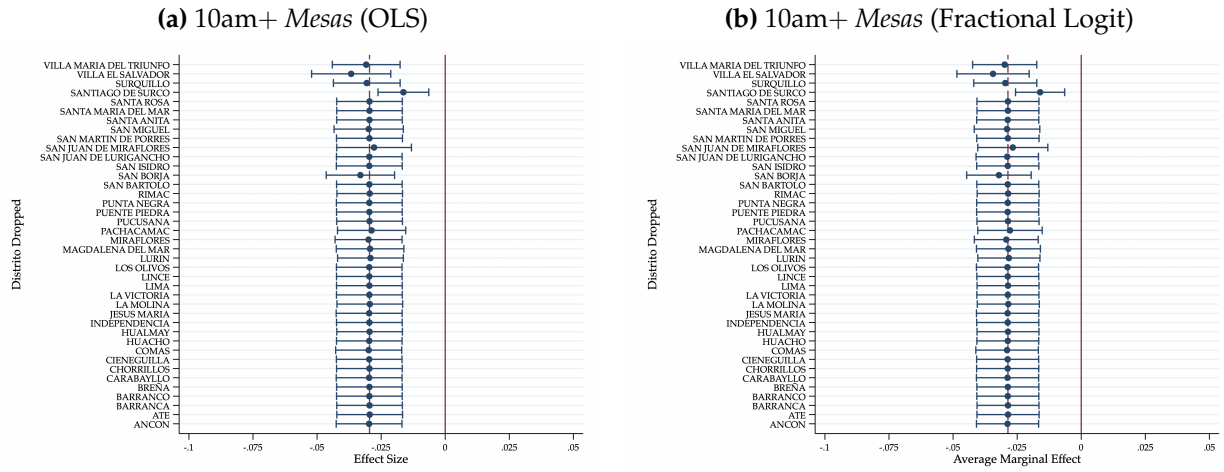


Figure C.7.1: OLS and Fractional Logit Results Dropping Each *Distrito*

Note: panel (a) uses OLS (panel A, column 1 of Table 2) and panel (b) uses fractional logit (panel B, column 1 of Table 2). Each coefficient is from a separate regression with the corresponding *distrito* dropped along the *y*-axis. Confidence intervals shown at the 95% level, with standard errors clustered by voting site.

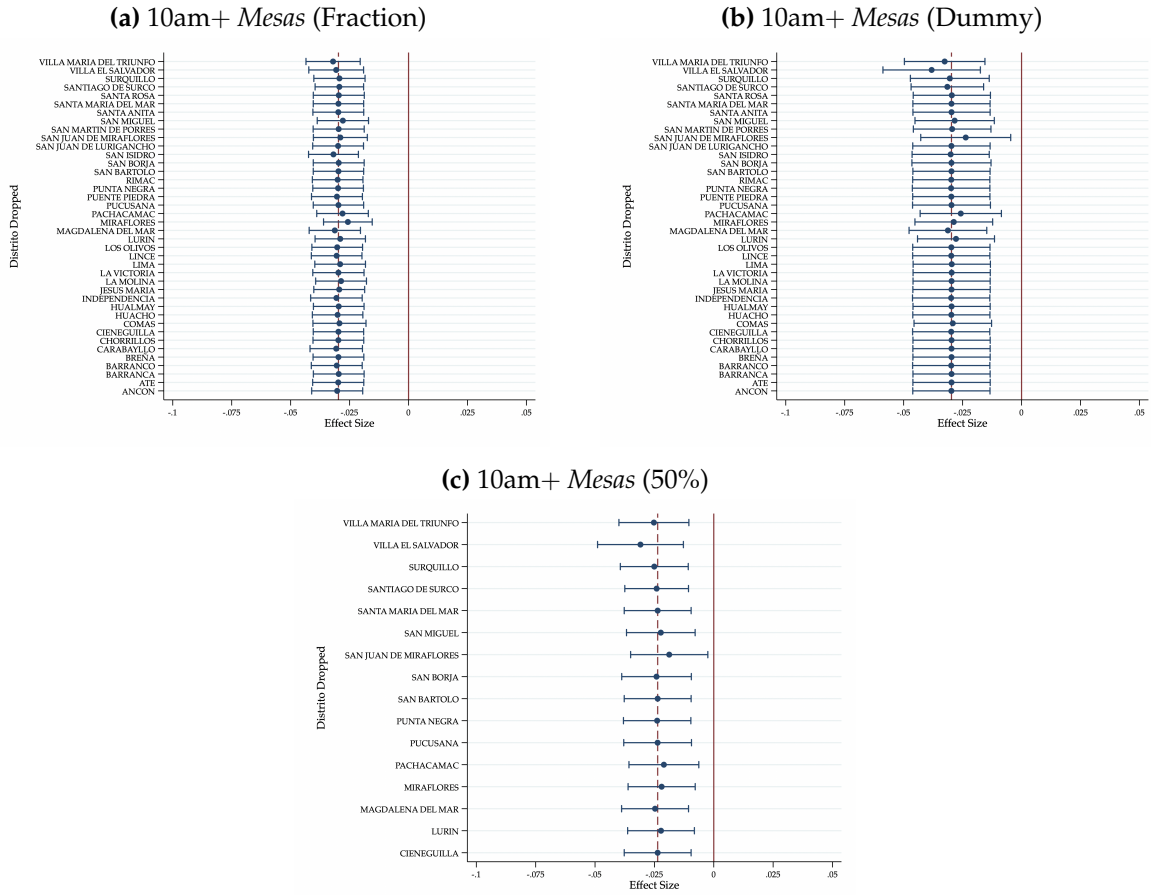


Figure C.7.2: TWFE Results Dropping Each *Distrito*

Note: panel (a) uses the fraction of *mesas* that opened after 10am on Sunday within a voting site as the treatment variable (panel A, column 1 of Table 3). Panel (b) uses a dummy variable for whether *any mesa* opened after 10am on Sunday within a voting site as the treatment variable (panel B, column 1 of Table 3). Panel (c) uses a dummy variable for whether at least 50% of *mesas* within a voting site opened after 10am on Sunday as the treatment variable (panel C, column 1 of Table 3). Each coefficient is from a separate regression with the corresponding *distrito* dropped along the *y*-axis. Confidence intervals shown at the 95% level, with standard errors clustered by voting site.

Appendix C.8 Sample Robustness

Table C.8.1: Effect of Late Opening on Turnout (*Mesa*-Level, Ever-Treated *Distritos*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: OLS</i>								
	Dependent variable is Turnout Rate							
Late Opening	-0.030*** (0.007)	-0.029*** (0.008)	-0.016** (0.008)	-0.023** (0.009)	-0.034*** (0.011)	-0.044*** (0.016)	-0.053* (0.030)	
ln(Opening Hour)								-0.076*** (0.013)
Observations	24,286	24,286	24,286	24,286	24,286	24,286	21,708	24,151
<i>Panel B: Fractional Logit</i>								
Late Opening	-0.029*** (0.006)	-0.028*** (0.007)	-0.016** (0.007)	-0.022*** (0.009)	-0.032*** (0.010)	-0.041*** (0.014)	-0.051* (0.027)	
ln(Opening Hour)								-0.074*** (0.012)
Observations	24,286	24,286	24,286	24,286	24,286	24,286	21,708	24,151
<i>Distrito</i> FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday	Non-JNE

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by voting site in parentheses. Treated sample refers to the definition of the treatment variable. JNE refers to *mesas* observed opening after 2pm by the [Jurado Nacional de Elecciones \(2026\)](#) report. Panel A reports OLS estimates. Panel B reports average marginal effects from fractional logit models. Column 7 only uses *mesas* that opened before 10am on Sunday as the control group. Column 8 drops all *mesas* flagged in the [Jurado Nacional de Elecciones \(2026\)](#) report given installation times cannot be confirmed before 2pm. Sample uses only *distritos* that comprise an ever-treated *mesa*.

Table C.8.2: Effect of Late Opening on Turnout (Site-Level, 3+ Elections Observed)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Fraction of Mesas</i>							
	Dependent variable is Turnout Rate (Site-Level)						
Late Opening	-0.032*** (0.010)	-0.030*** (0.011)	-0.027** (0.012)	-0.037*** (0.011)	-0.039*** (0.008)	-0.041*** (0.007)	-0.049** (0.022)
Observations	3,862	3,862	3,862	3,862	3,862	3,862	3,240
<i>Panel B: Any Late Mesa</i>							
Late Opening	-0.031*** (0.006)	-0.022*** (0.007)	-0.017** (0.008)	-0.011 (0.009)	-0.015 (0.010)	-0.034*** (0.009)	-0.049** (0.022)
Observations	3,862	3,862	3,862	3,862	3,862	3,862	3,240
<i>Panel C: At Least 50% of Mesas Late</i>							
Late Opening	-0.025*** (0.008)	-0.023** (0.009)	-0.025** (0.012)	-0.039*** (0.010)	-0.035*** (0.009)	-0.041*** (0.007)	-0.049** (0.022)
Observations	3,862	3,862	3,862	3,862	3,862	3,862	3,240
Voting Site FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Distrito</i> × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by voting site in parentheses. All specifications include site and district-year fixed effects and control for the number of mesas within each site. Panel A uses the continuous share of late-opening mesas within a site. Panel B defines treatment as any late-opening mesa within the site. Panel C defines treatment as at least 50% of mesas opening late. All specifications only include voting sites that are observed across three elections in the panel dataset. Sample uses only voting sites that we can observe across at least three distinct elections.

Appendix C.9 Alternative Error Structures

Table C.9.1: Effect of Late Opening on Turnout (Mesa-Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: OLS</i>								
	Dependent variable is Turnout Rate (Mesa-Level)							
Late Opening	-0.030** (0.013)	-0.029** (0.014)	-0.016 (0.012)	-0.023** (0.009)	-0.034*** (0.006)	-0.044*** (0.009)	-0.053 (0.041)	
ln(Opening Hour)								-0.076*** (0.024)
WCB <i>p</i> -value	0.000	0.000	0.048	0.018	0.006	0.028	0.112	0.000
WCB 95% CI	[-0.042, -0.017]	[-0.045, -0.013]	[-0.032, -0.001]	[-0.042, -0.004]	[-0.059, -0.010]	[-0.083, -0.007]	[-0.155, 0.024]	[-0.099, -0.052]
Observations	28,800	28,800	28,800	28,800	28,800	28,800	26,221	28,665
<i>Panel B: Fractional Logit</i>								
Late Opening	-0.029** (0.012)	-0.028** (0.013)	-0.016 (0.011)	-0.022*** (0.008)	-0.032*** (0.005)	-0.041*** (0.007)	-0.052 (0.037)	
ln(Opening Hour)								-0.074*** (0.023)
Observations	28,800	28,800	28,800	28,800	28,800	28,800	26,221	28,665
Distrito FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday	Non-JNE

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by *distrito* in parentheses. For panel A, WCB refers to wild cluster bootstrap *p*-values and 95% confidence intervals (Webb weights, 9,999 replications, clustered by voting site) (Cameron et al. 2008). Both panels include *distrito* fixed effects. Treated sample refers to the definition of the treatment variable. JNE refers to *mesas* observed opening after 2pm by the *Jurado Nacional de Elecciones* (2026) report. Panel A reports OLS estimates. Panel B reports average marginal effects from fractional logit models. Column 7 only uses *mesas* that opened before 10am on Sunday as the control group. Column 8 drops all *mesas* flagged in the *Jurado Nacional de Elecciones* (2026) report given installation times cannot be confirmed before 2pm.

Table C.9.2: Effect of Late Opening on Turnout (Site-Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Fraction of Mesas</i>							
	Dependent variable is Site Turnout Rate						
Late Opening	-0.030** (0.015)	-0.028 (0.026)	-0.028 (0.028)	-0.046** (0.019)	-0.065*** (0.021)	-0.065** (0.025)	-0.069 (0.042)
WCB <i>p</i> -value	0.001	0.004	0.019	0.000	0.000	0.002	0.019
WCB 95% CI	[-0.046, -0.013]	[-0.047, -0.009]	[-0.052, -0.005]	[-0.070, -0.022]	[-0.099, -0.034]	[-0.105, -0.030]	[-0.127, -0.015]
Observations	5,391	5,391	5,391	5,391	5,391	5,391	4,582
<i>Panel B: Any Late Mesa</i>							
Late Opening	-0.030*** (0.009)	-0.024*** (0.009)	-0.018 (0.013)	-0.023 (0.017)	-0.037** (0.016)	-0.057*** (0.017)	-0.069 (0.042)
WCB <i>p</i> -value	0.000	0.000	0.009	0.004	0.000	0.002	0.019
WCB 95% CI	[-0.040, -0.019]	[-0.036, -0.013]	[-0.032, -0.004]	[-0.038, -0.007]	[-0.057, -0.016]	[-0.095, -0.023]	[-0.127, -0.015]
Observations	5,391	5,391	5,391	5,391	5,391	5,391	4,582
<i>Panel C: At Least 50% of Mesas Late</i>							
Late Opening	-0.024** (0.012)	-0.020 (0.020)	-0.027 (0.025)	-0.044** (0.018)	-0.058*** (0.016)	-0.065** (0.026)	-0.069 (0.042)
WCB <i>p</i> -value	0.001	0.016	0.022	0.000	0.000	0.002	0.019
WCB 95% CI	[-0.038, -0.010]	[-0.036, -0.004]	[-0.050, -0.004]	[-0.068, -0.022]	[-0.090, -0.028]	[-0.105, -0.030]	[-0.127, -0.015]
Observations	5,391	5,391	5,391	5,391	5,391	5,391	4,582
Voting Site FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distrito × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by *distrito* in parentheses. WCB refers to wild cluster bootstrap *p*-values and 95% confidence intervals (Webb weights, 9,999 replications, clustered by voting site) (Cameron et al. 2008). All specifications include voting site and *distrito* × year fixed effects, and control for the number of *mesas* within each site. Panel A uses the (continuous) share of late-opening *mesas* within a site. Panel B defines treatment as any late-opening mesa within the site. Panel C defines treatment as at least 50% of *mesas* opening late.

Appendix C.10 de’Chaisemartin and D’Haultfœuille Estimator

Although we do not have variation in timing of treatment adoption as we only have a single treated period of 2026, we also use the estimator proposed by [Chaisemartin and D’Haultfœuille \(2024\)](#) for additional robustness. Letting $h_{s,t}$ denote treatment status for voting site s in election year t , the estimator for event time ℓ can be expressed as:

$$\beta_\ell = \mathbb{E} \left[(\tau_{s,t+\ell} - \tau_{s,t-1}) \mid h_{s,t-1} = 0, h_{st} = 1 \right] - \mathbb{E} \left[(\tau_{s,t+\ell} - \tau_{s,t-1}) \mid h_{s,t-1} = 0, h_{st} = 0 \right], \quad (7)$$

where the first term captures the average change in turnout for units switching into treatment between $t - 1$ and t , while the second term captures the corresponding evolution among units remaining untreated. Given treatment adoption occurs only in the 2026 election, our setting effectively corresponds to a single-cohort adoption design, with prior elections serving as untreated comparison periods. As shown below, our results for both the 10am+ (dummy) treatment and the 2pm+ JNE-listed *mesas* are identical to our standard event study results in [Figure 2](#).

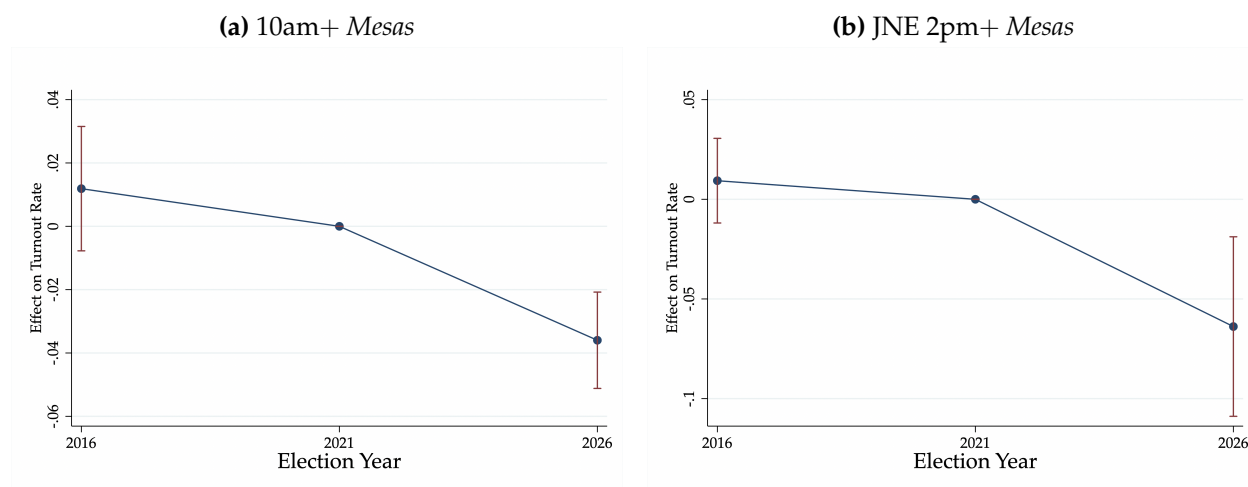


Figure C.10.1: Results Using de’Chaisemartin and D’Haultfœuille Estimator

Note: error bars represent 95% confidence intervals, with standard errors clustered at the voting site level. Both panels include *distrito*-year trends (*distrito*×*year* fixed effects remains computationally demanding with the estimator due to including several hundred extra parameters for estimation). Only a single pre-treatment “placebo” period can be estimated as we only have one post-treatment period for estimation ([Chaisemartin and D’Haultfœuille 2024](#)).

Appendix C.11 Income-Elasticity of Turnout

Table C.11.1: Effect of Late Opening and GDP on Turnout (*Mesa*-Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: OLS</i>								
	Dependent variable is Turnout Rate							
Late Opening	-0.020*** (0.006)	-0.022*** (0.007)	-0.011 (0.008)	-0.018** (0.008)	-0.034*** (0.011)	-0.045*** (0.016)	-0.038* (0.021)	
ln(Opening Hour)								-0.045*** (0.011)
ln(GDP)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.006*** (0.002)
Observations	28,800	28,800	28,800	28,800	28,800	28,800	26,221	28,665
<i>Panel B: Fractional Logit</i>								
Late Opening	-0.020*** (0.006)	-0.021*** (0.007)	-0.011 (0.007)	-0.017** (0.008)	-0.032*** (0.009)	-0.042*** (0.014)	-0.036* (0.018)	
ln(Opening Hour)								-0.045*** (0.011)
ln(GDP)	0.007*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	0.007*** (0.002)	0.006*** (0.002)
Observations	28,800	28,800	28,800	28,800	28,800	28,800	26,221	28,665
<i>Provincia</i> FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday	Non-JNE

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by voting site in parentheses. All specifications include log GDP as a covariate from [Chen et al. \(2022\)](#)'s dataset. Treated sample refers to the definition of the treatment variable. JNE refers to *mesas* observed opening after 2pm by the [Jurado Nacional de Elecciones \(2026\)](#) report. Panel A reports OLS estimates. Panel B reports average marginal effects from fractional logit models. Column 7 only uses *mesas* that opened before 10am on Sunday as the control group. Column 8 drops all *mesas* flagged in the [Jurado Nacional de Elecciones \(2026\)](#) report given installation times cannot be confirmed before 2pm.

Table C.11.2: Effect of Late Opening and GDP on Turnout (Site-Level)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Fraction of Mesas</i>							
	Dependent variable is Site Turnout Rate						
Late Opening	-0.020*** (0.005)	-0.031*** (0.007)	-0.040*** (0.010)	-0.053*** (0.013)	-0.078*** (0.015)	-0.083*** (0.018)	-0.050*** (0.013)
ln(GDP)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.004)
Observations	5,742	5,742	5,742	5,742	5,742	5,742	4,929
Treated Sites	272	179	88	52	33	11	12
<i>Panel B: Any Late Mesa</i>							
Late Opening	-0.011*** (0.004)	-0.017*** (0.004)	-0.021*** (0.006)	-0.032*** (0.008)	-0.043*** (0.010)	-0.077*** (0.017)	-0.050*** (0.013)
ln(GDP)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.004)
Observations	5,742	5,742	5,742	5,742	5,742	5,742	4,929
Treated Sites	272	179	88	52	33	11	12
<i>Panel C: At Least 50% of Mesas Late</i>							
Late Opening	-0.020*** (0.005)	-0.027*** (0.007)	-0.039*** (0.010)	-0.050*** (0.012)	-0.072*** (0.014)	-0.083*** (0.018)	-0.050*** (0.013)
ln(GDP)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.003)	-0.045*** (0.004)
Observations	5,742	5,742	5,742	5,742	5,742	5,742	4,929
Treated Sites	134	76	36	26	13	10	12
Voting Site FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Provincia</i> × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Treated Sample	10am+	11am+	12pm+	1pm+	2pm+	JNE	Monday

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Robust standard errors clustered by voting site in parentheses. All specifications include voting site and *provincia*-year fixed effects and control for the number of mesas within each site and ln(GDP). Panel A uses the continuous share of late-opening mesas within a site (treated count = sites with any positive fraction, matching Panel B). Panel B defines treatment as any late-opening mesa within the site. Panel C defines treatment as at least 50% of mesas opening late.

Appendix D Missing Voters Estimation

Appendix D.1 Site-Cluster Bootstrap Discussion

For the combined post-10am Sunday and Monday-opening estimates, we define two components, indexed by $k \in \{10, M\}$, where 10 denotes units that opened after 10am on Sunday due to the logistical errors and M denotes units that opened on Monday. For each component, the implied number of foregone votes is

$$\widehat{\Delta\text{Votes}}_k = \widehat{\beta}_k \left(\sum_i h_{ik} N_i \right),$$

where i indexes either *mesas*, m , or voting sites, s , depending on the specification. The combined estimate of these two effects is:

$$\widehat{\Delta\text{Votes}}_{10+M} = \widehat{\Delta\text{Votes}}_{10} + \widehat{\Delta\text{Votes}}_M.$$

Equivalently,

$$\widehat{\Delta\text{Votes}}_{10+M} = \widehat{\beta}_{10} \left(\sum_i h_{i,10} N_i \right) + \widehat{\beta}_M \left(\sum_i h_{i,M} N_i \right).$$

To construct uncertainty intervals for this combined quantity, we use a site-cluster bootstrap. In each bootstrap sample b , we re-estimate both treatment effects and recompute the composite effect:

$$\widehat{\Delta\text{Votes}}_{10+M}^{(b)} = \widehat{\Delta\text{Votes}}_{10}^{(b)} + \widehat{\Delta\text{Votes}}_M^{(b)}.$$

The bootstrap variance of the composite effect is therefore

$$\text{Var}_b \left(\widehat{\Delta\text{Votes}}_{10+M}^{(b)} \right) = \text{Var}_b \left(\widehat{\Delta\text{Votes}}_{10}^{(b)} \right) + \text{Var}_b \left(\widehat{\Delta\text{Votes}}_M^{(b)} \right) + 2 \text{Cov}_b \left(\widehat{\Delta\text{Votes}}_{10}^{(b)}, \widehat{\Delta\text{Votes}}_M^{(b)} \right).$$

Thus, by bootstrapping the composite statistic directly, the resulting interval incorporates both component variances as well as their covariance under the site-cluster resampling scheme.

Appendix D.2 TWFE Extensive Margin Missing Votes and Margin Estimates

Table D.2.1: Estimates of Missing Voters from Delayed Openings (TWFE Dummy)

<i>TWFE (Dummy)</i>	(1)	(2)	(3)	(4)
	$\widehat{\beta}$	Exposure-Weighted Registered Voters	$\widehat{\Delta\text{Votes}}$	95% Confidence Interval for $\widehat{\Delta\text{Votes}}$
10am+	-0.030	1,814,602	-53,977	[-73,364, -34,590]
2pm+ (JNE)	-0.057	44,508	-2,544	[-3,967, -1,121]
10am+ and Voted Monday	–	1,865,978	-57,543	[-82,102, -38,205]

Note: exposure-weighted registered voters refers to $\sum_s h_s N_s$ in the estimation of missing voters, where h_s is treatment exposure and N_s is the number of registered voters at the voting site, s , level. Results report TWFE estimates where treatment assignment is based on any *mesa* opening late within a site. Combined 10am+ and Voted Monday rows sum the predicted foregone votes cast from the two constituent estimates, with uncertainty computed using a site-cluster bootstrap.

Table D.2.2: Estimates of Missing López Aliaga-Sanchez Margin from Delayed Openings (TWFE Dummy)

<i>TWFE (Dummy)</i>	(1)	(2)	(3)
	$\widehat{\beta}$	$\widehat{\Delta\text{Margin}}$	95% Confidence Interval for $\widehat{\Delta\text{Margin}}$
10am+	-0.030	-12,084	[-17,849, -7,393]
2pm+ (JNE)	-0.057	-272	[-521, -55]
10am+ and Voted Monday	–	-12,687	[-18,731, -8,101]

Note: $\widehat{\Delta\text{Margin}}$ estimates the change in the López Aliaga-Sanchez margin from delayed openings by combining the estimated turnout effect with the local counterfactual López Aliaga-Sanchez vote-share margin among untreated voting units. Results report TWFE estimates where treatment assignment is based on any *mesa* opening late within a site. Combined 10am+ and Voted Monday rows sum the predicted margin effects from the two constituent estimates, with uncertainty computed using a site-cluster bootstrap.

Appendix D.3 Evidence of Irregular Reporting in *Actas*

Figure D.3.1: Irregular *Actas*

MESA DE SUFRAGIO N.° **903212** **ACTA ELE**

DEPARTAMENTO **LIMA** PROVINCIA **LIMA**

A ACTA DE INSTALACIÓN
Siendo las 14:00 hrs. del 12 de abril de 2026, se conformó e instaló la Mesa de sufragio, continuando luego con la revisión del material electoral recibido. El material se recibió en buen estado: SI NO

CANTIDAD DE CÉDULAS DE SUFRAGIO RECIBIDAS
→ Doscientos ochenta y cinco **285**
(en letras) (en números)

OBSERVACIONES: SE INSTALÓ LA MESA A LAS 15:00 HRS Y SE PROCEDIÓ A SUFRAGAR A LAS 15:22 HRS.

(a) Irregular *Acta* Example 1

MESA DE SUFRAGIO N.° **045730** **ACTA ELE**

DEPARTAMENTO **LIMA** PROVINCIA **LIMA**

A ACTA DE INSTALACIÓN
Siendo las 4:30 pm. del 12 de abril de 2026, se conformó e instaló la Mesa de sufragio, continuando luego con la revisión del material electoral recibido. El material se recibió en buen estado: SI NO

CANTIDAD DE CÉDULAS DE SUFRAGIO RECIBIDAS
→ Trescientos **300**
(en letras) (en números)

OBSERVACIONES: hubo un error en la hora 4:30 pm, cuando la instalación fue a las 13:50 pm.

(b) Irregular *Acta* Example 2