Climbing the Ranks: Incumbency Effects in Party-list Systems*

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Abstract

Incumbents tend to have a solid electoral advantage in candidate-centered electoral settings. Do similar incumbency effects exist in more party-centered environments? We estimate incumbency effects in an open-list proportional representation system, exploiting that seats are first allocated *across* parties, and then to candidates *within* party lists. Using data from Norwegian local elections 2003-2015, we document that a candidate that barely wins a seat in the local council has about a 9 percentage points (43 percent) higher probability of being elected in the next election compared to a candidate that just misses out on a seat on the same party list. We find no evidence that voters contribute to this personal incumbency advantage. Rather, it seems as if party elites are instrumental in securing the electoral success of their party affiliates. We show that incumbents and non-incumbents run again in the subsequent election at about equal rates, but that incumbents tend to advance in the party hierarchy and obtain safer ballot positions in future elections, which is what ultimately leads to electoral success.

Keywords: Incumbency Advantage, Proportional Representation, Regression Discontinuity Design.

JEL Classification: C21, D72

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1 Introduction

Citizens' ability to 'throw the rascals out' is the ultimate guarantee of a connection between citizens and policymakers in modern representative democracy (Powell, 2000). Political commentators often complain about the electoral dominance of incumbents, and trace many of the perceived ills of modern American politics to the electoral advantages created for, and by, incumbent legislators (Carson, Engstrom and Roberts, 2007; Fowler, 2016). An extensive political economy literature is therefore devoted to understanding the causes and consequences of the incumbency advantage; important early contributions include Erikson (1971), Gelman and King (1990), Cox and Katz (1996), and Katz and King (1999).

The existing literature emphasize strategies *incumbents* use to improve their electoral chances. An incumbent's popularity may, for example, stem from pork-barreling (e.g., Mayhew, 1974; Alvarez and Saving, 1997), the deterrence of high-quality challengers (e.g., Cox and Katz, 1996; Hall and Snyder, 2015), and activities that increase name recognition (e.g., Mann and Wolfinger, 1980; Kam and Zechmeister, 2013). Most of these studies come from the United States or other candidate-centered electoral settings.[1] Incumbency effects may also exist in party-centered environments, but the relevant mechanisms are likely to differ. In list-based electoral systems, *party elites* may contribute to the electoral success of incumbents through the nomination process (Llaudet, 2014; Golden and Picci, 2015).

In this paper we use data from Norwegian local elections to estimate various incumbency effects. The open-list electoral setting facilitates two types of regression discontinuity (RD) designs that together allow us to isolate various components of the incumbency advantage. Specifically, we exploit that seats are first allocated *across* parties, and then to candidates *within* lists. In our main analysis we compare bare winners to bare losers run-

[1] Eggers and Spirling (2017) review the literature and estimate the incumbency advantage in the United Kingdom and the United States over the 1900-2010 period. A related literature uses regression discontinuity designs to investigate how the party of the incumbent affects voting in *other* races (e.g., Ade and Freier, 2013).

ning for office *on the same party list*. This allows us to isolate the *personal incumbency advantage*, and explore the underlying mechanisms.[2] The second RD design exploits discontinuities in the seat allocation formula to obtain quasi-experimental variation in parties' representation in the council. By putting results from these two RD designs together, we aim to elucidate how the electoral advantages from holding office materialize in party-list systems.

The existing literature on incumbency effects in PR systems is sparse and the evidence is mixed. For example, Golden and Picci (2015) find that incumbents are more likely to be *re-selected*, but not *re-elected* under open-list PR in Italy. Using random election outcomes, Hyytinen et al. (2017) find no evidence of a personal incumbency advantage in open-list local elections in Finland. However, recent studies from the other Nordic countries show that incumbency matters despite the party-centered nature of these elections (Lundqvist, 2013; Dahlgaard, 2016; Fiva and Smith, 2016).[3]

In our main RD analysis, we find evidence of a substantial personal incumbency advantage. The probability of winning a seat in the subsequent election is estimated to jump from 0.21 to 0.30 when comparing bare losers to bare winners. Three supplementary pieces of evidence suggest that the jump at the cut-off should be interpreted causally. First, pre-determined covariates, such as age, gender, and previous election outcomes are well balanced around the cut-off for a seat change. Second, our RD estimates remain basically unaltered if we vary the estimation window around the cut-off for a seat change.

[2]In a seminal study, Lee (2008) finds that a narrow victory in United States House elections results in an eight percentage point increase in the vote shares of the *incumbent's party* in the next election. Subsequent studies have pointed out that these estimates should be interpreted as a combination of the personal and partisan incumbency advantages (Fowler, 2016; Fowler and Hall, 2014; Erikson and Titiunik, 2015). Fowler and Hall (2014) exploit close elections in conjunction with term limits in U.S. state legislatures to disentangle the sources of the incumbency advantage. They find that candidates get a nine percentage point increase in their vote share because of their personal incumbency, while non-incumbent candidates enjoy no electoral advantage as a result of *their party* having held the seat in the preceding term.

[3]Lundqvist (2013) estimates a personal incumbency advantage of 6 percentage points in Swedish local elections, Dahlgaard (2016) finds a 12 percentage point effect in local elections in Denmark, and Fiva and Smith (2016) find a 25 percentage point effect in national closed-list elections in Norway. Outside the Nordic countries, da Fonseca (2017) finds evidence of a personal incumbency advantage for mayors in Portugal, where local elections are conducted under PR. In the open-list binomial elections of Chile, Salas (2016) finds that holding two congressional seats in a district increases the probability of re-electing two candidates from the *same coalition* by 28 percentage points.

Finally, we do not find similar "incumbency effects" when conducting a set of falsification checks on non-marginal candidates.

The personal incumbency advantage could arise at various stages of the electoral process. Our results point clearly towards party elites rather than voters as the key political agents driving the personal incumbency advantage. We document how incumbents tend to advance in the party hierarchy and obtain safer positions on the ballot in the next election. This, in turn, contributes strongly to their electoral fortunes in the next election.

If voters prefer incumbents over non-incumbents, then parties should be able to capitalize on this, and they should also have an electoral advantage in the next election. We find no evidence that this is the case using our across-parties RD design. Parties that just miss out on their first seat receive about the same share of seats in the next election as parties that just won their first seat. Taken together with our other findings, this suggests that voters' contribution to the personal incumbency advantage that we document is small or non-existent.

2 Institutional setting

Norwegian local governments, about 430 in all, are important entities of the Norwegian welfare state. They are responsible for the provision of key welfare services, such as child care, schooling, and elderly care, and also provide traditional local public goods, such as sewage, water supply, and waste collection and management. Seventeen percent of the Norwegian labor force is currently employed in the local government sector.[4]

Each local government is run by a local council (*kommunestyre*) whose decisions are based on a simple majority rule. The local councils are elected every fourth year in September.[5] All Norwegian citizens aged 18 or over in the election year, and foreign

[4] `https://www.ssb.no/offentlig-sektor/nokkeltall`

[5] Local governments have the discretion to set the size of their council, subject to restrictions imposed by the Local Government Act of 1992. The number of councilors must be uneven. With less than 5,000 inhabitants, the number of councilors must be at least 11; above 5,000 but below 10,000, at least 19; above 10,000 but below 50,000, at least 27; above 50,000 but below 100,000, at least 35, and finally, above 100,000, at least 43.

nationals who have resided in Norway for the last three years, are entitled to vote. In the four local elections held in the 2000s, the participation rate has been about 60 percent.

The open-list electoral system offers both voters and local party elites means for influencing candidate selection (Andersen, Fiva and Natvik, 2014). Parties can opt to give certain candidates an increased share of the poll (25 percent of the total number of votes received by the party). Candidates with such a *pre-advantage* are listed at the top of the ballot paper in boldface (Appendix Figure A.1 shows an example). Local party organizations decide how many and which candidates to give such a pre-advantage.[6] Party elites have considerable control over which candidates wins elections, as pre-advantage status is often decisive for the within-party allocation of seats. In our definition, a candidate holds a *safe position* if the candidate is awarded a pre-advantage and the total number of pre-advantaged candidates on the party list does not exceed the number of seats won in the previous election. In our sample, 85 percent of candidates with a pre-advantage won a seat on the local council. Candidates with a safe position, according to our definition, win a seat in 94 percent of the cases.[7]

The local party organization typically appoints a *nomination committee* to recruit candidates for the ballot. They often start by asking incumbents if they are interested in seeking re-election, before looking at previous candidates, and finally other local party members and sympathizers (Ringkjøb and Aars, 2010). The final party ballot is usually decided at a *nomination meeting*, which is typically open to all local party members.

Voters may cast *personal votes* for candidates on *any* party list.[8] Together with

[6]The number of such candidates depends on the size of the local council. In councils with less than 23 members, parties can give a pre-advantage to a maximum of four candidates. For councils with 23 to 53 members, the maximum is six, and for councils with more than 53 members, ten is the limit (`https://www.regjeringen.no/no/portal/valg/valgordningen/id456636/`). On average, party lists in our sample pre-advantaged 2.4 candidates, and 11 percent of the lists pre-advantaged the maximum number of candidates.

[7]17 percent of the lists have exactly as many candidates with a pre-advantage as seats won (see also Appendix Figure A.2). About 7 percent of the lists in our sample do not give any candidate a pre-advantage.

[8]The number of personal votes cast on other party lists is limited to a quarter of the council size. Voters may nevertheless add as many as five candidates from other party lists. Fifteen percent of respondents in the 2007 Local Election Survey (*Lokalvalgsundersøkelsen*) report having given "side votes" to candidates from other parties, while 46 percent of respondents report giving additional personal votes to candidates on their chosen party list. Bergh, Bjørklund and Hellevik (2010) document that 25 percent

candidates' pre-advantage status, the number of personal votes yields the personal *poll* that forms the basis of the *within-party* distribution of seats. More specifically, the poll for candidate i running for party l is given by:

$$Poll_{il} = \begin{cases} PersonalVotes_i & \text{if } i \text{ has no pre-advantage} \\ PersonalVotes_i + 0.25 \cdot PartyVotes_l & \text{if } i \text{ has a pre-advantage for list } l \end{cases} \tag{1}$$

The initial ranking on the ballot decides which candidate is elected *only* in the event of a tie in the personal poll between two or more candidates. The poll also decides which candidates become deputy councilors. This means that candidates who just miss out on a council seat become their party's first deputy councilor. This person will substitute for indisposed regular councilors from their own party at local council meetings.[9]

The distribution of seats *across* parties is determined by the number of *party votes* (*listestemmer*) cast for the respective parties and the application of a modified Sainte-Laguë method (Fiva and Folke, 2016). When a ballot is cast without preferential votes accorded to other parties, a party will get as many list votes as there are seats up for election. If ballots include "side votes" for other parties, the list votes are transferred accordingly. For example, if a voter chooses party A's ballot, but adds a name from party B, then party A gets 24 list votes and party B 1 list vote, if the size of the local council is 25.[10]

3 Within-list RD design

Incumbents are likely to possess political abilities - which got them elected in the first place - that challengers do not (Erikson, 1971; Salas, 2016). This type of selection bias plagues the interpretation of observational estimates of the incumbency advantage. The idea behind RD designs is to exploit the fact that political representation changes discon-

of all candidates that were elected in the 2007 election, got elected exclusively due to preferential votes.

[9]Each list should have as many deputies councilors as regular councilors plus three.

[10]Party lists need a minimum of seven candidates to participate in the election. The average number of candidates per list in our sample is 24.

tinuously at particular thresholds. As long as we compare future political outcomes that are sufficiently close to the seat-change threshold, the RD design will provide estimates that are as credible as those from a randomized experiment.

3.1 Research design

To estimate the *personal incumbency advantage* we use a within-list RD design. Our starting point is the universe of candidates running for office in the 2003, 2007, 2011, and 2015 elections (about 60,000 candidates each year). We exclude party lists that do not win any seats (2% of observations), as well as non-standard lists (i.e. joint lists of the main parties, party-independent lists, and minor party lists) (14% of observations). In addition, we exclude all municipalities where we lack information on personal votes for any candidate in any election year.[11] We also drop candidates from the 12 municipalities involved in mergers in our sample period. This leaves us with a balanced panel of 189 municipalities and 2,977 party lists.[12] Since ties are broken by the initial ranking of the candidates on the list, not by a coin-toss, as in Finland (Hyytinen et al., 2017), we also exclude ties (142 party lists). The final sample consists of 2,835 party lists.

To implement the within-list RD design, we start by computing the number of seats party list l won, S_l, using the party votes of list l ($PartyVotes_l$) and the party votes of all other lists participating in the election. We then sort candidates based on their $Poll_{il}$ (see Equation 1) to get their within-list rank, R_{il}. The *Win Margin* is then given by:

[11] We have perfect data for 419 municipalities in 2003, 354 municipalities in 2007, 228 municipalities in 2011, and 428 municipalities in 2015. Data from the 2003 and 2007 elections are primarily from Christensen et al. (2008), but we correct some errors and supplement missing observations through direct contact with municipalities. The 2011 data is collected directly from the municipalities, while the 2015 data come from Statistics Norway.

[12] Our estimation sample does not differ much from the excluded municipalities with regards to municipality size. In our balanced panel the average population size is 11,621 and the median 4,644. If all municipalities were included, the average would have been 11,228 and the median of 4,554.

$$WinMargin_{il} = \begin{cases} \frac{Poll_{il} - Poll_l^{S_l+1}}{PartyVotes_l} & \text{if } R_{il} \leqslant S_l \text{ [elected candidates]} \\ \\ \frac{Poll_{il} - Poll_l^{S_l}}{PartyVotes_l} & \text{if } R_{il} > S_l \text{ [non-elected candidates]} \end{cases} \tag{2}$$

where $Poll_l^{S_l+1}$ is the poll of the *borderline defeated* candidate for list l, and $Poll_l^{S_l}$ is the poll of the *borderline elected* candidate.[13]

The left-hand panel of Figure 1 shows the frequency of observations by this distance metric; 45 percent of candidates have a win margin less than 10 percentage points. Many candidates win comfortably because of their pre-advantage. The bunching of observations somewhat above 0.25 is driven by situations where the elected candidates from the list have a pre-advantage, while the borderline defeated at the list does not (the bunching somewhat below -0.25 can be similarly explained).

We limit the estimation sample to candidates that are borderline defeated or borderline elected. This results in a symmetric density around zero, as shown in the right-hand panel of Figure 1. The sample restriction serves two purposes. First, it ensures that all list characteristics (e.g., party labels) and municipality characteristics (e.g, population size) exactly balance at the cut-off. Second, the sample restriction leads to well-defined treatment and control groups: we can compare future election outcomes for candidates that just won a seat in the current election (incumbents at $t+1$) to candidates that just missed out on a seat and therefore become the party's first deputy councilor. Because deputy councilors may have received some of the same "treatment" as candidates that just win (Kotakorpi, Poutvaara and Terviö, 2017), we include RD analyses of becoming the first, second, third, and fourth deputy member for the party, as an extension.

Table 1 shows that a naïve comparison of means between borderline defeated and borderline elected is unlikely to isolate the causal effect of winning office. Borderline

[13] We could, alternatively, construct the forcing variable in the within-list RD using *raw votes* rather than *vote shares* (as in Equation 2). In Appendix B, we present results using the alternative forcing variable. Our main findings are insensitive to the choice of forcing variable.

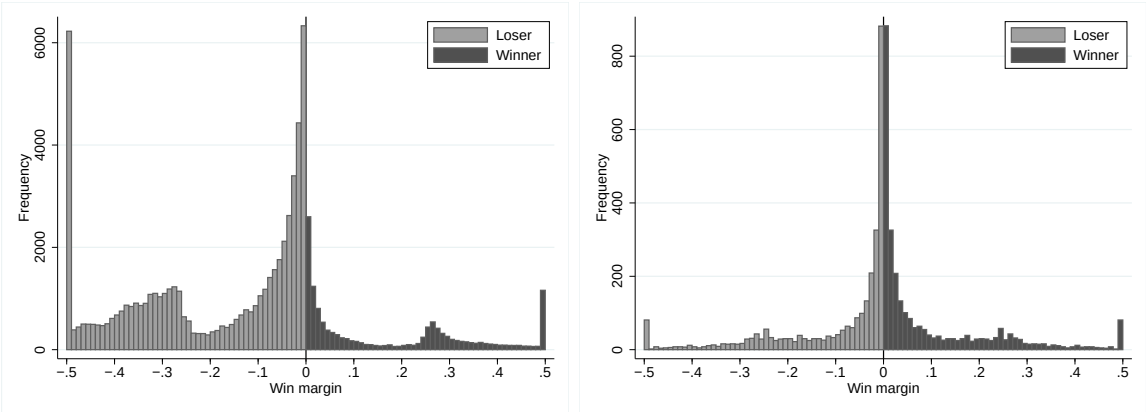
Table 1: Summary statistics

	(1) Bordeline defeated		(2) Bordeline elected		(3) Difference	
	Mean	SD	Mean	SD	Estimate	SE
Female	0.396	(0.489)	0.393	(0.489)	-0.002	(0.014)
Age	46.146	(12.771)	46.546	(12.449)	0.399	(0.420)
Candidate, t-1	0.316	(0.465)	0.349	(0.477)	0.033***	(0.014)
Pre-advantage, t-1	0.269	(0.444)	0.377	(0.485)	0.108***	(0.025)
Personal votes, t-1	0.119	(0.131)	0.164	(0.175)	0.045***	(0.011)
Elected, t-1	0.111	(0.314)	0.176	(0.381)	0.066***	(0.011)
Ballot position, t[a]	0.678	(0.240)	0.741	(0.231)	0.063***	(0.009)
Advancement, t	0.169	(0.375)	0.184	(0.388)	0.016	(0.012)
Pre-advantage, t	0.242	(0.428)	0.413	(0.493)	0.172***	(0.017)
Safe, t	0.080	(0.271)	0.204	(0.403)	0.125***	(0.010)
Personal votes, t	0.127	(0.168)	0.189	(0.334)	0.061***	(0.016)
Elected, t	0.000	(0.000)	1.000	(0.000)	1.000	(0.000)
Candidate, t+1	0.558	(0.497)	0.563	(0.496)	0.005	(0.018)
Advancement, t+1	0.230	(0.421)	0.208	(0.406)	-0.022**	(0.013)
Pre-advantage, t+1	0.142	(0.349)	0.233	(0.423)	0.091***	(0.013)
Safe position, t+1	0.078	(0.269)	0.141	(0.348)	0.062***	(0.010)
Personal votes, t+1	0.074	(0.130)	0.109	(0.181)	0.036***	(0.006)
Elected, t+1	0.191	(0.393)	0.311	(0.463)	0.120***	(0.014)
Socialist left party	0.131	(0.337)	0.131	(0.337)		
Labor party	0.178	(0.382)	0.178	(0.382)		
Center party	0.165	(0.371)	0.165	(0.371)		
Liberal party	0.118	(0.322)	0.118	(0.322)		
Christian dem. party	0.121	(0.326)	0.121	(0.326)		
Conservative party	0.157	(0.364)	0.157	(0.364)		
Progress party	0.131	(0.337)	0.131	(0.337)		

[a]The ballot position is standardized assigning the top candidate the value 1 and the bottom candidate 0 on all lists using the following formula: $BallotPosition = 1 - (Rank - 1)/(ListSize - 1)$

*Note: Sample is restricted to candidates from one of the seven main parties who are either next in line to win a seat (borderline elected), or first in line to lose a seat (borderline defeated) in the 2003 (N = 1,882), 2007 (N = 1,906) and 2011 elections (N = 1,882). Summary statistics for variables measured in election t and $t+1$ are based on 5,670 observations. We do not condition on a candidate to run meaning that personal votes in t-1 or in t+1 is zero if a candidate in t did not run in either t-1 or t+1. Summary statistics for variables measured in election $t-1$ and "Advancement, t" are based on 3,788 observations as these variables only can be measured in 2007 and 2011. Standard errors in column (3) are clustered at the municipality level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

Figure 1: Frequency of observations by distance to threshold



Note: The figure to the left shows the number of observations by the distance to the threshold for winning a seat, while the figure to the right shows the number of observations when limiting the sample to borderline defeated and borderline elected candidates. The figures are truncated at ± 0.5*. Each bin is for an interval of* 0.01*. Ties are excluded.*

elected are, for example, more likely to have won a seat in the previous election than borderline defeated (18 percent vs. 11 percent); they are more likely to have been pre-advantaged in the current election (41 percent vs. 24 percent), and so on.[14] To pin down the causal effect of winning office we compare candidates that *just missed out on a seat* to candidates that *just won a seat*, and estimate specifications of the form:

$$Y_{il} = \beta_0 + \beta_1 Seat_{il} + \psi(WinMargin_{il}) + \xi_{il}, \tag{3}$$

where $Seat_{ij}$ is a dummy equal to one if candidate i won a council seat for list l in the current election. In our main analysis, Y_{il} is a dummy variable equal to one if candidate i won a seat in the *next* election for list l. The personal incumbency advantage, captured by β_1, could come into play at different stages of the electoral process. To explore potential mechanisms contributing to our main estimate we run several auxiliary analyses where Y_{il} represents various outcome variables. First, we consider whether candidate i runs again in the subsequent election for list l. Then we consider whether the candidate's

[14] We have available two candidate level background variables: gender and age. As in most other electoral settings, women are underrepresented in Norwegian local politics. In our sample, 39 percent of the candidates are female, and candidates are, on average, 46 years old.

list rank improves from the focal election to the next, a clear sign of within-party political *advancement*, and whether the candidate holds a *safe position* on the ballot in the next election. Finally, we investigate whether the number of personal votes obtained by candidate i in the next election is affected by winning a seat in the current election.[15] We include a low order polynomial of the forcing variable on each side of the discontinuity, $\psi(WinMargin_{il})$. Our baseline specification is based on a local linear control function, but we also report results from local quadratic specifications. ξ_{il} is an error term (we cluster standard errors at the municipality level).

In any RD design, one needs to strike a balance between precision and bias when choosing the estimation window, or *bandwidth*. Several data-driven algorithms have recently been developed to guide this trade-off according to various criteria (Imbens and Kalyanaraman, 2012; Calonico, Cattaneo and Titiunik, 2014; Calonico et al., 2016). We rely on the mean-squared-error optimal bandwidth developed by Calonico et al. (2016). In practice, the choice of bandwidth has no substantial impact on our results.

3.2 Covariate balance

The electoral RD design is only effective when relevant actors do not have *precise* control over election results. Eggers et al. (2015) argue that there exists no convincing theoretical reason to expect close winners and losers of large-scale elections to differ systematically. What about the small-scale elections that we study? We believe it is highly unlikely that high-quality (or low-quality) candidates are able to sort systematically around the discontinuity threshold. First, it would require that candidates are able to accurately predict the number of seats their party list is going to win.[16] Second, high-quality candidates would have to obtain extremely precise information about the expected number of

[15] While the RD design makes it straightforward to estimate the effect of winning unconditional on running, estimating the conditional effect requires addressing selection into future candidacy (Anagol and Fujiwara, 2016). In our paper we do not attempt to address this selection issue, rather our RD analyses includes all candidates who ran for office at time t (i.e., it is not conditional on running at time $t+1$).

[16] In our sample, the average number of seats won by the party list is 4.6 (SD=3.9).

personal votes, while low-quality candidates must lack the ability or willingness to do so.

If candidates lack the ability to precisely manipulate the number of personal votes obtained, candidates just above and just below the cut-off should be similar in variables that are determined prior to treatment. It is therefore unnecessary to include covariates capturing candidate characteristics in Equation (3) for identification. We can instead use pre-determined candidate characteristics, like age and gender, to empirically assess the validity of the RD design. An important principle applies to this type of falsification test: All predetermined covariates should be analyzed in the same way as the outcomes of interest (Skovron and Titiunik, 2015).

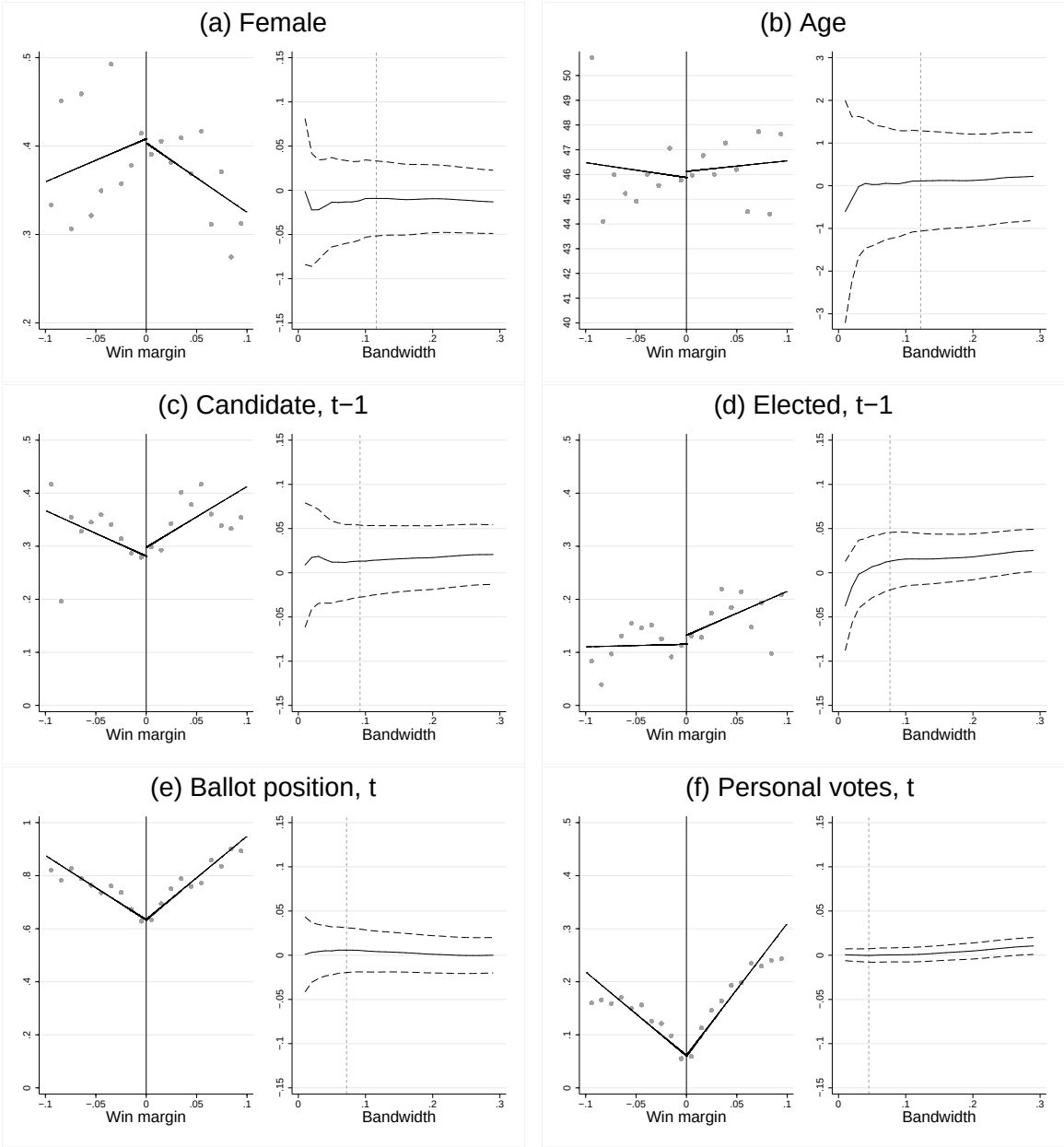
Figure 2 presents results for six different pre-determined covariates. Each sub-panel, denoted (a)-(f), consists of two plots. The plots to the left are standard RD plots using a bandwidth of 10 percentage points. The vertical line in each plot represents a zero win margin, indicating the transition from barely missing out on a seat to barely winning. Each dot represents a binned average for a one percentage-point interval. The plots to the right display RD estimates and 95% confidence intervals as a function of the bandwidth chosen.

In contrast to the simple comparison-in-means analysis, which compares all borderline defeated to all borderline elected (column (3) in Table 1), we find that covariates balance well around the cut-off for a seat change. Winning a seat in the previous election, arguably the best and most important predictor of a candidate's future electoral performance (Fouirnaies, 2016), does not differ systematically between close winners and close losers.

3.3 Main results

Figure 3 illustrates how winning a seat in the current election (t) affects the probability of winning a seat in the next election ($t + 1$). Again, we provide a standard RD plot in the left-hand panel, while we plot RD estimates as functions of the bandwidth chosen in the right-hand panel. Visual inspection of the data indicates that incumbents have an electoral advantage over non-incumbents in the next election. When comparing bare

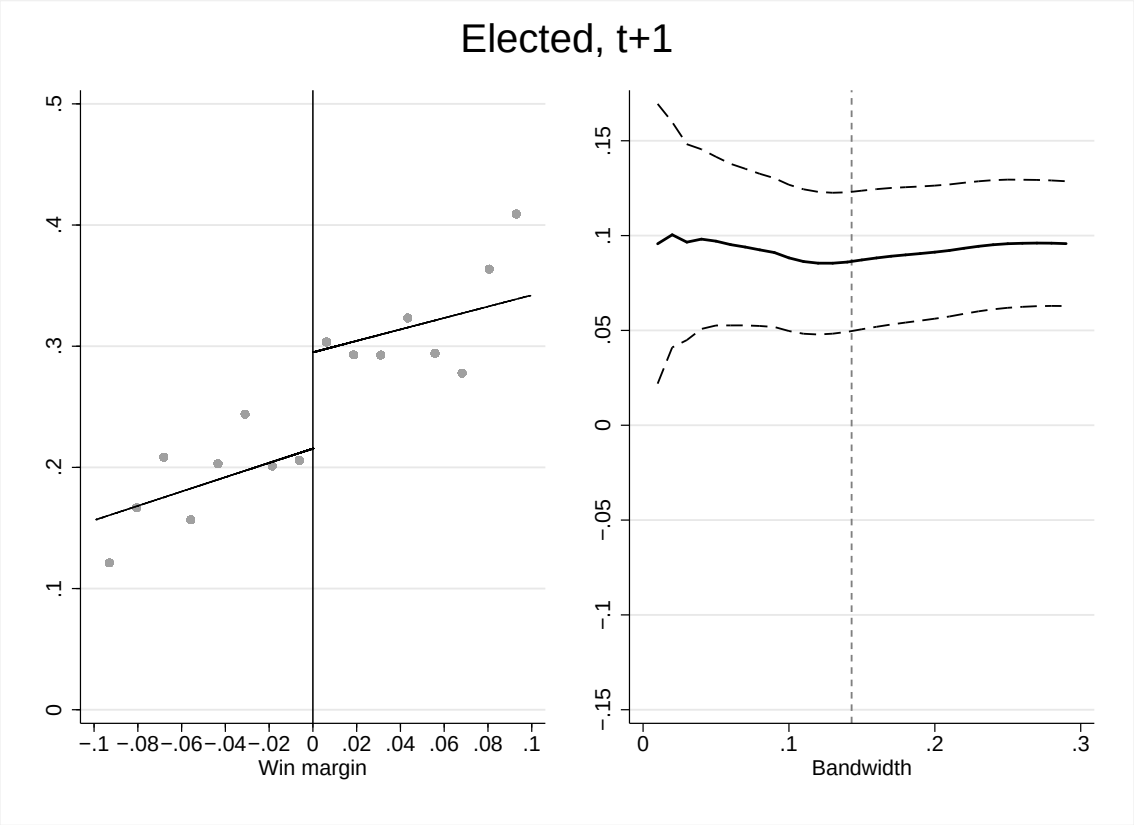
Figure 2: RD plots: Pre-determined covariates



Note: The figure presents results on covariate balance for six different variables (given in the title of each panel). Plots to the left are standard RD plots using a bandwidth of 10 percentage points (N = 3,908). Separate linear lines are estimated below and above the discontinuity using the underlying data, not the binned scatter points. The vertical line in each plot represents a zero win margin, indicating the transition from barely missing out on a seat to barely winning. Each dot represents a binned average for 1 percentage point intervals. The plots to the right display RD estimates and 95% confidence intervals as a function of the bandwidth chosen. The dashed vertical line indicates the optimal bandwidth calculated by the Calonico et al. (2016) algorithm. RD estimates are based on local linear specification using a triangular kernel. Standard errors are clustered at the municipality level.

winners to bare losers, the personal incumbency advantage seems to be slightly below ten percentage points. The statistical analysis, presented in the first column of Table 2, confirms this impression. Our baseline specification, in Panel A, is a local linear specification using a triangular kernel. The point estimate (8.6 percentage points) when using the optimal bandwidth is indicated by the vertical line in the right-hand panel of Figure 3.

Figure 3: RD Plot: The personal incumbency advantage



Note: The plot to the left is a standard RD plots using a bandwidth of 10 percentage points (N = 3,908). Separate linear lines are estimated below and above the discontinuity using the underlying data, not the binned scatter points. The solid vertical line represents a zero win margin, indicating the transition from barely missing out on a seat to barely winning. Each dot represents a binned average for 1 percentage point intervals. The plot to the right display RD estimates and 95% confidence intervals as a function of the bandwidth chosen. The dashed vertical line indicates the optimal bandwidth calculated by the Calonico et al. (2016) algorithm. RD estimates are based on local linear specification using a triangular kernel. Standard errors are clustered at the municipality level.

If we replace the triangular kernel with a uniform kernel (Panel B), i.e., one that puts no particular weight on observations close to the cut-off, or use a local quadratic

specification (Panel C and D), the estimated incumbency effect is basically unaltered. The effect is statistically significant at all conventional levels in all specifications.[17]

Our estimate of the personal incumbency advantage of about nine percentage points is similar to the RD estimates presented by Lundqvist (2013) for local Swedish elections (six percentage points) and by Dahlgaard (2016) for local Danish elections (twelve percentage points). In local Finish elections, where voters exclusively decide which candidates are eventually elected from a given list, the personal incumbency advantage is modest or perhaps non-existent (Hyytinen et al., 2017; Kotakorpi, Poutvaara and Terviö, 2017).

Our RD estimate is considerably smaller than the incumbency effects documented for national closed-list elections in Norway (Fiva and Smith, 2016). Political advancement in closed-list systems depends on the regard in which the candidates are held by the party, especially the party leadership, whereas advancement in open-list systems also depends on the candidate's relative attractiveness to voters. This may explain why the incumbency effects documented by Fiva and Smith (2016) are larger than what we find. It may be easier for party elites to orchestrate re-election of current incumbents in (national) closed-list elections, than at (local) open-list elections.[18] In the next subsection we explore the sources of the personal incumbency advantage in more detail.

3.4 Mechanisms

The interaction of various political actors may contribute to the observed personal incumbency advantage. The existing literature, the focus of which is predominantly on candidate-centered electoral contexts, emphasizes various strategies incumbents themselves can make use of to improve their electoral chances. In PR systems, party elites

[17]Using the bias-correction and robust inference procedure of Calonico, Cattaneo and Titiunik (2014), the 95% confidence interval ranging from 0.047 to 0.129 (Table 2, panel A, column 1). We have seen that covariates are well balanced across the cut-off for a seat change (Figure 2). It is therefore unsurprising that adding covariates or list fixed effects in the RD specification leaves the point estimates of interest basically unaltered (see, Appendix Table A.1). Also, there is no gain precision from including these covariates. In Appendix Table A.2, we show that the personal incumbency advantage is estimated to be slightly larger in above-median size municipalities (eleven percentage points) than in below-median size municipalities (eight percentage points), but the 95% confidence intervals largely overlaps.

[18]Fiva and Smith (2016) find an incumbency advantage of about 25 percentage points.

Table 2: Results from regression discontinuity estimation

Panel A: Local linear specification - triangular kernel

	(1) Elected	(2) Candidate	(3) Advance	(4) Safe	(5) Votes
RD estimate	0.086***	0.011	0.041**	0.041***	0.005
	(0.019)	(0.025)	(0.019)	(0.013)	(0.004)
Bandwidth	0.143	0.083	0.125	0.100	0.043

Panel B: Local linear specification - uniform kernel

	(1) Elected	(2) Candidate	(3) Advance	(4) Safe	(5) Votes
RD estimate	0.080***	0.013	0.036*	0.039***	0.005
	(0.020)	(0.025)	(0.019)	(0.014)	(0.005)
Bandwidth	0.091	0.060	0.092	0.073	0.028

Panel C: Local quadratic specification - triangular kernel

	(1) Elected	(2) Candidate	(3) Advance	(4) Safe	(5) Votes
RD estimate	0.086***	0.005	0.045**	0.042***	0.004
	(0.021)	(0.025)	(0.021)	(0.015)	(0.004)
Bandwidth	0.163	0.204	0.179	0.162	0.070

Panel D: Local quadratic specification - uniform kernel

	(1) Elected	(2) Candidate	(3) Advance	(4) Safe	(5) Votes
RD estimate	0.087***	-0.002	0.039*	0.048***	0.002
	(0.022)	(0.025)	(0.022)	(0.015)	(0.005)
Bandwidth	0.125	0.165	0.142	0.122	0.055

*Note: Each cell represent RD estimates based on equation 3 using the optimal bandwidth calculated by the Calonico et al. (2016) algorithm. In the first, second, third, and fourth column the outcome variables are equal to one if the candidate in the next election win a seat, run, improve his/her list position, and hold a safe position on the list, respectively. In the fifth column the outcome variable is the ratio of candidate personal votes to party votes in the next election. If the candidate is not running this variable is zero. Standard errors clustered at the municipality level are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.*

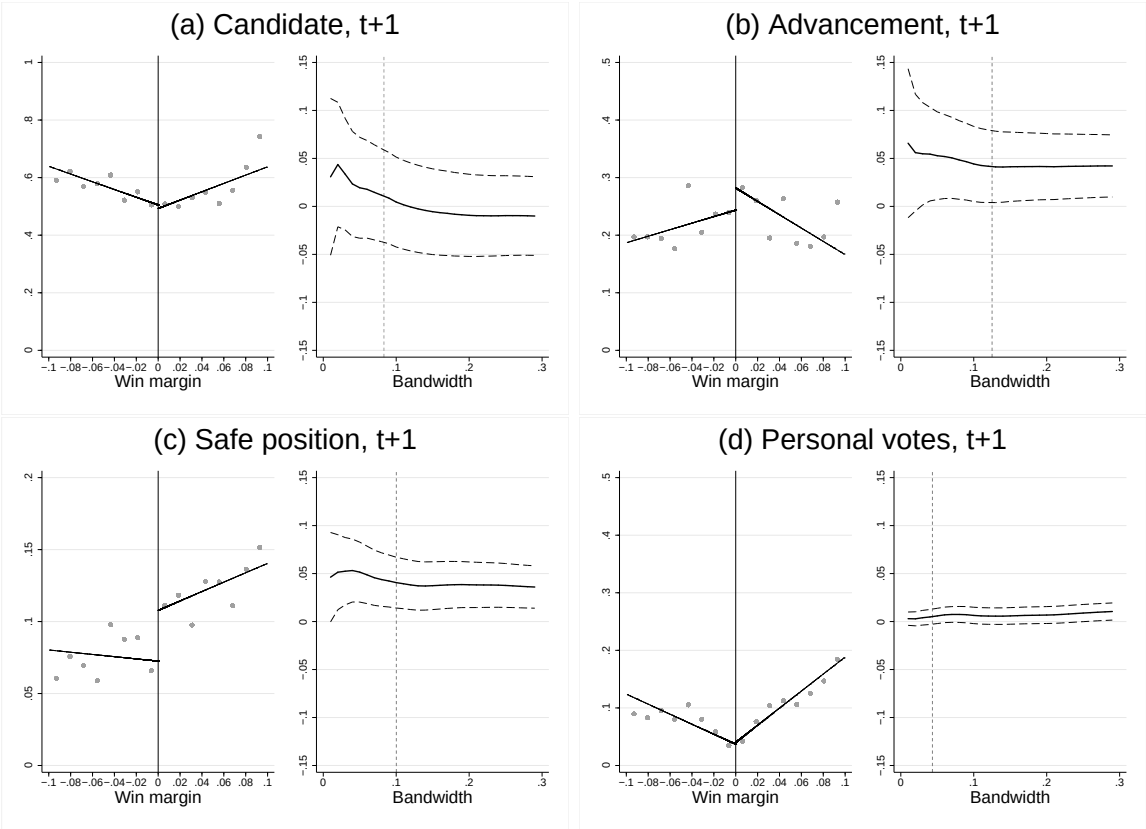
may also contribute to the incumbency advantage through the ordering of the electoral lists (Llaudet, 2014; Golden and Picci, 2015). In the following we investigate whether winning a council seat increases candidates' tendency to run again, whether incumbents rise faster in the party hierarchy than non-incumbents, and whether voters are more likely to cast personal votes for incumbents than for non-incumbents. As before, we exploit close elections for inference.

Panel (a) of Figure 4 shows that the personal incumbency advantage is not driven by an increased propensity for incumbents to run again: Both bare losers and bare winners tend to run again in about half the cases. Incumbents tend, however, to get re-nominated and placed higher up (lower numerically) on the list in the next election (see panel (b) of Figure 4). The probability of advancing up the list increases by about 4 percentage points as a result of winning a seat in the current election. The probability of landing a *safe position* in the next election also increases by about 4 percentage points from winning a seat in the current election (see panel (c) of Figure 4). Both these effects are statistically significant at conventional levels (see column (3) and (4) in Table 2).

Figure 5 illustrates in more detail how bare winners rise in the party hierarchy in comparison to bare losers. We restrict the sample to candidates less than 10 percentage points away from winning or losing their seat in election t who also run in election $t+1$. In the current election bare winners and bare losers have similar ballot rankings (left-hand panel), while in the next election, bare winners move towards the top of the list (right-hand panel).

Both supply and demand factors are likely to contribute to this result. On the one hand, candidates' *willingness* to take on a high-ranked position on the party list may be influenced by just winning or losing the last election. On the other hand, incumbents gain political experience and become more central in the local party organization. When the nomination committee starts to *recruit candidates* for the next election, incumbents are likely to be the first they consider (Ringkjøb and Aars, 2010). We believe that when incumbents climb the party ranks, it is likely due to a combination of candidate

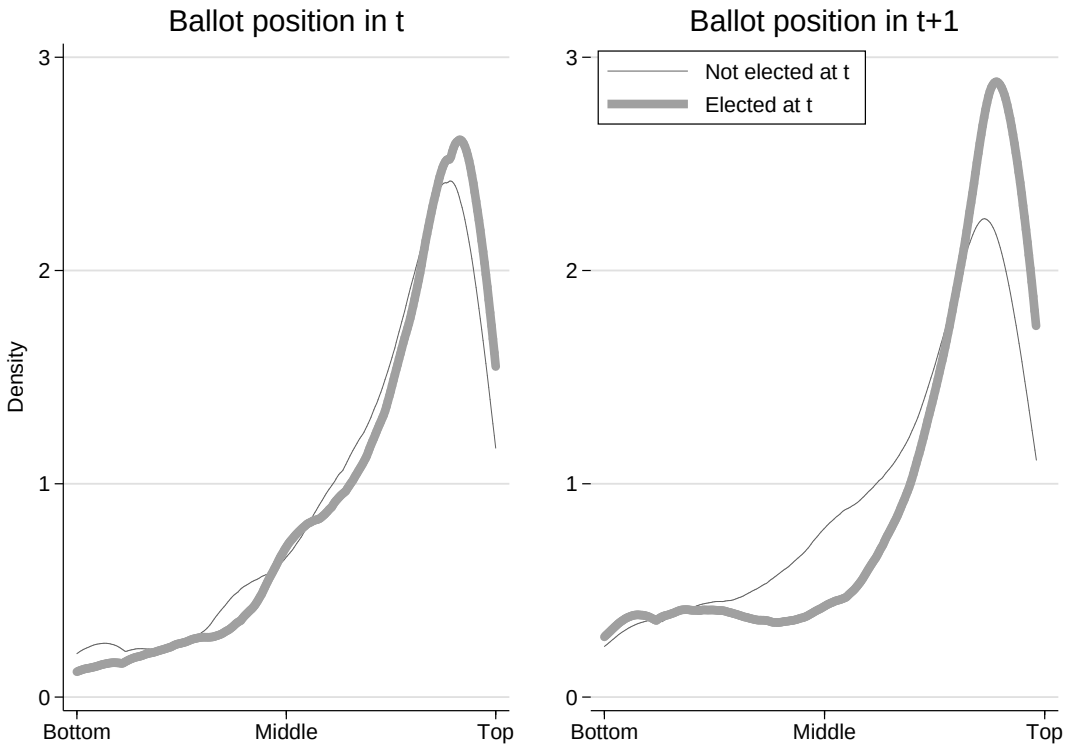
Figure 4: RD Plots: Mechanisms



Note: Plots to the left are standard RD plots using a bandwidth of 10 percentage points (N = 3,908). Separate linear lines are estimated below and above the discontinuity using the underlying data, not the binned scatter points. The vertical solid line in each plot represents a zero win margin, indicating the transition from barely missing out on a seat to barely winning. Each dot represents a binned average for 1 percentage point intervals. The plots to the right display RD estimates and 95% confidence intervals as a function of the bandwidth chosen. The dashed vertical line indicates the optimal bandwidth calculated by the Calonico et al. (2016) algorithm. RD estimates are based on local linear specification using a triangular kernel. Standard errors are clustered at the municipality level.

motivation (supply) and within-party popularity (demand).

Figure 5: Kernel density plot of candidates' ballot position in elections t and $t+1$



Note: The figure shows kernel density plots of candidates' standardized ballot position in elections held at time t (left-hand panel) and $t+1$ (right-hand panel). The thick lines provide density plots for bare winners, while the thin lines provide density plots for bare losers. The sample is restricted to candidates less than 10 percentage points away from winning/losing a seat in election t who also run in election $t+1$ (N = 3,908). The standardized ballot position take values from 0 (bottom position) to 1 (top position).

Survey evidence indicates that local political experience is important when voters decide whether to cast a personal vote for particular candidates.[19] It is, however, not straightforward to identify the extent to which voters contribute to the personal incumbency advantage. Panel (d) of Figure 4 shows that candidates that just missed out on a seat in election t receive about the same number of personal votes in $t+1$ than candidates that just won a seat. Two factors complicate the interpretation of this figure. First, winning the current election possibly affects the decision to run again and hence whether

[19]See Appendix Figure A.3.

candidates receive *any* votes at $t+1$. In our setting, this does not seem to be a major concern since bare losers and bare winners run again at about equal rates (cf. Panel (a) of Figure 4). Second, winning the current election affects candidates' list position (and pre-advantage status) in the next election. From our main analysis we know that incumbents tend to obtain better list positions at $t+1$ than non-incumbents (cf. Table 2). Hence, ballot position likely acts as an informational cue to voters, and may increase the number of personal votes that incumbents receive.[20] In any case, there is no evidence in our data that personal votes drive the personal incumbency advantage.[21]

3.5 Placebo checks

To assess the validity of our RD design we estimate "placebo treatment effects" by comparing sets of candidates all of whom won seats in the local election. More specifically, we identify candidates that would have been borderline elected or borderline defeated if the party had won one, two, three, or four fewer seats, respectively. Then, we construct the "win margin" as in the baseline RD, and implement four placebo RD designs. If we find evidence of any "incumbency advantage" in these placebo checks it would cast doubt on our identification strategy.

Figure 6 shows the results of this falsification exercise. In panel (a), the actual borderline elected are treated as *borderline defeated*, while candidates who are second in line to lose their seats are treated as *borderline elected*. There is no evidence of any "incumbency effect" in this placebo checks. The same is true for the three other placebo tests (panel b, c, and d).

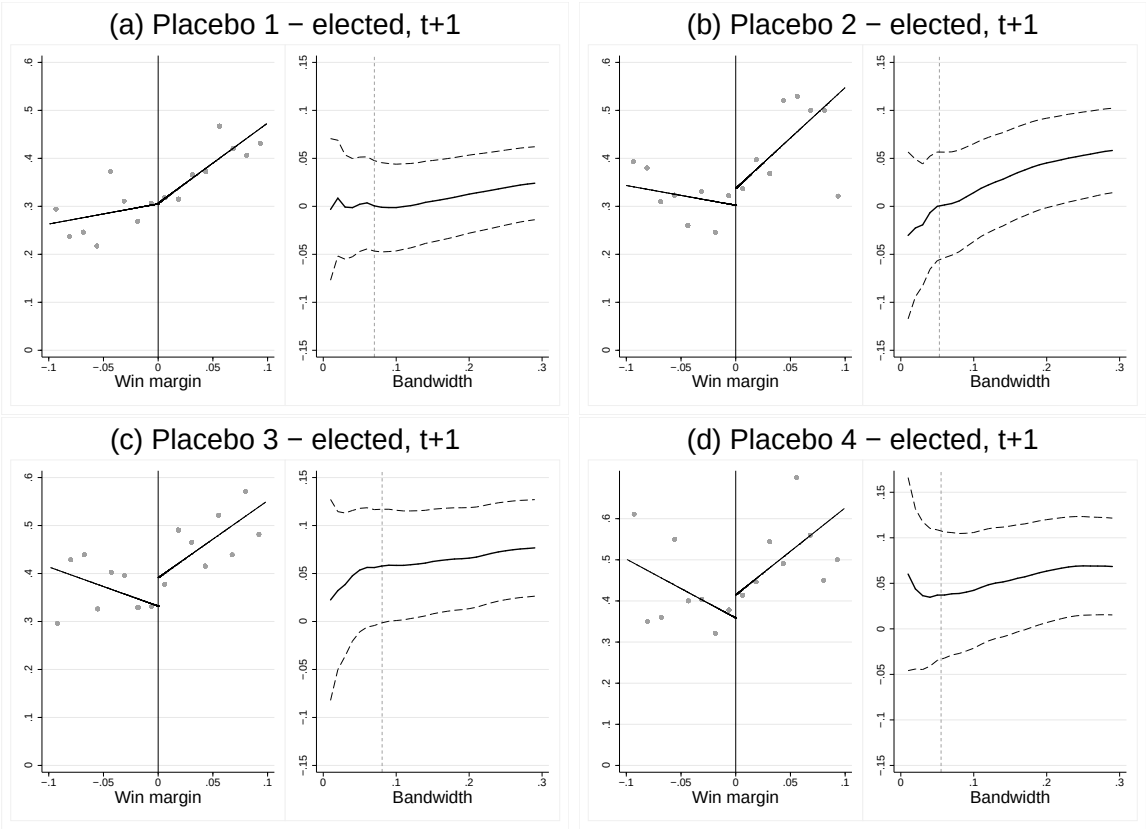
Folke, Persson and Rickne (2016) study how the distribution of personal votes across candidates affect the nomination and promotion behavior of political parties in Sweden

[20] It is also possible that the ballot order *in itself* affects election outcomes (Miller and Krosnick, 1998). In our data set, candidates ranked higher on the ballot tend to obtain more personal votes (cf. Appendix Figure A.4). This strong empirical regularity has been documented previously by Christensen et al. (2008).

[21] The 95% confidence interval ranges from –0.003 to 0.013 (see panel A, column (5) in Table 2). This implies that if there is an incumbency effect on personal votes, it is *at most* around 1 percentage point.

and Brazil. They find that personal votes matter beyond their direct impact on electoral outcomes. The lack of any significant effects in the placebo analyses presented in Figure 6, suggest that such "primary effects" are less important in our context.

Figure 6: RD Plots: Placebo analysis



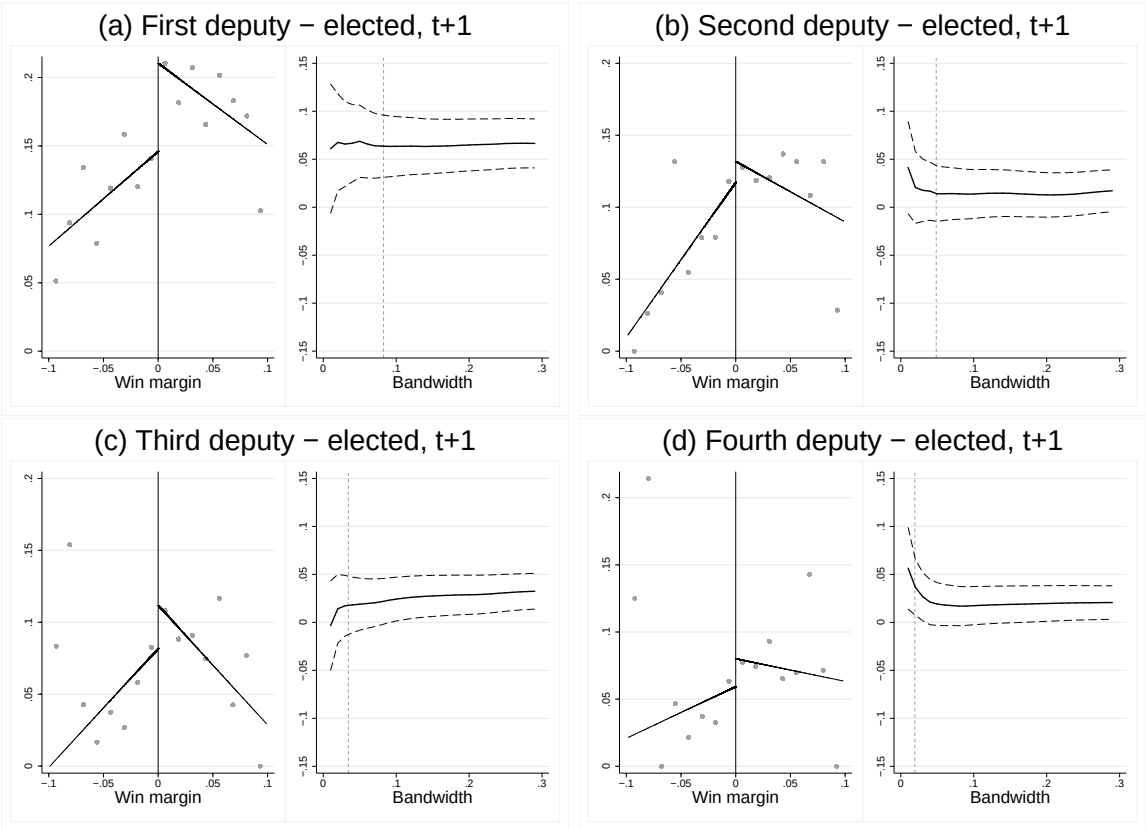
Note: The left-hand panels show RD plots for winning at $t+1$ when each party is assumed to be winning (a) one, (b) two, (c) three or (d) four less seats. The sample is limited to party lists that won at least two, three, four, and five seats, in panels (a), (b), (c), and (d), respectively (without restricting the bandwidth the number of observations are 5,242, 4,216, 3,224, and 2,513, respectively). Separate linear lines are estimated below and above the discontinuity using the underlying data, not the binned scatter points. Each dot represents a binned average for 1 percentage point intervals. The plots to the right display RD estimates and 95% confidence intervals as a function of the bandwidth chosen. The dashed vertical line indicates the optimal bandwidth calculated by the Calonico et al. (2016) algorithm. RD estimates are based on local linear specification using a triangular kernel. Standard errors are clustered at the municipality level.

3.6 RD estimates of deputy councilors

Following the set-up of the placebo analyses, we estimate four RD analyses assuming that the party had won one, two, three, or four *more* seats. These estimates should not be

interpreted as placebo checks because the ranking of the losers on a party's list determines the ranking of substitute representatives. Panel (a) of Figure 7 shows that becoming the first deputy councilor (rather than the second deputy councilor) increases the likelihood that the candidate wins a seat in $t+1$ with six percentage points, while, becoming the second, third, or fourth ranked deputy appear not to substantially affect future election chances (panel b, c, and d). Like elected candidates, first deputy councilors also tend to climb the party ranks (see Appendix Table A.3).

Figure 7: RD Plots: Deputy analysis



Note: The left-hand panels show RD plots for winning at $t+1$ when each party is assumed to be winning (a) one, (b) two, (c) three or (d) four more seats. Separate linear lines are estimated below and above the discontinuity using the underlying data, not the binned scatter points. Each dot represents a binned average for 1 percentage point intervals. The right-hand panels display RD estimates and 95% confidence intervals as a function of the bandwidth chosen. The dashed vertical line indicates the optimal bandwidth calculated by the Calonico et al. (2016) algorithm. RD estimates are based on a local linear specification with a triangular kernel. Standard errors are clustered at the municipality level.

4 Across-parties RD design

So far, we have focused on individual candidates' electoral advantages from barely winning a council seat. Do *parties* also benefit from winning a seat in the council? If voters prefer incumbents over non-incumbents, then parties should be able to capitalize on this, and have an advantage in the seat share in the following election. To investigate this hypothesis, we implement an across-parties RD design, where we compare electoral outcomes of party lists that won a single seat (the treatment group) to party lists that did not win any seats (the control group). We construct the forcing variable as in Folke (2014) and Fiva, Folke and Sørensen (2017) and rely on a balanced panel of 413 municipalities for the 2003-2015 period.[22] We pool observations over party lists that won zero or one seat in the 2003, 2007, and 2011 elections, leaving us with a sample of 1,575 observations. As in the preceding analysis, we only include party lists from the seven dominant parties in Norwegian politics.

Figure 8 provides six key plots for the across-parties RD analysis. The top left-hand panel shows the density of observations by the distance to the seat threshold. We see no evidence of any bunching of observations around the threshold for the seat change.[23] This is not surprising since it is essentially impossible for a party to know ex-ante where the seat thresholds are going to be (Fiva, Folke and Sørensen, 2017). In the top middle panel we see that parties' vote shares at time t remain stable at the threshold for a seat change, providing further support for the identifying assumption of this RD design. The top right-hand panel of Figure 8 shows the 'first stage': Barely winning a seat in the council increases the seat share at time t from zero to 0.04. This implies that local councils consists, on average, of about 25 councilors in the estimation sample.

The bottom left-hand panel of Figure 8 plots the relation between the party running

[22] In 2015, 428 municipalities existed. We drop municipalities that were involved in mergers during our sample period (Vindafjord, Kristiansund, Aure, Inderøy, Bodø, and Harstad), municipalities displaying inconsistencies between the distribution of votes and the distribution of seats (Aukra, Sør-Varanger, Hobøl, Skedsmo, Karlsøy, Modalen, Molde, and Gjemnes), and one municipality with a tie in vote shares in one year (Flatanger).

[23] Appendix Figure A.5 shows the McCrary density plot.

in the next election ($t+1$) and the distance to the seat change (at time t). The jump at the cut-off indicates that parties that barely win a seat in the council are about 15 percentage points more likely to run again in comparison to party lists that just missed out on a seat.[24] Still, judging from the bottom right-hand panel Figure 8, it appears that incumbent parties are no more likely to be re-elected at $t+1$ over a party list that barely misses out on its' first seat.[25] This result is consistent with our previous findings which suggest that the personal incumbency advantage materializes primarily within the party and not as a consequence of voter demand for incumbents over non-incumbents.

5 Conclusion

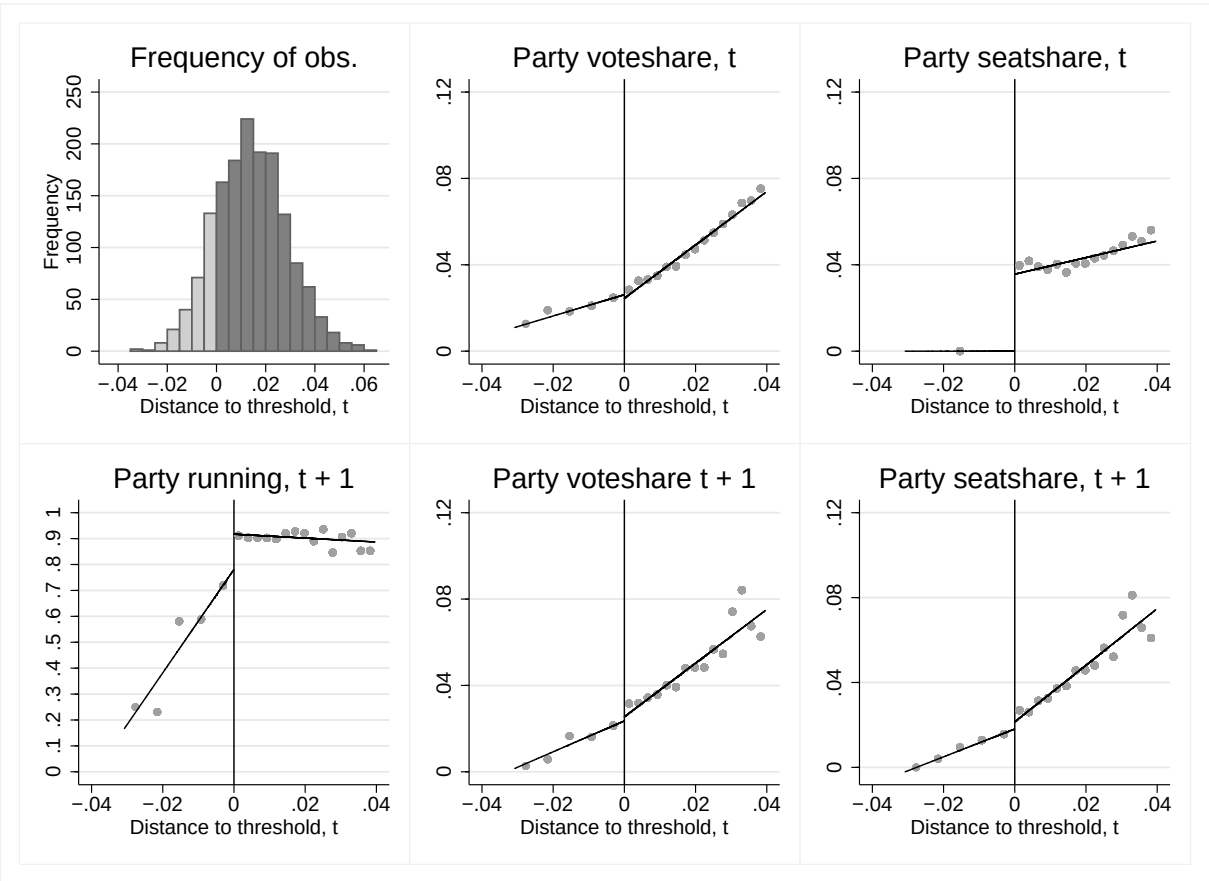
Previous studies from the United States have found that *individual candidates* benefit electorally from holding office while other candidates from the *incumbent party* do not (Fowler and Hall, 2014; Erikson and Titiunik, 2015). We document a similar pattern in the context of open-list PR elections in Norway. The mechanisms that drive the personal incumbency advantage differ markedly, however, across the different electoral settings. While voters are instrumental in creating the personal incumbency advantage in candidate-centered contexts, this appears not to be the case in more party-oriented environments.

We document that councilors who just won a seat in the last election are more likely to win again compared to party peers who just missed out on a seat. We find no clear evidence that voters contribute to this personal incumbency advantage. Rather, our study shows, the personal incumbency advantage appears to be created within party organizations. Incumbents and non-incumbents alike run again in the subsequent election

[24] Using a local linear specification with a triangular kernel, the optimal bandwidth is 0.01 according to the algorithm developed by Calonico et al. (2016). Using this estimation window, the point estimate is 0.18 with a standard error of 0.08.

[25] The jump at the cut-off is positive, but close to zero. Using the optimal bandwidth, we find a point estimate of 0.005 and a standard error of 0.007. We have also implemented an across-parties RD design that exploits variation in incumbency status both at the extensive (winning the first seat) and intensive margin (winning multiple seats). Also in this analysis, there is no evidence that parties benefit from winning an additional seat. Results omitted for brevity.

Figure 8: Across-parties RD design



Note: The top left-hand panel shows the density of observations by the distance to the seat threshold. The other panels plot the relation between various outcome variables (given in the panel heading) against the distance to the seat threshold. If the party is not running at $t+1$, vote shares and seat shares are set to zero. The sample is restricted to the seven main parties in Norwegian politics. We only include lists winning zero or one seat in the council (N=1,575). Separate linear lines are estimated below and above the discontinuity using the underlying data, not the binned scatter points.

at about equal rates, but incumbents tend to advance in the party hierarchy and obtain safer positions on the ballot in the next election.

We should highlight that our study does not fully separate elites' and voters' respective contributions to the incumbency advantage. The fact that incumbency has such a large effect on list placement strongly suggests that elites drive much of the personal advantage. However, other interpretations are possible: For example, it could be that voters always support the same party, but within that party, they prefer incumbents, and the parties respond endogenously by throwing their support behind incumbents. We find it unlikely that parties perfectly anticipate voter reactions, but our findings are consistent with such an interpretation.

Political commentators often complain about the electoral dominance of incumbents, conjecturing that mediocre politicians exploit the political system in order to keep themselves in power (Fowler, 2016). However, a more optimistic interpretation, supported by evidence in Fowler (2016), is that open elections effectively select for good leaders who naturally continue to win reelection. Is this the case in our party-centered context? Are incumbents that rise to the top of the party hierarchy high-quality leaders that match the preferences of the electorate? Our current study cannot answer these questions, but we hope that future research will extend our knowledge in this direction.

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