Online Appendix (not for printing) to
The Primary Effect: Preference Votes and Political Promotions

July 25, 2015

Who gets preference votes?

Here, we ask if Swedish voters make an informed choice when casting their preference votes, and if they reward politicians with higher competence. To examine these questions, we run a simple regression for the relationship between candidate competence, as well as key socioeconomic characteristics of the politician, and the share of preference votes in the party group that the candidate receives. The socioeconomic characteristics are gender, country of birth (Nordic country or not), age, re-election (at least once), and education (tertiary education or not). The last two of these have been used to measure politician quality in previous work, but we also include three other measures.

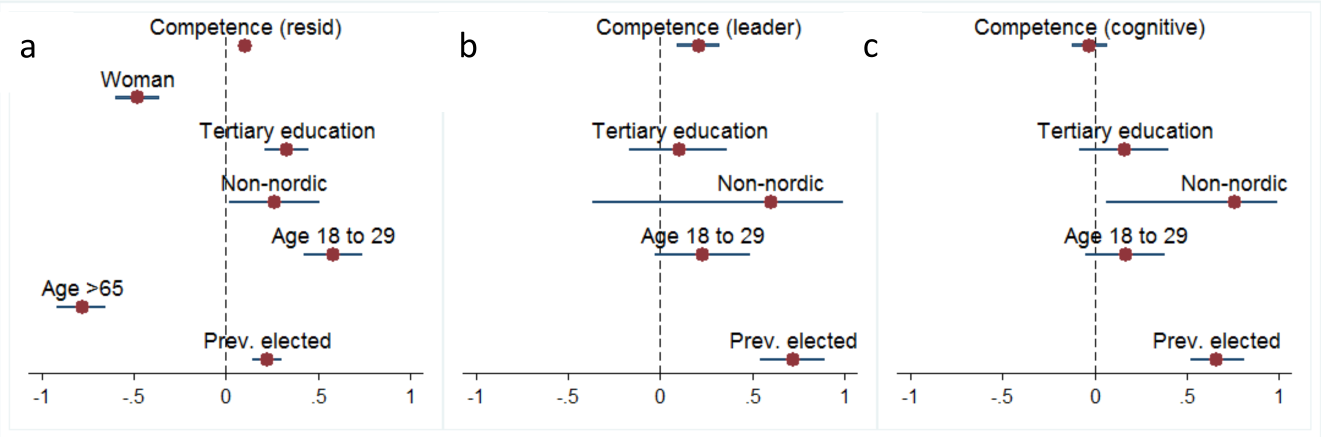
Our main competence measure is the one described in Section 4 of the paper, which assumes that competence can be gauged from average income differences (over 20 years) between people with the same education, occupation, age, and gender. The other two competence measures are from Sweden's prior mandatory military draft: one cognitive score from a written IQ-type test and one leadership score from a formalized interview with a psychologist meant to sort recruits across training programs. However, the draft data are only available for men born between 1951 and 1980.

The results from this analysis are presented in Figure A1, which plots the point estimates and a 95% confidence interval for each candidate characteristic. To reduce the risk of conflating socioeconomic characteristics with trends in preference voting and/or list rank, all specifications include non-parametric trends in preference voting over time (year-fixed effects); non-parametric trends within each specific party group (year-fixed effects interacted with party-group-fixed effects)[1]; and preference vote differences across list ranks (list-rank fixed effects interacted with four categorical dummy variables for the size of the party group). Since

[1] When we use the draft data to measure competence, we often only have a measure for one member, or none, of each party group. To avoid throwing these observations away we do not include the party-group-year fixed effect directly in the regressions. Instead we take the residual from a regression for the full sample in which we include the fixed effect. The estimates are not sensitive to using this procedure or not, but the standard errors of the estimates become much smaller.

they include list-rank fixed effects, our estimates are likely to be downward biased as more competent politicians more likely have higher ranks. The control for list rank may thus partly absorb the correlation between competence and preference votes.

Figure A1 – Share of Preference Votes and Politician Characteristics



Notes: Each graph shows the coefficients and their 95% confidence bands in an OLS regression of the share of preference votes for an individual candidate on her list rank and a number of personal characteristics, as described in the text.

The income-residual and leadership measures are positively correlated with preference votes, even though we control for list rank. Higher wage-residual competence by one standard deviation is associated with a 0.1 percentage point higher share of preference votes. The estimate based on the leadership score has the same magnitude. Even though the estimates are relatively small, they are still striking as they refer to a characteristic that voters cannot learn about from ballot information. One potential reason is that charismatic politicians with good leadership skills are more likely to run personal campaigns or be known through the local media. Some Swedish local politicians do run campaigns for preference votes, but these are often modest with spending typically falling below 5,000 SEK (~ USD 800) in the early 2000s.

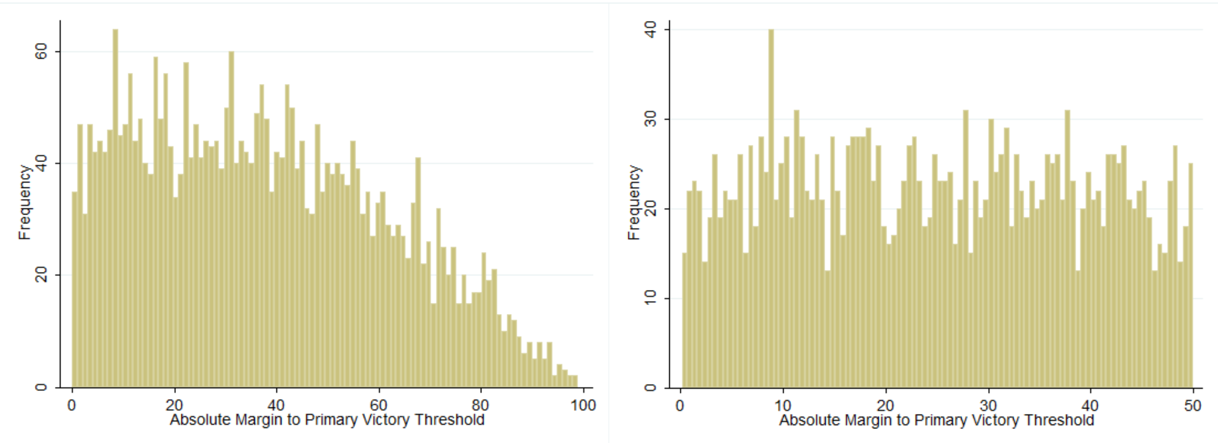
For the competence measure based on cognitive skills, we do not find a statistically significant association. This may be due to the limited sample of male politicians for whom we know the military draft results. In the full sample (Figure 8a), the coefficient on tertiary education is significant, but it is not in the smaller sample, suggesting that the analysis may pick up a different sample rather than voters disregard for cognitive skills. It is also notable that preference votes are positively and strongly associated with previous election, a common measure of politician quality.

All in all these results suggests that voters do reward competent politicians when casting their presence vote. Of course, this also suggests that at least some voters make an informed choice when casting a preference vote.

Data Description

In Figure A2, we show the density of observations for the Swedish municipal party groups as a function of the win margin. We show this for the full sample and for the sample where the relative margin between the two candidates is less than 50 percent. In both graphs we use 100 bins. We only include positive margins, as the negative margins would only mirror the positive ones (i.e., the loss margin of the runner up is identical to the win margin of the winner).

Figure A2 – The Density of Observations by Win Margin



Notes: The figures illustrate the density of observations as a function of the absolute win/loss margin. In the left-side graph we show the full distribution, while the right-side graph shows our estimation sample with an absolute margin of less than 50 percent. Each figure contains 100 observation bins.

The left figure shows a clear concentration of observation within a win margin of 50 percent. Outside of the 50 percent margin, the density of observations goes down as we move away from the threshold. When examining our estimation sample in the right-hand graph, we see that the observations are evenly spread out within the 50 percent window.

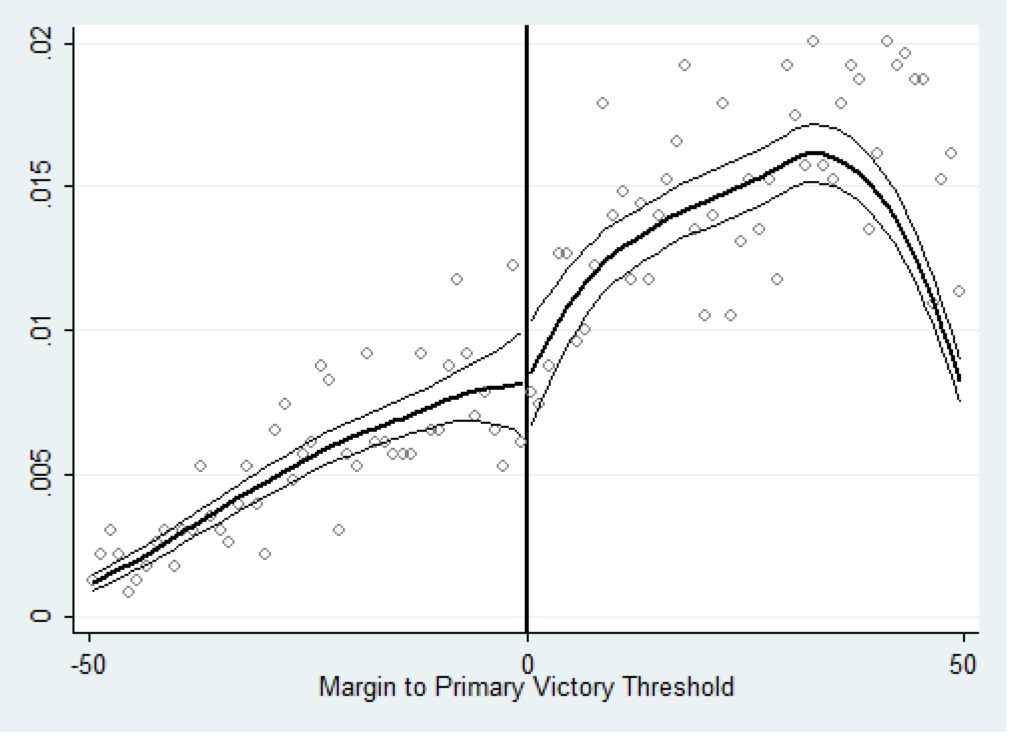
Robustness Checks for the Baseline Results for Sweden

Manipulation around the Threshold

An important condition for an RDD design to yield unbiased results is that the density of observations is smooth around the threshold defining the treatment. In our Swedish data, this is not clear a priori. For example, the current leader could have more resources to draw preference votes. S/he could also have more information about the expected distribution of the votes and use his or her resources to tilt the odds of winning. If such attempts were successful, this would show up as a higher density of observations to the right of the threshold, the cases where the current leader narrowly won the preference vote primary.

To test for such manipulation, we rely on the two-step procedure proposed by McCrary (2008). In the first step, the forcing variable is partitioned into equally spaced bins and frequencies are computed within those bins. In the second step, the frequency count within each bin is used as the outcome variable in a local linear regression. Our main analysis always has balanced frequency counts around the threshold, by definition, since we include both the winner and the runner up. To examine possible manipulation around the threshold, we therefore restrict the sample to the current party leaders to see if they are more likely to win close races. We present the results of this test graphically, with the regression lines as well as the raw density of observations.

Figure A3 – Test for Balance around the Threshold



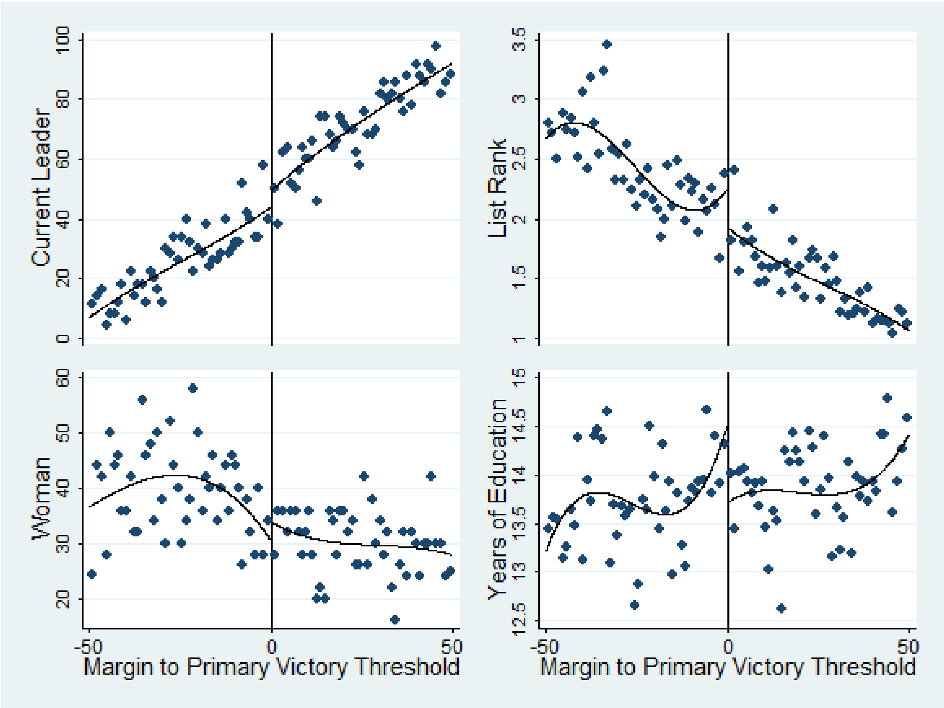
Notes: The graph illustrates a McCrary test for balance in the density of observations around the thresholds for being the primary winner. The sample is restricted to the current party leaders. The estimated discontinuity jump is 0.0433 with a standard error of 0.1705 – thus the jump at the density is not statistically significant.

Figure A3 shows the distribution of observations, by their margin to the primary victory threshold. Over the full support, the distribution is skewed to the right, which is expected given the advantage of current leaders in the preference vote (recall Figure 2). However, neither the raw data nor the local linear regression lines suggest any systematic sorting around the threshold. Thus, there is little indication that the current party leader can affect his/her preference vote tally relative to the top challenger, when the competition is neck and neck.

Placebo Outcomes

We now turn to some placebo analysis. This is organized in the same way as the baseline analysis, except that we consider different outcomes variables and exclude all control variables. We examine four outcomes: (i) being the current leader, (ii) current list placement, (iii) female gender, and (iv) years of education. These outcomes are strong predictors of receiving preference votes and of being selected as party leader in the next election. The placebo outcomes are also determined prior to treatment. Therefore, they should not be affected by passing the threshold if our analysis is correct. A graphical analysis is presented in Figure A4 and regression results in Table A1.

Figure A4 – Graphical Analysis of Being the Primary Winner on Placebo Outcomes



Notes: The figure contains binned averages of four placebo outcomes as a function of the distance to the list-winning threshold. Each bin contains 50 observations. The lines are fitted third-order polynomials.

The results from the graphical analysis and the regressions are clear: there is no evidence for treatment status being significantly related to any of the predetermined outcomes. The graphical analysis shows no indication of a shift as we pass the threshold for any of the four outcomes; nor do the regressions provide any evidence of sorting into treatment. All estimates for the specifications that include some sort of control function are very close to zero and all but one is statistically insignificant (see columns marked "Flexible Polynomials" and "Local Linear" in Table A1 below). The few significant estimates are found for list placement and being the current leader in the “close 10 percent” and “close 5 percent” specifications (see columns marked "Close Margin"). These significant results can be explained by the strong relationship between the forcing variable (i.e., preference votes as a share of the party vote) and list placement.[2] The steep slope of this relationship leads to an imbalance in sample means between the two sides of the threshold as we expand the window but do not include any control function for this slope. Also, for years of education, we find a weakly significant effect in some of the specifications with control functions, which seems to be a product of the control functions adjusting to outliers close to the threshold.

Table A1 – Regression Analysis of Being the Primary Winner on Placebo Outcomes

		Flexible Polynomials		Local Linear		Close Margin	
Party leader current election	OLS	2^{nd} Pol	3^{rd} Pol	I-K test	10% window	10 % window	5% Window
Primary Effect	46.19***	6.53	5.61	0.36 [a]	1.66	14.80***	12.44*
	(1.77)	(5.95)	(8.22)	(8.45)	(9.41)	(4.43)	(6.62)
Observations	4,486	4,486	4,486	1,112	892	892	402
Current list rank							
Primary Effect	-0.94***	-0.10	-0.33	-0.20 [b]	0.00	-0.34***	-0.21
	(0.05)	(0.16)	(0.22)	(0.17)	(0.26)	(0.11)	(0.17)
Observations	4,422	4,422	4,422	1,703	881	881	399
Woman							
Primary Effect	-9.41***	1.65	3.44	2.29 [c]	3.67	-1.12	1.00
	(1.58)	(4.90)	(6.65)	(5.04)	(7.44)	(3.57)	(5.28)
Observations	4,486	4,486	4,486	1,962	892	892	402
Years of education							
Primary Effect	0.14*	-0.17	-0.82**	-0.38 [d]	-0.46	-0.22	-0.22
	(0.08)	(0.25)	(0.34)	(0.29)	(0.39)	(0.18)	(0.27)
Observations	4,483	4,483	4,483	1,570	892	892	402

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the party-group level are reported in parentheses. Bandwidths [a]12.1%; [b]18.5%, [c]21.4%; [d]17.2%.

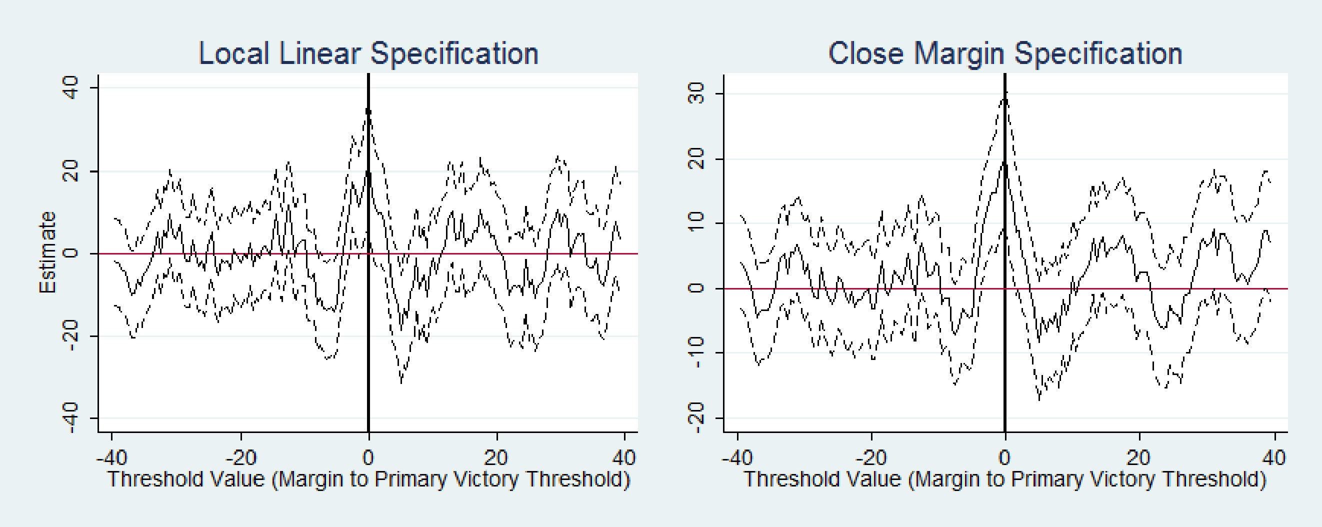
[2] Snyder et al. (2014) show that this type of imbalance is not a problem in RDD specifications. If one controls for the relationship between the outcome and the forcing variable, the imbalance will also be controlled for.

In sum, we find no evidence of violations to the identifying assumptions.

Placebo Thresholds

By varying the placement of the treatment threshold, we can ascertain that our analysis is not picking up one of many jumps. In Figure A5, we let the treatment threshold vary and plot the estimated treatment effects along with a 95-percent confidence interval. The figure shows the placebo analysis for two specifications with local-linear regression and an estimation window of 10% (graph to the left), and the specification without any control function and a window of 5% (graph to the right). These figures reassure us that the spikes in the estimated treatment effects are found at the true threshold values – these spikes are also the only ones that reach statistical significance.

Figure A5 – Placebo Analysis of Estimated Primary Effects

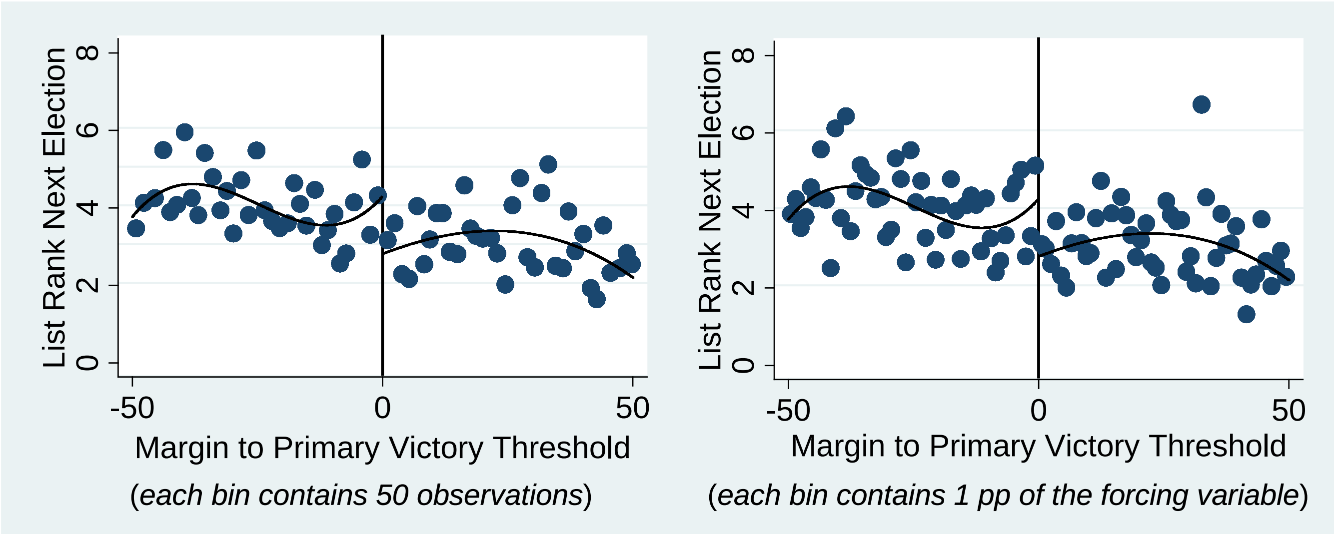


Notes: The graphs show the estimated treatment effect on becoming party leader in the next election of being the preference vote primary winner in the current election for different values of the treatment threshold. Zero corresponds to the true threshold value, while positive and negative numbers correspond to different false placebo values (for different percentages away from the true value). The black line shows the point estimates, and gray lines show a 95 percent confidence interval.

Extended Analysis with List Rank as the Outcome

Figure A6 shows the graphical analysis with list rank as the outcome variable, and Table A2 follows up with the regression evidence. The graphical evidence is not as convincing as for the main outcome variables, and most of the estimates are only statistically significant at the 10 percent level.

Figure A6 – Graphical Analysis of the Primary Effect on List Rank in the Next Election



Notes: The graphs show binned averages of list rank in *t+1* as a function of the win/loss margin to being the preference vote primary winner. In the left graph, each bin contains 50 observations. In the right, each bin contains all observations within a 1 percentage-point interval for the win/loss margin. The lines in the graphs are third-order polynomials fitted to the observations.

The shakiness of the analysis is driven at least in part by the long upper tail of the dependent variable. Because politicians who quit active political work sometimes remain as "list pullers" on very low-ranked positions on the party ballot, we have a group of individuals with very high list ranks in *t*+1. Truncating the list-rank variable at a lower number, gives substantially smaller standard errors in the regressions.

Table A2 – Estimates of the Relationship between Winning the Preference Vote Primary in Election *t*, and List Rank in *t*+1

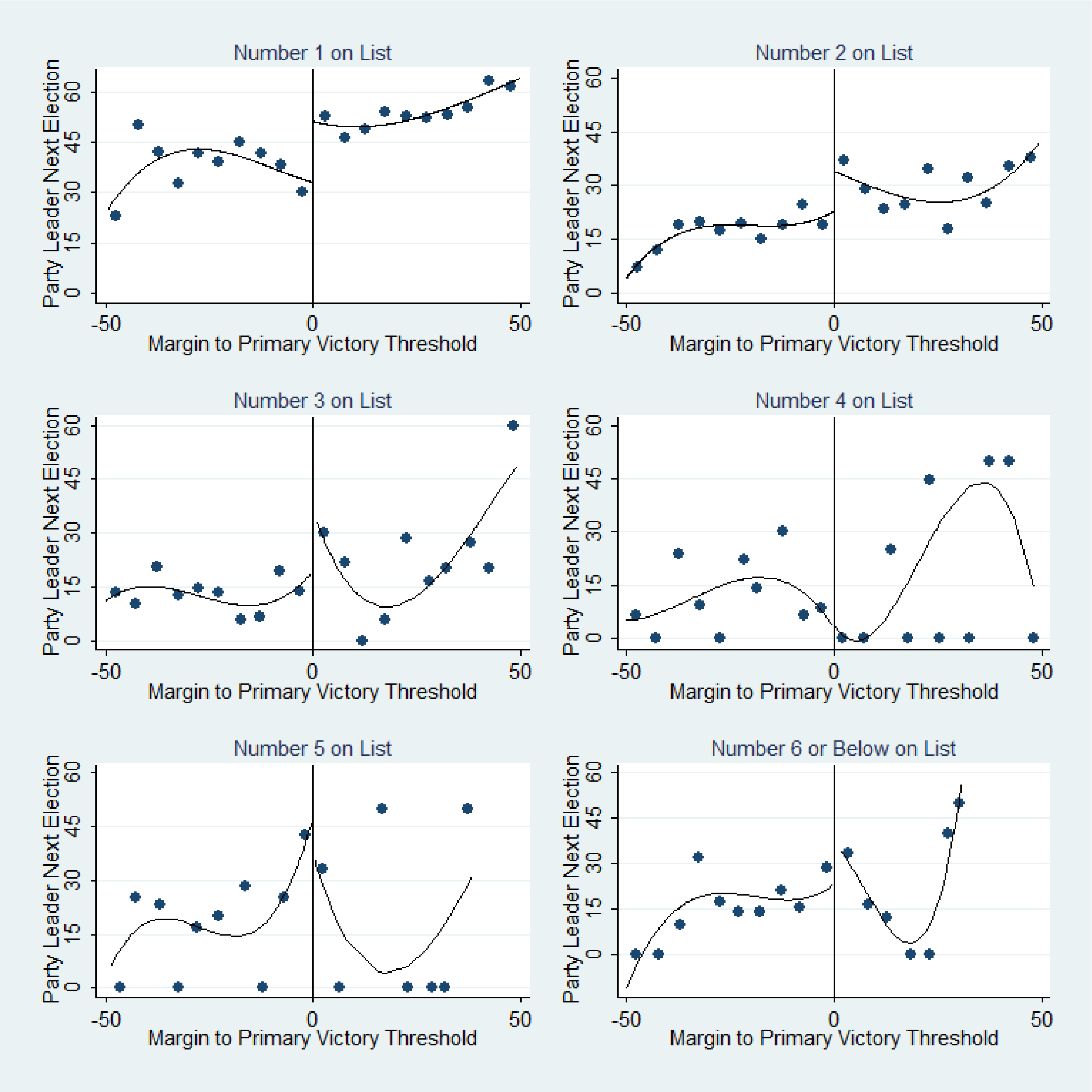
		Flexible Polynomials		Local Linear		Close Margin	
Without Controls	OLS	2nd Pol	3rd Pol	I-K test	10% window	10 % window	5% Window
Primary Effect	-0.94***	-0.80	-1.49*	-0.90 [a]	-1.78*	-0.63*	-1.29*
	(0.18)	(0.58)	(0.84)	(0.60)	(0.96)	(0.38)	(0.67)
Observations	3,448	3,448	3,448	1,427	686	686	318
With Controls							
Primary Effect	-0.38**	-0.77	-1.49*	-0.97 [a]	-1.83**	-0.65*	-1.37**
	(0.18)	(0.55)	(0.79)	(0.57)	(0.87)	(0.33)	(0.59)
Observations	3,440	3,440	3,440	1,394	685	685	318

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the party-group level are reported in parentheses. Dummy variables are scaled as 0 or 100. Control variables are listed under Table 1. Bandwidth: [a]20.0% [b]19.4%.

Primary Effect Conditional on Exact List Rank

Figure A7 shows the graphical analysis of examining the primary effect conditional on exact list rank, and Table A3 follows up with the regression evidence.

Figure A7 – Graphical Analysis of the Primary Effect Conditional on Exact List Rank



Notes: The graphs show binned averages of list rank in *t+1* as a function of the win/loss margin to being the preference vote primary winner. In the left graph, each bin contains 50 observations. In the right, each bin contains all observations within a 1 percentage-point interval for the win/loss margin. The lines in the graphs are third-order polynomials fitted to the observations.

The graphical evidence suggests a clear positive effect for those ranked first and second and an effect of potentially similar size for those ranked third. For lower ballot positions there no indication of a primary effect. The regression results, presented in Table A3 confirm the graphical analysis. The estimated effects for the three highest ballot positions are remarkably similar. However, the standard errors become much larger as we move down in rank due to the smaller sample sizes. Thus the estimated effect for those second ranked are only weakly significant, while those ranked third are insignificant. For those ranked lower there is no indication of there being a primary effect.

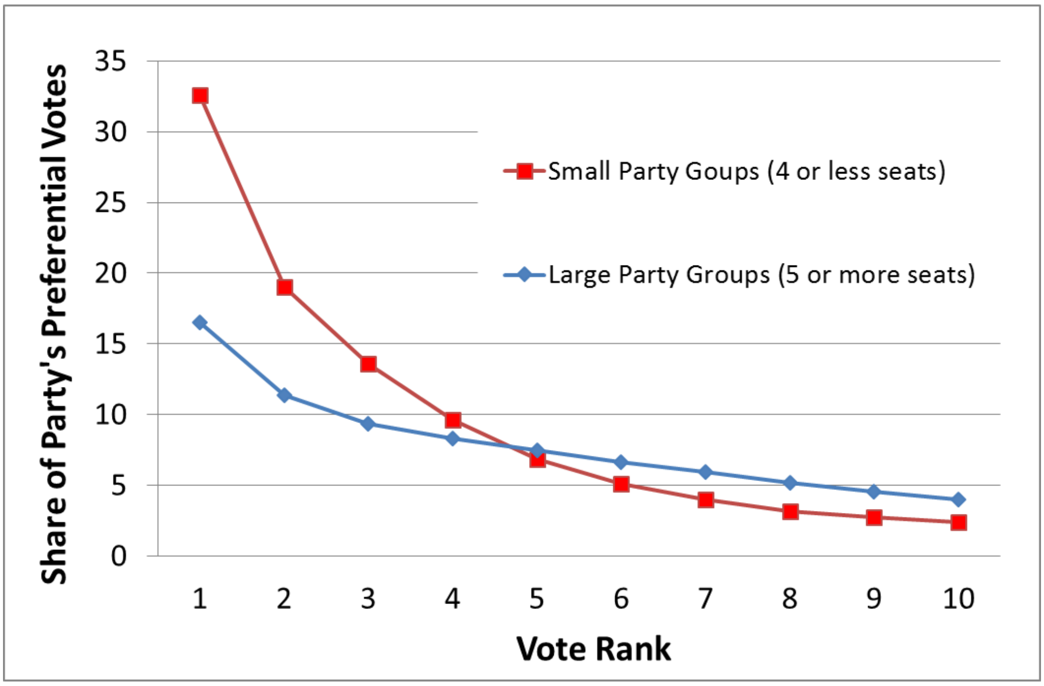
Table A3 – Estimates of the Relationship between Winning the Preference Vote Primary in Election *t*, and Becoming Party Leader in *t*+1, by Party List Rank

		Flexible Polynomials		Local Linear		Close Margin	
Party List Rank 1	OLS	2nd Pol	3rd Pol	I-K test	10% window	10 % window	5% Window
Primary Effect	15.97***	19.70***	18.10**	22.09*** [a]	23.95**	14.74***	22.66***
	(2.40)	(6.70)	(9.13)	(7.79)	(10.11)	(4.78)	(7.00)
Observations	2,194	2,194	2,194	715	426	426	191
Party List Rank 2							
Primary Effect	11.92***	17.17**	11.12	12.07 [b]	21.81*	10.35*	17.99**
	(2.62)	(7.61)	(10.33)	(7.88)	(11.65)	(5.48)	(8.30)
Observations	1,316	1,316	1,316	571	269	269	117
Party List Rank 3							
Primary Effect	6.42*	17.72	16.43	19.72 [c]	17.86	9.22	16.21
	(3.77)	(11.97)	(16.90)	(14.08)	(20.03)	(8.40)	(12.34)
Observations	513	513	513	156	98	98	49
Party List Rank 4							
Primary Effect	6.00	-16.08	1.07	-11.94 [d]	-11.76	-7.41	-8.33
	(6.18)	(10.02)	(13.89)	(13.81)	(12.76)	(5.16)	(8.43)
Observations	174	174	174	54	42	42	19
Party List Rank 5+							
Primary Effect	-0.69	-1.58	-10.28	-27.84 [e]	2.39	-13.33	-9.52
	(9.81)	(27.20)	(38.86)	(29.71)	(39.63)	(18.30)	(29.17)
Observations	97	97	97	39	25	25	13

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the party group level reported in parentheses. The table shows estimates the primary effect, by list placement at time *t*. Bandwidth: [a]16.6%; [b]19.7%. [c]15.9%; [d]14.6%. [e]21.6%

Additional Results for Brazil

Figure A8 – Preference Votes Obtained by Vote Rank in Brazil



Notes: The figure shows the preference votes obtained based on vote rank. The sample is divided based on party-group size.

Table A4 – Estimates of the Relationship between Winning the Preference-Vote Primary in Election *t* and Becoming a Mayoral Candidate in *t* + 1. Results for Brazil.

		Flexible Polynomials		Local Linear		Close Margin	
	OLS	2nd Pol	3rd Pol	I-K window	10% window	10 % window	5% window
Candidate Same Party							
Primary Effect	4.41***	2.12**	2.33*	2.37** [a]	2.58**	2.77***	3.13***
	(0.40)	(1.03)	(1.36)	(1.17)	(1.30)	(0.56)	(0.90)
Observations	10,878	10,878	10,878	5,984	4,694	4,694	2,042
Candidate Any Party							
Primary Effect	4.51***	1.52**	2.11**	1.21* [b]	1.65*	2.25***	2.11***
	(0.27)	(0.68)	(0.89)	(0.73)	(0.84)	(0.37)	(0.59)
Observations	24,354	24,354	24,354	14,220	10,584	10,584	4,542

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the party-group level are reported in parentheses. Dummy variables are scaled as 0 or 100. Control variables are listed under Table 1. Bandwidth: [a]12.4%; [b]13.2%;

The results in Table A4 show that the estimated primary effect on the probability of becoming a mayoral candidate for the politician's current party is about 2.5 percentage points. This result maintains its statistical significance across every specification. With a baseline probability just over 3 percentage points, the relative effect (about 80%) is about the same magnitude as in the Swedish case. The primary effect on becoming a candidate for any party is slightly smaller at about 1.5 percentage points, but still statistically significant in every specification.

References

McCrary, Justin 2008. Manipulation of the Running Variable in the Regression Discontinuity Design: a Density Test. *Journal of Econometrics*, 142, 698-714.

Snyder, James, Shigeo Hirano, and Olle Folke 2014. Partisan Imbalance in Regression Discontinuity Studies Based on Electoral Thresholds. *Political Science Research and Methods*, forthcoming.