The Positional Value and Redistributive Consequences of the Ballot Grid

Evidence from the Philippine Party-List Switch from an Alphabetical to a Randomized Ballot Grid

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Working paper – comments welcome

Abstract

The crowded ballots of modern democracies are grids, not lists. Where a candidate sits in the grid decides whether voters ever reach them, with real consequences for representation. We exploit the Philippine party-list election, whose four-column grid of up to 187 organizations was ordered alphabetically in 2010 and randomized in five elections since. The raffles identify the causal effect of grid position on vote share: moving from the tenth slot to the hundredth costs an organization 30 percent of its votes. Because positions vary by row and column, the grid reveals which scanning behavior produces the losses, something the single-column ballots of prior research cannot. Under the alphabet, the weakest organizations captured the best positions through strategic renaming; the raffle ended the capture and prevented its entrenchment. However, it still reallocated four to five seats every cycle to lucky drawers, suggesting a need to address ballot randomization's redistributive effects.

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1 Introduction

Democratic representation depends on more than what voters want. Between a voter's preference and a seat in the legislature stands a chain of rules and technologies. Electoral formulas and district magnitudes decide how votes become seats (Lijphart, 1994; Cox, 1997); registration and administrative rules decide who gets to vote at all (Rosenstone and Wolfinger, 1978); parties, campaigns, and the information environment shape which options voters see and what they know about them (Lupia and McCubbins, 1998); and after the voter is done, counting and canvassing technologies determine whether votes are counted as cast (Fujiwara, 2015; Callen and Long, 2015). This paper is about the link in this chain where the choice itself is made: the ballot, the piece of paper on which a preference becomes a vote.

A long line of research shows that the ballot is not a neutral instrument either. Candidates listed first receive more votes, especially when voters know little about their options (Bain and Hecock, 1957; Miller and Krosnick, 1998; Koppell and Steen, 2004; Ho and Imai, 2008; Meredith and Salant, 2013; Webber et al., 2014; Freeder, de Benedictis-Kessner and Bernhard, 2025), and voters who face many decisions on a long ballot begin to skip races and rely on shortcuts (Augenblick and Nicholson, 2016). Yet most democracies treat the ballot's layout as a minor administrative detail, and most research treats the ballot as a simple list, where the only design question is who comes first.

Both habits break down when the ballot grows crowded, and crowded ballots are common in the world's large democracies. Indonesian voters choose among parties whose ballot numbers are drawn by lot (Komisi Pemilihan Umum, 2022). Australian Senate voters in 2013 faced a ballot of forty-two party columns, so wide it was nicknamed the tablecloth (Australian Electoral Commission, 2025). Danish municipal voters face multi-column candidate grids (Blom-Hansen et al., 2016), and Polish local voters choose from lists that together carry thousands of names (Flis and Kamiński, 2022).[1] On ballots like these, position is no longer just a place in a sequence. An option sits in a row and a column, and where it sits decides whether a hurried voter ever reaches it. And the stakes are high because the margins are thin. In the proportional systems that produce such ballots, a party enters or misses the legislature over one or two percent of the vote, so the votes that come with a good position can decide who is represented at all. The ballot, a technology so simple it gets taken for granted, has real consequences for democratic representation.

The trouble begins with the default rule most democracies use to order their ballots: the alphabet. Because ballot order effects are well established, the top of the ballot is a known prize, and the alphabetical rule lets contestants compete for it through the one instrument it leaves in their hands – their names. Australian parties long stacked their tickets with early-alphabet surnames, the practice that gave "donkey voting" its name (Orr, 2002), and Spanish Senate candidates listed near the top of the alphabetical ballot ran consistently ahead of their own list-mates (Lijphart and López Pintor, 1988). The advantage thus accrues, election after election, to the small subset of contestants opportunistic enough to engineer their way to the top. And because ballot position helps decide who wins office, and holding office confers an advantage in the next election (Lee, 2008; Querubin, 2016), what begins as a quirk of voter cognition hardens into two distinct pathologies: the *capture* of good positions by the well-named, and their *entrenchment* across elections.

Election administrators around the world have converged on a remedy: take the choice away. A lottery severs the link between a contestant's name and its place on the ballot, so that no one can buy, engineer, or inherit a good position (Orr, 2002; Lijphart and López Pintor, 1988; Miller, 2010). But fairness in assignment is all it delivers. A randomized ballot still has a first position and a last one, and the voter who works through it still meets the

[1]Section 1 of the Supporting Information surveys these systems and their ordering rules.

options in some order. If position moves votes, the lottery does not shrink the advantage; it re-deals it.

In this paper, we argue that randomization cures the capture and the entrenchment but leaves the advantage itself intact, because the advantage originates in voters, not in rules. As the ballot grows from a short list into a crowded grid, voters lean harder on cognitive shortcuts. Theories of choice under scarce attention disagree about which shortcut they use: voters may *satisfice*, reading the options in order and stopping at the first acceptable one (Simon, 1955; Krosnick, 1991), scan the page in an F-shaped pattern (Nielsen, 2006; Pernice, 2017), or favor particular regions of the visual field (Jewell and McCourt, 2000; Tatler, 2007). All imply that some positions get reached and others do not, and nothing about a lottery changes this. The argument yields three testable hypotheses. First, position should continue to move votes even when a lottery assigns it, and the spatial pattern of the losses should reveal which shortcut voters use. Second, randomization should end the capture but not the advantage: on every draw, the prize should pass undiminished to that raffle's lucky few. Third, the redrawn lottery should end entrenchment, yet each cycle should still crown its advantaged few, with consequences for who is represented.

We put these hypotheses to the test in the Philippine party-list election, which fills a fifth of the House of Representatives and has operated a crowded ballot under both ordering rules. In each of the six automated elections from 2010 through 2025, between 115 and 187 organizations competed for a single nationwide vote on a four-column, cue-free ballot grid. The 2010 election listed organizations alphabetically. Every election since 2013 has assigned positions by public raffle, drawn anew each cycle. The grid lets the competing shortcut theories make different predictions, the repeated raffle identifies the causal effect of position within organizations, the rule change supplies the before-and-after comparison the capture hypothesis requires, and linked rosters across all six elections track persistence. From official ballot facsimiles and canvasses we assemble each organization's exact grid coordinates and votes, and balance tests confirm the raffles were random in practice.

Our first finding is that position moves votes even when a lottery assigns it, and that the pattern of the losses shows voters reading the grid in order and stopping early. Comparing each organization to itself as successive raffles moved it around the ballot, we estimate an elasticity of vote share with respect to ballot rank of -0.16: an organization moved from the tenth slot to the hundredth loses roughly 30 percent of its votes. The effect appears in every raffle, and we cannot reject that it is the same across draws spanning twelve years. The losses fall exactly where satisficing predicts and where the rival scanning theories say they should not. The effect is also strikingly uniform: well-known and obscure organizations, rich and poor municipalities, all lose at the same rate.

Our second finding is that the alphabetical default had indeed been captured, and that randomization ended the capture without retiring the prize. On the 2010 ballot, 115 of 187 slots carried names beginning with a digit or "A," and the organizations that renamed themselves this way were roughly a quarter weaker than the rest once the causal position effect is netted out. In short, the default rule subsidized the weakest entrants. Randomization ended the naming competition, and engineered names fell from over two thirds of the printed roster to under a third by 2025. The prize, however, remained: feeding position-purged vote shares through the statutory seat formula, the draw moved four to five seats in every cycle, twenty-four seats over the five raffled elections. In other words, the lottery did not shrink the prize but changed who wins it.

Our third finding is that a lucky draw does not harden into lasting advantage. Organizations that drew well in one raffle do no better in the next, so chance does not compound. The one nuance is survival: organizations that draw badly are less likely to contest the next election at all. Because the draw is repeated, no organization can hold the good positions for long. In a polity where political advantage is otherwise heritable to an unusual degree (Querubin, 2016), randomization does not neutralize position advantages, but it does prevent their entrenchment.

Our paper makes three contributions. To the study of voter cognition, we provide what the single-column literature cannot: a research design that separates competing models of how voters scan a ballot. When Miller and Krosnick (1998) attributed name-order effects to satisficing, the attribution had to rest on theory, because on a list every scanning model predicts the same decline. The grid supplies the missing test, and satisficing passes it. To the study of electoral institutions, we document that randomization redistributes position advantages rather than removing them, quantify the redistribution in seats, and show that what randomization does accomplish is preventing their accumulation. To the political economy of reform, the shift from alphabet to raffle offers a before-and-after picture of an electoral rent being taken away: the rent was held by the weakest organizations, paid for with their names, and reassigned to chance after the reform.

Beyond the ballot, the findings speak to any setting in which many options compete for scarce attention on an administratively ordered list: school-choice menus, judicial retention ballots, referendum bundles, digital marketplaces. The Philippine case shows that position advantages in such settings are large, persistent, and uniform, and that randomizing the list solves a fairness problem while leaving the attention problem intact.

The paper proceeds as follows. Section 2 describes the party-list system and its two ordering rules. Section 3 develops the scanning models, Section 4 the data, and Section 5 the econometric framework. Section 6 reports the results in hypothesis order. Section 7 concludes with the implications for ballot design and representation.

2 The Party-List Election and Its Two Ordering Rules

2.1 The party-list system

The Philippine House of Representatives combines single-member plurality districts with a nationwide party-list tier. The 1987 Constitution reserves twenty percent of House seats for party-list representatives. The entire country votes as a single at-large district, and each voter casts exactly one party-list vote. Seats are allocated by a statutory formula: organizations surpassing two percent of the national party-list vote are guaranteed a seat, no organization may hold more than three, and since a 2009 Supreme Court ruling all reserved seats must be filled, which extends seats to organizations well below the threshold in descending order of votes.[2]

Two features of this system matter for our design. First, because the contest is a single nationwide district, every voter in the country faces the identical party-list ballot section: the same organizations, in the same order, in the same physical arrangement. Position on the ballot is therefore a national-level treatment, assigned once per organization per election. This distinguishes our setting from those in which ballot order rotates across precincts or districts (Ho and Imai, 2008; Koppell and Steen, 2004). Second, the two-percent threshold and the three-seat cap generate sharp seat discontinuities at low vote shares. Even small position-induced shifts in vote share can therefore be pivotal for representation.

2.2 A crowded, low-information electoral market

The party-list ballot is extraordinarily crowded, and voters process it with limited information. Between 115 and 187 organizations appeared on the ballot in the six elections we study, most of them small: in 2022 the median organization received about a third of one percent of the vote, and no organization in any election received more than 9.3 percent (Table 1). Ideological differentiation is weak. Many groups are sectoral franchises, celebrity vehicles, or local machines with national labels, and watchdog groups have documented organizations

[2]1987 Constitution, art. VI, §5; Republic Act No. 7941 (Party-List System Act of 1995), §§11–12; *BANAT v. COMELEC*, G.R. Nos. 179271 and 179295, April 21, 2009. Section 2 of the Supporting Information compiles the full legal and administrative record, including the accreditation litigation that shaped each roster.

functioning as fronts for established political families.[3] Voters meet this menu near the end of a very long ballot, and invalid or blank party-list votes ran between a fifth and two fifths of turnout. These are precisely the conditions under which choice architecture should matter most: many options, weak priors, and high search costs (Miller and Krosnick, 1998; Augenblick and Nicholson, 2016).

2.3 Automated elections and the physical ballot

The 2010 election was the Philippines' first fully automated national election, and automation is what makes our design possible. Voting shifted to precinct-count optical scan machines. Each voter receives a single pre-printed paper ballot, shades ovals beside chosen candidates, and feeds the ballot to a scanner at the precinct.[4] Three consequences follow. First, the ballot is standardized. The machine-readable form has fixed typography, fixed cell sizes, and no party logos or photographs, so the party-list section is, visually, a dense matrix of near-identical entries. Second, because the ballot must fit the scanner's paper path, the party-list contest cannot run as one long column. All six ballots set it as a four-column grid, with the sequential ballot numbering running down the leftmost column first and then down each successive column. Reading order on this ballot is therefore column-major, a fact our tests of the scanning theories exploit throughout. Third, the Commission on Elections (COMELEC), the country's election administrator, publishes facsimile ballots before each

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[3] Section 2 of the Supporting Information provides sources.

[4] Republic Act No. 8436 (1997), as amended by Republic Act No. 9369 (2007). The same optical-scan technology, with updated hardware in later cycles, was used in every election through 2025. Section 2 of the Supporting Information details the statutes and the hardware succession.

election, which lets us reconstruct each organization's exact grid coordinates. Section 3 of the Supporting Information reproduces the party-list section of all six ballots.

The overall ballot is long: presidential-year ballots (2010, 2016, 2022) place the party-list section after the presidential, vice-presidential, and senatorial contests, and midterm ballots drop only the top two, so the party-list decision always arrives late, after many prior choices (Augenblick and Nicholson, 2016).

2.4 Two ordering regimes: the alphabet and the raffle

Through the 2010 election, organizations appeared on the ballot in alphabetical order of their registered ballot name, with numerals sorting ahead of letters. The ballot name is chosen by the organization itself, subject to COMELEC approval, so under this rule an organization's position followed from a choice the organization controlled.

Beginning with the 2013 election, COMELEC severed the link between names and position by publicly raffling ballot order, explicitly to deny any advantage to groups whose names begin with "1" or "A."[5] The raffle assigns each accredited organization a number, and the numbers map deterministically onto grid positions through the column-major layout. COMELEC has drawn the raffle anew in every election since, so the five draws of 2013 through 2025 are mutually independent. Because each raffle is conducted over a set of accredited organizations fixed before the draw, an organization's position is exogenous to its quality, resources, and prior performance. The balance tests in Section 5 of the Supporting Information verify this claim draw by draw.

One administrative detail deserves note. Party-list accreditation is contentious, and

[5]COMELEC Resolution No. 9591 (2012) set the rules for the first raffle. Section 2 of the Supporting Information documents each cycle's raffle resolution and disposition, including groups added to the ballot by court order after a draw.

Table 1: Party-list ballots, 2010–2025

	2010	2013	2016	2019	2022	2025
Election type	Pres.	Midterm	Pres.	Midterm	Pres.	Midterm
Ordering regime	Alpha.	Raffle	Raffle	Raffle	Raffle	Raffle
Groups printed on ballot	187	123	115	134	177	155
Groups with canvassed votes	187	113	115	134	176	154
Ballot-number range	1–187	1–136	1–115	1–181	1–178	1–156
Gaps in printed numbering	0	13	0	47	1	1
Grid (columns × max rows)	4 × 47	4 × 31	4 × 29	4 × 34	4 × 45	4 × 39
Party-list votes (millions)	30.09	27.69	31.31	27.46	36.80	41.95
Invalid/blank party-list (%)	21.0	31.0	30.4	41.5	33.7	26.9
Median vote share (%)	–	0.56	0.59	0.47	0.32	0.38
Maximum vote share (%)	5.06	4.59	5.22	9.31	5.74	6.63
Seats allocated	57	58	59	61	63	64

Notes: Elections held on May 10, 2010; May 13, 2013; May 9, 2016; May 13, 2019; May 9, 2022; and May 12, 2025. Sources: official COMELEC sample ballots (grid structure, our transcription) and official COMELEC canvass and turnout statistics (votes, seats). Section 2 of the Supporting Information gives full sources. Invalid/blank shares are one minus party-list votes over voters who actually voted. The 2016 and 2019 totals are our constructed party-list totals (Section 4 of the Supporting Information); other years use official canvass totals. Vote shares are percentages of all party-list votes cast, including those of printed-but-uncanvassed organizations; the 2010 median is omitted because the 187-organization roster mechanically depresses it.

litigation often continues past ballot printing, so the set of organizations printed on each ballot differs somewhat from the set that entered the raffle and from the set whose votes were canvassed (Table 1). Section 2 of the Supporting Information records these cases election by election, and Section 4 describes how we handle them. None of this churn is related to the raffle draw.

3 Ballots as Choice Architecture under Scarce Attention

Ballot position features prominently in the study of voter behavior. A long line of research, running from the founding study of Bain and Hecock (1957) to the present, documents that candidates listed first receive more votes, particularly in races where voters know little about their options (Miller and Krosnick, 1998; Koppell and Steen, 2004; Ho and Imai, 2008; Meredith and Salant, 2013; Webber et al., 2014; Freeder, de Benedictis-Kessner and Bernhard, 2025). The canonical interpretation is cognitive: voters facing many unfamiliar options satisfice, evaluating alternatives in the order encountered and stopping at the first acceptable one (Simon, 1955; Krosnick, 1991), with fatigue compounding the tendency as the number of prior decisions grows (Augenblick and Nicholson, 2016). Implicitly, however, this literature conceptualizes the ballot as a one-dimensional list: the treatment is ordinal rank, and the prediction is monotone decay. That conceptualization fits the ballots most often studied, a handful of candidates presented in a single column. It becomes strained once ballots grow crowded enough that election administrators must typeset them as multi-column grids. On a grid, two options with adjacent ranks may sit in different regions of the visual field, and two options in the same visual region may hold distant ranks. Position becomes spatial as well as ordinal. Only a small literature has engaged this two-dimensional reality (Blom-Hansen et al., 2016), and none, to our knowledge, in a setting where the grid coordinates of every option are randomly assigned.

We build on these literatures and conceptualize the crowded ballot as choice architecture processed under scarce attention. A voter confronting well over a hundred options, set in near-identical typography without logos, photographs, or informative cues, cannot evaluate them all. Most options are unfamiliar, and many voters approach the choice with weak preferences or none at all. In such an environment the binding constraint on choice is not preference but exposure: an option can only be chosen if the voter's scan path reaches it before search terminates. The voter's scanning strategy therefore determines the distribution of electoral attention across ballot positions. Where the eyes enter the grid, in what direction they travel, and when they stop decide which options are ever evaluated. Randomization of positions turns that distribution into an estimable object. The theoretical payoff is that competing models of visual search, which are observationally equivalent on short single-column ballots, make distinct predictions about the spatial pattern of vote shares across a randomized grid.

3.1 Scanning models and their spatial signatures

We organize the theoretical possibilities into six families, each drawn from an established literature in cognitive psychology, survey methodology, or human–computer interaction. Throughout, northing denotes an option's row within its column (1 = top), easting its column (4 = leftmost, 1 = rightmost), and ballot number its printed sequential number, which runs down the leftmost column first (column-major order). Table 2 summarizes each family's signature. The table lists the two lateral-bias models on separate rows and adds one auxiliary pattern, recency, in which evaluation recovers for the last options scanned rather than declining throughout (Murdock, 1962). We test it alongside the six families. Figure 1 draws the six families' predictions on a stylized grid, using the same shading convention as the empirical heatmaps of Section 6, so that the competing predictions can be compared to the data at a glance.

Ordinal primacy (serial search). If voters process options in the printed sequence and evaluation propensity declines with each option scanned (Murdock, 1962; Carney and Banaji, 2012), vote share decays in ballot number, and spatial coordinates carry no information beyond it: decay down every column and across columns left to right, with equal within-column slopes.

Satisficing with early stopping (convex primacy). If, in addition, voters stop scanning once an acceptable option is found or attention is exhausted (Simon, 1955; Krosnick, 1991; Augenblick and Nicholson, 2016), decay is convex in ballot number: steepest at the beginning of the sequence and flattening thereafter, which spatially concentrates the down-column penalty in the leftmost column and mutes it in later ones. We emphasize this signature because it is easily mistaken for a spatial-attention pattern while being purely ordinal in origin.

F-pattern spatial scanning. Eye-tracking research on how people skim dense, unformatted text finds fixations concentrated along the top rows and down the left edge, the

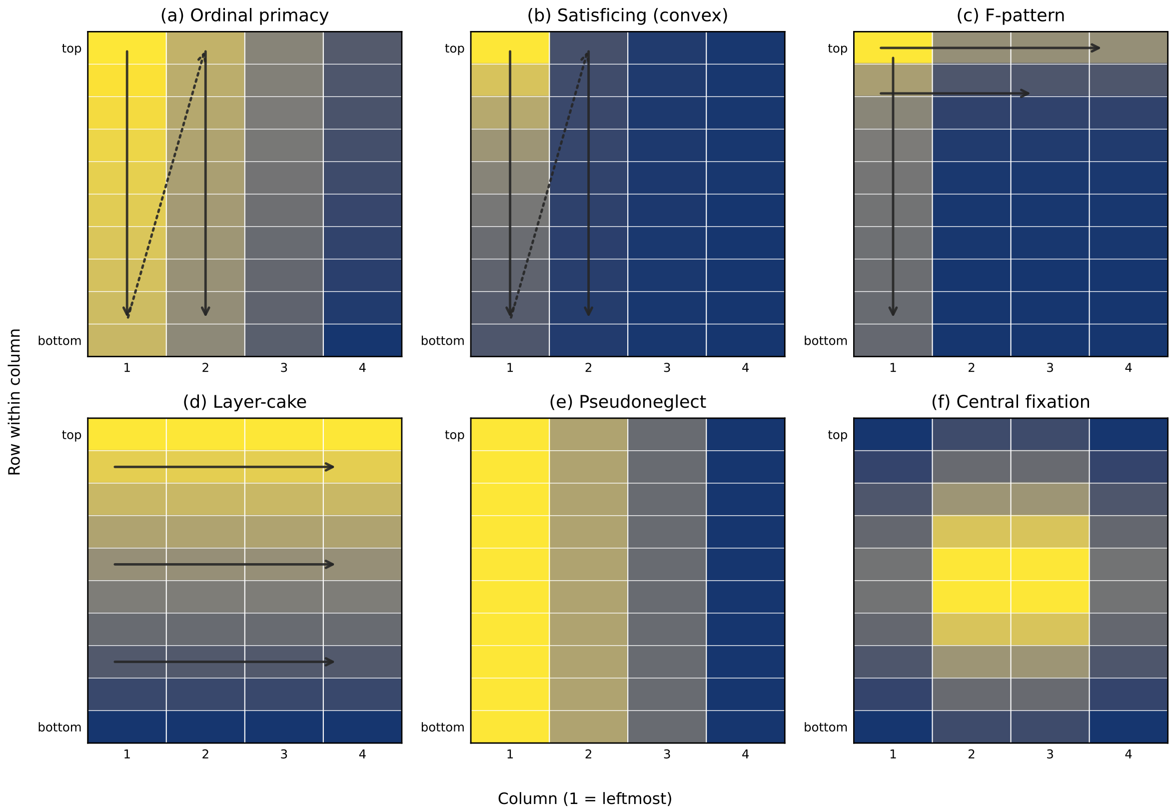

Figure 1: Stylized spatial predictions of the six scanning families on a four-column grid. Lighter cells receive more votes; arrows trace scan paths. Panels (b) and (c) disagree most sharply in the leftmost column: satisficing concentrates the down-column penalty there; the F-pattern's stem protects it.

"F-pattern" (Nielsen, 2006; Pernice, 2017). Treating the ballot as a visual field rather than a sequence, this model predicts a down-column penalty that is weakest in the leftmost column, where the F's stem sustains attention, and strongest toward the right: the opposite interaction from convex primacy, with upper rows favored in all columns.

Row-wise scanning (layer-cake). If voters instead sweep horizontally across rows (Pernice, 2017), vertical position dominates: vote share declines in northing, with no independent effect of easting and no northing-by-easting interaction.

Lateral biases and central fixation. Visuospatial attention exhibits a small systematic leftward bias (pseudoneglect), and initial fixations gravitate toward the center of a visual field (Jewell and McCourt, 2000; Tatler, 2007). These predict, respectively, a leftward main

Table 2: Spatial signatures of competing scanning models

Model	Ballot number	Northing × Easting	Other signature
Ordinal primacy	linear decay	none (absorbed by number)	equal within-column slopes
Convex primacy	convex decay	down-penalty steepest left	flattening in number
F-pattern	–	down-penalty weakest left	top rows favored in all columns
Layer-cake	–	none	northing only; no easting effect
Pseudoneglect	–	none	easting main effect only
Central fixation	–	–	inverted-U in both dimensions
Recency	U-shape	–	recovery at column/list ends
Directed search	≈0 for salient parties	–	effects concentrate among obscure

Notes: Northing denotes an option's row within its column (1 = top); easting its column (4 = leftmost); ballot number the printed sequential number, which runs in column-major order. Entries marked "–" indicate that the model makes no prediction for that dimension. Recency is an auxiliary pattern tested alongside the six families.

effect without vertical structure, and an inverted-U advantage for middle rows and columns.

Directed search. Finally, voters may arrive with a chosen option and search for it by name or by a memorized ballot number, as when campaigns advertise their drawn number in advance of the election. Position effects should then be near zero for options with salient brands or the resources to advertise, concentrating instead among obscure options chosen by voters who arrive undecided (cf. Ho and Imai, 2008; Pirolli and Card, 1999).

3.2 Hypotheses

Given these considerations, we derive the three hypotheses previewed in the introduction, now stated in testable form.

1. **[Hypothesis 1]:** *Ballot position affects vote shares even when positions are randomly assigned, and the spatial pattern of the effects reveals the scanning model.*
   a) Randomly assigned position variables jointly predict vote shares under random assignment, and within organizations, moving to a better position across successive draws increases vote share.
   b) The spatial signature of the estimated effects discriminates among the scanning models of Table 2, using the specifications developed in Section 5.
   c) If scarce attention is the mechanism, effects should concentrate where information is scarce: among options without prior office, prior ballot presence, or high prior

vote shares, and in localities with lower education, income, and urbanization.

2. **[Hypothesis 2]:** *Under the alphabetical rule the position advantage was captured through names; randomization redistributes the advantage without removing it.*
   a) The position–vote gradient under alphabetical ordering exceeds the causal gradient identified under randomization. The excess identifies the selection premium of lexical engineering.
   b) Strategic naming decays once randomization severs names from positions.
   c) Under every draw, the causal position effect continues to move votes, and through the seat-allocation rule, seats.
3. **[Hypothesis 3]:** *Randomization prevents the entrenchment of position advantage.* Because draws are independent across elections and the attention mechanism operates at the moment of choice, a favorable position confers no durable asset. Persistence could still arise indirectly, through office-holding or campaign resources won by a lucky draw (Lee, 2008), so the hypothesis is an empirical claim rather than a logical necessity.
   a) A favorable draw in one election does not raise an organization's vote share in the next.
   b) Any persistence operates through survival: organizations that draw badly are less likely to contest the next election.

3.3 Alternative explanations and scope

While the logic of the scanning framework is appealing, two other explanations could plausibly generate position–vote correlations, and our design must confront both. The first is mechanical: with a hundred-plus options and shares that sum to one, chance alone will produce some association between positions and votes in any single draw. We address this with randomization inference built on the actual draw-over-slots structure and, more powerfully, with replication across independent draws and within-organization re-randomization: a chance pattern in one election will not survive repeated draws. The second is a nonattentional channel: where ballot numbers are printed and campaigned on, a low or memorable number may be advertised more effectively regardless of where the eyes wander. This number-memorability channel operates through campaign communication rather than in-booth scanning, and campaigns do demonstrably reallocate their efforts in response to drawn positions (Gulzar, Robinson and Ruiz, 2022). But it predicts effects of ballot number conditional on grid position, concentrated among organizations with the resources to advertise. That prediction is nearly the mirror image of Hypothesis 1c. Distinguishing revealed attention from advertised numbers is therefore an empirical task for our heterogeneity tests rather than an identifying assumption.

A final word on scope. We deliberately study the extreme case: the most crowded, least cue-rich ballot for which randomized position data exist. This is the environment where attention-based theories claim their strongest jurisdiction. Failure here would travel farther than success. The institutional-design implications of what we find, and the remedies available, are taken up in Section 6.5 and the conclusion.

4 Measuring Positions, Votes, and Organizations

4.1 Ballot layouts and position variables

We measure each organization's ballot position from the official sample ballots COMELEC published for the six elections, transcribing the party-list section of each into a slot-level dataset that records every printed entry's ballot number, printed name, and grid coordinates. Section 3 of the Supporting Information reproduces the six party-list sections as printed.

Because the grid is read in column-major order (Section 2), we define three position variables for organization i in election t. Sequence rank, $r_{it} \in \{1, \ldots, K_t\}$, is the organization's position in the printed reading order, where K_t is the number of printed slots (Table 1). Row, n_{it}, is the organization's row within its column (1 = top; columns hold 28–47 rows depending on the election). We scale row to $\tilde{n}_{it} = (n_{it} - 1)/(N_{ct} - 1) \in [0, 1]$, where N_{ct} is the length of column c in election t, so that after scaling 0 denotes the top of a column and 1 the bottom. Column, $e_{it} \in \{1, 2, 3, 4\}$, indexes horizontal position, coded so that larger values are further left, the direction of earlier ranks. Because grids differ in size across elections, scaled variables are used whenever elections are pooled.

Ballot numbers and sequence ranks are not identical, because post-raffle disqualifications leave gaps in the printed numbering. Some printed organizations, moreover, were never canvassed or had their registrations cancelled after the election. Our position variables count all printed slots, and our outcome regressions exclude the uncanvassed organizations' vote observations. Section 4 of the Supporting Information details both issues and shows that the exclusion is conservative.

4.2 Electoral returns

Our outcome is the organization's share of all party-list votes cast, the quantity COMELEC itself uses for seat allocation. For 2010, 2013, 2022, and 2025 we take national vote totals from the official canvass. For 2016 and 2019 we additionally construct municipal-level returns covering more than 1,600 municipalities per election. These aggregate to within 1 to 4 percent of official national totals and permit the municipality-level analyses reported below.[6] Table 1 reports the resulting vote-share distributions: the median organization receives roughly half of one percent of the party-list vote, and the largest between 4.6 and 9.3 percent. Throughout, the outcome in estimation is $y_{it} = \ln s_{it}$, where s_{it} is organization i's share of party-list votes in election t. The logarithm both respects the heavy right skew of shares and gives coefficients the interpretation of proportional effects.

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[6]Section 4 of the Supporting Information documents the construction and the validation against official totals.

4.3 Municipal characteristics

For the municipality-level analyses, we draw municipal covariates from the 2007 and 2010 municipal census aggregates assembled in the replication materials of Ravanilla (2025): educational attainment shares, total municipal income, population, land area, household structure, and an indicator for large multidistrict cities. We use the 2010 values, measured before the first raffle and therefore predetermined with respect to every position draw we study. Matching by province and municipality name links 1,632 of the 1,644 municipalities in the returns (99.3 percent). The unmatched few are small municipalities created after 2007 or absent from the census files. The resulting organization-by-municipality panel for 2016 and 2019 contains 396,854 observations, and aggregating it back to national totals reproduces the national estimates almost exactly, a check we report with the results.

4.4 Organization characteristics and cross-election linkage

To measure predetermined characteristics and to follow organizations across elections, we link the six rosters by organization. Linkage uses printed ballot names, official registered names, and documented renamings (for example, Ako Bicol contested as AKB in 2010 and 2013 and as AKO BICOL thereafter). We verify each cross-election match against official results and, where names alone are ambiguous, against contemporaneous accreditation lists. The resulting panel contains between 64 and 87 organizations with canvassed votes in both elections of each consecutive raffled pair, 180 organizations in at least two raffles, 74 in three or more, and 24 in all five. Thirty-eight organizations appear in every election from the alphabetical 2010 benchmark through 2019, and twenty-two span all six.

For each organization-election we record: prior-election vote share (zero if absent from the prior ballot); an indicator for having appeared on the prior ballot; an indicator for holding a seat in the outgoing Congress, constructed from official seat allocations of the preceding election; and printed-name characteristics (leading character class and length), which matter under alphabetical ordering and serve as placebo characteristics under the raffle.

5 Econometric Framework

The raffle randomly assigned each organization's grid position in each of the five elections from 2013 through 2025, and the draws are mutually independent. Within an election, differences in vote shares across positions are therefore causally attributable to position. Across elections, re-randomization permits within-organization designs that difference away all time-invariant organizational characteristics. The specifications below map onto the hypotheses of Section 3.

5.1 Cross-sectional specifications (Hypothesis 1a)

Within each raffled election, we estimate variants of

$$y_{it} = \alpha_t + \beta_1 \, \tilde{n}_{it} + \beta_2 \, e_{it} + \beta_3 \, (\tilde{n}_{it} \times e_{it}) + \epsilon_{it}, \tag{1}$$

with variables as defined in Section 4 and α_t an election intercept. The signatures of Section 3 map onto the coefficients as follows. Convex ordinal primacy concentrates the downward penalty where the earliest ranks sit, in the leftmost column, implying $\beta_3 < 0$ together with a leftward level advantage, $\beta_2 > 0$. F-pattern scanning protects the left edge, implying $\beta_3 > 0$. Layer-cake scanning implies $\beta_1 < 0$ with $\beta_2 = \beta_3 = 0$, and pseudoneglect implies $\beta_2 > 0$ with $\beta_1 = \beta_3 = 0$.

Ordinal theories are estimated directly on the sequence rank:

$$y_{it} = \alpha_t + \gamma_1 \ln r_{it} + \epsilon_{it}, \tag{2}$$

where γ_1 is the elasticity of vote share with respect to rank. A quadratic in scaled rank tests convexity directly, and quadratics in $\tilde{n}_{it}$ and e_{it} added to equation (1) test central fixation and recency. Two further tests separate ordinal from spatial accounts at the one place they disagree: an indicator for the top rows of columns two through four, for which spatial scanning predicts a premium and ordinal scanning none, and a test of whether column indicators add explanatory power once rank enters flexibly, which they should not under ordinal scanning. Finally, because post-raffle disqualifications drove printed numbers and sequence ranks apart, including both in equation (2) would separate scanning from advertised numbers. In practice the two remain nearly collinear (Section 4 of the Supporting Information), so we treat that regression as suggestive.

5.2 Model comparison (Hypothesis 1b)

Because the scanning models imply non-nested functional forms, within-sample fit is weak grounds for choice. Our primary criterion is leave-one-election-out prediction: we fit each model on four raffled elections and evaluate its predictions on the fifth, cycling through all five. A scanning model worth generalizing should predict a raffle it has not seen. Vuong-style non-nested comparisons within elections complement this criterion (Vuong, 1989).

5.3 Within-organization designs (Hypothesis 1a)

For organizations contesting consecutive raffled elections, first-differencing eliminates all time-invariant characteristics:

$$\Delta y_i = \delta + \gamma^{FD}\,\Delta \ln r_i + \Delta\epsilon_i, \tag{3}$$

where $\Delta \ln r_i$ is exogenous because the later position is drawn by an independent raffle. For organizations contesting three or more raffled elections, we estimate

$$y_{it} = \mu_i + \alpha_t + \sum_{\tau} \gamma_\tau \left(\ln r_{it} \times \mathbf{1}[t = \tau]\right) + \epsilon_{it}, \tag{4}$$

with organization fixed effects μ_i, so the election-specific elasticities γ_τ are identified solely from within-organization movement across raffles. A joint test of $\gamma_{2013} = \cdots = \gamma_{2025}$ asks whether the position effect changed as the electorate gained experience with the raffled ballot. Comparing the fixed-effects estimates with the year-by-year cross-sections validates each design against the other's threat, between-organization heterogeneity in one case and survivorship selection in the other.

5.4 Heterogeneity (Hypothesis 1c)

Scanning and campaign-communication accounts part ways on who is exposed. We estimate

$$y_{it} = \alpha_t + \gamma_1 \ln r_{it} + \gamma_2 M_{it} + \gamma_3 (\ln r_{it} \times M_{it}) + \epsilon_{it}, \tag{5}$$

where M_{it} is, in turn, an indicator for first-time organizations, an indicator for holding a seat in the outgoing Congress, and prior-election vote share. If attention substitutes for information, position matters most where voters know least: $\gamma_3 < 0$ for first-timers and $\gamma_3 > 0$ for incumbents and high prior shares. The number-memorability account predicts the opposite pattern, and uniform sequential scanning predicts $\gamma_3 = 0$ throughout. For 2016 and 2019 we estimate the municipal analogue of equation (5), replacing M with predetermined 2010 census characteristics and including organization-election and municipality-election fixed effects, with standard errors clustered by organization-election, the level of treatment.

5.5 The alphabetical benchmark (Hypothesis 2a)

Positions on the 2010 ballot were chosen through names rather than assigned, so its cross-section identifies not a causal effect but the equilibrium relationship between position and votes under sorting. Estimating equation (2) on the 2010 roster yields the alphabetical-regime gradient γ^A, and Hypothesis 2a predicts $\gamma^A < \gamma^{FE} < 0$, where γ^{FE} is the pooled within-organization elasticity from equation (4). The gap measures the sorting premium. A complementary decomposition nets position out of 2010 outcomes: the residual $\hat{q}_i = y_{i,2010} - \hat{\gamma}^{FE} \ln r_{i,2010}$ is a position-adjusted quality index, and comparing it between the lexically engineered digit-and-"A" block and the remainder of the roster tests whether selection into early slots was adverse on quality. Section 6.4 reports both tests.

5.6 Persistence (Hypothesis 3)

Independence of successive draws makes persistence directly testable. For organizations contesting consecutive raffled elections, we regress vote share on the previous election's position, conditional on the current position. Under Hypothesis 3a the earlier draw carries no weight. For Hypothesis 3b, we estimate whether the earlier draw predicts contesting the next election at all. Because the earlier position is randomly assigned, both regressions estimate causal effects of a lucky draw.

5.7 Inference

The treatment is drawn once per organization per election, 704 printed draws across the five raffles, so all main estimation is at the organization-election level and inference respects the randomization directly: for every reported statistic we compute randomization-inference p-values by re-drawing placebo assignments to the realized slot structure 5,000 times, permuting within election when pooling. These are exact under the sharp null. For magnitudes we also report HC1 intervals, and cluster-robust intervals by organization in the fixed-effects design. The identifying assumption is verifiable: predetermined characteristics do not predict position in any raffle, and successive draws are mutually uncorrelated (Section 5 of the Supporting Information). Because vote share is itself an outcome of the earlier raffle, all balance tests are level-on-level, never changes on changes.

6 Empirical Results

Our goal in this section is to establish what a ballot position is worth, how voters generate that value, and who collects it under each ordering rule. The raffles do the identifying work:

five independent draws are five natural experiments, and because many organizations contest repeatedly, re-randomization adds a sixth, within-organization design that differences away everything an organization carries across elections. The subsections test the hypotheses in order: whether randomized position moves votes (Section 6.1), which scanning model produces the losses (Section 6.2), whether the effect concentrates where information is scarce (Section 6.3), who held the advantage under the alphabet (Section 6.4), what the draw is worth in seats (Section 6.5), and whether luck persists (Section 6.6).

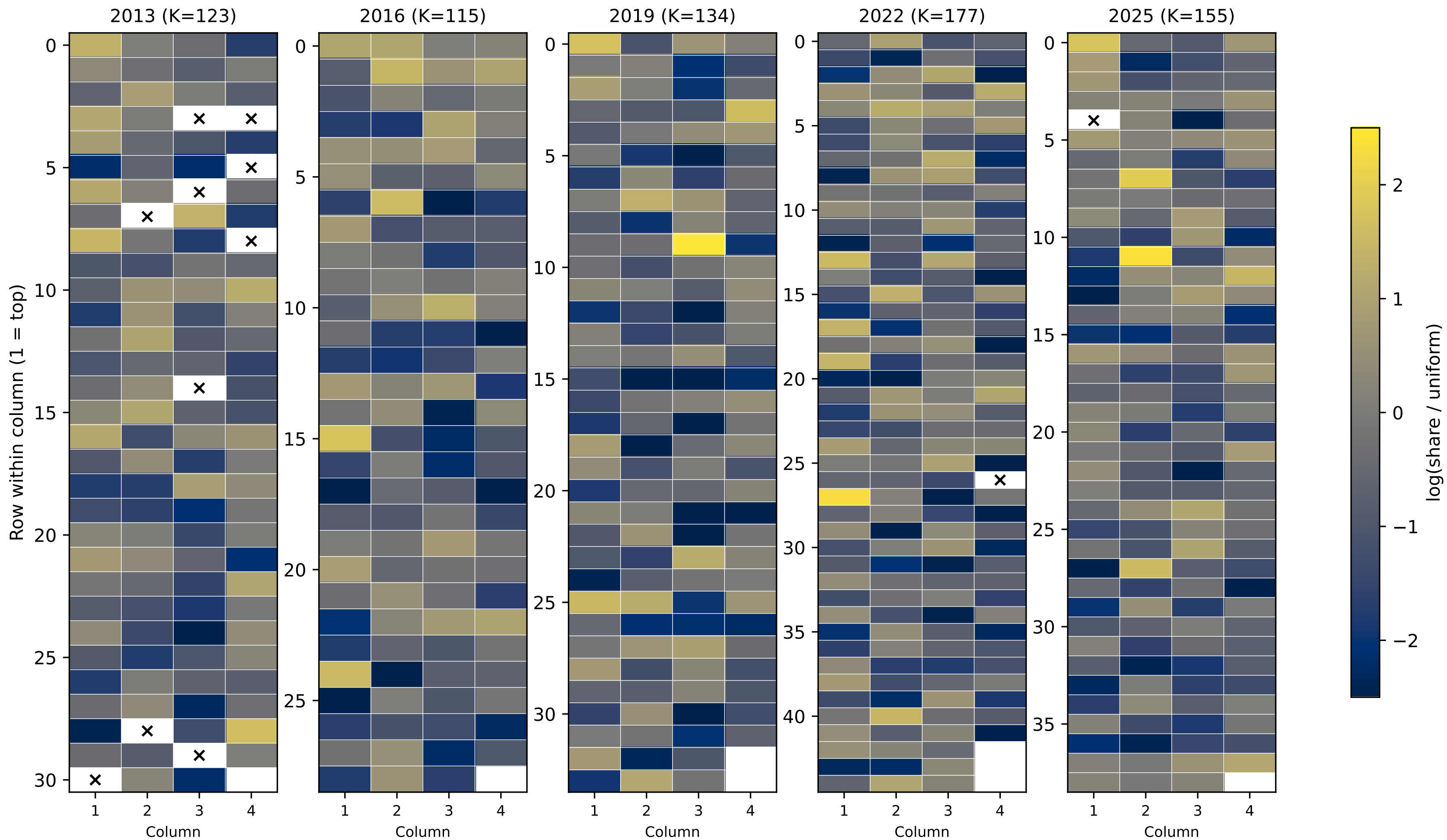

Figure 2: Party-list vote share by ballot grid position, five raffled elections. Cells show log(vote share relative to uniform); lighter cells receive more votes; crosses mark printed-but-uncanvassed slots; blank cells lie beyond the last printed slot.

6.1 Does a lottery neutralize position? (Hypothesis 1a)

The lottery does not neutralize position: randomly assigned position moves votes, by a lot and by the same proportion every time. Comparing each organization to itself as successive raffles moved it around the ballot, the elasticity of vote share with respect to sequence rank is -0.155 (se 0.046; RI $p < 0.001$): an organization moved from the tenth slot to the hundredth loses roughly 30 percent of its votes. The effect appears in every raffle, and we cannot reject a common elasticity across draws spanning twelve years ($p = 0.84$).

Table 3 shows how the estimate is built. Column (1) pools all five raffled cross-sections with election fixed effects: the rank elasticity is -0.131 (cluster-robust se 0.044; RI $p = 0.005$). Column (2) restricts the sample to the 180 organizations contesting at least two raffles and shows a noisier, somewhat smaller cross-sectional estimate. Column (3) adds organization fixed effects, so that the elasticity is identified solely from the raffle moving the same organization across elections, and the estimate strengthens to -0.155. Column (4) restricts to the 74 organizations randomized three or more times and finds -0.206 (se 0.057; RI $p < 0.001$). The pattern is exactly what unbiased but noisy cross-sections imply: purging between-organization heterogeneity does not shrink the estimate, it sharpens it. The raw data show the same thing. Figure 2 displays each organization's vote share, relative to uniform, at its raffled grid position: in 2013, vote shares visibly concentrate in the upper rows and leftward columns, the region scanned earliest in reading order, and the pattern fades in later elections as between-organization noise grows.

Two corroborations complete the picture. If an organization's next raffle moves it deeper into the ballot it should lose vote share, and it does: the first-difference estimates are negative in all four consecutive-election pairs (-0.180, -0.230, -0.082, and -0.059 from 2013$\rightarrow$2016 through 2022$\rightarrow$2025). And no single draw flips the sign of the election-specific elasticity: -0.243, -0.238, -0.199, -0.103, and -0.247 for 2013 through 2025.

The cross-sections tell the same story where power permits. The joint randomization-inference tests, reported in Section 6 of the Supporting Information, are strong in 2013

Table 3: Ballot position and vote share under randomization

	(1)	(2)	(3)	(4)
ln sequence rank	-0.131^*	-0.106^*	-0.155^*	-0.206^*
	(0.044)	(0.049)	(0.046)	(0.057)
	[0.005]	[0.035]	[<0.001]	[<0.001]
Election fixed effects	Yes	Yes	Yes	Yes
Organization fixed effects	No	No	Yes	Yes
Sample (raffles contested)	All	≥ 2	≥ 2	≥ 3
Organizations	367	180	180	74
Observations	692	505	505	293

Notes: Outcome: log share of party-list votes, organization-election level, five raffled elections (2013–2025). Cluster-robust standard errors by organization in parentheses; randomization-inference p-values in brackets (5,000 permutations of positions within each election). $^*p < 0.05$, based on randomization-inference p-values.

(RI $p = 0.002$; $R^2 = 0.21$) and insignificant thereafter, as between-organization differences in baseline popularity dwarf the position effect. Figure 3 shows the fading is a matter of power: cross-sectional elasticities are negative in every election and sit close to the within-organization estimates throughout. The agreement defuses the panel's one vulnerability, survivor selection, since estimates identified by persistent organizations match the full roster.

One election deserves transparency. 2013 is the only cross-section where position variables are jointly significant on their own, and the sharpest spatial patterns below come from it. The headline estimate does not depend on it: dropping 2013 yields a pooled within-organization elasticity of -0.135 (se 0.056), still significant at the five percent level, and dropping any other single election yields estimates between -0.123 and -0.205. What 2013 uniquely contributes, for the compositional reasons documented in Section 6.4, is the clearest single-election picture of an effect present in every raffle. Hypothesis 1a is confirmed in both designs, and the pooled estimate is insensitive to alternative outcome and treatment definitions and to gapped versus gap-free numbering (Section 6 of the Supporting Information).

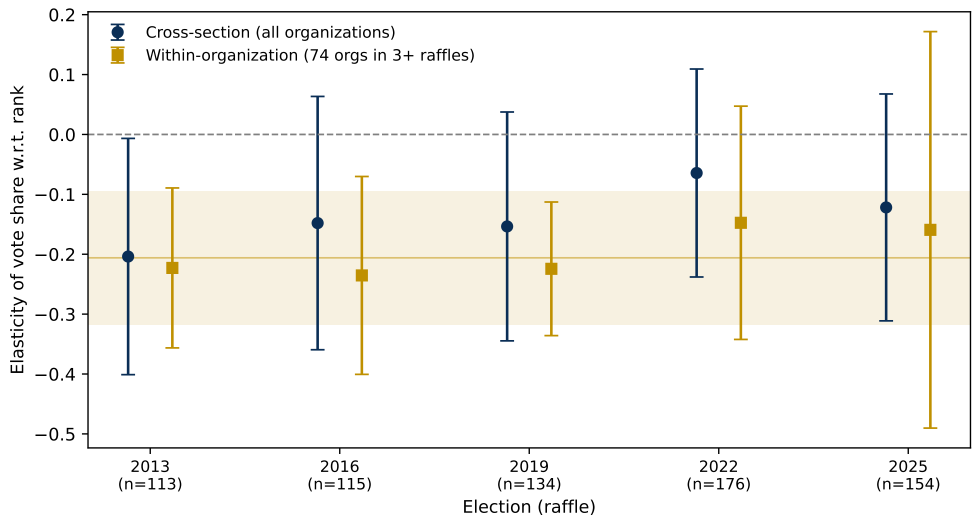

Figure 3: Elasticity of vote share with respect to sequence rank, by election: cross-sectional estimates (HC1 intervals) and within-organization estimates (organization and election fixed effects, cluster-robust intervals). The shaded band is the pooled within-organization elasticity, −0.206 (cluster se 0.057), with its 95 percent interval; a joint test cannot reject equal elasticities across the five raffles ($p = 0.84$). Election labels report canvassed organizations.

6.2 How do voters scan the grid? (Hypothesis 1b)

Voters read the crowded grid the way they would read a list: in order, stopping early. Wherever the data can discriminate among the scanning models, the spatial pattern of the losses matches convex ordinal primacy, the satisficing signature, and contradicts the spatial-attention alternatives. The one recurring exception is a modest top-of-column premium, a small spatial residual to which we return below.

The signature is visible in the raw data. Figure 4 pools all five raffles and removes organization and election averages, using no information about position. What remains reproduces the satisficing prediction of Figure 1: a premium of roughly 40 log points at the top of the leftmost column that fades to zero by mid-column, against much flatter profiles further right.

The formal tests agree. The signature battery requires cross-sectional resolution, and the joint tests of Section 6.1 identify where it exists: the 2013 cross-section, where position

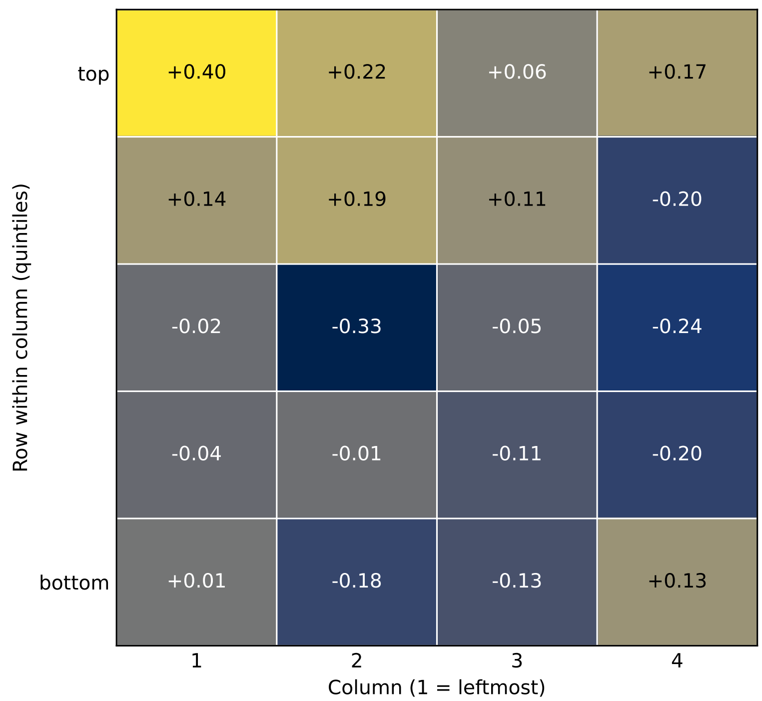

Figure 4: The pooled within-organization grid: mean residual log vote share by row quintile and column, five raffled elections, after removing organization and election fixed effects. Rows are binned into quintiles because column lengths differ across elections (29 to 47 rows); each cell then averages 20 to 33 organization-elections. Lighter cells receive more votes. Compare Figure 1, panel (b).

alone explains a fifth of the variance. There the shape is unambiguous. Four results establish it (Section 6 of the Supporting Information reports the full battery). First, vote share is convex in sequence rank (RI $p = 0.01$): losses are steepest among the earliest slots and flatten thereafter, as stopping-rule models require. Second, the downward penalty is steepest in the leftmost column ($p = 0.01$), which is where the earliest ranks sit and exactly opposite the F-pattern's signature prediction. Third, leftward columns carry a level advantage ($p = 0.01$), as reading order requires. Fourth, and decisively for the ordinal interpretation, there is no top-of-column premium ($p = 0.47$), and once the rank function is given a flexible spline form, column indicators add nothing ($p = 0.30$): an organization atop column two fares exactly as its sequence rank implies. Lateral-bias and central-fixation signatures find no consistent support in any year. In the four later cross-sections no coherent signature emerges: isolated coefficients cross thresholds with signs that disagree from year to year, as eight underpowered tests per election will produce by chance.

The signature battery can also be run within organizations, pooling all five raffles: each spatial coordinate is re-randomized by every draw, so organization fixed effects purge the between-organization noise from the shape tests just as from the level. The panel confirms the reading-order account's central elements: the decline in scaled rank (RI $p = 0.004$) and in row within column ($p = 0.001$) is strong, leftward columns carry a level advantage ($p = 0.02$), and vertical decay is convex ($p = 0.002$), as stopping rules imply. One element falls just short of significance: a top-of-column premium at RI $p = 0.051$, echoing the 2016 cross-section where it is unambiguous ($p < 0.01$). This is the one persistent hint that a minority of voters skim column tops while the majority read in order.

Three qualifications complete the horse race, each anticipated in Section 5. The pronounced convexity of 2013 does not survive within organizations, so we place no weight on precise curvature. The rank–number decoupling test is uninformative, because the two variables remain correlated above 0.999, and the Vuong comparisons cannot separate the ordinal and spatial forms within any single election. Out of sample, position models improve on a no-position benchmark by one to two percent of mean squared error, with the simple monotone forms generalizing best, which is what a modest, stable effect embedded in heavy heterogeneity describes. One residual deserves future work: the 2016 top-of-column premium matches the pattern documented on Danish multi-column ballots (Blom-Hansen et al., 2016).

6.3 Where do position effects concentrate? (Hypothesis 1c)

The position penalty is uniform. Contrary to Hypothesis 1c, it does not concentrate where information is scarce: not among obscure organizations, and not in poorer or less educated municipalities. The uniformity is itself informative.

Across organizations, the position penalty is the same for everyone. In the tests of equation (5), reported in Section 6 of the Supporting Information, the rank elasticity does not differ between first-time and veteran organizations (interaction -0.020, RI $p = 0.64$), between incumbents and the rest (-0.010, $p = 0.75$), or by prior-election vote share ($+0.016$, $p = 0.78$). Each interaction is an order of magnitude smaller than the main effect and tightly estimated, so meaningful moderation is ruled out rather than undetected. The picture is of a uniform, multiplicative exposure effect: position scales each organization's votes by a common factor, consistent with sequential scanning determining whether an organization is considered at all. This uniformity cuts against information-based accounts in which position substitutes for knowledge (cf. Ho and Imai, 2008; Miller and Krosnick, 1998), against the heterogeneity documented on Australian ballots (King and Leigh, 2009), and against a strong directed-search story.

The uniformity extends across places. The organization-by-municipality panel for 2016 and 2019 covers 396,854 observations across 1,620 municipalities, and aggregating it back to national totals reproduces the national elasticities almost exactly (-0.148 in 2016, -0.149 in 2019). The municipal data also reveal an extensive margin the national data cannot: an organization drawn one log unit deeper into the ballot is 3.6 percentage points less likely (se 0.5) to record any votes at all in a municipality, from a base of 90 percent, so position governs geographic reach and not only vote counts. In the municipal moderation tests of Section 6 of the Supporting Information, with organization-election and municipality-election fixed effects, the rank penalty does not vary with a municipality's college-educated share, income per capita, or population density. The one significant moderator is the indicator for the country's large multidistrict cities, where the penalty is about a quarter flatter than elsewhere. The direction is the information account's, but the dose is modest and the reach narrow.

The uniformity also holds over time.[7] The transformation of the campaign environment left no mark: ballot-number jingles and sample-ballot distribution became staples of party-list campaigning, and by 2019 the leading vote-getters were media-branded organizations winning from unremarkable positions.[8] The durable fact is persistence, not adaptation: five independent lotteries, twelve years end to end, the same elasticity each time.

6.4 Who captured the alphabet? (Hypotheses 2a and 2b)

The top of the alphabetical ballot was captured, and captured by the weakest organizations. Netting the causal position effect out of 2010 outcomes, the lexically engineered block that filled the first 115 of that ballot's 187 slots was roughly 25 percent weaker than the rest of the roster. Adverse selection on this scale overturns Hypothesis 2a's prediction: the observed 2010 gradient, -0.070 (HC1 se 0.084), is flatter than the causal elasticity of -0.155, not steeper, because a negative causal slope and a positive composition slope nearly cancel (Figure 6).

Before 2013, organizations chose their ballot position through their names. The ordering was alphabetical, with numerals sorting ahead of letters, so the surest route to the top of the

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[7] It holds across ballot lengths as well: choice fatigue would predict stronger position effects on the longer presidential-year ballots, and the within-organization elasticities of presidential and midterm years are statistically indistinguishable. With two presidential and three midterm raffled elections, however, the comparison is underpowered.

[8] Dwight de Leon, "Things to Know: Campaign Jingles, a Staple in Philippine Elections," *Rappler*; Michelle Abad, "Ang Probinsyano Party List Is Heading to Congress. Where Did Its Votes Come From?" *Rappler*, May 2019. Full URLs in Section 2 of the Supporting Information.

PARTY LIST			(Vote for 1)
1. 1 ANG PAMILYA FORMERLY ANC	48. AGBIAG	95. ANG TRABAHANTE	142. DIWA
2. 1-AANI	49. AGHAM	96. ANG TSINOY	143. EMMANUEL
3. 1-ABAA	50. AGILA	97. ANUPA	144. FFW
4. 1-AHAPO	51. AGRI	98. APEC	145. FIL-MUS
5. 1-AK	52. AHON	99. APELA	146. FIRM 24-K
6. 1-CARE	53. AK	100. APO	147. GABRIELA
7. 1-NET	54. AKAP	101. APOI	148. GREEN FORCE
8. 1-TUBIG (FORMERLY AAWAS)	55. AKAP BATA	102. ARAL	149. IVAP
9. 1-UTAK	56. AKAP.TAO	103. ARARO	150. KAAGAPAY
10. 1GANAP/GUARDIANS	57. AKB	104. ARC	151. KAAKBAY
11. 1ST KABAGIS	58. AKBAYAN	105. ARCAPP	152. KABATAAN
12. 1ST PRISA	59. AKI	106. AS	153. KABAYAN
13. A BLESSED PARTY-LIST	60. AKMA-PTM	107. ASAHAN MO	154. KAKUSA
14. A TAMBAY	61. AKO	108. AT	155. KALAHI
15. A TEACHER	62. AKO AGILA	109. ATING KOOP	156. KALINGA
16. A-IPRA	63. AKO BAHAY	110. ATM	157. KASAPI
17. AA-KASOSYO PARTY	64. AKSI	111. ATONG PAGLAUM	158. KATRIBU
18. AAMA	65. ALAGAD	112. ATS	159. KATUTUBO
19. AAMBIS-OWA	66. ALAY BUHAY	113. AVE	160. KLBP
20. AANI	67. ALE	114. AVPAP	161. LPGMA
21. AAPS	68. ALIF	115. AWAT	162. LYPAD
22. AASCA	69. ALIM	116. BABAE KA	163. NCCP
23. ABA	70. ALLUMAD	117. BAGO	164. OPO
24. ABA ILONGGO	71. ALMA	118. BANAT	165. ORAGON
25. ABAKADA	72. ALMANA	119. BANDILA	166. PACYAW
26. ABAMIN	73. ALNA	120. BANGON TRANSPORT	167. PBA
27. ABANG LINGKOD	74. ALON	121. BANTAY	168. PCL
28. ABANTE KA	75. ALUM	122. BAYAN MUNA	169. PEP
29. ABAY PARAK	76. ALYANSA NG OFW	123. BAYANI	170. PFP
30. ABBA-AMA	77. AMA	124. BH	171. PM (MANGGAGAWA)
31. ABC	78. AMANA	125. BIDA (BATANG IWAS)	172. PM (MASDA)
32. ABO	79. AMANG	126. BIDA (BINIGKIS)	173. SABOD
33. ABONO	80. AME	127. BIGKIS	174. SAGIP
34. ABOT TANAW	81. AMIN	128. BINHI	175. SB
35. ABP-BICOLNON	82. AMS	129. BIYAHENG PINOY	176. SENIOR CITIZENS
36. ABROAD	83. AN WARAY	130. BUHAY	177. SMART
37. ABS	84. ANAD	131. BUKID	178. TUCP
38. ACMA	85. ANAK	132. BUKLOD FILIPINA	179. UCAP
39. ACT TEACHERS	86. ANAKALUSUGAN	133. BUTIL	180. UFS
40. ACTS	87. ANAKPAWIS	134. CHINOY	181. UNI-MAD
41. ADA	88. ANG KASANGGA	135. CIBAC	182. UNLAD PILIPINAS
42. ADAM	89. ANG LADLAD	136. COCOFED	183. VENDORS PARTY LIST
43. ADD	90. ANG MINERO	137. COFA	184. VFP
44. ADD-TRIBAL	91. ANG NICP	138. CONSLA	185. WOMENPOWER
45. AFPSEGCO	92. ANG PADER	139. COOP-NATCCO	186. YACAP
46. AG	93. ANG PDR	140. CPM	187. YES WE CAN
47. AGAP	94. ANG SAMAKA	141. DAMAYAN	

Figure 5: The party-list section of the official 2010 ballot (authors' transcription of the COMELEC facsimile). Shading marks entries whose printed name begins with a digit or "A"; they fill the first 115 of 187 slots.

ballot was a name beginning with a digit, and the next surest was a name beginning with "A." The 2010 ballot records the equilibrium of that contest (Figure 5): twelve digit-first names followed by 103 consecutive "A" names, so that the first 115 of 187 slots, the entire first two columns and half of the third, were occupied by lexically engineered entries. Alphabetic bias under chosen names has been documented before, in Spanish Senate elections (Lijphart and López Pintor, 1988). What distinguishes the Philippine case is how completely political actors organized around it.

The logic is a decomposition: the observed gradient equals the causal effect plus the sorting of quality into position, so Hypothesis 2a predicted a steeper alphabetical gradient. The difference instead runs the other way (+0.085, se 0.096, statistically indistinguishable from zero), and the resolution lies in who chose to queue-jump. The organizations crowding the digit-and-"A" block were disproportionately marginal groups whose entire electoral strategy was their ballot position, while the established brands of Philippine party-list politics (Buhay, Gabriela, Bayan Muna, Kabataan, Senior Citizens) sat mid-alphabet on legacy names their supporters already knew, and had no reason to give up those names for placement.

The direct test uses the position-adjusted quality residual $\hat{q}_i$ of Section 5: the block gap is -0.28 log points (se 0.19), ranging from -0.17 to -0.39 across the confidence bounds of the causal elasticity, imprecise but uniformly negative at every value our interval admits. A second check comes from within the block, where lexical engineering is near-universal and composition roughly constant, so the observed gradient should revert toward the causal one. It does: the within-block 2010 gradient steepens to -0.156 (se 0.103), essentially the causal elasticity itself.

The naming record supplies the final evidence (Hypothesis 2b): strategic naming decays once the raffle severs names from positions. Computed on printed ballot names across all six elections, the share of entries beginning with a digit or "A" was 61.5 percent in 2010 and 68.3 percent in 2013, a roster still composed of names registered under the old regime, and then fell monotonically as the raffle removed the incentive: 51.3 percent in 2016, 43.3 in 2019, 36.2 in 2022, and 29.7 percent in 2025, less than half the alphabetical-era level. The persistence of obsolete lexical engineering, eroding only as rosters turn over, is itself evidence that the names were chosen for position rather than for branding. It is also the composition fact behind the strength of the 2013 cross-section documented in Section 6.1: the first raffled roster was a survivors' roster, compressed in baseline popularity, on which position had unusually little competition from fame.

Position choice thus did not amplify the observed gradient, because the option was exercised mainly by organizations with little else to offer. The queue-jumpers' behavior was nonetheless rational: the slots were genuinely valuable, and an arms race is precisely what a

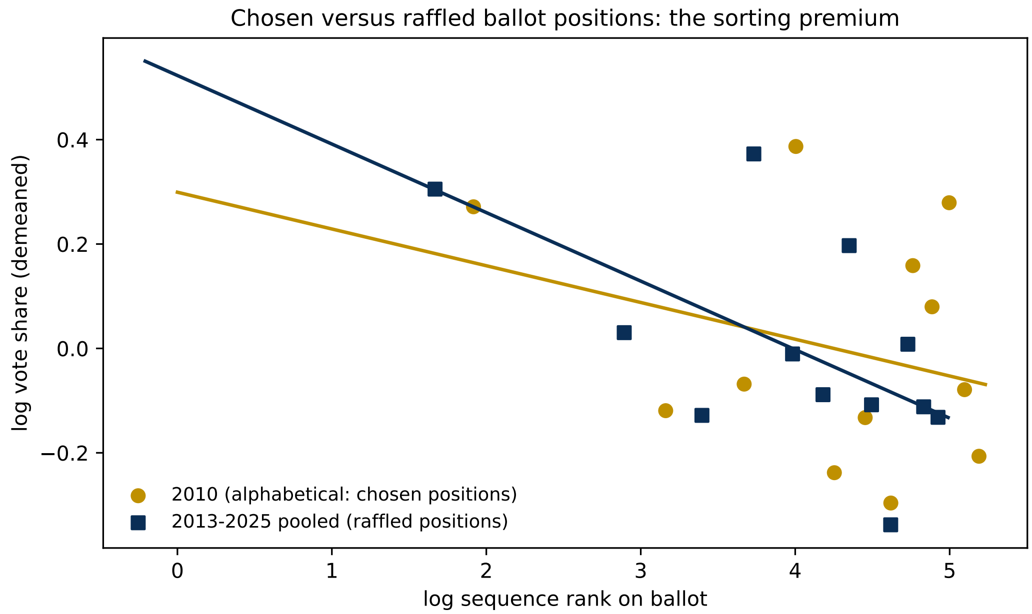

Figure 6: Chosen versus raffled positions: binned means of demeaned log vote share against log sequence rank, 2010 (chosen) and pooled 2013–2025 (raffled), with OLS fits.

valuable, freely appropriable rent predicts. What the 2013 reform changed was not the existence of position rents but their allocation, from organizations willing to burn their names for placement to the luck of the draw. Who gained and who lost from that reallocation, measured in seats, is the subject of the next subsection. Section 7 of the Supporting Information pursues three companion analyses, including a compositional test on the predetermined 2007 baseline that finds the digit-and-"A" block roughly a third weaker on pre-reform performance, confirming the adverse-selection signature independently.

6.5 What remains at stake: seats (Hypothesis 2c)

The draw moves real representation: four to five seats in every cycle at the point estimate, twenty-four congressional seats over the five raffled elections, roughly eight percent of the party-list chamber each time, with eight to ten organizations affected per election (Table 4).

The count is mechanical. The Philippine seat formula converts vote shares into seats, so we can replay each election with the position effect removed and tally the seats that change hands. For organization i in election t, the counterfactual log share is

$$\ln s_{it}^{cf} = \ln s_{it} + \hat{\gamma}\left(\ln r_{it}^{cf} - \ln r_{it}\right), \tag{6}$$

where $\hat{\gamma} = -0.155$ is the pooled within-organization elasticity (Table 3, column 3), r_{it} is the actual raffled sequence rank, and r_{it}^{cf} is the counterfactual rank, with shares renormalized to sum to one. Applying one elasticity to every organization is licensed by the uniformity results of Section 6.3: famous and obscure organizations lose the same proportion of their votes to a worse draw.

We consider two counterfactuals. The de-lucked counterfactual sets every organization to the same position, the mean log rank, so that no one gains or loses from placement at all. It isolates the seats attributable purely to the luck of the draw. The alphabetical counterfactual instead assigns each organization the rank it would have held had the printed roster been sorted alphabetically, holding names fixed. It measures how different the raffle's allocation is from the one the old regime's rule would have produced with the same contestants.[9] Seats are allocated to actual and counterfactual shares by the statutory formula of Section 2. Applied to actual vote shares, where official organization-level allocations are on record, the implementation reproduces them for 95 to 98 percent of organizations, with differences concentrated in documented proclamation disputes (Section 6 of the Supporting Information).

The affected organizations are not marginal curiosities. Bayan Muna, one of the country's oldest progressive party-lists, drew the first slot in 2019, and purging that advantage costs it one of its three seats. 4Ps drew the very first slot on the 2025 ballot, and purging

[9] Holding names fixed is the short-run counterfactual. Under a durable alphabetical regime organizations would re-engineer their names, as Section 6.4 documents.

Table 4: Seats allocated by the luck of the draw

Counterfactual		2013	2016	2019	2022	2025	Total
De-lucked	Seats moved	4	5	5	5	5	24
(position effect purged)	[range over $\hat{\gamma}$ CI]	[1–5]	[4–7]	[2–7]	[1–8]	[2–7]	[10–34]
	Organizations affected	8	10	10	10	9	47
Alphabetical ordering	Seats moved	4	5	6	5	6	26
(names held fixed)	[range over $\hat{\gamma}$ CI]	[1–6]	[4–8]	[3–10]	[3–10]	[2–13]	[13–47]
	Organizations affected	8	10	12	10	12	52

Notes: Counterfactual shares from equation (6) with $\hat{\gamma} = -0.155$; ranges evaluate $\hat{\gamma}$ at the endpoints of its cluster-robust 95% interval $[-0.245, -0.064]$. "Seats moved" counts seats changing hands under the counterfactual allocation. Party-list quotas: 58, 59, 61, 63, and 64.

that advantage costs it two seats. The reason modest vote shifts move whole seats is institutional. The two-percent threshold and the three-seat cap concentrate marginal seats among mid-sized organizations, so a proportional shift of a few tenths of a percentage point crosses a seat boundary precisely where the party-list system's distributive stakes are highest.

Two implications follow. First, the seats in Table 4 are purely redistributed: fairness ex ante, lottery ex post. Every cycle, a public drawing of lots reallocates a bloc of seats comparable to the margin between major party-list factions. Second, the alphabetical counterfactual moves a comparable number of seats, twenty-six across the period, because alphabetical ordering is itself just another assignment of the same valuable slots. The reform re-rolled the advantage rather than removing it. Only rotation across ballots or precincts, as several American jurisdictions practice (Ho and Imai, 2008), averages each organization's advantage toward zero within a single election. We return to this design point in the conclusion.

6.6 Does a lucky draw persist? (Hypothesis 3)

A lucky draw does not persist. The previous draw's total effect on current vote share is a precise zero (+0.005, se 0.054; RI $p = 0.94$) beside a contemporaneous effect of -0.141, and organization fixed effects do not change the conclusion (Table 5, Hypothesis 3a). The one exception operates through survival, to which we return below.

The test is clean because each draw is independent of the last: lagged position is a randomized test of whether the advantages a lucky draw confers (a seat, office resources, visibility) persist into the next contest. This is the incumbency advantage the close-elections literature identifies from near-ties (Lee, 2008), here identified from a true lottery, and its absence also validates the first-difference designs of Section 6.1.

The extensive margin is different (Hypothesis 3b). A worse draw makes an organization less likely to be on the ballot at all next time: one log unit of rank lowers the probability of contesting the next election by 4.6 percentage points (cluster se 2.1; RI $p = 0.039$) from a base of 58 percent, so a tenth-slot draw against a hundredth-slot draw is worth roughly eleven points of survival. The lottery therefore does not create compounding advantages among the organizations that persist, but it does help select which organizations persist. Part of the raffle's redistribution operates not through the seats of any single cycle but through the composition of the roster that contests the next one.[10]

The comparison of regimes matters here. Under the alphabet, the same organizations held the survival-relevant positions election after election. The raffle re-deals them every cycle, so they can come to rest on no one. In a political system built to an unusual degree on dynastic persistence (Querubin, 2016), the finding that this one institution entrenches no one is not a small one. Whatever else randomization fails to do, it is an anti-entrenchment device.

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[10]This selection also implies that the no-carryover estimate conditions on survival. Organizations that return despite bad draws may be unusually resilient, which would, if anything, mask a positive carryover among them. The agreement between within-organization and full-roster estimates in Section 6.1 bounds how much such selection can matter.

Table 5: Does luck compound? Lagged draws, current votes, and survival

Outcome	(1) log share	(2) log share	(3) contested next election
ln rank, current draw	-0.141^{*}	-0.175^{*}	-0.046^{*}
	(0.063)	(0.082)	(0.021)
ln rank, previous draw	+0.005	−0.046	
	(0.054)	(0.053)	
Election fixed effects	Yes	Yes	Yes
Organization fixed effects	No	Yes	No
Observations	309	204	538

Notes: Columns (1)–(2): organizations contesting consecutive raffled elections; the previous draw's coefficient is its total causal effect on current votes (randomization-inference $p = 0.94$ in column 1, permuting previous draws within election). Column (3): linear probability of appearing on the next raffled ballot, all canvassed organizations in 2013–2022 (base rate 0.58); RI $p = 0.039$. Standard errors clustered by organization; column (2) drops singleton organizations. $^{*}p < 0.05$.

7 Conclusion

The crowded ballots of modern democracies are grids, not lists, and the difference has consequences for representation. More so than on the list, options on the grid outnumber the attention a voter brings into the booth. Ballot position therefore becomes valuable, and someone must decide how positions are assigned. There are two broad options: let political actors compete for positions, as the Philippines did under alphabetical ordering, or assign them by lottery, as it has done since 2013. Each rule is an answer to a question rarely asked out loud: who should receive the votes that come from the ballot's layout itself? We have used a system that tried both rules in sequence to measure how large these vote flows are, to identify the scanning behavior by which voters produce them, and to trace who received them under each rule.

The finding we emphasize first is persistence. The elasticity we estimate, roughly −0.16, held across five raffles spanning twelve years, through documented changes in the campaign environment that could have weakened it. The elasticity did not move. Attention rents on a crowded ballot are structural rather than transitional. They come from a simple arithmetic: more options than a voter has time or information to consider. Familiarity with the raffle has not changed that arithmetic.

Two lessons follow. The first concerns randomization as institutional design. The 2013 reform genuinely delivered fairness: no organization can now buy, game, or inherit the top of the ballot. But fairness before the draw does not translate into neutrality after it. A single national draw hands the entire position advantage to whoever happens to be lucky, every cycle, and the twenty-four seats we count are the cumulative result. The raffle's one dynamic advantage is that draws do not repeat, so advantages cannot accumulate to any organization.

The second lesson concerns what happens when a reform takes away a captured advantage. The organizations that had invested in position, by giving up their names for placement, were precisely the ones with the least else to offer voters. When the reform removed their advantage, it did not remove the advantage itself but passed it from strategic actors to lucky ones. Reformers who close off one channel of capture should expect the underlying prize, if it survives, to find a new owner. In our setting the new owner is chance, which is more equitable than lexical engineering but not the same as neutrality.

The complete remedy has been available all along. Rotation, printing different orders on different ballots, would remove the position advantage rather than reassign it, averaging every organization's advantage toward zero within a single election. Several American jurisdictions already rotate candidate order across precincts (Ho and Imai, 2008). Nothing technological stands in the way: the automated election system already prints precinct-specific ballots for local contests.[11] What rotation awaits is not capacity but recognition. The lesson travels beyond ballots. Wherever options compete for attention on an administratively ordered list, randomizing the order equalizes expectations. Only rotation equalizes outcomes.

[11]Each ballot is printed for, and readable only by, the machine of a particular precinct (Section 2 of the Supporting Information).

Our evidence concerns the allocation of votes and seats, not the quality of representation those seats produce, so our normative claims are procedural. The raffle allocates representation more fairly than the alphabet did. Whether the resulting Congress governs better is a question our design cannot answer, though work tracing downstream policy effects of ballot order shows it is answerable (Shi and Singleton, 2023). Eye-tracking studies on facsimile ballots would convert our revealed-attention inferences into direct observation (Jenke and Sullivan, 2025; Coronel et al., 2021). The ballot is the last piece of choice architecture in the voter's hands, and our basic finding is that it is never neutral. It always confers an advantage on someone. The one thing institutions can decide is on whom.

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Supporting Information

SI 1 Ballot-Order Lotteries Around the World

Crowded ballots ordered by lottery are standard equipment in the world's large democracies. Indonesia, the third largest, assigns each party's ballot number by public draw, and Indonesian parties, like Philippine party-lists, campaign on the numbers they happen to pull (Komisi Pemilihan Umum, 2022). Australia, whose "donkey vote" named the phenomenon (Orr, 2002), orders the party columns of its Senate ballot by drawing numbered balls from a rotating container, a procedure that in 2013 arranged a ballot of forty-two party columns so large it was nicknamed the tablecloth (Australian Electoral Commission, 2025). Danish municipal voters face multi-column candidate grids (Blom-Hansen et al., 2016), Polish local elections array thousands of candidates on randomly ordered party lists (Flis and Kamiński, 2022), and American jurisdictions from California onward randomize name order precisely because they concede that order matters (Ho and Imai, 2008). Every one of these systems has adopted the fair-lottery half of the solution and left the attention problem untouched, which is exactly the configuration whose consequences we measure.

SI 2 The Party-List System: Legal and Administrative Record

SI 2.1 The Legal Architecture of the Party-List System

The 1987 Constitution reserves twenty percent of House seats for representatives "elected through a party-list system of registered national, regional, and sectoral parties or organizations" (art. VI, §5(2)). Republic Act No. 7941 (1995) operationalizes the system: §§5–8 govern registration and accreditation by COMELEC, §11 sets the two-percent guarantee, proportional additional seats, and the three-seat cap, and §12 mandates a single nationwide tally.[12] Two Supreme Court decisions define the modern regime. *BANAT v. COMELEC* (2009) struck down the earlier *Veterans* formula and required that all reserved seats be filled, producing the three-round allocation we implement in Section 6.5 of the main text: one guaranteed seat per organization above two percent; additional seats proportional to vote share, capped at three; and remaining seats assigned one each in descending vote order until the quota is exhausted.[13] *Atong Paglaum v. COMELEC* (2013) then liberalized entry, holding

[12] `lawphil.net`.

[13] `lawphil.net`.

that national and regional parties need not represent "marginalized and underrepresented" sectors and relaxing nominee requirements, which reshaped the roster from 2013 onward.[14]

SI 2.2 Automation Statutes and Counting Hardware

Republic Act No. 8436 (December 22, 1997) authorized an automated election system, and Republic Act No. 9369 (2007) amended it into the form first implemented nationally in 2010; §11 of R.A. 8436 is the statutory basis for the two-sided official ballot.[15] The counting hardware succeeded in three generations, all optical-scan: Smartmatic precinct-count optical scan (PCOS) machines in 2010 and 2013; Smartmatic vote-counting machines (VCMs) in 2016, 2019, and 2022; and automated counting machines (ACMs) leased from Miru Systems of South Korea in 2025, about 110,000 units.[16] In every generation the voter's task is identical: shade the oval beside the chosen entry and feed the sheet to the scanner, which is why we treat the six elections as technologically comparable.

SI 2.3 The Raffles, Cycle by Cycle

2013. Resolution No. 9591 set the raffle rules; the first raffle was held January 4, 2013 over 136 groups (see Section 2 of the main text, including the contemporaneous statements of purpose).

2016. Resolution No. 10025 (December 3, 2015) set the rules, and the raffle was held December 14, 2015 over 185 groups, but final ballot numbers were assigned only after disqualification petitions were resolved, so the printed numbering is gap-free, the procedural comparison Section SI 6 exploits.

2019. Resolution No. 10448 governed an electronic raffle conducted December 5, 2018 over 181 groups; Bayan Muna drew the first position. When later delistings removed 47 groups, COMELEC retained the raffled sequence rather than renumbering, producing the

[14] `lawphil.net`.

[15] `lawphil.net`; `lawphil.net`.

[16] Michael Bueza, "How the Philippine Automated Election System Works," *Rappler* (updated 2022), `rappler.com`; "Filipinos Will Use Comelec's New Voting Machines in 2025," *Rappler*, June 2024, `rappler.com`.

gapped 2019 ballot.[17]

2022. Resolution No. 10733 (with Resolutions 10735, 10735-S, and Minute Resolution 21-1397) governed an electronic raffle conducted December 14, 2021, witnessed virtually under pandemic protocols. One feature requires care: seven groups that had been denied accreditation and later obtained Supreme Court relief were appended at the bottom of the listing without re-raffling, so the final printed slots of the 2022 ballot were assigned by litigation timing rather than by the draw.[18] This does not threaten our estimates. The 2022 balance tests (Section SI 5) cover the full printed roster and detect nothing; excluding the last seven printed slots leaves the 2022 cross-sectional gradient (a null in either case) and the pooled within-organization elasticity essentially unchanged (-0.150 against -0.155), and only one of the appended groups enters the panel at all.

2025. Resolution No. 11068 governed an electronic raffle conducted October 18, 2024 at the COMELEC office in Intramuros, livestreamed, over 156 groups; 4Ps drew the first position and Uswag Ilonggo the last, exactly as our transcribed layout records.[19]

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[17] Dona Magsino, "Comelec to Maintain Raffled Sequence of Party-List Groups in Ballots for 2019 Polls," *GMA News Online*, January 16, 2019, `gmanetwork.com`; Faye Orellana, "Comelec Releases Initial Order of Party-List Groups," *INQUIRER.net*, December 5, 2018, `inquirer.net`.

[18] Evelyn Macairan, "Comelec Sets Virtual Raffle of Party-List Order on Ballot," *Philippine Star*, November 30, 2021, `philstar.com`; Cathrine Gonzales, "7 Party-Lists Earlier Denied to Join Polls Still Included in Ballot Listing," *INQUIRER.net*, December 29, 2021, `inquirer.net`.

[19] Giselle Ombay, "Comelec to Raffle Party-List Order of Listing on Oct. 18," *GMA News Online*, October 15, 2024, `gmanetwork.com`; Rhodina Villanueva, "156 Party-List Groups to Be Included in Ballot," *Philippine Star*, October 19, 2024, `philstar.com`. The resolution's full text is preserved at `lawphil.net`.

SI 2.4 Accreditation Churn, Election by Election

The set of organizations printed on each ballot differs from both the set that entered the raffle and the set whose votes were ultimately canvassed, because party-list accreditation is contentious and the litigation routinely straddles ballot printing. This subsection records the chronology, cycle by cycle, with sources. In 2010, COMELEC approved 144 new accreditation petitions (Resolution No. 8744) and denied 162; with subsequent accreditations and previously registered organizations, the certified list closed at 187 printed names, and several winners' proclamations were disputed after the fact, including the Ang Ladlad restraining order and the unimplemented Philippine Guardians Brotherhood status quo order. In 2013, COMELEC purged the roster from nearly 200 groups to just over 100 between October and December 2012 (69 delisted by mid-November), accredited 83 of 289 applicants, and printed 36 more under status quo ante orders; the *Atong Paglaum* remand then arrived weeks before election day. Thirteen raffle numbers (16, 24, 34, 36, 41, 45, 47, 63, 69, 80, 83, 101, 102) were removed after the draw but before printing, and ten printed groups were never canvassed. In 2016 the gap-free procedure described above left no scars. In 2019, delistings after the raffle removed 47 of 181 numbers. In 2022 and 2025, a single gap each remained. In 2022, An Waray's 385,460 votes were counted and it was proclaimed to a seat, which passed to next-ranked Akbayan in September 2024 after the Supreme Court affirmed the cancellation of its registration. In 2025, Duterte Youth's 2,338,564 counted votes placed it second, but its proclamation was suspended and, upon finality in September 2025, its three seats passed down the ranking without recomputation, on the An Waray precedent; the 64th seat (Gabriela) was a separate adjustment to the constitutional twenty-percent share.[20]

For identification, this churn is mostly benign and arguably helpful. Disqualifications occur for reasons unrelated to the raffle draw, and each removal shifts every lower-numbered survivor's grid position in a way no organization controls. The treatment we code is the printed position, not the raffle number (Section SI 4); the balance tests of Section SI 5, which cover the full printed rosters, detect no selection from the churn; and the 2016 procedure, whose numbers were assigned only after disqualifications were resolved, provides a gap-free comparison (Section SI 6).

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[20]Sources: on 2013, Amita Legaspi, *GMA News Online*, May 28, 2013, `gmanetwork.com`; Sheila Crisostomo, *Philippine Star*, May 7, 2013, `philstar.com`; *ANAD v. COMELEC*, G.R. No. 206987, and *COCOFED v. COMELEC*, G.R. No. 207026, at `lawphil.net`. On 2022 and 2025: *GMA News Online*, May 26, 2022, `gmanetwork.com`; *Manila Bulletin*, September 25, 2024, `mb.com.ph`; *Philippine Star*, October 1, 2025, `philstar.com`; *GMA News Online*, October 2, 2025, `gmanetwork.com`; *INQUIRER.net*, September 14, 2025, `inquirer.net`.

SI 2.5 The Physical Ballot

The official ballot is a single pre-printed sheet, marked by shading ovals and machine-read at the precinct; both sides are used (in 2022 the national contests occupied the front and the 177 party-list entries the reverse), with statutory authority in §11 of R.A. 8436. Reported dimensions are 8.5 by 25 inches in 2010 (longer in the ARMM) and 26 inches long for domestic voters in 2022 (30 in the BARMM, 25 overseas).[21] Ballots are precinct-specific: each sheet is printed for, and can only be read by, the machine of a particular precinct, since it carries that precinct's local contests. COMELEC's chairman put it directly ahead of the 2025 election: a ballot for a voter in Precinct 1A cannot be used in Precinct 2A.[22] This fact carries the design implication developed in the conclusion: the infrastructure for printing position-rotated party-list sections across precincts already exists; only the party-list section is frozen nationwide.

SI 3 The Party-List Ballots, as Printed

Figures SI-1 through SI-6 reproduce the party-list section of each of the six ballots we study. Each reconstruction is built directly from our transcription of the official COMELEC sample ballot: every printed entry appears at its printed grid position, with its printed ballot number and name. Typography is schematic; grid structure, numbering (including the gaps left by post-raffle disqualifications), and naming are as printed.

SI 4 Data Construction and Validation

SI 4.1 Printed Numbers, Sequence Ranks, and Uncanvassed Slots

Two institutional details require care. First, ballot numbers and sequence ranks are not identical: organizations disqualified after the raffle but before printing leave gaps in the printed numbering (thirteen numbers in 2013, forty-seven in 2019, one each in 2022 and 2025), so an organization's printed number can exceed its sequence rank by up to 47 places. Sequence rank, not printed number, is the treatment implied by scanning theories, and the

[21] Ana de Villa Singson, "Frequently Asked Questions," *Philippine Star* Starweek Magazine, April 4, 2010, `philstar.com`; Dona Z. Pazzibugan, "Comelec Finalizes List of Bets, Starts Printing of Ballots," *INQUIRER.net*, January 18, 2022, `inquirer.net`.

[22] Dexter Cabalza, "Comelec: No Excess Ballots for 2025 Polls," *INQUIRER.net*, October 28, 2024, `inquirer.net`.

PARTY LIST *(Vote for 1)*

1. 1 ANG PAMILYA FORMERLY ANC
2. 1-AANI
3. 1-ABAA
4. 1-AHAPO
5. 1-AK
6. 1-CARE
7. 1-NET
8. 1-TUBIG (FORMERLY AAWAS)
9. 1-UTAK
10. 1GANAP/GUARDIANS
11. 1ST KABAGIS
12. 1ST PRISA
13. A BLESSED PARTY-LIST
14. A TAMBAY
15. A TEACHER
16. A-IPRA
17. AA-KASOSYO PARTY
18. AAMA
19. AAMBIS-OWA
20. AANI
21. AAPS
22. AASCA
23. ABA
24. ABA ILONGGO
25. ABAKADA
26. ABAMIN
27. ABANG LINGKOD
28. ABANTE KA
29. ABAY PARAK
30. ABBA-AMA
31. ABC
32. ABO
33. ABONO
34. ABOT TANAW
35. ABP-BICOLNON
36. ABROAD
37. ABS
38. ACMA
39. ACT TEACHERS
40. ACTS
41. ADA
42. ADAM
43. ADD
44. ADD-TRIBAL
45. AFPSEGCO
46. AG
47. AGAP
48. AGBIAG
49. AGHAM
50. AGILA
51. AGRI
52. AHON
53. AK
54. AKAP
55. AKAP BATA
56. AKAP.TAO
57. AKB
58. AKBAYAN
59. AKI
60. AKMA-PTM
61. AKO
62. AKO AGILA
63. AKO BAHAY
64. AKSI
65. ALAGAD
66. ALAY BUHAY
67. ALE
68. ALIF
69. ALIM
70. ALLUMAD
71. ALMA
72. ALMANA
73. ALNA
74. ALON
75. ALUM
76. ALYANSA NG OFW
77. AMA
78. AMANA
79. AMANG
80. AME
81. AMIN
82. AMS
83. AN WARAY
84. ANAD
85. ANAK
86. ANAKALUSUGAN
87. ANAKPAWIS
88. ANG KASANGGA
89. ANG LADLAD
90. ANG MINERO
91. ANG NICP
92. ANG PADER
93. ANG PDR
94. ANG SAMAKA
95. ANG TRABAHANTE
96. ANG TSINOY
97. ANUPA
98. APEC
99. APELA
100. APO
101. APOI
102. ARAL
103. ARARO
104. ARC
105. ARCAPP
106. AS
107. ASAHAN MO
108. AT
109. ATING KOOP
110. ATM
111. ATONG PAGLAUM
112. ATS
113. AVE
114. AVPAP
115. AWAT
116. BABAE KA
117. BAGO
118. BANAT
119. BANDILA
120. BANGON TRANSPORT
121. BANTAY
122. BAYAN MUNA
123. BAYANI
124. BH
125. BIDA (BATANG IWAS)
126. BIDA (BINIGKIS)
127. BIGKIS
128. BINHI
129. BIYAHENG PINOY
130. BUHAY
131. BUKID
132. BUKLOD FILIPINA
133. BUTIL
134. CHINOY
135. CIBAC
136. COCOFED
137. COFA
138. CONSLA
139. COOP-NATCCO
140. CPM
141. DAMAYAN
142. DIWA
143. EMMANUEL
144. FFW
145. FIL-MUS
146. FIRM 24-K
147. GABRIELA
148. GREEN FORCE
149. IVAP
150. KAAGAPAY
151. KAAKBAY
152. KABATAAN
153. KABAYAN
154. KAKUSA
155. KALAHI
156. KALINGA
157. KASAPI
158. KATRIBU
159. KATUTUBO
160. KLBP
161. LPGMA
162. LYPAD
163. NCCP
164. OPO
165. ORAGON
166. PACYAW
167. PBA
168. PCL
169. PEP
170. PFP
171. PM (MANGGAGAWA)
172. PM (MASDA)
173. SABOD
174. SAGIP
175. SB
176. SENIOR CITIZENS
177. SMART
178. TUCP
179. UCAP
180. UFS
181. UNI-MAD
182. UNLAD PILIPINAS
183. VENDORS PARTY LIST
184. VFP
185. WOMENPOWER
186. YACAP
187. YES WE CAN

Figure SI-1: Party-list section of the 2010 ballot (alphabetical ordering; 187 organizations). Authors' reconstruction from the official COMELEC sample ballot.

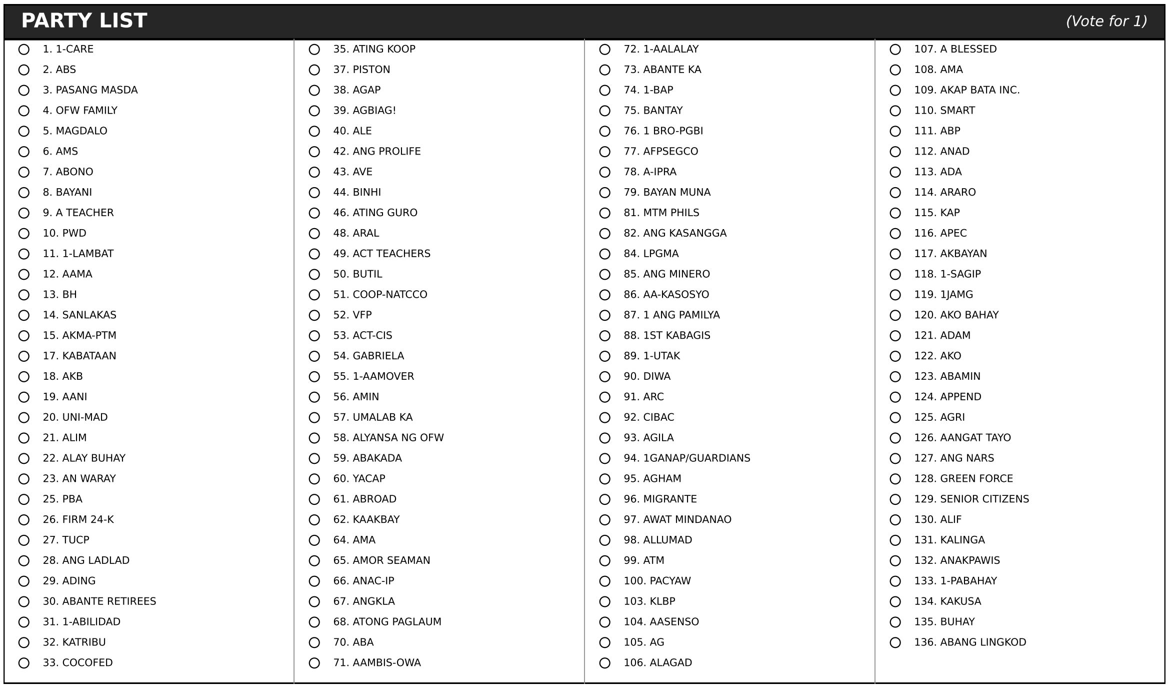

Figure SI-2: Party-list section of the 2013 ballot (first raffle; 123 organizations, numbers 1–136 with thirteen gaps). Authors' reconstruction from the official COMELEC sample ballot.

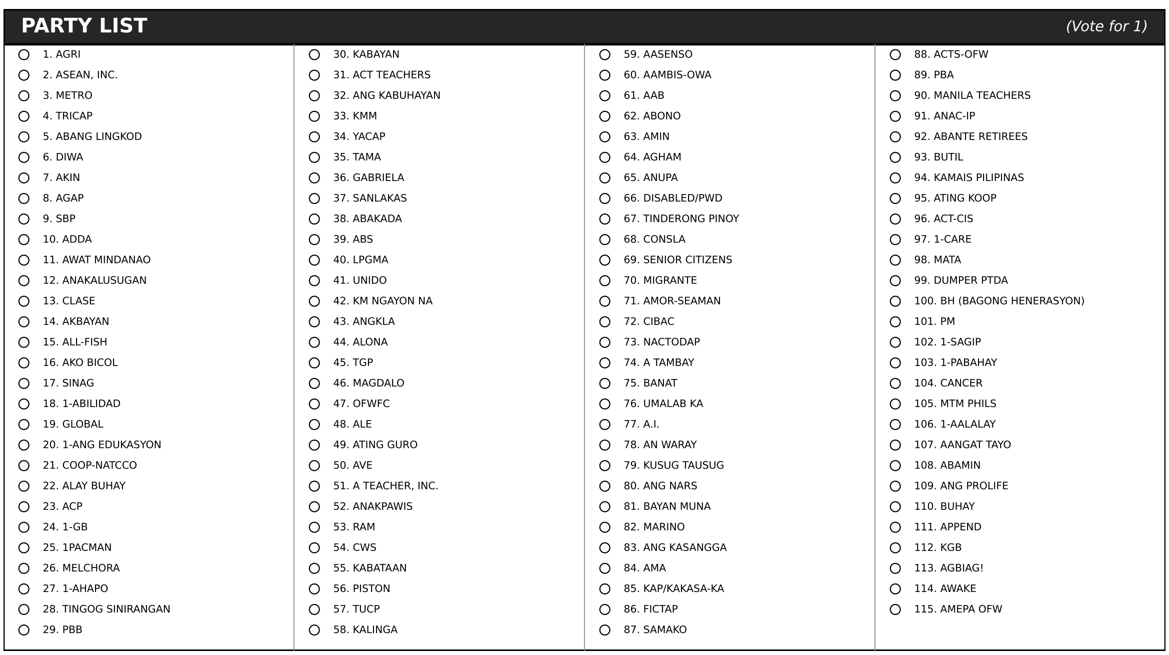

Figure SI-3: Party-list section of the 2016 ballot (raffle; 115 organizations, gap-free numbering). Authors' reconstruction from the official COMELEC sample ballot.

PARTY LIST			*(Vote for 1)*
1. BAYAN MUNA	46. ALL-FISH	92. AKO BISAYA	138. MAGDALO
2. KABAYAN	47. ANAKALUSUGAN	93. AVE	139. ANUPA
3. MAGSASAKA	48. TGP	94. METRO	140. 1-CARE
4. PEACE	49. ABEKA	95. AASENSO	141. AKO BICOL
6. KONTRA BROWNOUT PARTYLIST	51. AKMA-PTM	96. ALONA	142. COOP-NATCCO
7. OFW FAMILY	52. ABS	97. TRICAP	143. MAYPAGASA
8. 1-UTAK	53. BH (BAGONG HENERASYON)	98. BANAT	144. ATING KOOP
9. KUSUG TAUSUG	54. ANG PROBINSYANO	99. TINGOG SINIRANGAN	147. ALENG ENTREP
10. GLOBAL	55. PM	100. MANILA TEACHERS'	148. AANGAT TAYO
12. ANG NARS	58. ANAC-IP	101. ACT-CIS	149. KABALIKAT
14. ANAKPAWIS	59. SINAG	103. APPEND	150. BHW
16. CWS	60. PATROL	104. CONSLA	151. RECOBODA
17. PTA	63. KOOP-KAMPI	105. KMM	152. AKO PADAYON
18. RAM	65. UNIDO	108. ABAKADA	153. AMIN
20. DUMPER PTDA	67. SBP	109. KALINGA	155. ITO ANG TAMA
21. LUNTIAN	68. PPP	110. MARVELOUS TAYO	156. 1 UTAP BICOL
22. AKO BISDAK	69. INANG MAHAL	112. AAMBIS-OWA	157. DUTERTE YOUTH
23. ASEAN	71. 1-ANG EDUKASYON	113. TINDERONG PINOY	158. BUTIL
24. APEC	72. KAMAIS	114. AGRI	163. ABANG LINGKOD
26. PBA	73. ALIF	115. AGAP	164. AGBIAG!
27. AMEPA OFW	74. MATA	116. ACTS-OFW	166. TUCP
29. BAHAY	75. LPGMA	117. FICTAP	168. FFP
30. PRAI	76. ABONO	121. SAMAKO	169. WOW PILIPINAS
31. JUAN MOVEMENT	78. AKO	122. 1PACMAN	170. SAGIP
32. BUKLOD FILIPINO	79. ABANTE PILIPINAS	124. ABAMIN	171. DIWA
33. CIBAC	80. MARINO	126. PLM	172. PHILRECA
35. MURANG KURYENTE	82. TAO MUNA	127. AWAKE	173. 1P
36. ANGKLA	83. ACT TEACHERS	130. SENIOR CITIZENS	175. PBB
37. GABRIELA	84. PEOPLE'S CHAMP	131. A TEACHER	176. LAANG KAWAL
38. AKO AN BISAYA	86. ALAY BUHAY	132. GP	177. 1AAAP
39. 1-LAMBAT	87. BUHAY	133. 1-APTO	180. GRECON
42. KABATAAN	88. AKBAYAN	134. 1-AHAPO	181. AA-KASOSYO PARTY
43. AN WARAY	89. PARTIDO SANDUGO	136. ANG KABUHAYAN	
44. SULONG DIGNIDAD	91. PROBINSYANO AKO	137. YACAP	

Figure SI-4: Party-list section of the 2019 ballot (raffle; 134 organizations, numbers 1–181 with 47 gaps). Authors' reconstruction from the official COMELEC sample ballot.

PARTY LIST *(Vote for 1)*

○ 1. KAMALAYAN	○ 46. ANGAT	○ 91. KONTRA BROWNOUT	○ 137. KOOP-KAMPI
○ 2. KM NGAYON NA	○ 47. SOLID-CHANGE	○ 92. TULUNGAN TAYO	○ 138. MAYPAGASA
○ 3. PSIS	○ 48. BICOL SARO	○ 93. DUTERTE YOUTH	○ 139. PVAID
○ 4. AGAP	○ 49. OFW	○ 94. ABEKA	○ 140. TUTOK TO WIN
○ 5. KABAYAN	○ 50. ANG PROBINSIYANO	○ 95. KABATAAN	○ 141. BAYAN MUNA
○ 6. HOME OWNER	○ 51. ANAKALUSUGAN	○ 97. ACTS-OFW	○ 142. API
○ 7. KAPUSO-PM	○ 52. PBA	○ 98. RAM	○ 143. ARTE
○ 8. PDP CARES	○ 53. PEACE	○ 99. USWAG ILONGGO	○ 144. ASAP
○ 9. MARVELOUS TAYO	○ 54. P3PWD	○ 100. AKO BISAYA	○ 145. BUNYOG
○ 10. AKO OFW	○ 55. S.M.I.L.E	○ 101. KAPAMILYA	○ 146. AKBAYAN
○ 11. UNITED SENIOR CITIZEN	○ 56. AAMBIS-OWA	○ 102. PUSONG PINOY	○ 147. DAMAYAN
○ 12. WOW PILIPINAS	○ 57. BUTIL	○ 103. CWS	○ 148. WIFI
○ 13. ALTERNATIBA	○ 58. LUNAS	○ 104. SILBI	○ 149. CANCER PARTY LIST
○ 14. 1-RIDER PARTYLIST	○ 59. ANG KOMADRONA	○ 105. CIBAC	○ 150. 1-ANG EDUKASYON
○ 15. 1-CARE	○ 60. KABAKA	○ 106. SUBANEN	○ 151. UFCC
○ 16. ABP	○ 61. SAGIP	○ 107. AKKK	○ 152. AGRI
○ 17. AKO MUSIKERO	○ 62. ANG KABUHAYAN	○ 108. BG PARTY-LIST	○ 153. MAHARLIKA
○ 18. AKO BICOL	○ 63. AKO I.P.	○ 109. PAMILYANG MAGSASAKA	○ 154. ANAKPAWIS
○ 19. PRAI	○ 64. PHILRECA	○ 110. COOP NATCCO	○ 155. 1TAHANAN
○ 20. 4PS	○ 65. BABAE AKO	○ 111. FRONTLINERS ANG BIDA	○ 156. TURISMO
○ 21. MAAGAP	○ 66. KB	○ 112. KASAMA	○ 157. APEC
○ 22. ABANTE PILIPINAS	○ 67. GABRIELA	○ 113. AKO BISDAK	○ 158. SENIOR CITIZENS PARTYLIST
○ 23. AKTIBONG KAAGAPAY	○ 68. KALINGA	○ 114. GP PARTY	○ 159. AASENSO
○ 24. ALSA BISAYA	○ 69. STL	○ 115. BAHAY	○ 160. AMIN
○ 25. PROBINSYANO AKO	○ 70. RECOBODA	○ 116. MAGSASAKA	○ 161. 1-PACMAN
○ 26. YACAP	○ 71. ANGAT PINOY	○ 117. MARINO	○ 162. ABB-NFCPI
○ 27. MAGDALO	○ 72. ANAC-IP	○ 118. UNITED FRONTLINERS	○ 163. AN WARAY
○ 28. ACT-CIS	○ 73. DIWA	○ 119. L.O.G.R.O. KUSINERO	○ 164. AP PARTYLIST
○ 29. HUGPONG FEDERAL	○ 74. ASENSO PINOY	○ 120. BPO	○ 165. AKAP PINOY
○ 30. TGP	○ 75. PPP	○ 121. ABANG LINGKOD	○ 166. BUHAY
○ 31. ONE COOP	○ 76. PAMILYA MUNA	○ 122. KUSUG TAUSUG	○ 167. ARISE
○ 32. BARKADAHAN	○ 77. AYUDA SANDUGO	○ 123. PLM	○ 168. IWI
○ 33. DUMPER PTDA	○ 78. BUKLOD FILIPINO	○ 124. OK PARTYLIST	○ 169. ANG TINIG NG SENIORS
○ 34. MALABUNG	○ 79. BTS	○ 125. A TEACHER	○ 170. LBP
○ 35. BH (BAGONG HENERASYON)	○ 80. MOCHA	○ 126. PASADA-CC	○ 171. IPEACE EPANAW
○ 36. AKMA-PTM	○ 81. ACT TEACHERS	○ 127. OFW FAMILY	○ 172. APAT-DAPAT
○ 37. IPATUPAD	○ 82. ALONA	○ 128. ANGKLA	○ 173. UMA ILONGGO
○ 38. PINUNO	○ 83. KABALIKAT	○ 129. COCOMAN	○ 174. AYUDA
○ 39. LPGMA	○ 84. ALIF	○ 130. ACT AS ONE	○ 175. SAMBAYANAN
○ 40. CLICK PARTY	○ 85. 1-UTAP BICOL	○ 131. AKO ILOCANO AKO	○ 176. AKO BREEDER
○ 41. TODA	○ 86. TINGOG	○ 132. TRABAHO	○ 177. FPJ
○ 42. BHW	○ 87. H.E.L.P. PILIPINAS	○ 133. PATROL	○ 178. LIBRO
○ 43. MALASAKIT@BAYANIHAN	○ 88. TUCP	○ 134. AKO PADAYON	
○ 44. ABS	○ 89. PTA	○ 135. ABONO	
○ 45. PASAHERO PARTYLIST	○ 90. AGIMAT	○ 136. MANILA TEACHERS	

Figure SI-5: Party-list section of the 2022 ballot (raffle; 177 organizations, one gap). Authors' reconstruction from the official COMELEC sample ballot.

PARTY LIST *(Vote for 1)*

○ 1. 4PS	○ 40. BABAE AKO	○ 80. BOSES PARTY-LIST	○ 119. COOP-NATCCO
○ 2. PPP	○ 41. ARISE	○ 81. ARANGKADA PILIPINO	○ 120. KABAYAN
○ 3. FPJ PANDAY BAYANIHAN	○ 42. MAGDALO	○ 82. AANGAT TAYO	○ 121. 1MUNTI
○ 4. KABATAAN	○ 43. APEC	○ 83. OFW	○ 122. PINOY WORKERS
○ 5. DUTERTE YOUTH	○ 44. MAGBUBUKID	○ 84. BIDA KATAGUMPAY	○ 123. API PARTY
○ 6. ML	○ 45. SSS-GSIS PENSYONADO	○ 85. KAMANGGAGAWA	○ 124. AKO BISAYA
○ 7. PBBM	○ 46. GABRIELA	○ 86. BFF	○ 125. KAMALAYAN
○ 8. P3PWD	○ 47. TINGOG	○ 87. BUNYOG	○ 126. AKO TANOD
○ 9. MURANG KURYENTE	○ 48. APAT-DAPAT	○ 88. AGRI	○ 127. PROBINSYANO AKO
○ 10. BICOL SARO	○ 49. AHON MAHIRAP	○ 89. SENIOR CITIZENS	○ 128. KABABAIHAN
○ 11. IPATUPAD	○ 50. UGB	○ 90. 4K	○ 129. RAM
○ 12. PATROL	○ 51. AKBAYAN	○ 91. PBP	○ 130. ALONA
○ 13. JUAN PINOY	○ 52. AGIMAT	○ 92. ONE COOP	○ 131. AKO BICOL
○ 14. ARTE	○ 53. PHILRECA	○ 93. CIBAC	○ 132. GP (GALING SA PUSO)
○ 15. WIFI	○ 54. KAPUSO PM	○ 94. BH - BAGONG HENERASYON	○ 133. KAUNLAD PINOY
○ 16. MAAGAP	○ 55. ILOCANO DEFENDERS	○ 95. 1AGILA	○ 134. ABP
○ 17. UNITED SENIOR CITIZENS	○ 56. 1-RIDER PARTY-LIST	○ 96. EDUAKSYON	○ 135. CWS
○ 18. EPANAW SAMBAYANAN	○ 57. TICTOK	○ 97. ANG TINIG NG SENIORS	○ 136. LPGMA
○ 19. AKO PADAYON	○ 59. BAYAN MUNA	○ 98. BG PARTY-LIST	○ 137. A TEACHER
○ 20. TUCP	○ 60. ANG PROBINSIYANO	○ 99. PINOY AKO	○ 138. SWERTE
○ 21. ACT TEACHERS	○ 61. BANAT	○ 100. H.E.L.P. PILIPINAS	○ 139. GABAY
○ 22. 1PACMAN	○ 62. SBP	○ 101. HEALTH WORKERS	○ 140. MALASAKIT@BAYANIHAN
○ 23. TGP	○ 63. BUHAY	○ 102. PEOPLE'S CHAMP	○ 141. AKAY NI SOL
○ 24. DUMPER PTDA	○ 64. TULUNGAN TAYO	○ 103. AA-KASOSYO PARTY	○ 142. LUNAS
○ 25. ANAKALUSUGAN	○ 65. SAGIP	○ 104. SOLID NORTH PARTY	○ 143. DIWA
○ 26. AKSYON DAPAT	○ 66. BTS BAYANING TSUPER	○ 105. ABAMIN	○ 144. PINUNO
○ 27. BHW	○ 67. VENDORS	○ 106. TRABAHO	○ 145. PAMILYA MUNA
○ 28. SULONG DIGNIDAD	○ 68. ACT-CIS	○ 107. ANGKASANGGA	○ 146. BAGONG PILIPINAS
○ 29. BATANG QUIAPO	○ 69. AKTIBONG KAAGAPAY	○ 108. TODA AKSYON	○ 147. HUGPONG FEDERAL
○ 30. PBA	○ 70. ASENSO PINOY	○ 109. TURISMO	○ 148. TUPAD
○ 31. GILAS	○ 71. SOLO PARENTS	○ 110. ABONO	○ 149. LAANG KAWAL
○ 32. AKO ILOCANO AKO	○ 72. ANG KOMADRONA	○ 111. ASAP NA	○ 150. PAMILYA KO
○ 33. PAMILYANG MAGSASAKA	○ 73. PROMDI	○ 112. LINGAP	○ 151. BBM
○ 34. CLICK PARTY	○ 74. PUSONG PINOY	○ 113. UNITED FRONTLINERS	○ 152. HEAL PH
○ 35. ABANTE BISDAK	○ 75. KUSUG TAUSUG	○ 114. KASAMBAHAY	○ 153. ABANG LINGKOD
○ 36. MANILA TEACHERS	○ 76. DAMAYANG FILIPINO	○ 115. TUTOK TO WIN	○ 154. MAGSASAKA
○ 37. PAMANA	○ 77. MPBL	○ 116. AKO OFW	○ 155. MAHARLIKA
○ 38. NANAY	○ 78. ANGAT	○ 117. AGAP	○ 156. USWAG ILONGGO
○ 39. KM NGAYON NA	○ 79. KALINGA	○ 118. 1TAHANAN	

Figure SI-6: Party-list section of the 2025 ballot (raffle; 155 organizations, one gap). Authors' reconstruction from the official COMELEC sample ballot.

two are too collinear ($r > 0.999$) for their effects to be separated empirically. Second, some printed organizations' votes ultimately carried no representation: ten organizations on the 2013 ballot were disqualified before canvassing, and the registrations of An Waray (2022) and Duterte Youth (2025) were cancelled after the elections, with their seats passed down the ranking (Section SI 2). All of these organizations occupy grid slots, and therefore enter the construction of every other organization's sequence rank; our outcome regressions exclude their vote observations. This exclusion is conservative (Section SI 6). We also verify that results are robust to excluding the disqualified organizations' grid neighborhoods entirely.

SI 4.2 Electoral Returns: Sources and Aggregation

National vote totals for 2010, 2013, 2022, and 2025 come from the official canvass results. For 2016 and 2019 we additionally constructed returns from the official disaggregated results files, cleaned and summed to municipal and national totals; the resulting municipal files cover 1,644 municipalities across the two elections.

Coverage validates well. The constructed 2016 and 2019 national totals fall within one to four percent of the official figures, and, more stringently, estimates computed from the constructed data reproduce estimates computed from official totals almost exactly: the cross-sectional rank elasticities from the aggregated municipal file are -0.148 (2016) and -0.149 (2019), against -0.15 in both years from the official-totals file (Section 6 of the main text).

All construction code, the slot-level layouts, the crosswalks, and the constructed datasets in Stata, R, and CSV formats are part of the replication archive.

SI 5 Balance and Randomization Inference Detail

SI 5.1 Balance Across the Five Raffles

If the raffles were sound, nothing an organization brought to the draw should predict where the draw put it. Table SI-1 tests this for every raffle and every position variable. Each cell reports the joint F statistic from regressing the position variable (sequence rank, row within column, or column) on six predetermined characteristics: prior-election vote share, an indicator for having appeared on the prior ballot, an indicator for holding a seat in the outgoing Congress, indicators for names beginning with a digit or with "A," and name length. Randomization-inference p-values, the share of 5,000 permuted raffles producing an F at least as large, appear in brackets. Nothing predicts position: across the fifteen joint tests the RI p-values range from 0.16 to 0.95, and the name-based characteristics that mechanically occupied the top of the 2010 ballot are included in every test.

SI 5.2 Independence of Successive Draws

The within-organization designs additionally require that successive raffles be mutually independent, so that an organization's new position is a fresh draw rather than an echo of its old one. Pooling all consecutive raffle pairs and regressing an organization's current log rank on its previous log rank, with election fixed effects, yields a coefficient of -0.039 (se 0.052), indistinguishable from zero.

Table SI-1: Balance: do predetermined characteristics predict raffled position?

Raffle	Sequence rank	Row within column	Column
2013 ($n = 123$)	0.52 [0.79]	0.55 [0.77]	0.61 [0.71]
2016 ($n = 115$)	1.34 [0.25]	0.91 [0.49]	1.58 [0.16]
2019 ($n = 134$)	0.55 [0.74]	1.04 [0.40]	0.39 [0.85]
2022 ($n = 177$)	0.66 [0.69]	0.28 [0.95]	0.61 [0.74]
2025 ($n = 155$)	0.33 [0.92]	0.29 [0.95]	0.34 [0.92]

Notes: Each cell reports the joint F from regressing the position variable on the six predetermined characteristics listed in the text, for the full printed roster of the given raffle; randomization-inference p-values in brackets (5,000 permutations of positions across organizations within the election; p = share of permuted F statistics at least as large as the observed one).

SI 5.3 The Randomization-Inference Algorithm

Every randomization-inference p-value in the paper is computed the same way. The realized slot structure of the relevant election is held fixed: the set of printed grid positions, including the positions of printed-but-uncanvassed entries, is exactly the set COMELEC printed. A placebo assignment permutes the canvassed organizations over those positions, mimicking a re-run of the raffle; the statistic of interest is recomputed; and the procedure repeats 5,000 times (2,000 for the per-cell balance tests) with a fixed seed. The reported p-value is the share of placebo statistics at least as extreme as the observed one, with the observed draw included in the numerator and denominator. For pooled statistics, permutation is within election, respecting the fact that each raffle randomized only within its own roster. Because assignment really was random, these p-values are exact under the sharp null of no effect for any organization and require no asymptotic or distributional assumptions.

SI 6 Additional Results

SI 6.1 Validation of the Seat-Allocation Implementation

Section 6.5 of the main text allocates counterfactual seats with our implementation of the statutory formula. Table SI-2 validates that implementation by applying it to actual vote shares and comparing organization-level seat counts with the official allocations. Agreement is 94.7 to 98.3 percent in the cycles for which an organization-level official allocation is on record (2013, 2022, 2025), and the disagreements coincide with each cycle's documented proclamation disputes rather than with the formula's mechanics. For 2016 and 2019, organization-level official seat lists are not in our files, so the check is coarser: the formula predicts 46 and 50 seat-winning organizations, respectively; for 2019, against the official count of 51, it reproduces both official three-seat winners (ACT-CIS and Bayan Muna), while awarding Ako Bicol a third seat where the official allocation gave two.

Table SI-2: Seat-formula validation against official allocations

Cycle	Organizations	Exact agreement	Organizations differing
2013	113	94.7%	6
2016	115	(coarse check; see text)	–
2019	134	(coarse check; see text)	–
2022	176	98.3%	3
2025	154	98.1%	3

Implementation: guaranteed seat at two percent; proportional additional seats capped at three; remaining seats one each in descending vote order to the quota. Differences concentrate in proclamation disputes (for example, the An Waray second seat in 2013 and the two deferred proclamations of 2025).

SI 6.2 Robustness of the Headline Elasticity

Table SI-3 re-estimates the pooled within-organization specification of Table 3 of the main text, column (3), under alternative outcome and treatment definitions. The untransformed outcome scales each share by the number of canvassed organizations, so that a value of one is a uniform share; the scaled-rank treatment replaces log rank with rank as a fraction of the printed roster. The estimate is also unchanged when every organization printed adjacent to an uncanvassed slot (same column, one row above or below) is excluded, which addresses the concern that printed-but-uncanvassed entries distort their neighbors' visual environment. Municipality-level estimates, which weight places equally rather than in proportion to their electorates, appear in Section 6 of the main text and are steeper, reflecting the extensive margin of geographic reach.

The exclusion of the two post-election cancellations is likewise conservative. Both later cancellations were counted at canvass (385,460 votes for An Waray in 2022; 2,338,564 for Duterte Youth, the second-largest total of 2025, from the fifth ballot slot). Including these officially counted votes strengthens the estimates: the 2025 cross-sectional gradient moves from -0.12 to -0.17, and the pooled within-organization elasticity from -0.155 to -0.162.

Table SI-3: Headline elasticity under alternative definitions

Specification	Estimate	(se)
Baseline: log share on log rank	-0.155	(0.046)
Untransformed outcome (share $\times$ number canvassed)	-0.215	(0.058)
Scaled-rank treatment (rank as fraction of roster)	-0.422	(0.148)
Excluding uncanvassed neighborhoods	-0.158	(0.049)

All rows: organization and election fixed effects, organizations contesting two or more raffles (505 observations; 496 in the final row), cluster-robust standard errors by organization. The scaled-rank coefficient is a semi-elasticity over the whole roster and is not directly comparable in magnitude to the log-rank rows.

Table SI-4: Position effects under randomization: joint tests

	2013	2016	2019	2022	2025
Parties (canvassed)	113	115	134	176	154
Joint F (position basis)	3.51	1.37	0.76	1.49	1.11
R^2	0.213	0.094	0.046	0.067	0.058
RI p-value	0.002*	0.221	0.631	0.171	0.355

Outcome: log share of party-list votes. Position basis: row/column-length, its square, column indicators, and row-by-column interactions (8 terms). RI p-values from 5,000 permutations over the realized slot structure of each election. $^*p < 0.05$.

SI 6.3 Cross-Sectional Test Tables

Tables SI-4 through SI-7 report the joint existence tests, the signature battery, and the two uniformity moderation tables discussed in Section 6 of the main text.

Table SI-5: Signature tests of competing scanning models

Test	Cross-sections 2013	2016	2019	2022	2025	Within-organization (pooled, all raffles)
Sequence rank (linear)	−0.39 [0.22]	−0.53 [0.15]	−0.26 [0.46]	−0.54 [0.07]	−0.08 [0.78]	−0.42* [0.004]
Rank^2	+3.06* [0.01]	−0.65 [0.65]	+1.44 [0.29]	−2.57* [0.02]	+1.10 [0.34]	+0.68 [0.19]
Row × column	−0.76* [0.01]	−0.01 [0.98]	+0.14 [0.65]	+0.31 [0.25]	−0.29 [0.26]	−0.18 [0.15]
Column main effect (leftness)	+0.45* [0.01]	+0.08 [0.65]	−0.02 [0.89]	−0.03 [0.85]	+0.13 [0.40]	+0.16* [0.02]
Row main effect only	−0.56 [0.09]	−0.92* [0.01]	−0.36 [0.29]	−0.36 [0.22]	−0.64* [0.03]	−0.55* [0.001]
Top-of-column jump	+0.25 [0.47]	+1.12* [<0.01]	+0.03 [0.95]	−0.14 [0.73]	−0.20 [0.60]	+0.31 [0.051]
Row^2 / Column^2	n.s.	n.s.	n.s.	−0.17 [0.05][a]	n.s.	+1.62* [0.002][b]

Key coefficients from the indicated specifications; outcome is log share of party-list votes, positions scaled, column coded so that larger values are further left. RI p-values in brackets (5,000 permutations). Each row tests a distinct model from Section 3 of the main text: a linear rank term is the common prediction of all primacy models; a positive Rank^2 term indicates convex primacy (losses concentrated among the earliest slots, as stopping-rule models require); the row-by-column interaction separates convex primacy (negative: the downward penalty is steepest in the leftmost column) from F-pattern scanning (positive: the penalty would be mildest at the left); a column main effect is implied by reading order and by pseudoneglect; a row-only gradient with no column structure indicates layer-cake scanning; the top-of-column jump (an indicator for rows 1–3 of columns 2–4, conditional on smooth rank controls) separates spatial from ordinal accounts, which predict a positive jump and zero, respectively; and row and column quadratics test central fixation ([a]the reported 2022 coefficient is the column quadratic; the row quadratic is insignificant there; [b]the reported within-organization coefficient is the row quadratic, indicating convex vertical decay; the column quadratic is insignificant). The final column pools all five raffles with organization and election fixed effects (organizations contesting two or more raffles; 505 observations; standard errors clustered by organization; RI permutes grid assignments within each election). $^*p < 0.05$.

Table SI-6: Organizational uniformity: moderators of the rank penalty

Moderator (standardized)	$\ln r \times$ moderator	se	RI p
First-time organization	-0.020	(0.040)	0.64
Seat in outgoing Congress	-0.010	(0.033)	0.75
Prior-election vote share	$+0.016$	(0.038)	0.78

Outcome: log share of party-list votes, organization-election level, five raffled elections ($n = 692$). Each row is a separate estimate of equation (5) of the main text: election fixed effects, the moderator's main effect included, moderator standardized. Standard errors clustered by organization; RI p-values from 5,000 permutations of positions within each election. First-time indicates no appearance on any earlier ballot (2010 onward); incumbency is constructed from the statutory seat allocation of the preceding election; prior share is zero for organizations absent from the preceding ballot. $^{*}p < 0.05$.

SI 6.4 Why the 2013 Cross-Section Speaks Loudest

Why does 2013 speak loudest? The question deserves a direct answer, because the obvious explanation, that voters learned their way out of the effect after the first raffle, is the one the data reject. Had learning eroded the penalty, the within-organization elasticity would have declined across raffles, and it did not (Table 3 of the main text). What faded is the cross-section's signal-to-noise ratio, and Section 6.4 of the main text makes the reason intelligible. The 2013 roster was composed of alphabetical-era survivors: organizations that had competed under a regime in which names, not fame, determined placement, and whose baseline popularity was correspondingly compressed. The largest 2013 vote-getter won eight times the median organization's votes; by 2022 the ratio was thirty. Position explained a fifth of the variance in 2013 in part because there was little else to explain. The transformation that followed was on the supply side. Media-branded organizations entered and demonstrated that national fame converts into votes from any slot, inflating between-organization variance and burying the cross-sectional signature without touching the per-organization penalty. There is learning in this story, but it is the learning of political entrepreneurs about what wins votes, not of voters about how to read a ballot. A residual possibility remains that the first raffle also caught the electorate at its most position-dependent, before any voter arrived with a memorized ballot number and before any campaign had printed one on a poster; the 2013 cross-section carries the largest rank slope and the sharpest convexity, as that story would predict. But those differences are not statistically distinguishable from a constant effect, so we flag the possibility and rest on the panel's verdict: the mechanism did not weaken, the world around it got noisier.

SI 6.5 Gapped versus Gap-Free Numbering

The 2016 raffle assigned final ballot numbers only after disqualification petitions were resolved, so its printed numbering is gap-free, while the 2013, 2019, 2022, and 2025 ballots

Table SI-7: Geographic uniformity: municipal moderators of the rank penalty

Moderator (2010 census, standardized)	$\ln r \times$ moderator	se
College-educated share	$+0.011$	(0.013)
Log income per capita	$+0.000$	(0.004)
Log population density	-0.002	(0.013)
Multidistrict-city indicator	$+0.008^{*}$	(0.003)

Outcome: $\ln((\text{votes} + 0.5)/\text{municipal party-list votes})$, organization $\times$ municipality $\times$ election (2016, 2019); $n = 396{,}854$ (density: 394,248). All specifications include organization-election and municipality-election fixed effects; standard errors clustered by organization-election. Moderators are 2010 municipal census aggregates, standardized. $^{*}p < 0.05$.

carry gaps left by post-raffle delistings (thirteen, 47, one, and one). If gap handling distorted our position coding, the 2016 estimates should differ systematically from the gapped elections. They do not: the election-specific within-organization elasticity for 2016 (-0.238) sits between the gapped elections' estimates, and the joint test of Section 6 of the main text cannot reject a common elasticity across all five raffles ($p = 0.84$). Throughout, sequence rank is defined over printed slots, not printed numbers, so gaps affect neither the treatment definition nor its interpretation.

SI 7 The Reform as Treatment: Composition, Incidence, and Sorting

This appendix reports three companion analyses to Section 6.4 of the main text.

SI 7.1 A Direct Compositional Test: The 2007 Baseline

The adverse-selection reading of Section 6.4 of the main text rests on a residual: quality inferred by netting the causal position effect out of 2010 outcomes. A cleaner test uses performance that predates the outcome entirely. The May 2007 party-list election, the last before automation, provides it: 93 organizations contested, and the official canvass is preserved in the vote tabulation reproduced in *BANAT v. COMELEC* itself.[23] We link 76 of the 93 to the 2010 roster by name, acronym, and documented renamings.

[23] *BANAT v. COMELEC*, G.R. Nos. 179271 and 179295, April 21, 2009, `lawphil.net`, which reproduces the National Board of Canvassers ranking of all 93 participants. We cross-checked the seat allocation against contemporaneous reports of the April 2009 re-

Table SI-8 compares the lexically engineered digit-and-“A” block (2010 slots 1–115) with the rest of the 2010 roster on three predetermined measures. Block organizations were less likely to have contested 2007 at all (−4.8 percentage points, se 7.4), won a smaller mean share of the 2007 vote (−0.26 percentage points, se 0.18), and held fewer 2007 seats (−0.11, se 0.10). Conditional on having contested, the average block organization's 2007 vote share was 1.01 percent against 1.50 percent for the rest: about a third weaker (difference −0.49, se 0.35). No single contrast is significant at the five percent level, but all four point estimates are negative, and the conditional gap sits close to the 25 percent implied by the residual decomposition of Section 6.4 of the main text. The direct and the residual-based tests agree: the organizations that engineered their way to the top of the 2010 ballot were weaker before they got there.

Table SI-8: Predetermined 2007 strength of the 2010 digit/“A” block

2007 measure	Digit/“A” block	Rest of roster	Difference (se)
Contested 2007	0.38	0.43	−0.048 (0.074)
Vote share, % (all)	0.39	0.65	−0.260 (0.177)
Seats	0.24	0.35	−0.112 (0.103)
Vote share, % (if contested)	1.01	1.50	−0.493 (0.351)

The block comprises 2010 ballot slots 1–115 ($n = 115$); the rest, slots 116–187 ($n = 72$). Unconditional rows assign zero to organizations absent from the 2007 ballot. Conditional row: $n = 44$ block, $n = 31$ rest. Standard errors from two-sample comparisons of means.

SI 7.2 Reform Incidence Among Continuing Organizations

For the 82 organizations printed on both the 2010 and 2013 ballots, the reform is a treatment: a chosen position replaced by a drawn one. Column (1) of Table SI-9 regresses the change in log vote share across the reform on the randomly drawn 2013 log rank and the chosen 2010 log rank. Both signs are what causal reallocation predicts. Organizations the draw pushed deeper lose share (−0.078, HC1 se 0.077; RI $p = 0.34$), and the forfeited 2010 advantage protects no one (+0.012, se 0.078). At $n = 82$ the estimates are attenuated and imprecise, and the sample conditions on surviving the 2013 purge, so we report the exercise as corroboration rather than evidence.

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proclamation.

SI 7.3 Sorting on Gains

If the organizations with the most to gain from placement were the ones that bought it, position-adjusted quality should decline with chosen 2010 rank among survivors. Column (2) of Table SI-9 uses 2013 outcomes purged of the causal position effect as the quality measure and regresses them on chosen 2010 rank. The slope is +0.054 (HC1 se 0.103): weakly positive, meaning the organizations that chose the earliest slots were, if anything, the weaker ones. This is the adverse-selection signature a third time, in the survivor sample.

Table SI-9: The reform as treatment: incidence and sorting among continuing organizations

Outcome	(1) Δ log share, 2010→13	(2) Adjusted 2013 quality
ln rank 2013 (drawn)	−0.078	
	(0.077) [0.34]	
ln rank 2010 (chosen)	+0.012	+0.054
	(0.078)	(0.103)
Observations	82	82

Sample: organizations printed on both the 2010 and 2013 ballots with canvassed votes in both years. Column (2)'s outcome is $y_{i,2013} - \hat{\gamma}^{FE} \ln r_{i,2013}$ with $\hat{\gamma}^{FE} = -0.155$. HC1 standard errors in parentheses. The bracketed randomization-inference p-value permutes the 2013 draw among the continuing organizations (5,000 permutations); no RI p-value accompanies the 2010 rank because 2010 positions were chosen, not randomized. $^{*}p < 0.05$.