How Campaigns Respond to Ballot Position: A New Mechanism for Order Effects

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Abstract

An established finding on ballot design is that top positions on the ballot improve the electoral performance of parties or candidates because voters respond behaviorally to salient information. This paper presents evidence on an additional unexplored mechanism: campaigns, that can act before voters, adjust their behavior when allocated a top position on the ballot. We use a constituency-level lottery of ballot positions in Colombia to establish first that a ballot-order effect exists: campaigns randomly placed at the top earn more votes and seat shares. Second, we show that campaigns react to being placed on top of the ballot: they raise and spend more money on their campaign, and spending itself is correlated with higher vote shares. Our results provide the first evidence for a new mechanism of ballot-order effects examined in many previous studies.

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Voting is at the heart of democracy, but what explains voter choice? Among many explanations, past work on ballot design shows that voters have a tendency to vote disproportionately for whoever is listed at the top of the ballot, regardless of the identity of the party or candidate. The primary postulated reason in the long literature examining ballot order effects argues that this occurs because voters respond behaviorally to salient parts of the ballot (Krosnick, Miller and Tichy, 2004; Ho and Imai, 2008; Blom-Hansen et al., 2016). This paper argues and presents evidence for an additional mechanism that may augment or countervail behavioral reasons for ballot order effects: before the election takes place, campaigns may adjust strategically once the ballot order is revealed, impacting directly voters' decisions on election day.

We test campaign responses to ballot order effects in Colombia, which is an ideal context for this purpose; party positions on ballots for local councils are assigned through lotteries held in each constituency and there is systematic data on campaign spending. We combine election data, scans of 1099 ballots, and novel revenue and expenditure data for each campaign to present three results. First, we confirm that a ballot order effect exists in Colombia – party-lists assigned the top row get higher vote and seat shares. Second, we present novel evidence that those assigned the top row of the ballot raise 12.28 percent more money and spend an equivalent amount on campaigning, mostly on publicity and electioneering. Finally, we show that there exists a correlation between higher expenditure and vote share in our sample, opening the possibility that changes in vote shares are, in part, due to increased expenditure.

This paper makes several contributions. First, our paper presents novel evidence for a new mechanism of ballot order effects (Brians and Grofman, 2001; Addonizio, Green and Glaser, 2007; Ansolabehere, 2009). Our evidence – that ballot order effects can arise through a campaign strategic response channel in addition to a voter channel – potentially explains some null effects in the literature (Lau and Redlawsk, 2001; Augenblick and Nicholson, 2015). More broadly, these results are in line with recent work that argues that voters may be more strategic (and less behavioral) than is commonly assumed (Ashworth, De Mesquita and Friedenberg, 2018), since they could be reacting to increased campaign spending. Second, we add evidence on ballot order effects for elections from a developing country to a literature that is dominated by research on the US and other developed democracies (Blom-Hansen et al., 2016; Darcy and McAllister, 1990). Instead of effects being

larger, as predicted by previous work for contexts where voters may rely more on heuristics, the effects we observe are similar in magnitude to studies from the US. More generally, our results are relevant for the literature on campaigns and how they strategically adjust their behavior in light of new information on election day factors (Carsey et al., 2011; Hartman, Pattie and Johnston, 2017) or how ballot design can affect the number of invalid votes cast (Pachon, Carroll and Barragán, 2017). Most importantly, our study is the first to provide evidence that campaigns react to ballot design.

A Campaign-Based Mechanism

The order in which candidates and parties appear on the ballot can affect their electoral performance (see Appendix Table A1 for a summary of previous findings). A large body of work either explicitly tests for (Ho and Imai, 2008; Koppell and Steen, 2004; Meredith and Salant, 2013; Kim, Krosnick and Casasanto, 2015; Geys and Heyndels, 2003; Blom-Hansen et al., 2016), or suggests that, a voter-based mechanism explains why ballot-order effects exist (Alvarez, Sinclair and Hasen, 2006; Chen et al., 2014; Darcy, 1986; Miller and Krosnick, 1998; Gold, 1952; Faas and Schoen, 2006; King and Leigh, 2009; Krosnick, 1991). Decision-fatigued voters, operating in low information environments, use the ballot order to help make their choice. Our *additional* account focuses on campaign responses for order effects. While work shows that candidates care a lot about the order in which they appear on the ballot (e.g. Krosnick, Miller and Tichy, 2004), and Ho and Imai (2008) even highlight the possibility that campaigns might respond to ballot order, to date this mechanism has not been empirically tested.

The order in which names appear on the ballot is often announced weeks before an election, allowing campaigns the opportunity to adjust their behavior. For example in the case analyzed by King and Leigh (2009), Australia uses a random ballot order at the federal level which is announced several weeks in advance of the election date. In 2004, this randomization was conducted on the 17th September and the election held on the 9 October, giving parties over three weeks to react to their ballot order placement. We code several previous studies that document ballot order effects in real world settings and find that, in 9 out of 12 cases, campaigns had the ability and time to adjust their strategy after the announcement of the ballot order and the election day (See Table A1). One way campaigns could adjust behavior in response to the announced ballot order is by raising and spending campaign money differently.

If campaigns can plausibly adjust their strategy in either direction, how we interpret the effects in the literature will differ by the direction of this adjustment. On the one hand, campaigns allocated a prominent spot on the ballot may reduce campaigning efforts to account for the possibility of getting a vote bump in the upcoming election due to their increased salience. If this effect holds, then the existing literature that stresses election-day voter-based explanations for ballot order effects understates the effect of a prominent spot on the ballot, since campaigns are taking mitigating action. Conversely, campaigns with a prominent position on the ballot may increase their campaigning efforts if the expected vote bump brings them close to the possibility of winning (more). That is, additional campaigning to convert voters might now take them over the edge and help them win. In this case, the campaign response is working in the same direction as the ballot order effects identified in existing work. Therefore, the observed effects on electoral outcomes in the current literature might be at least partially explained by campaigns' reactions.

Background–Local Elections in Colombia

Colombia is currently divided into 1099 municipalities where local elections are held every 4 years. In each local election, politicians are elected to fill positions on a council that serves as the local legislative body. The council's main role is to approve the annual budget on projects proposed by the municipal mayors and play a supervisory role for these projects. While Colombia was historically bipartisan, it currently has many parties that contest elections across municipalities. For a municipality, these party organizations may present a single list of candidates under their banner. We refer to this list as the 'party-list'. Within each party-list for a specific municipality, there are multiple candidates. We distinguish between "party-organizations" (understood as the centralized party apparatus that can operate across several municipalities), "party-lists" (for specific municipalities), and candidates throughout the remainder of this paper.

According to Hangartner, Ruiz and Tukiainen (2019), party organizations usually just lend their credentials to party-lists, with minimal intervention in terms of funding and expenditure. In 2015 – for which campaigning data is available for local councils – there were an average of 8 party-lists on each municipality ballot. Council ballots have a conventional ballot design with a single column layout, with party logos and without candidate pictures. A proportional representation system is used to elect councils where parties can choose, before the ballot lottery, between an open list (voters choose a party-list and candidate) or closed list (voters choose the party-list only, and votes

are distributed according to a predetermined ranking of candidates). An example of the council ballot can be found in Figure A1, where Liberals presented a party-list for Pacora municipality and the list was placed in the second-row of the ballot.

Random assignment of position in ballots: During the electoral cycle, parties announce their intention to run within municipalities, pick a list type, and submit an ordered list of candidates to their local registry. Election administrators then conduct a random lottery to assign each party-list to a position in the ballot. Importantly, the party-list candidates and list type is unaffected by the position in the ballot since the randomization takes place after registration. Between the ballot lottery and election day, party-lists (and the candidates within them) have about 3 months to react to their ballot position (see Table A2). Seats are allocated to party-lists on the basis of their aggregated vote share and for open party-lists candidates who receive the most votes are assigned those seats. Party-lists using a closed-list ballot assign seats based on their initial ranking of candidates. The ballot randomization is conducted by a non-partisan entity, is independently verified, and campaign representatives can be present at the lottery.

Empirics and Data

Data: We use the electoral data compiled by Pachón and Sánchez (2014) with updated results from the *Registraduría Nacional del Estado Civil*. In order to code ballot position we obtained scans of all council elections ballots in 2015 (N=1,099). Using hand coding and an optical character recognition package in Python, we coded if a party-list is placed on top of the ballot.[1] We obtained data on campaign income and spending from the National Electoral Commission (*Cuentas Claras*). For each candidate, these data report the total income of the campaign, broken down by source of income from the candidate's own sources, donations, or the party organization. Similarly, the data report total expenditure, broken down by expenditure items. We calculate the total income and spending for party-lists per voter – by adding all spending/income of the candidates in the list and dividing by the number of registered voters in the constituency. A detailed breakdown is available in Table A3. The reporting system has imperfect compliance; 90% of party-lists disclose campaign income, but missingness is uncorrelated with being assigned the top row of the ballot (see Table A4 column 8).

Estimation: We assemble a party-list level dataset and run regressions of the following form:

[1] We randomly picked 5 percent of the ballots and manually checked ballot position and found no errors.

$$Y_{pc} = \beta Top\ Row_{pc} + \alpha_c + \gamma_p + \varepsilon_{pc},$$

where outcomes, Y_{pc}, are measured for each party-list: a list for party organization p in constituency (municipality) c. $Top\ Row_{pc}$ is an indicator variable for whether the party-list enters the ballot in the top row. We include ballot/constituency fixed effects (α_c) in the regression to account for common shocks at the constituency level. Finally, party organization fixed effects (γ_p) control for party organization characteristics. We cluster standard errors at the ballot/municipality level.

Balance: We test the validity of the random assignment of ballot position. For instance, one concern is that bigger party organizations are able to manipulate the system to be systematically on top of the ballot. We code party size (measured as the number of municipalities the party contests), whether the party has participated in more than one election, their previous vote share in the municipality, use coding by (Fergusson et al., 2020) to see if the party is right leaning or a main traditional party, as well as missing reports on campaign data, and list type chosen. We also test for differences in candidate characteristics in party-lists. Overall the results described in Appendix C.1, Table A4 and Table A5, show good balance, allaying concerns that the lottery was systematically manipulated.

Results

Table 1: **The Effect of Party-List in Top Row of the Ballot**

	Electoral Outcomes			Campaign Finance		
	Vote Share	Seat Seat	# Seats	Revenue	Total Expenditure	Publicity Expenditure
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Row = 1	0.009***	0.012***	0.145***	84.966**	81.966**	37.195*
	(0.003)	(0.004)	(0.04)	(39.477)	(39.183)	(22.532)
Mean if Row > 1	0.126	0.127	1.415	679.397	667.314	261.874
Effect Size (%)	7.245	9.761	10.277	12.506	12.283	14.204
# Ballots	1099	1099	1099	1099	1099	1099
# Observations	7886	7886	7886	7886	7886	7886
Ballot FE	Yes	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Standard errors, clustered at the municipality/ballot level, are in parentheses. Each observation is a party-list within a ballot. Races with more than one row on the ballot are included.

Table 1 columns 1-3 confirm existing results in the literature by showing that the top position on the ballot translates into better electoral performance. The top row increases a party-list's vote share by 0.9 percentage points, a treatment effect of about 7.3 percent. The treatment also affects actual electoral outcomes: the seat share of the top row party-list increases by 1.2 percent points, and there is a 14.5 percent increase in the probability of winning an additional seat. This is verified by

the increase in the number of seats won by the party-list. We also check if there is a ballot order effect in rows other than the top row by comparing each succeeding row with subsequent rows (see Appendix Figure A2 and A3). We find that ballot order effects are only present for the first row. We therefore focus the remaining analyses on comparisons between the first and all other rows.

Next, we analyze party-list income and spending data to test if getting the top position affects the way party-lists behave before election day. Looking first at income, the results in Table 1 column 4 show that party-lists who are allocated the top row raise about 12.5 percent more funds. We also break this increase in income into official reporting categories to show that the increase come primarily from candidates' own pockets and not from additional donations, loans, or transfers from the party organization – confirming the primacy of the candidate's role over the party organization (see Appendix Table A10 for results, and Table A8 for details of coding).

Looking at expenses next, in Table 1, we find that party-lists allocated the top row of the ballot spend about 12.3 percent more on campaigning. We further demonstrate how ballot order affects campaign activity by coding *transaction-level* data from campaign expenditures. We code if transaction details include words such as 'posters' and 'flyers' that signal campaigning (see Table A9 for all key words used). We sum these publicity expenditures for party-lists and normalize them by the number of registered voters in the constituency. Table 1 column 6 shows that being allocated the top position on the ballot increases the amount spent on 'publicity' by 14.2 percent. We also find that top row party-lists spend more money on Administrative, Transport, and Mailing expenses (see Table A11). These results are consistent with the party-list reacting to their top row position by sending more mailers to voters. Finally, a concern with these results is that expenditure occurs *before* the ballot order was known. This is because spending is allowed a week before the lottery is conducted. However, the evidence shows that spending is balanced before the lottery and that the order effect is only present post-lottery, and more specifically only for the weeks right before the election (see Appendix C.4 and Table A7).

Furthermore, we find that party-list spending is correlated with vote shares: Appendix Table A12 shows that every 1,000 pesos spent per registered voter is correlated with 3.1 percentage points

higher vote share and 4.1 percentage points higher seat share.[2] Speculatively, using the previous results in Table 1 column 5, the additional spending of 81.97 pesos per voter by party-lists suggests that campaigns at the top of the ballot potentially increased their vote and seat shares by 0.25 and 0.33 percentage points respectively.[3] This could be a sizable effect via spending given that the total effect for being placed on top is 0.9 and 1.2 percentage points, for vote shares and seats shares respectively.

Unpacking campaign responses

So far our results have focused on the party-list level, and in this section we conduct exploratory analysis to unpack the results by looking at within-list candidate actions. This helps us explore if ballot order reactions by campaigns are strategic (rather than purely behavioral).

We explore this by exploiting two unique features of our case, both determined *before* the lottery takes place. *First*: party-lists in Colombia can run under either open or closed lists in each constituency. Open lists incentivize candidates to exert more campaigning effort since they can obtain votes directly. Comparatively, candidates lower placed in closed lists have fewer incentives to campaign since their additional spending would yield benefits for candidates at the top of the list rather than themselves. *Second*: in PR elections, parties present a list with candidates placed in different positions strategically. Candidates at the top of the *list* tend to be party leaders who are well-recognized and carry a higher chance of winning, while candidates placed in the middle or bottom have less recognition and a relatively lower chance of winning. For instance, Mustillo and Polga-Hecimovich (2020) suggest that highly placed candidates within lists do confer electoral advantages, and Hangartner, Ruiz and Tukiainen (2019) via interviews confirm that in the case of Colombia; higher placed candidates tend to have local recognition and be party leaders. Given this set-up, we have two predictions: First, candidates in *open* lists are more likely to change campaigning when their party-list is randomly assigned the top position on the ballot since candidates can reap the benefits of additional campaigning (as opposed to closed lists). Second, we expect that candidates placed at the top of open lists – those that have documented recognition and are more sure of victory – are less likely to spend more when the party-list is placed at the top of the ballot,

[2]We omit the top party-list to avoid compounding the potential voter driven ballot order effect. For robustness, we also include the top party and the results are similar (see Table A13).

[3]In Appendix Section D.4 we provide a further discussion on estimating mediation effects.

while candidates in the middle and bottom of the list would spend more, given that the top ballot position of the party-list improves their chances of victory.

We find that *only* in open lists do candidates seem to spend more when their party-list is placed on the top row of the ballot (see Figure A6). Moreover, estimating these comparisons with municipality and party organization fixed effects, and grouping by candidate positions within the list – where *top* is the first position, *middle* is positions 2 to 4, and *low* is 5 or lower – we find that in open lists top candidates do not increase spending, while candidates in the middle and bottom positions do (see Figure A9). In the case of closed lists, however, there is no positive adjustment of spending across the three groups of candidates. In summary, campaign spending does seem to be a strategic reaction that varies by candidate position within the party-list as well as the type of list.

Conclusion

We present evidence that campaigns react to ballot positions by raising and spending more money on campaigning before elections. Our key contribution is to show that these results illustrate an unexplored mechanism of a long established effect of ballot order in the literature. Existing explanations of ballot order effects may be overstating the contribution of a behavioral channel as the only mechanism for the observed ballot order effect: campaigns also strategically adjust their behavior once they are allocated a prominent position on the ballot, and this strategy can lead to more votes. Further study of campaign reactions is therefore a fruitful area of research that should be included in the vast literature of ballot order effects and election administration more broadly.

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FOR ONLINE PUBLICATION: APPENDIX

Contents

A Review of ballot order effect studies

Table A1 briefly reviews studies that have considered ballot position effects using natural experiments. We provide a summary of the substantive findings as well as information about the cases under consideration in each study:

Table A1: **Natural experimental studies of ballot position effects from Blom-Hansen et al. (2016) and others, with information added on campaign spending**

Natural experimental studies	Identified ballot position effect	Campaign spending allowed?	Election
Natural experiments from the USA (random rotation of order of candidates)			
Alvarez, Sinclair and Hasen (2006)	Positive effect of being listed first	Allowed	California, 1998 All statewide races
Chen et al. (2014)	Positive effect of being listed first	Allowed	North Dakota, 2000-2006 All statewide elections
Darcy (1986)	No position effect	Allowed	Colorado, 1984 All statewide races
Ho and Imai (2006)	Positive effect of being listed first	Allowed	California, 2003 Gubernatorial recall election
Ho and Imai (2008)	Positive effect of being listed first	Allowed*	California, 1978-2002 All statewide races
Koppell and Steen (2004)	Positive effect of being listed first	Allowed	New York City, 1998 All statewide Democratic primaries
Krosnick, Miller and Tichy (2004)	Positive effect of being listed first	Allowed	Ohio (All statewide races), North Dakota(All statewide races) and Californa(President and Senate), 2000
Meredith and Salant (2013)	Positive effect of being listed first	-	City council & California, 1995-2008 City council and school board elections
Miller and Krosnick (1998)	Positive effect of being listed first	Allowed	Ohio, 1992 All statewide and countywide races
Pasek et al. (2014)	Positive effect of being listed first	Allowed	California, All statewide general election, 1976-2006 (where name order was rotated)
Natural experiments from outside the USA			
Faas and Schoen (2006):	Positive effect of being listed first	Regulated spending: Broadcast through TV/Radio granted during a period before the election	Bavarian state elections in Germany
Geys and Heyndels (2003):	Positive effect of being listed first	Strictly regulated spending	Regional elections in Brussels
King and Leigh (2009):	Positive effect of being listed first	Allowed	Australian federal elections
Blom-Hansen et al. (2016):	Positive effect of being listed first	Allowed	Danish local/regional elections

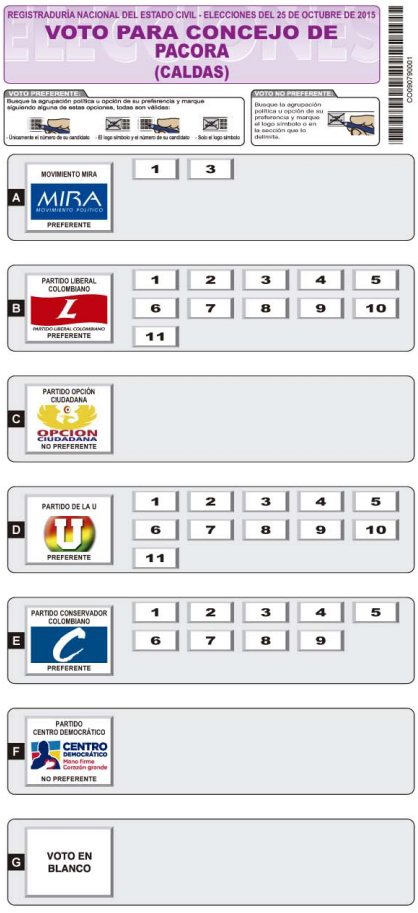
* California employs a randomization-rotation scheme, where the order is randomized in the first assembly district and rotated for all subsequent 80 assembly districts. In this context it less likely that parties can respond to ballot order effects.

B Information on the 2015 Colombian Council Ballots Design

B.1 Open and closed list ballots

Figure A1 demonstrates the use of both open and closed lists on the same ballot. Parties can choose, prior to the submission of candidates to the electoral authority, whether to use an open- or closed-list in a given municipality. Therefore the same party organization can choose different list-types in different municipalities, and different parties can choose different list-types within the same municipality. This decision is made prior to the randomisation ballot position. In Table A2 we present a timeline of the major procedural milestones in the 2015 electoral cycle. In Figure A1 the party-lists in positions C and F used a closed list, whereas the others chose open lists.

Figure A1: **Example Ballot - 2015 Council Elections**



B.2 Election timeline

Table A2: Deadlines for list type decisions

Deadline	*List type decision*
25th of July 2015	Inscription of candidates in the local registry and Limited initiation of political advertisement (only via public space)
28th of July 2015	Parties can initiate all political advertisement
31st of July 2105	Last day to announce changes in the party lists only if a candidate quits
2nd of August 2015	Publication on the web-page of final list of candidates
4th of August	**Lottery of party places in the ballot**
25th of October 2015	Election date
1st of January 2016	Elected officials take office

B.3 Summary Statistics

Table A3: **Descriptive Statistics for Council Elections**

Variable	N	Mean	Sd	Min	Max
Panel A. Election Result					
Vote Share	7886	0.13	0.09	0	0.646
Seat Share	7886	0.131	0.115	0	0.778
Party Seats	7886	1.448	1.234	0	10
Registered Voters (Thousands)	7886	45.482	248.637	0.759	5188.174
Row = 1 on Ballot	7886	0.125	0.331	0	1
Panel B. Party Characteristics					
Num. of Municipalities Contested	7886	802.682	236.433	1	1029
2011 Vote Share(**)	4622	0.168	0.105	0.001	0.71
2011 Seat Share(**)	4622	0.177	0.129	0	0.778
Right Party(***)	4907	0.041	0.199	0	1
Traditional Party	7886	0.223	0.417	0	1
Minority Party	7886	0.094	0.293	0	1
Party Participated in the Last Election	7886	0.586	0.493	0	1
Panel C. Campaign Financing					
Revenues(*)					
Total	7886	702.978	1011.789	0	21953.41
Candidate Income	7886	649.682	983.753	0	21953.41
Private Donations	7886	34.296	156.706	0	4565.79
Financial Credits	7886	1.344	41.844	0	3048.012
Events	7886	0.806	15.724	0	634.016
State	7886	0.081	3.771	0	217.752
Party Contributions	7886	16.769	124.965	0	3768.794
Expenditures(*)					
Total	7886	690.385	997.991	0	21953.41
Advertising	7886	270.723	504.924	0	16868.436
Administrative	7886	76.976	223.395	0	5874.105
Office	7886	22.834	99.92	0	3144.519
Material	7886	53.591	185.705	0	6126.482
Public Acts	7886	164.16	440.916	0	15999.613
Transport and Mail Service	7886	123.22	320.525	0	9252.906
Research	7886	2.15	31.369	0	1912.261
Judicial Cost	7886	32.571	115.373	0	3705.98
Electioneering	7886	161.406	361.793	0	13157.075
Financial Fees	7886	0.346	5.539	0	376.611
Exceed	7886	0.065	2.136	0	119.119
Other	7886	53.066	187.62	0	6399.881

Notes: *Total Colombian Pesos /Registered voters. ** Only available for parties which participated in the previous election. *** Not all parties were coded for ideology

C Robustness tests for results presented in the main paper

In this section we provide further tests that assess the robustness of the findings reported in the main text. Table A4 reports the results of conventional balance tests; Figures A2 and A3 repeat the analysis treating other rows on the ballot as the "treatment" row, to see if the effect varies by row. Given the different scales that the outcomes in Table 1 are measured with, we standardize these outcomes (relative to the control means and standard deviations) and display results in Table A6.

C.1 Balance in covariates

Table A4 displays balance tests for a host of contextual features of the parties in our dataset, including the choice of open or closed list. The randomisation is effective – there are no significant differences between the types of party that are placed in the first row of the ballot compared to other rows.

Table A4: **Balance Table**

	# Munis Contesting (1)	2011 Vote Share (2)	2011 Seat Share (3)	Old Party (4)	Right Party (5)	Minority Party (6)	Traditional Party (7)	Campaign Data Missing (8)	Open List (9)
Effect of Row = 1	0.000	0.004	0.006	−0.009	0.000	−0.000	−0.000	−0.001	0.004
	(0.000)	(0.004)	(0.006)	(0.009)	(0.000)	(0.000)	(0.000)	(0.009)	(0.006)
# Observations	7886	4622	4622	7886	4907	7886	7886	8833	7886
Ballot FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors, clustered at the ballot level, are in parentheses. Each observation denotes a party within a ballot. All races with more than one row on the ballot are included in the regression. Note that 2011 vote and seat share are only available for parties which participated in the previous election. Also, not all parties were coded for ideology. Also note that balance tests were conducted for party-lists that go into the main analysis since they do report campaign data (7886), and column 8, tests whether missingness is correlated to being placed in the top-row and includes for all party-lists (8833).

One particular balance concern is imperfect compliance in terms of reporting campaign finance revenue and expenditure: not all candidates, and therefore party-lists, report in the *Cuentas Claras* system. For the purpose of our paper this could bias our results if reporting is affected by being assigned to the top row of the ballot. Descriptively, of the 8,833 lists for councilors in our dataset, 921 lists are missing campaign finance information (10.4 percent). 10 percent of parties placed in the top row lack campaign finance information (110 out of 1100 parties). 10.5 percent of parties in all other row positions (811 out of 7733) are missing campaign finance information. Most importantly, this missingness, while not insubstantial, is not correlated with row position in our data. As column 8 in A4 shows, there is no statistically significant difference in missing data for the top row

compared to the others.

Table A5 reports balance tests for characteristics of the candidates within the party-lists. In particular, we compare the proportion of women, those with previous disciplinary faults, those registered to vote in the same municipality as the party-list, as well as the average number of candidate lawsuits at the time of the election. We also check balance for the number of candidates in list in column 5. In each case, the data is balanced and uncorrelated with being assigned the top position on the ballot.

Table A5: **Balance Table for Candidate Characteristics within Party-Lists**

	Women (1)	Previous Discip. Sanctions (2)	Same District (3)	No. of Lawsuits (4)	List Size (5)	Withdrawn From Election (6)
Effect of Row = 1	0.002	−0.001	0.00003	−0.005	0.064	0.0001
	(0.002)	(0.002)	(0.002)	(0.011)	(0.060)	(0.001)
# Observations	7886	7886	7883	7886	7886	7886
Ballot FE	Yes	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Standard errors, clustered at the ballot level, are in parentheses. Each observation denotes a party within a ballot. All races with more than one row on the ballot are included in the regression.

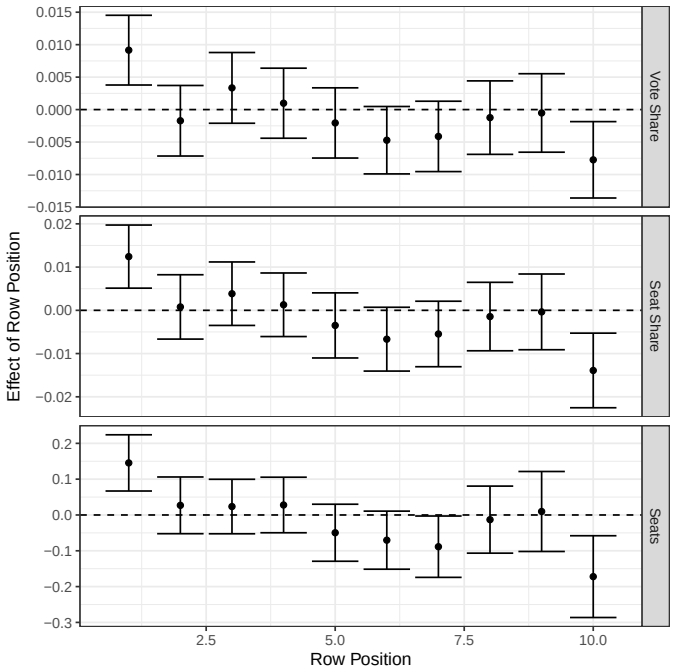
C.2 Alternative treatment rows

Figure A2 presents the estimated row effect comparing a given row to all other rows in the dataset for the three outcomes reported in the main text.[4] There is no positive and significant row effect for any other than the first row.

We also compute the estimated effect comparing each row to all rows below. Figure A3 shows the estimated effects for each row omitting all observations in rows above the 'treatment' row. In general the results show positive, albeit mostly insignificant, results for most row positions. Substantively, no effect is as large as the row effect when the Row = 1.

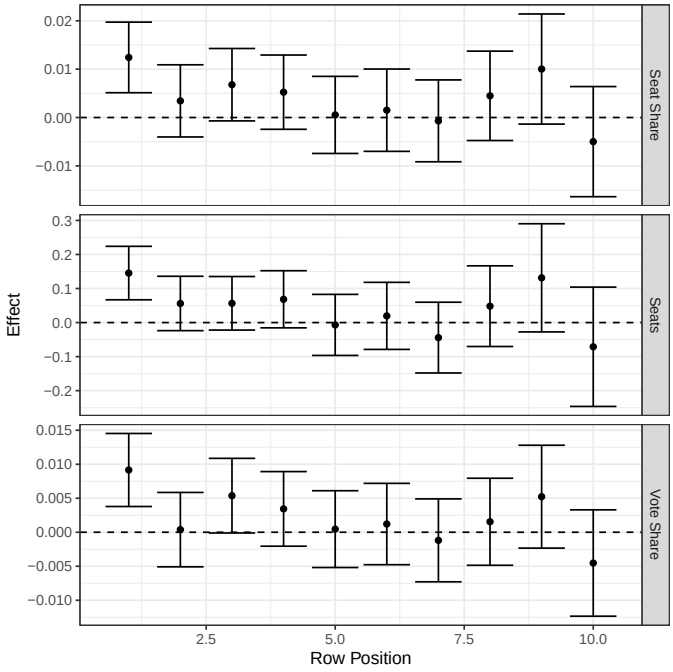
[4]We restrict our analysis to the top ten rows since there are many fewer ballots with more than ten rows, and so the variance is much larger.

Figure A2: **Effect of row position compared to all other rows (including those above)**



Effect estimates shown with 95% confidence intervals. Positions beyond the 15th row are not reported due to very large confidence intervals.

Figure A3: **Effect of row position compared to all rows *below***



Effect estimates shown with 95% confidence intervals. Positions beyond the 10th row are not reported due to small sample sizes.

C.3 Standarized regression results

To aid comparison of the top-row effects across electoral and campaign finance outcomes, which have different scales, we standardize the outcomes and re-run our regression analyses. Table A6 reports the results.

Table A6: Main Regression Results from Table 1, With Standardized Outcomes

	Electoral Outcomes			**Campaign Finance**		
	Vote Share	Seat Share	# Seats	Revenue	Total Expenditure	Publicity Expenditure
	(1)	(2)	(3)	(4)	(5)	(6)
Effect of Row = 1	0.105***	0.11***	0.119***	0.091**	0.089**	0.082*
	(0.031)	(0.033)	(0.033)	(0.042)	(0.043)	(0.05)
# Ballots	1099	1099	1099	1099	1099	1099
# Observations	7886	7886	7886	7886	7886	7886
Ballot FE	Yes	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Each outcome measure is standardized by deducting the variable's control mean ($\mathbb{E}[Y|\text{Row} > 1]$), and dividing through by the respective standard deviation. Standard errors, clustered at the ballot level, are in parentheses. Each observation is a party within a ballot. Races with more than one row on the ballot are included.

The campaign finance estimates' variances are higher than the respective electoral outcomes, but in standardized terms this difference is relatively small. We are therefore confident that inferences across the outcomes should be comparable in their precision. The coefficients for campaign finance outcomes, moreover, while smaller than those for the electoral outcomes, are nevertheless substantively similar in size.

C.4 Timing of campaign spending

The electoral calendar allows parties to initiate political advertisement in a limited way, via the public space, on the 25th of July, and all forms of political advertisements from the 28th of July onward. However, the ballot lottery results are announced on the 4th of August. This means, that there is over a week of potential spending *before* the ballot order lottery is announced, while there are about ten and a half weeks until the election date on the 25th of October for political advertisement after the lottery.

Our results would be confounded if the expenditure effects are being driven by campaign spending that occurs *before* the announcement of the ballot lottery (when party-lists (and the candidates within them) are unaware of their ballot position). To check if spending, and the effects we observe, indeed emerge post-lottery announcement we use the transaction date in the data to estimate the effects separately before and after the lottery. We conduct this analysis both at the party-list level, which is the level the ballot position is assigned, and as a robustness check at the candidate level. Table A7 reports these results, where we find an effect only post-lottery.

To examine systematically the change of expenditure across time, we also take a difference-in-differences approach, where we compare spending between top- and non-top placed party-lists and candidates before and after the ballot order lottery. Following Angrist and Pischke (2008), we estimate the following model:

$$\text{Total Expenditure}_{pct} = \beta \text{Post-Lottery}_t \times \text{Top Row}_{pc} + \text{Post-Lottery}_t + \text{Top Row}_{pc} + \alpha_c + \gamma_p + \epsilon_{pc},$$

where β is the coefficient of interest, total expenditures are measured for each party-list before and after the lottery: a party organization p, in each constituency c, and for each period t. Top Row_{pc} is an indicator variable for whether the party-list enters the ballot in the top row. We include post-lottery dummy Post-Lottery_t to account for common shocks across the campaigning period, as well as ballot/constituency α_c and party organization fixed effects γ_p.

Results are available in column 3 in Table A7. Figure A4 visualises the results and shows the effect

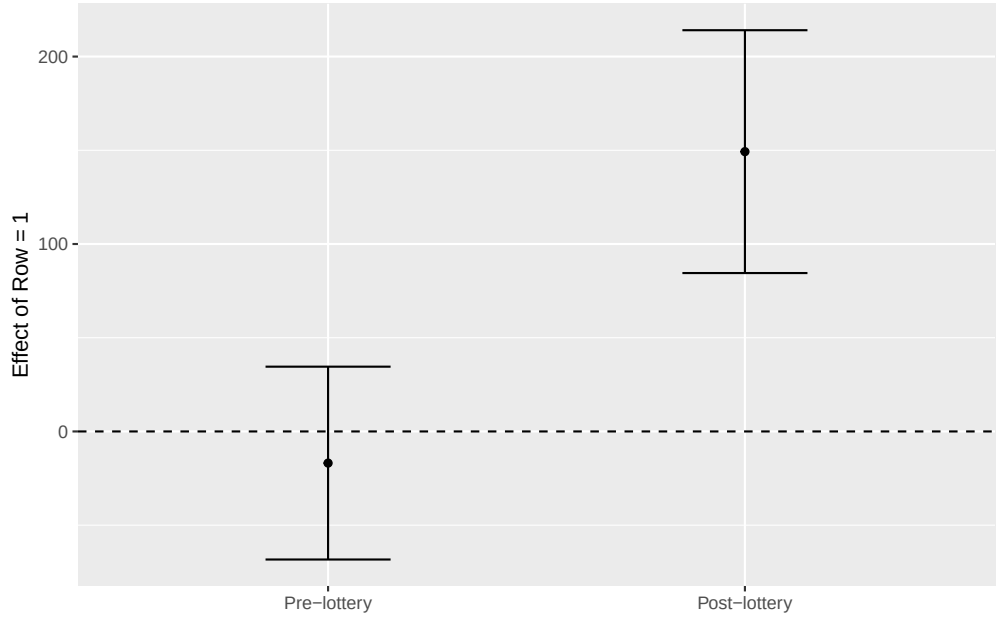
Table A7: **Balance in Expenditure Before and After Lottery Announcement**

	Total Expenditure		
	Pre-Lottery Sample	Post-Lottery Sample	Difference-in-Differences Model
Panel A: Total Expenditure by Party-List			
Row = 1	31.077	84.449**	-16.88
	(25.854)	(39.281)	(26.244)
Post-Lottery	-	-	530.328***
			(14.683)
Row = 1 x Post-Lottery	-	-	149.286***
			(33.05)
N	7886	7886	15772
Panel B: Expenditure by Individual Candidates			
Row = 1	3.808	9.413**	-3.718
	(2.574)	(3.819)	(3.034)
Post-Lottery	-	-	62.457***
			(1.171)
Row = 1 x Post-Lottery	-	-	20.658***
			(4.667)
N	66690	66690	133380
Ballot FE	Yes	Yes	Yes
Party FE	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Standard errors, clustered at the municipality-party level for candidate-level analysis and at the ballot-level for party-list analysis, are in parentheses. Campaigning began in a limited form on the 25th July 2015 and fully on 28th July, while the ballot order lottery was run on the 4th August 2015. All dates between 28th July and 4th August (inclusive) are coded as "Pre-Lottery", and all dates afterwards as "Post-Lottery". The number of observations increase in column 3 because the data are set up in a panel form. Note that "Row=1" coef. for col. 3, is for difference between top-row and others in the pre-lottery period.

is only positive and statistically significant in the period after the ballot order lottery.

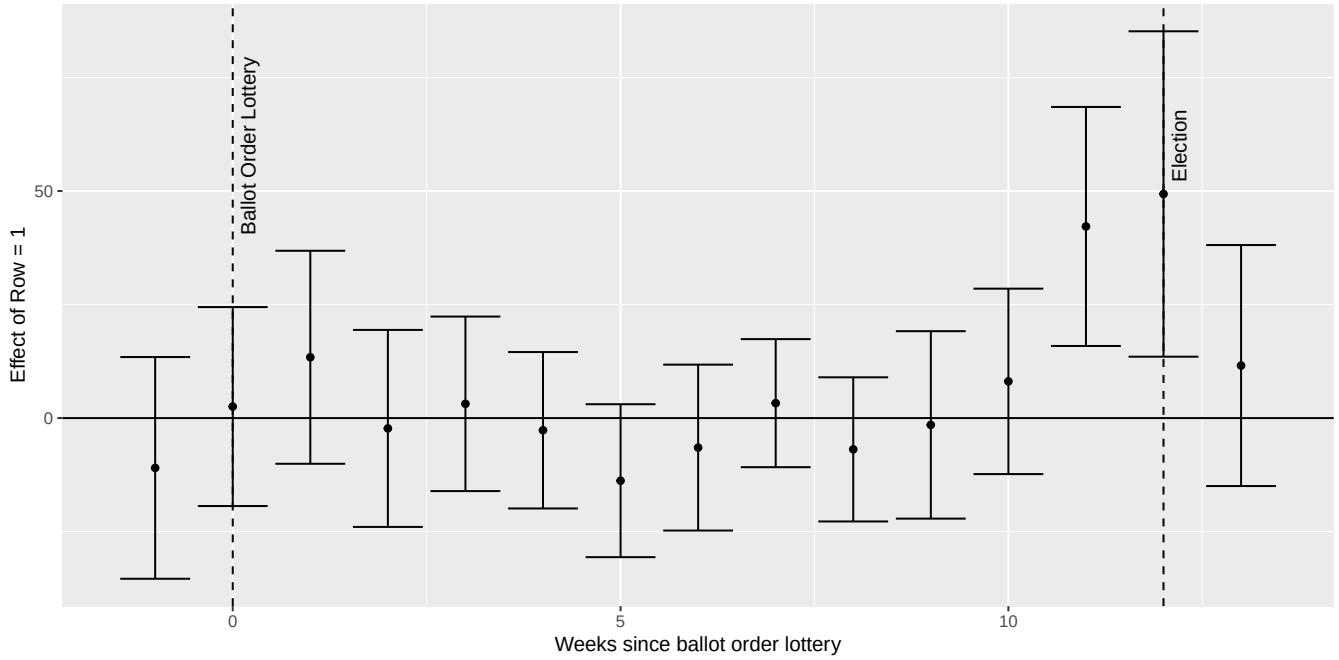
Figure A4: **Effect of Top Row on Total Expenditure by Party Campaigns, Pre- and Post-Ballot Order Lottery**



Interactive coefficients shown with 95% confidence intervals. Individual transactions by candidates are aggregated to the party campaign level per week.

Second, we run the model at the week-level, and show that spending differences between those in the top row compared to those further down the ballot occur in well after the lottery in the weeks closest to the election date. Note that the differences disappear right after the election, which is when parties are spending money on salaries and office closing costs. Figure A5 show these results.

Figure A5: **Week-by-week Effect of Top Row on Total Expenditure by Party Campaigns**



Interactive coefficients shown with 95% confidence intervals. Individual transactions by candidates are aggregated to the party campaign level per week. Some expenditures (those between 26 October 2015 and 31 October 2015) exceed election day to allow for the costs of closing campaigns and salaries. All expenditures are weighted by population.

Overall, our results shows that the effect is emerging *after* the lottery and close to the election date.

D Party-level campaign finance correlations

D.1 Codebook for revenues and expenditures

In this section we present further analysis of party-list revenue and expenditure activity. We report the official decomposition of revenue and expenditure categories, as well as our own key word search that helps identify publicity spending. We then consider the substantive size of the effects reported in the main text, and present further regression estimates of the correlation between randomized ballot position and campaign finance broken down into official categories. Finally, we present preliminary mediation analysis results assessing the direct and indirect effects of ballot order positioning on electoral outcomes.

Table A8: **Donations Codebook**

Revenues	
CandInc	Credits or contributions from the income of the candidates, or direct relatives
PvtContr	Contributions, grants and loans, in cash or kind, by private donors
Credits	Credits obtained in financial institutions to finance the campaign
Events	Income originating from public events, or publications by the party or movement
State	State Funding
Party	Political parties financing the candidate campaigns
Expenditure	
Admin	Administrative expenses
Office	Office expenses and acquisitions
Materials	Investment in materials and publications
PubActs	Public acts by the candidates
TransMail	Transport and mail service costs
Research	Political research and training of party members
Judicial	Judicial accountability and expenses related to campaign accounts
Election	Electioneering expenses
Fin	Financial costs
Exceed	Expenses that exceed the amount set by the National Electoral Council
Other	Other expenses

Table A9: **Key words for coding Publicity spending**

Words	**Words in spanish**
Public event	Evento público
Advertising	Publicidad
Speech	Locución
Banner	Pendon
Commercial	Cuña/Propaganda
Poster	Carteles/Afiches
Flyer	Volantes
Advertising schedule	Pauta publicitaria
Advertising buttons	Botones publicitarios
Publicist	Publicista
Marketing	Marketing
Prints	Estampados
Billboard	Valla publicitaria/Pasacalles
Sound	Sonido
Television	Televisión
Radio	Radio
Press	Prensa
Logistics	Logística
Mural	Mural
Stand	Stand
Vests	Chalecos
T-shirts	Camisetas
Hats	Gorras/Cachuchas

D.2 Decomposed results on revenues and expenditures

Table A10: **Decomposition of Row Effect on Official Revenue Categories**

	CandInc (1)	PvtContr (2)	Credits (3)	Events (4)	State (5)	Party Org (6)
Effect of Row = 1	79.309**	5.375	−0.935	−0.221	−0.077	1.515
	(39.069)	(5.970)	(0.838)	(0.422)	(0.048)	(4.265)
Mean if Row > 1	626.712	33.535	1.447	0.862	0.092	16.748
Effect Size (%)	12.655	16.028	-64.626	-25.659	-83.25	9.046
# Ballots	1099	1099	1099	1099	1099	1099
# Observations	7886	7886	7886	7886	7886	7886
Ballot FE	Yes	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Standard errors, clustered at the ballot level, are in parentheses. Each observation denotes a party within a ballot. All races with more than one row on the ballot are included in the regression. See Table A8 for a description of the variables. The outcomes are measured in persos per registered voters.

Table A11: **Decomposition of Row Effect on Official Expenditure Categories**

	Admin (1)	Office (2)	Material (3)	PubActs (4)	Trans/Mail (5)	Research (6)	Judicial (7)	Electioin (8)	Fin (9)	Exceed (10)	Other (11)
Effect of Row = 1	20.175**	−1.153	−9.021	16.390	31.570**	−0.910	−5.632	21.906	0.053	0.035	8.552
	(9.324)	(3.522)	(6.039)	(15.327)	(12.798)	(0.709)	(3.468)	(16.952)	(0.092)	(0.080)	(5.742)
Mean if Row > 1	73.574	22.651	53.953	156.692	117.104	2.247	32.321	157.299	0.341	0.062	51.07
Effect Size (%)	27.422	-5.09	-16.72	10.46	26.959	-40.505	-17.426	13.927	15.643	56.987	16.745
# Ballots	1099	1099	1099	1099	1099	1099	1099	1099	1099	1099	1099
# Observations	7886	7886	7886	7886	7886	7886	7886	7886	7886	7886	7886
Ballot FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Standard errors, clustered at the ballot level, are in parentheses. Each observation denotes a party within a ballot. All races with more than one row on the ballot are included in the regression. See Table A8 for a description of the variables. The outcomes are measured in persos per registered voters.

D.3 Correlation between Campaigning and Electoral Performance

Table A12: **Correlation of Campaigning and Electoral Performance (omitting party-lists in top row)**

	Total Revenue		Total Expenditure	
	Vote Share (1)	Seat Share (2)	Vote Share (3)	Seat Share (4)
Effect of 1k Peso/Registered Voter	0.031***	0.041***	0.031***	0.041***
	(0.004)	(0.005)	(0.004)	(0.005)
# Ballots	1098	1098	1098	1098
# Observations	6900	6900	6900	6900
Ballot FE	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Parties in the top row omitted. Standard errors, clustered at the ballot level, are in parentheses. Each observation denotes a party within a ballot. All races with more than one row on the ballot are included in the regression.

Table A13: **Correlation Between Campaign Finance and Electoral Performance**

	Total Revenue		Total Expenditure	
	Vote Share (1)	Seat Share (2)	Vote Share (3)	Seat Share (4)
Effect of 1k Peso/Registered Voter	0.028***	0.036***	0.028***	0.036***
	(0.003)	(0.004)	(0.003)	(0.004)
# Ballots	1099	1099	1099	1099
# Observations	7886	7886	7886	7886
Ballot FE	Yes	Yes	Yes	Yes
Party FE	Yes	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Standard errors, clustered at the ballot level, are in parentheses. Each observation denotes a party within a ballot. All races with more than one row on the ballot are included in the regression.

D.4 Mediation analysis

As noted in Green, Ha and Bullock (2010), doing mediation without invoking strong assumptions is hard because mediators – campaign spending and voter behavior in our case – are not randomly assigned, potentially interact with one another, and occur post-treatment on the causal chain.

Comparing the difference in regression coefficients excluding and including a campaign spending variable will likely result in 'post-treatment bias', since we are essentially conditioning on campaign spending that occurs downstream from the treatment on the causal chain. This is a potential problem that is discussed in several papers including Acharya, Blackwell and Sen (2016) who propose an alternative method for mediation that we now conduct.

We first run a model with campaign spending on the right hand side to find the independent effect of spending on the vote share outcome ($\beta_{\text{Spend.}}$). We then run a second model where the outcome is residualized to remove the direct effect of spending:

$$\tilde{y}_i = y_i - \beta_{\text{Spend.}} \times \text{Total Spending}_i,$$

and then regress this residualized component on the top row indicator. The resultant coefficient is the average conditional direct effect (ACDE), which is the effect of treatment that does not occur through campaign spending.

Table A14 summarizes these results. The ACDE calculated via this method shows, as above, that there is an effect on our outcomes outside of the effect of campaign spending. However, as before, the point estimates are attenuated that suggest that there are some effects via campaign spending.

The above analysis invokes strong assumptions about the specific model we chose for the demediation function ($\tilde{y}_i = y_i - \beta_{\text{Spend.}} \times \text{Total Spending}_i$,). The authors discuss how this analysis invokes a selection on observables assumption regarding this function, where, if we believe that the function is properly specified, then campaign spending is as good as randomly assigned, which it is not. In addition, the second key assumption invoked in the analysis above is that there are no intermediate interactions. Suppose a voter relies heavily on ballot order heuristics. Concurrently, a party-list randomly allocated to the top of the ballot increases spending to boost their salience. The

voter, now more aware of the top party-list, uses a combination of the ballot order heuristic and the name recognition afforded by political advertising to choose that party. These types of effects are assumed away by the proposed method.

We therefore, treat the analysis with this method as preliminary, and conclude that because doing mediation is a hard problem, we leave the exact estimation of the mediation effect for future work. Our main contribution in this paper is to show that the additional mechanism of campaign spending does exist and we provide evidence that points to this.

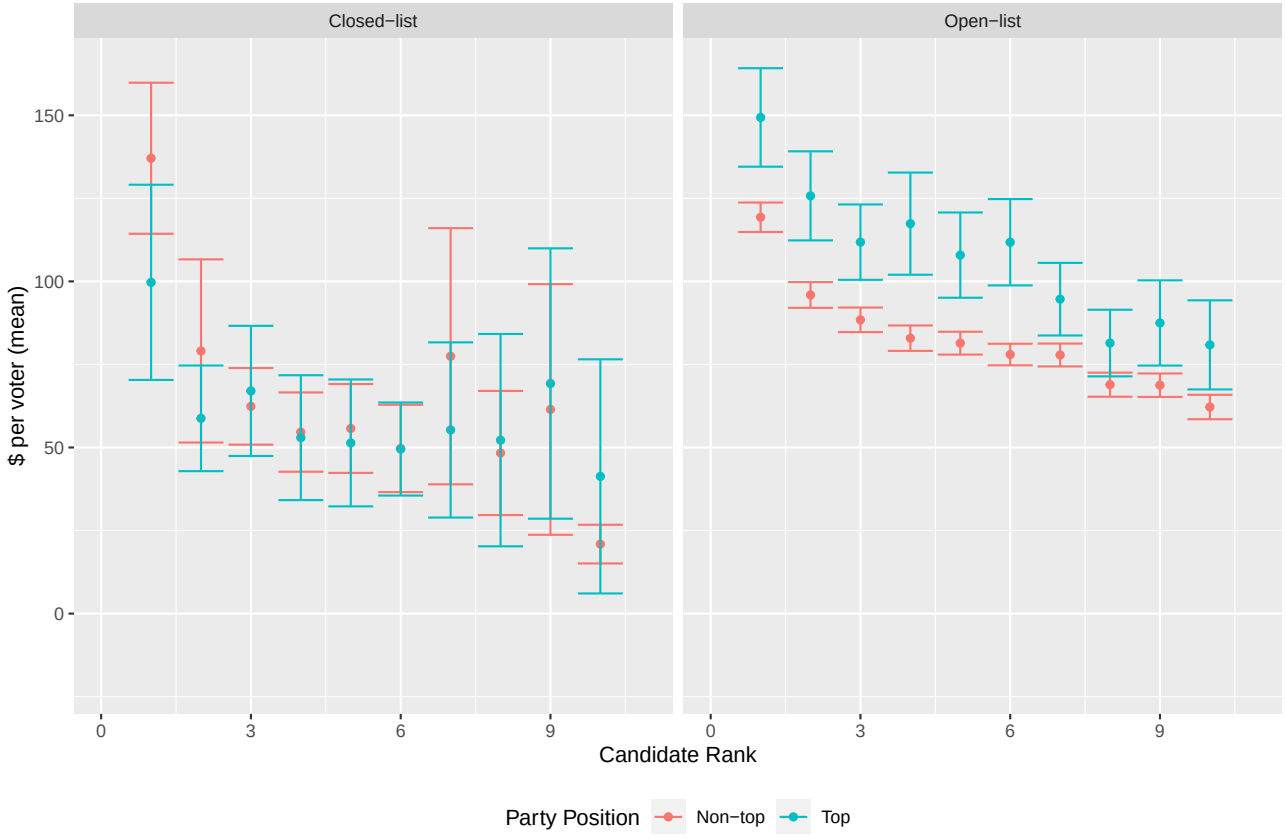
Table A14: **Average Conditional Direct Effects of Top Row**

	Vote Share	Seat Share	Total Party Seats
ATE (Total Effect)	0.009***	0.012***	0.145***
	(0.003)	(0.004)	(0.04)
ACDE (Direct Effect)	0.007***	0.009***	0.114***
	(0.003)	(0.004)	(0.039)
Ballot FE	Yes	Yes	Yes
Party FE	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Standard errors, clustered at the ballot level, are in parentheses. Each observation is a party within a ballot. Races with more than one row on the ballot are included. ATE is calculated as per the main paper. ACDE is calculated with methods discussed in Acharya, Blackwell and Sen (2016).

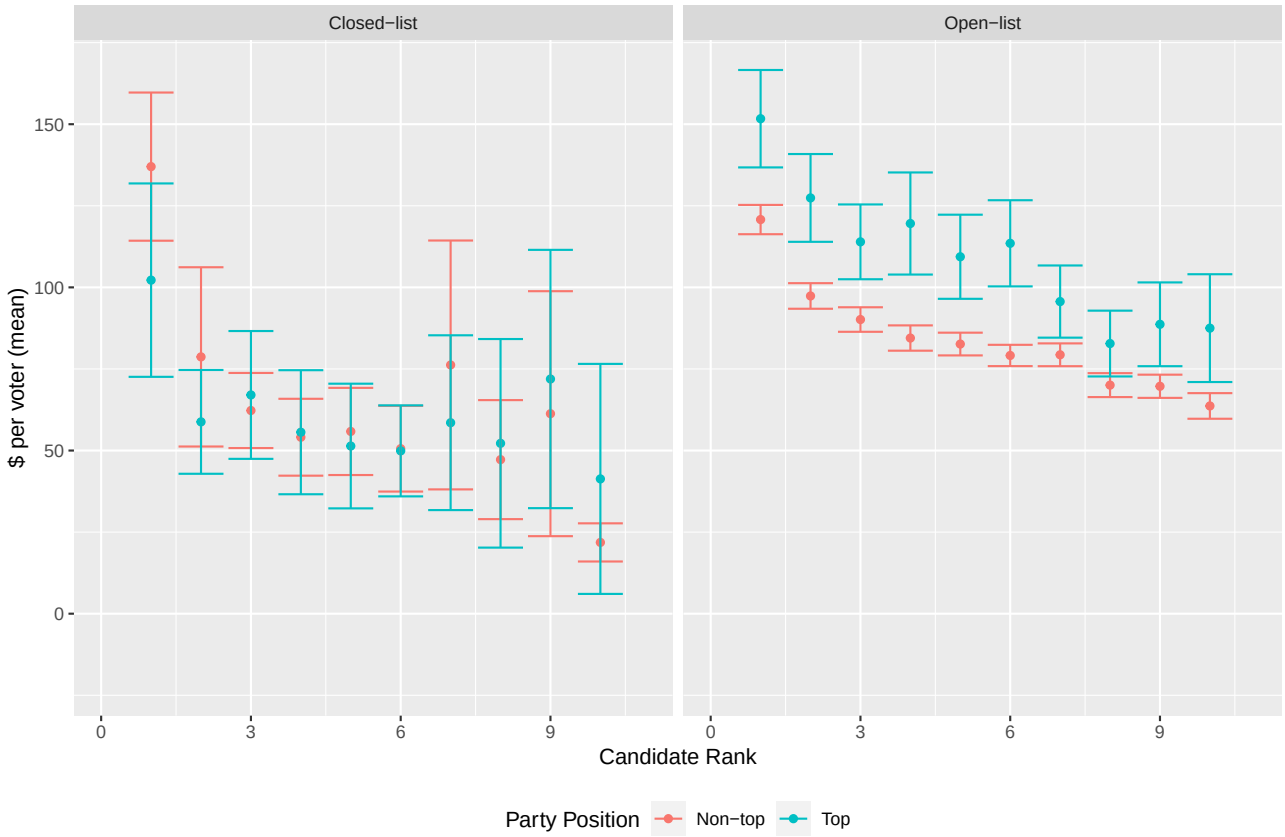
E Candidate-level campaign finance correlations

Figure A6: **Differences in Mean *Expenditure* at the Candidate-Level, by List Type and Candidate's Rank in the List**



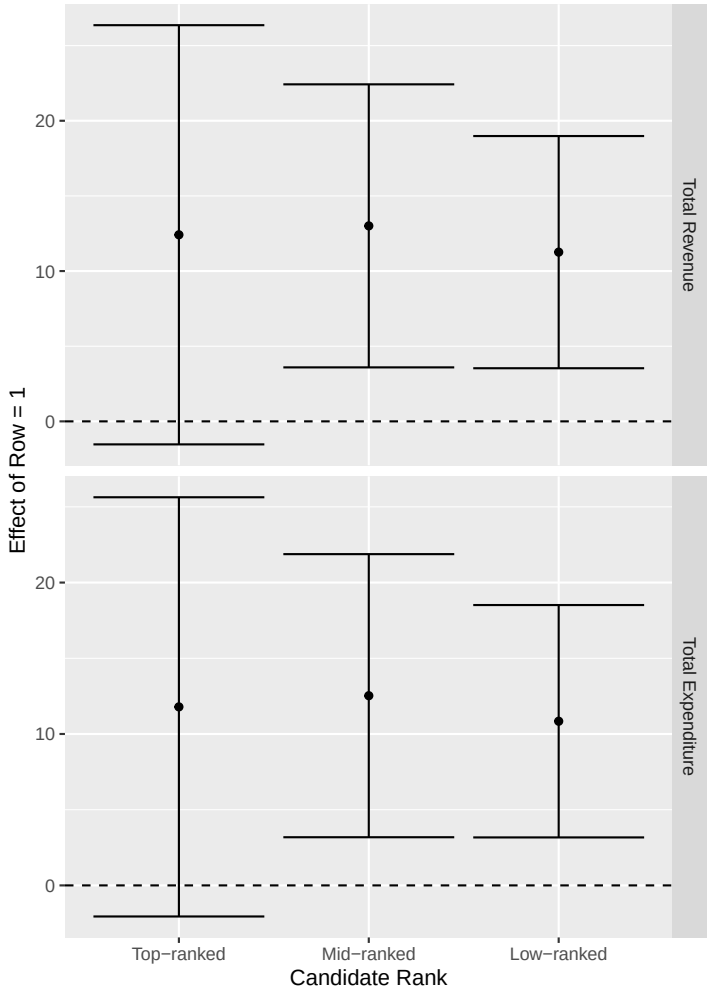
Means shown with 95% confidence intervals. Candidate rank is a candidate's initial position in the party list when the party registered, which is predetermined. Candidate list ranks beyond the 10th candidate are not reported due to very limited sample and large confidence intervals.

Figure A7: **Differences in Mean *Revenue* at the Candidate-Level, by List Type and Candidate's Rank in the List**



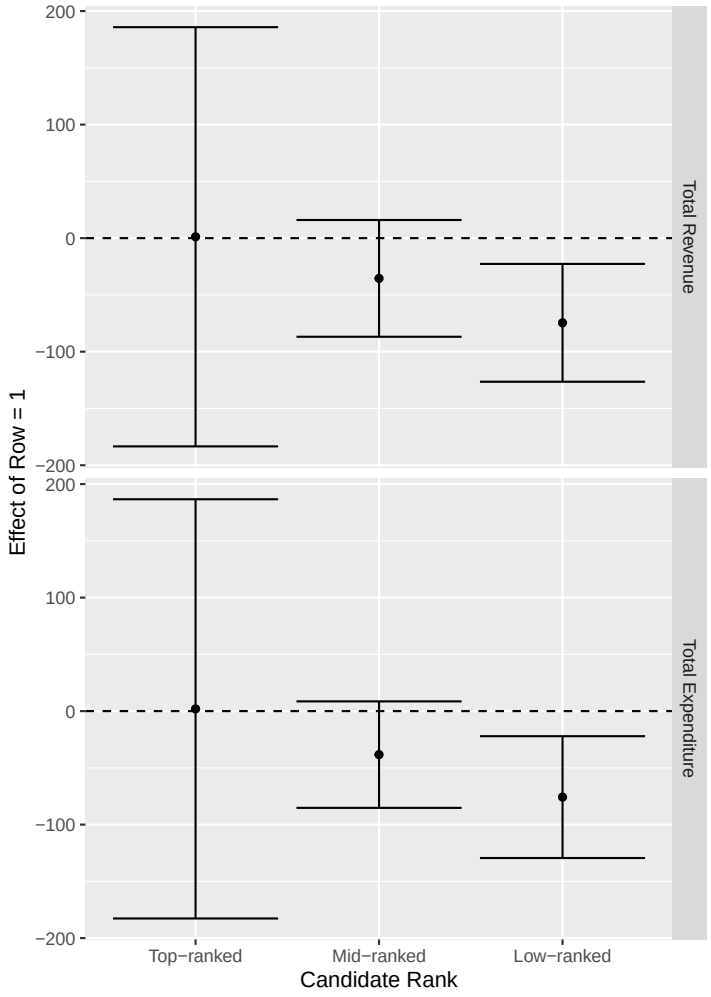
Means shown with 95% confidence intervals. Candidate rank is a candidate's initial position in the party list when the party registered, which is predetermined. Candidate list ranks beyond the 10th candidate are not reported due to very limited sample and large confidence intervals.

Figure A8: **Correlations Between Campaign Financing and Party-List Being on the Top Row, Estimated Separately for each Candidate Ranking and for *Open lists***



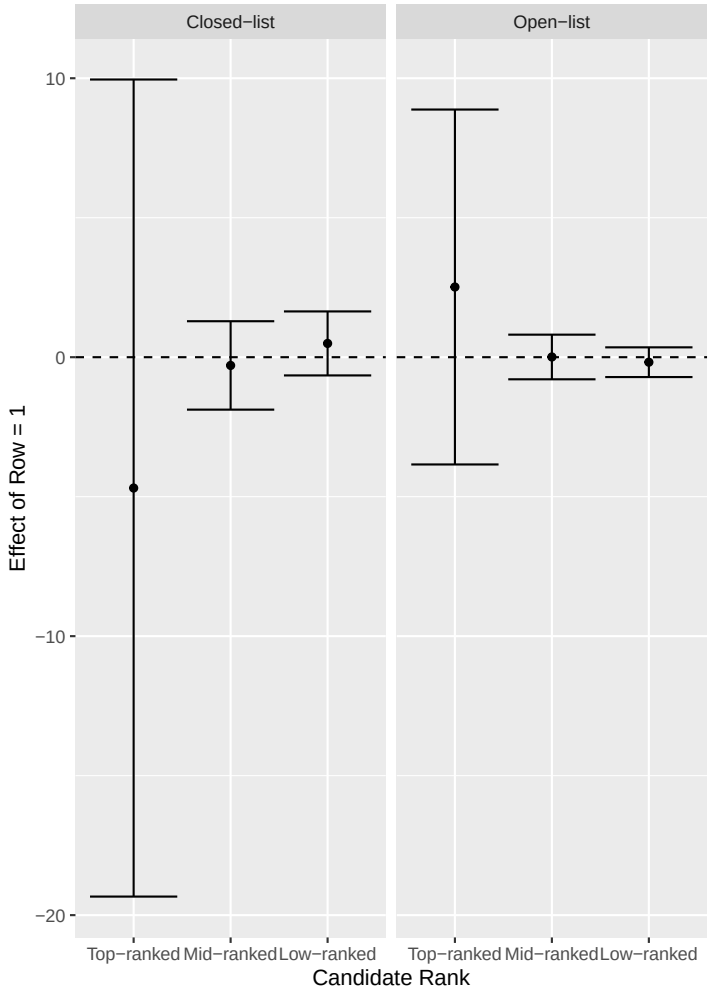
Regression coefficients shown with 95% confidence intervals (clustered by party campaign). Top-ranked candidates are those listed in position 1, mid-ranked candidates are those listed in position 2-4, and low-ranked candidates are those listed in position 5 or lower. Separate regression models are estimated for each of these groups. Estimates include party and municipality fixed effects.

Figure A9: **Correlations Between Campaign Financing and Party-List Being on the Top Row, Estimated Separately for each Candidate Ranking and for *Closed lists***



Regression coefficients shown with 95% confidence intervals (clustered by party campaign). Top-ranked candidates are those listed in position 1, mid-ranked candidates are those listed in position 2-4, and low-ranked candidates are those listed in position 5 or lower. Separate regression models are estimated for each of these groups. Estimates include party and municipality fixed effects.

Figure A10: **Correlations Between *Party Financing* of Candidates, at the Candidate-Level, by List Type and Candidate's Rank in the List**



Coefficients shown with 95% confidence intervals. Each coefficient is generated by running a separate regression, subsetting the data on list type and candidate rank.Candidate rank is a candidate's initial position in the party list when the party registered, which is predetermined. Candidate list ranks beyond the 10th candidate are not reported due to very limited sample and large confidence intervals.

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