What Happens When Women Win Elections? The Electoral Returns to Increased Representation of Women

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Abstract

How does the election of a woman shape the electoral performance of their party? Research suggests that electing women to political office shapes party behavior and improves voters' perception of the party. However, the downstream electoral consequences of electing women to office remain understudied. Drawing on administrative data and closely contested municipal elections in Denmark from 1997 to 2017, we find that marginally electing a woman over a man improves the electoral performance of the entire party in the succeeding election. We propose three mechanism that can produce these findings: the *competence*, *legitimacy*, and *party coherence mechanisms*. We find evidence partly consistent with the latter two mechanisms, but little to support the first one. This suggests that electing women changes the functioning of the party's elected delegation. These findings offer insights into the role of women in politics and prompt further inquiry into gender representation in elected offices.

How does the election of a woman shape the future electoral performance of their party? While women are largely discouraged from running for political office (Fox and Lawless, 2014; Kanthak and Krause, 2010; Lawless and Fox, 2010, 2005), we also know that when women actually are elected, they have profound effects: Women politicians change citizens' perceptions of their parties (Adams et al., 2023; O'Brien, 2019), and even improve certain public policy outcomes (Hessami and Fonseca, 2020). We also know that (at least in some dimensions) women are better representatives of their constituencies than men (Anzia and Berry, 2011). Even though it is well-established that there are profound effects of electing a woman, we know little about whether it has downstream electoral consequences for their party. In this paper, we posit that electing women to office improves the electoral outcomes of their parties.

To test this argument, we use rich administrative data linked to candidates in local elections in Denmark spanning from 1997-2017. The elections are proportional with most parties running on open-lists, where personal votes alone determine who would occupy the seats won by the party. This institutional feature makes Denmark a particularly suitable test bed for our study. First, it allows us to compare the future electoral performance of both the party and the woman candidate herself, thereby distinguishing between electoral returns at the party and the candidate level. Second, it generates numerous close elections, where a woman marginally beats a man or does not. We use this source of random variation in the share of elected women to causally identify how increasing a party's representation of women affects the party's future electoral performance.

We find that narrowly electing a woman over a man increases the share of women elected in the party and the personal votes cast for the party's women in the next election. Additionally, it increases the total votes cast for the party as well as the party's number of seats in the next election. Importantly, the effect is concentrated among parties with a under-representation of women relative to their ideological bloc. As we discuss in our concluding remarks, these results have important implications for the role of women in politics, and raise new questions about the under-representation of women in elected office.

The Election of a Woman and the Party's Electoral Performance

Women remain underrepresented across political assemblies even in countries where gender equality is comparatively high – Denmark being one example. This is puzzling as there are several reasons why parties can be expected to be rewarded for the election of a woman.

First, the election of women may influence voters' perception of the entire party. Women politicians tend to prioritize building coalitions and have a more collaborative legislative style (Barnes, 2016; Holman and Mahoney, 2018; Volden et al., 2013). In line with this, when parties are represented by more women, citizens tend to perceive the party as more moderate (O'Brien, 2019). Similarly, when a party, a voter does not support, is represented by more women, the voter tends to view the party with less hostility (Adams et al., 2023). The survey-based findings in Ben-Shitrit et al. (2022) show that women's representation in radical right parties leads to increased support for those parties. Adding to this, research also shows that when there is more gender equality among the decision-makers in an institution, survey-respondents tend to view them as more legitimate (Clayton et al., 2019).[1] Overall, the election of women may influence voters' perception of the party, as parties with a more equal representation of women are seen as more moderate, more collaborative, and therefore also more legitimate.

Second, if women candidates overall are more qualified and exercise more effort, this may cause an increase in the electoral performance in the following election. Here, we can measure the increase in personal votes for the woman as well as the votes for the entire party. There are several reasons why women candidates may be more qualified than men. If voters are biased against women candidates – or if potential candidates believe that they are – only the very best qualified women will choose to run for office (Anzia and Berry, 2011; Bauer, 2020). Once elected, women seem to work harder on a number of dimensions – for example by performing more constituent services (Lazarus and Steigerwalt, 2018) and securing more funding (Anzia and Berry, 2011).

[1] However, there is mixed evidence that women's representation increases political efficacy (Cowley, 2014; Lawless, 2004; Ulbig, 2007; West, 2017). This would cast doubt that efficacy could be a mechanism linking women's representation to party performance.

This gender gap in qualifications and effort may have downstream electoral consequences not just for the candidate herself but for the entire party. First, if women exert more effort on behalf of their constituents, they would see improved electoral returns themselves, leading to a gendered incumbency advantage. If this effect is strong enough, it would spill over onto the party, improving their collective performance in the upcoming election. Second, even if women do not see a larger incumbency advantage, their harder work may still lead to increased returns for the party as women politicians seek to foster a collaborative environment (Barnes, 2016; Holman and Mahoney, 2018; Lawless, 2015; Volden et al., 2013), and it has been shown that the presence of women in groups improves their collective functioning (Woolley et al., 2010), and that the work place environment is better if gender equality is higher (Rickne and Folke, 2023). Therefore, women incumbents may channel their efforts into improving their parties' abilities to work together, which could increase electoral returns in the form of votes cast for the party.

Overall, this leads us to expect the election of women to improve their party's performance in the next election.

Context, Data, and Identification Strategy

Denmark is a particularly good context to study the down-stream effect of the election of a woman. The latter implies that voters can easily locale women on the ballot. This is due to the open-list system and the way the ballot is designed, where voters can choose between voting for the candidate personally and their party. This context will both allow us to identify the causal effect of electing women, and to distinguish between effects on the candidate's own electoral performance, and that of her party. Local elections in Denmark follow a simple proportional representation (PR) system between parties. Voters can vote either for a party vote or cast a personal vote for a specific candidate, which also counts as a vote for the candidate's party. Council seats are allocated *between parties* by the total number of votes for the party and its candidates using D'Hondt's method. The allocation of seats *within the party* depends on whether the party deploys an open or semi-open list. Parties with open lists allocate seats exclusively based on the candidates' personal votes. Thus, if an open-list party wins n seats, the candidates with the n^{th} most personal votes win the marginal

seat. In contrast, parties with semi-open lists prioritize their candidates and allocate party votes to candidates starting from the top. Personal votes are extremely important, and account for between 70 and 80% of the votes cast in the elections we observe (Nørtoft, 2019). While the candidates at the top of the lists generally receive most votes (e.g. in Copenhagen, they received between 10 and 20% of their parties' votes), the median marginal candidates received 143 estimated personal votes in our data. This system operates in all 98 municipalities, each governed by a municipal council consisting of between nine and 31 members. The only exception is Copenhagen, which has 55 members. Elections are held every fourth year on fixed terms. The elections are generally high stakes, as municipal spending accounts for 35% of the Danish GDP (Egerod and Larsen, 2021). Therefore, the elections we study are high turnout (between 65 and 72%), and voters are quite well-informed (Pedersen et al., 2022). Curiously, women are not very well represented in municipal councils, and only make up a little more than 30% of the elected. In the Danish parliament, women made up 37-39% during the same years (Kjaer and Kosiara-Pedersen, 2019).

Data

Our data covers all candidates running in the Danish municipal elections from 1997 to 2017. The election data contains variables for each candidate including their municipality, party list, list structure, electoral success, and a metric expressing their win or lose margin. All Danes have a civil registration number, which allows us to uniquely link election data to administrative data for all permanent residents in Denmark born after December 13, 1912, including variables like gender, age, occupation, education, income, and country of origin (Thygesen et al., 2011). Combined, these two sources provide us with rich data for the totality of local political candidates over six election terms in a 20-year period.

Identification strategy

Following previous studies (Folke, 2014) we use close elections between the marginal candidates to identify the effects of increased women representation. We consider elections where two candidates of different gender from the same party compete for the same seat. Here, a woman might marginally beat a man or not. We use this as-if random treatment to study the effect of increased

women representation. For our main results, we only include parties with open-lists, because personal votes alone assign their marginal seats, which provides the best grounds for causal identification. In Appendix E.1, we run our main specifications on the subsets of parties using semi-open lists and obtain similar results to those of open-lists, alleviating concerns related to external validity.

Due to privacy concerns, administrative data cannot be linked to personal vote counts. We therefore rely on a regression discontinuity design, assuming local randomization (Cattaneo et al., 2015). However, we can obtain a measure of how close each election is within each municipality, party and election year. Our measure captures which percentile of election closeness each candidate was a part of. A candidate with a closeness of 5% ran in one of the five percent closest elections. We subset to extremely close elections and then estimate the causal effect of electing women using difference-in-means via OLS:

$$Y_{p,m,t+1} = \delta WomanWinner_{p,m,t=0} + \varepsilon_{p,m,t+1}$$

$WomenWinner_{p,m,t=0}$ is a binary indicator equaling 1, if a woman wins the marginal seat over a man on party list, p, in the municipality, m, and in election t=0. $Y_{p,m,t+1}$ is the outcome of interest in the subsequent election. The parameter of interest is δ, which captures how electing a woman affects downstream characteristics for the party. We investigate five different outcomes for the party in the subsequent election: 1) share of women among everyone elected for the party, 2) total number of votes for women candidates, 3) total number of votes for men candidates, 4) total number of votes for the party, and 5) total number of seats won in the local council. To limit outliers from driving our findings, we log outcome 2-4, i.e., the variables with vote totals.[2]

To focus on close elections, where δ is causally identified, we select the b% closest elections for each election year. We choose this bandwidth using the procedure proposed by Cattaneo et al.

[2]In 2005, a municipal amalgamation changed the number of municipalities from 271 to 98. For the 2001 election, we only include parties from the 32 unchanged municipalities, since their outcomes (in 2005), occur for the same electorate. For 2001, we exclude parties in amalgamated municipalities, since their treatment occurs in a different electorate than their outcomes.

(2016), which is designed for regression discontinuity designs relying on a local randomization assumption. We select the bandwidth where the lowest p value of any covariate is below 0.15. We add the additional criterion that the bandwidth should also allow for well-powered tests. This leads us to using the 18% bandwidth. Appendix B shows the balance tests used for selecting the bandwidth. Appendix H presents the power analysis.

Our identification strategy requires our data set to have a large number of close elections. For the 18% closest elections, we have 819 party-election observations. Importantly, elections are extremely close at this bandwidth. Using vote ranges provided by Statistics Denmark to ensure anonymity, we can estimate personal votes by extracting the mid-point between the two limits. On average, we estimate that only 47 votes separate winners and losers. The median is only 34. This corresponds to, respectively, 1.97% and 1.42% of the average list vote. Thus, we consider extremely close elections. In Appendix C, we also show that our treatment is balanced across covariates. We present more information on the role of personal votes in Appendix S.

Results

In Table 1 we examine whether increasing a political party's representation of women improves the electoral performance of the entire political party in the subsequent election. In Column 1, we show that electing an additional woman increases a party's share of women elected by 10.7 percentage points in the next election. This is a considerable effect when compared to the baseline (constant) of 0.238. In Column 2, we find that electing an additional woman increases the total number of votes for women on the party list in the next election by 42.8%. In Column 3, we find that electing an additional woman neither decreases nor increases the number of votes received by men on the party list in the next election. In Column 4, we show that electing an additional woman causes a large and significant increase in the party's number of votes by around 13.4%. Last, in Column 5, we show that the additional election of a woman causes the party to reap 0.31 additional seats in the local council. Importantly, the increase in votes for women is not caused by a redistribution of votes within the party from men to women. Rather, the party as a whole benefits.

Table 1: The Effect of Electing Women on Future Electoral Outcomes (t+1)

	Dependent variable:				
	Share Women Elected	Log Votes Women	Log Votes Men	Log Votes Party	Seats Party
	(1)	(2)	(3)	(4)	(5)
Woman win t=0	0.107***	0.428***	0.031	0.134**	0.311**
	(0.034)	(0.076)	(0.073)	(0.058)	(0.145)
Constant	0.238***	5.793***	7.323***	7.490***	4.562***
	(0.019)	(0.250)	(0.264)	(0.210)	(1.022)
Bandwidth Selector	Optimal	Optimal	Optimal	Optimal	Optimal
Bandwidth	18	18	18	18	18
Observations	819	819	819	819	819

Note: The estimated effects of electing a woman on parties' electoral outcomes. The bandwidth of 18 is the widest, where treatment and control group remain balanced covariates. This is defined by the smallest p value on any covariate being among 0.15. An observation is a municipality-party-year. Robust standard errors are clustered by municipality and party in parentheses. *, **, and *** indicate $p < 0.1$, < 0.05 and < 0.01, respectively.

A series of tests in the appendix confirm the robustness of these results. In Appendix G, we show estimates for all bandwidths. The results are reassuringly robust for all but the most narrow bandwidths where there are very few observations. In Appendix H, we show that the null finding in the narrow bandwidths can be explained by low power. A second concern is that the elected woman brings multiple skills with her. In Appendix G we run a number of placebo tests that suggests that our results are not driven by these alternative factors. There are also concerns related to external validity. In Appendix E.2, we show that our estimates are local to bigger parties with national representation among other characteristics. Additionally, we show in Appendix L that the effects are present in smaller municipalities, and in Appendix R, we show that the results are robust to leaving any municipality and party out. Fourth, the results could be driven by other women choosing to run, when a woman is elected. In Appendix J, we show that is not the case. In Appendix M, we show that we can obtain very similar, but more noisy, estimates using vote and seat shares instead of counts. In Appendix D, we use the close election as an instrument for the share of women serving the party. In Appendix T, we show that women and men have approximately equal probabilities of winning in the narrow bandwidths. Finally, one could worry that the effects are driven by other incumbents staying. In Appendix I, we show that is not the case.

Most importantly, in Appendix Q we replicate our main results using a different close election design. We use the fact that while we cannot obtain actual personal vote counts, we can obtain estimates of them. We can use this to calculate the vote differences between marginal winners and losers. Dong and Kolesár (2023) shows that using running variables measured with error in this way still produces valid results.

Discussion and Conclusion

While the findings we have presented are important in their own right, it is worthwhile discussing which mechanisms might drive them. Broadly speaking, the results could be produced by changes at the level of the individual candidate or the party. Additionally, they can come about, because voters view parties with equal gender representation as more legitimate, or because women candidates improve their parties' performance. We will discuss each of these mechanisms in turn.

First, if parties perform better after the election of a woman, because women exert more effort to serve their constituency it should manifest itself in women incumbents seeing higher future election returns than men. We call this the *competence mechanism*. In Appendix I, we find limited evidence to back this hypothesis. This provides important evidence on the mechanism because it shows two things. *1)* if the effect is brought about by women exerting more effort, it is a type of effort that does not also improve the woman candidates' own electoral performance. *2)* The observed effect is driven by women bringing about party-level changes. That is, we are looking for a party-level mechanism brought about by the election of an individual.[3]

Second, we take steps toward investigating which party-level changes the elected woman brings about. Broadly speaking, she can improve her party's internal functioning – *the party coherence mechanism* – or voters can improve their perception of the party even without the party changing its behavior – *the legitimacy mechanism*. In Appendix K, we provide evidence that is partly consistent with both these mechanisms. We show that the effect is concentrated among parties with an under-representation of women relative to their ideological bloc. This suggests that

[3]This provides additional evidence that our results are not driven by the other characteristics about the elected woman.

voters reward parties that "catch up" on the representation of women compared to competing parties. This story is consistent with the existing literature showing that citizens perceive institutions with gender-equal representation as more legitimate (Clayton et al., 2019). However, if the effect were purely driven by voter perceptions of candidates – and not the candidates' behavior – we would expect the effects to be strongest where women's under-representation was most visible. This would be when there are fewer women in the city council, and when the city council as well as the party's delegation are small. In Appendix F we find that these variables do *not* consistently moderate the effect across our outcomes. On the other hand, in Appendix S, we find evidence supporting the legitimacy mechanism. We show that the effect on votes for men and votes and seats for the party are driven by close elections, where the candidates received a very large number of personal votes. These are elections among particularly well-known candidates, which the voters will be more aware of. Hence, there is both evidence against and in favor of the legitimacy mechanism. Importantly, voters might not be aware of the gender distribution in the party as such. However, the information about a party's leading candidates is likely to be much more widespread. Hence, the best test of the legitimacy mechanism in this context would be to examine data on list placement. Unfortunately, we were not allowed to incorporate that due to privacy concerns. In Appendix P we present suggestive (but statistically insignificant) evidence that electing a woman might increase the presence of women in the list's top placement. However, future research would do well to examine this notion in detail.

The evidence in Appendix K is also consistent with the party coherence mechanism. If a political party has a lack of women representation, electing a woman may help to enhance the party's functionality. Studies from other Scandinavian countries indicate that having more women in the workplace can create a better working environment for both men and women (Rickne and Folke, 2023). This could suggest that women improve their colleagues' abilities to collaborate, which has been found to be the case in the US (Barnes, 2016). To test this mechanism fully, future research should examine whether parties with more women more often obtain their programmatic goals, they rate their work environment better, and whether they (in the Danish context) are better

able to service their constituents. Due to privacy concerns, we are not permitted to merge the information necessary to conduct these analyses into our dataset.

In sum, electing women improves a party's electoral performance. Our mechanistic evidence suggests that this happens, because women bring about changes at the party-level, and not because the elected woman herself out-performs other incumbents electorally. More research is needed to establish definitively whether this happens, because parties with more women are governed better, or because voters prefer parties with equitable gender representation.

Our findings raise the question of why parties do not run more women, when this improves their electoral performance. It is interesting to note that we estimate positive spillover effects: In Appendix N, we find that when one party elects a women, its competitor parties increase the proportion of women among its candidates in future elections. However, the effect is weak, only a couple of percentage points. The increased electoral performance of parties is only found among parties with less than 30% women among their candidates (as described in Appendix K). As the electoral rewards disappear for parties with more representation of women, this cannot incentivize parties to increase their representation of women beyond one-in-three. This suggests that parties may realize that there are positive returns to running women candidates, but that the competition only works at low levels of women representation, see also (Gilardi, 2015). Future research should examine whether this could be part of the explanation for the under-representation of women in elected office.

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Online Appendix for: What Happens When Women Win Elections?

Contents

A Measuring competence of political candidates

In a number of our robustness checks, we use a measure of candidate competence to ensure that our results are not driven by that.

It is not straight forward to define and measure politicians' competencies or abilities. Traditionally studies proxied competence by education (Besley and Reynal-Querol, 2011; Ferraz and Finan, 2009; Fisman et al., 2015), which however confounds competence and ability (Carnes and Lupu, 2023; Dal Bó et al., 2017).

Recent studies have instead suggested to use *mincer earnings regressions* to estimate income residual from an elaborate regression on income (Besley et al., 2017; Dal Bó et al., 2017; Gulzar, 2021; Meriläinen, 2022). If the labor market value abilities not accounted for by the variables in the regression, we would expect people who are high in unobservable ability to have positive residuals, since they would earn more than what we predict based on observable characteristics. Hence, we are interested in the candidates' income residual – how they deviate from their predicted income.

We follow this approach and run regressions on total earnings on a set of predictors in each year we have data. Specifically, we run the following:

$$Y_{i,t} = f(age_{i,t}, education_{i,t}, sector_{i,t}) + \alpha_{j,t} + \varepsilon_{i,t}$$

where $Y_{i,t}$ is income in year, t, for person, i. We model age by a set of indicator variables for five-year age groups. Education is measured in four main groups: primary, secondary, tertiary, or vocational education. For employment sector, we use a set of 19 sector categories from 2000 and later.[1] To maximize model flexibility, we include indicators for missing values on each of the variables and fully saturate the model with interactions between age group, education, and sector. To account for regional variability, we also include fixed effects, $\alpha_{j,t}$, for municipalities.[2]

[1] Due to a change in how Statistics Denmark track employment sector, we do not include sector in the models before 2000.

[2] Before 2007 we include fixed effects for all of the 271 municipalities. From 2007 and onwards we have fixed effects for the 98 municipalities after the merger.

Finally, we take into account that women, people with different ages, and non-native workers may experience different labor market outcomes. We therefore estimate the model individually for each of the sixteen configurations of two binary variables, gender and being a native Dane[3], and four age groups: 18-32 years, 33-47 years, 47-61 years, and 62 years or older.

To diminish the impact of yearly fluctuations in income between years, we use the average income in the four years prior to the election. The remaining residual, $\varepsilon_{i,t}$ is what we label as 'competence' throughout the paper. This measure will express whether a person's income is higher or lower than we would expect based on the income regression.

[3]We follow Statistics Denmark's official definition of native Danish as someone who has at least one parent who is both a Danish citizen and born in Denmark.

B Bandwidth selection using covariate balance

We follow the procedure for selecting bandwidths under a local randomization assumption proposed by Cattaneo et al. (2015, 2016). In particular, we subset the data to include only the closest elections (the 1% closest). Within that subset, we then regress our treatment variable (a woman beating a man for the marginal seat) on a number of covariates. We then retrieve the lowest p value on any covariate. We repeat this process for all remaining bandwidths between 2% and 50%. We use the following covariates of parties' candidate pools (all lagged by one election year); average age in the party, whether the election takes place in the biggest five cities, the amount of female candidates in the party, proportion with college degree, whether the party is in parliament, whether the party is part of the national left-wing grouping, the party's vote share in the municipality, proportion unemployed, proportion of immigrants, and average competence ("earning ability") following Besley et al. (2017) and as we outline in Section A. The standard errors are clustered at the party-municipality level, as in the main results.

Figure **??** shows the smallest p value of any covariate within each bandwidth. Cattaneo et al. (2016) recommend using the last bandwidth where the lowest p value is above 0.15. As we can see, the p value on the proportion of college graduates is below 0.15 in the second bandwidth. However, as we show in Appendix H, using this bandwidth would produce tests that are highly unlikely to be well-powered. Therefore, we use the bandwidth before the p value is below 0.15 and that is expected to produce a well-powered estimate (as per Appendix H). This leads us to use the 18% closest elections for our main results.

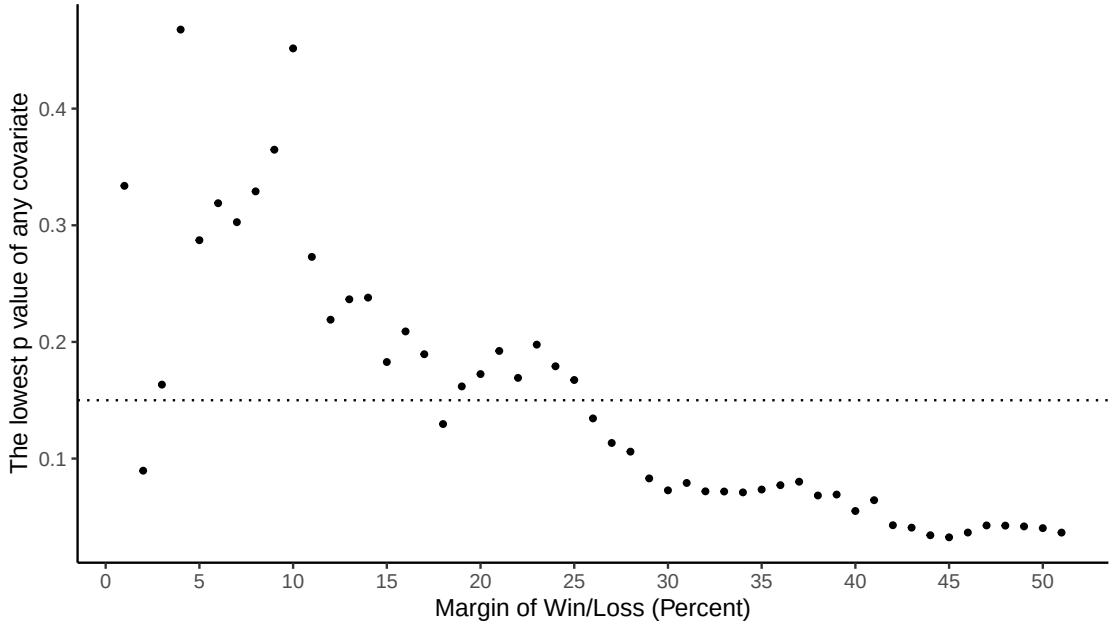

Figure B.1: Permutation test of covariate imbalance using Wald F Statistic. *The graph shows the minimum p value from a regression with the covariates described in the text above. The horizontal line shows the threshold of p = 0.15, which Cattaneo et al. (2016) recommends to use as the value for selecting the bandwidth.*

C Balance on pre-treatment covariates

To test if our treatment and control conditions are balanced on observables, Figure C.1 shows the estimated coefficients and confidence intervals that we estimated in the models in B. That implies that we use all the covariates from the previous appendix. We do not use the party's average income in this regression, because the competence measure is a function of it. However, we have separately estimated the balance of our treatment in respect to income, and it is not statistically significant across the bandwidths we examine. The standard errors are clustered at the party-municipality level. None of the differences are significantly different ($p > 0.05$), at our chosen bandwidth, which suggest that the parties in our sample are similar prior to the treatment assignment. In turn, this is strong evidence in favor of our design.

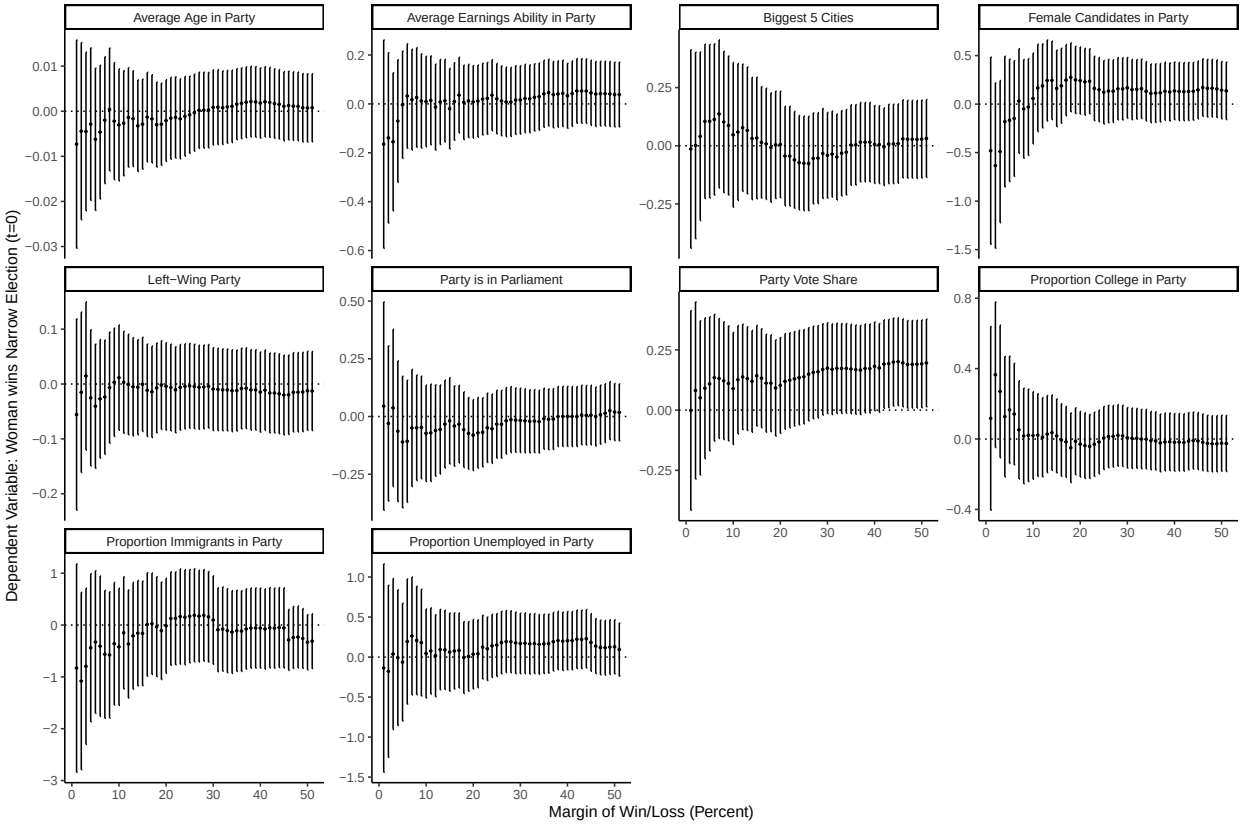

Figure C.1: Estimated difference in covariates between treated and untreated parties. *Note:* Balance on pre-treatment covariates across increasing bandwidths. 95% confidence intervals from robust standard errors are clustered by municipality and party.

D Effects Among Complier Parties

In Table D.1, we test whether our treatment works as expected. We do so by estimating how electing an additional woman in election $t = 0$ affects the party's share of women elected in the council that is seated immediately after. In other words, this analysis examines whether electing a women actually improves the party's gender representation – a prerequisite for voters changing their perception of the party. The estimated effect of 0.364 is statistically significant and indicates that narrowly electing an additional woman increases the proportion of women representing the party in the local council by about 33 percentage points. This is over a baseline of 15.7% (the constant), which is the share of women elected for parties where the marginal seat is won by a man over a woman. Hence, the marginal election of a woman over a man has a substantial effect on parties' representation of women. This strongly indicates that our treatment is valid and captures what we expected and intended it to capture.

Table D.1: Testing the validity of the treatment. The effect of electing an additional woman on share of elected women

	Dependent variable:
	Share Women Elected t=0
Woman Candidate Win t=0	0.338^{***}
	(0.083)
Constant	0.163^{***}
	(0.028)
Bandwidth	18
Observations	819

Note: The estimated effects of electing a woman on parties' share of women among their elected. Robust standard errors are clustered by municipality and party in parentheses. *, **, and *** indicate p $<$ 0.1, $<$ 0.05 and $<$ 0.01, respectively.

This strong effect on the share of women also allows us to examine the local average treatment effect (LATE) among complier parties. This is an interesting estimand, because all parties may not seat the woman they elect, or they may simultaneously increase their share of elected men. If we can obtain unbiased estimates of the LATE, we would be able to examine the effect among parties that only see their share of elected women increase, when they narrowly elect a woman. Of course, we need to impose the excludability assumption – that the effect of narrowly electing a woman is strictly mediated by the share of women representing the party in the municipal council. We worry that this will be violated in this setting, because there may be an independent effect of electing women, if that shapes voter perceptions. Still, it may be interesting to examine the instrumental variable estimates, which we present in Table D.2. The estimated effect of the share of women – among parties that increase this share when they narrowly elect a woman – is very large across all dependent variables This could suggest that the exclusion restriction is violated.

Table D.2: The LATE of electing women on future party performance

	Dependent variable:				
	Share Women	Votes Women	Votes Men	Votes Party	Seats Party
	(1)	(2)	(3)	(4)	(5)
(IV'd) Share Women	0.687***	12.399***	14.663***	15.196***	9.712***
	(0.064)	(1.764)	(2.242)	(2.143)	(3.013)
Bandwidth	18	18	18	18	18
Observations	819	819	819	819	819

Note: The estimated local average treatment effect of electing a woman on parties' future electoral performance. Robust standard errors are clustered by municipality and party in parentheses. *, **, and *** indicate $p < 0.1$, < 0.05 and < 0.01, respectively.

E External Validity

E.1 Results in Semi-Open Party Lists

Most parties in our sample's time period use open-lists (72%), yet the effects of electing women on open-lists might not generalize to semi-open lists, if the two types of parties attract different types of voters. Table E.1 shows the estimated effects of our treatment, a woman marginally beating a man, but for parties with semi-open lists. We keeping using the bandwidth of 18 and cluster standard errors at party-municipality level. Recall that since personal votes alone do not decide the marginal seats in the party, identification is more ambiguous in this specification. However, overall, these results are quite similar to our well-identified main specification for open-list parties. This alleviates concerns related to external validity.

Table E.1: The Effect of Electing Women in Parties with Semi-Closed Lists

	Dependent variable:				
	Share Women Elected	Votes Women	Votes Men	Votes Party	Seats Party
	(1)	(2)	(3)	(4)	(5)
Woman win t=0	0.081**	0.123	0.271	0.188*	1.097***
	(0.032)	(0.121)	(0.171)	(0.101)	(0.389)
Constant	0.256***	5.584***	6.589***	6.947***	3.366***
	(0.019)	(0.234)	(0.364)	(0.247)	(0.985)
Bandwidth	18	18	18	18	18
Observations	711	711	711	711	711
Note:					*p<0.1; **p<0.05; ***p<0.01

E.2 Are Parties with Close Elections Different?

In Table E.2, we show the mean differences between party-municipality-election year observations with narrow elections (defined by the 18% bandwidth) and the rest.

Table E.2: Comparing Parties With and Without Close Elections

	Close Elections (N=1090)		No Close Elections (N=780)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. E
Total Votes for Party	3513.2	5390.6	577.0	787.5	-2936.2	1
N Elected for Party	4.6	3.5	1.3	0.8	-3.2	
Represented in Parliament	0.9	0.3	0.6	0.5	-0.3	
Left Party	0.4	0.5	0.3	0.5	-0.1	
Big City	0.1	0.2	0.1	0.3	0.0	
Proportion Women	0.3	0.3	0.3	0.4	0.0	
Estimated Personal Votes	193.4	170.0	202.2	729.1	8.9	
Median Income	356885.1	109022.7	306522.3	128818.7	-50362.8	56
Average Age	48.7	4.6	48.8	7.6	0.1	

The results show that the parties with close elections are a) significantly larger (in terms of seats and total votes), b) more likely to have representation in parliament, and c) their candidates have higher incomes. On the other hand, they do not have a) a higher proportion of women candidates, b) more personal votes, c) older candidates, d) or take place in bigger cities.

This suggests that the results mostly describe larger national parties that are more strongly institutionalized in Danish politics across the country. On one hand, this implies that our results are of more international interest, as these parties – all else being equal – are more similar to parties in other contexts than purely local Danish parties. Additionally, it implies that our results are not driven purely by parties that already have a strong preference for running and electing women. On the other hand, it places us in a worse position to speak to purely local politics. While this might seem of less general interest, it constitutes an important aspect of local politics in advanced democracies.

F Are Effects Moderated by the Information Environment?

If the voter perceptions hypothesis (the legitimacy mechanism) is correct, and electing improves the party's future performance *independent of the elected woman's behavior*, then the information environment should moderate the effect. In particular, the effects should be larger when the elected woman is more visible, and the party's voters more strongly prioritize gender equality (i.e. among left-wing parties). Figure F.1 reveals the estimated interactions between the treatment, i.e., marginally electing a woman, and five different moderators: 1) the proportion of women in the city council as such (not just in the party), 2) the proportion of incumbents running for the party, 3) the number of seats held by the party at the time when the new woman is elected, 4) whether the party is part of the traditional left-wing or right-wing, and 5) the size of the city council. The first four moderators capture how visible the newly elected woman is likely to be for voters. The fifth moderator captures parties whose voters prioritize gender equality more highly.

We do not find any strong patterns of moderation across these variables and particularly there is limited evidence of partisan patterns–it seems that voters of left-wing and right-wing parties react similarly to the election of an additional woman. We find the strongest evidence of moderation when examining the proportion of the party's candidates that are incumbents. Notably, we uncover evidence that the effect of electing women is stronger when the proportion of incumbents is higher, which runs counter to the expectation, because the newly elected woman is likely to be less visible, when there are many incumbents in the party's delegation.

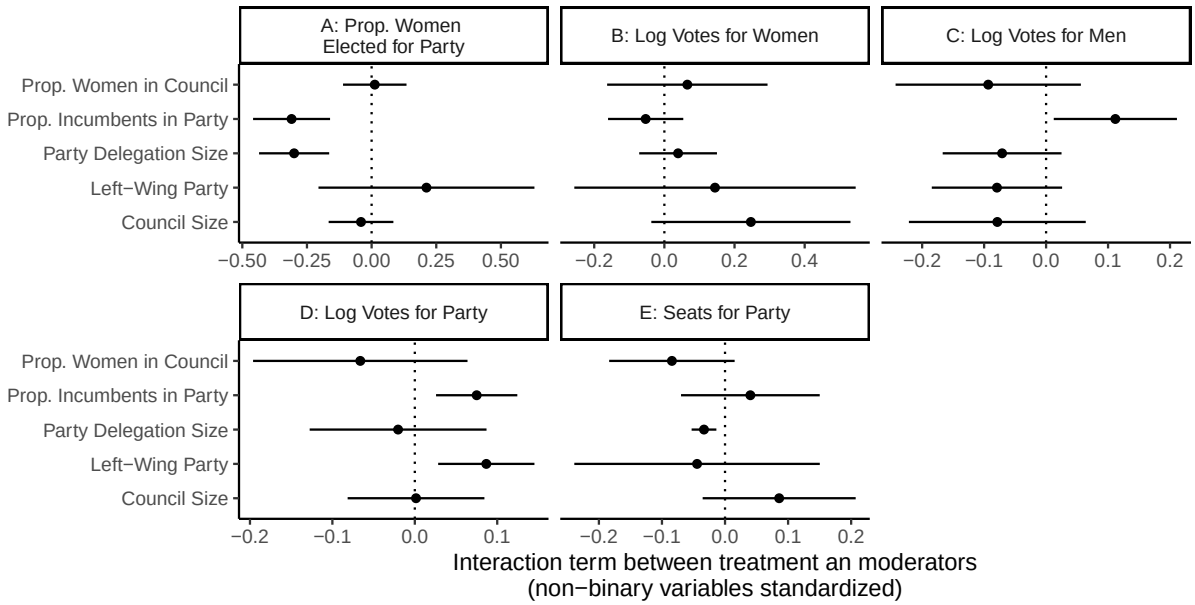

Figure F.1: Estimated interaction terms between treatment (marginally electing a woman to the party) and five different moderators for the five different outcomes *Note:* 95% confidence intervals. We use the bandwidth of 18 from the main specifications and cluster the standard errors at the municipal-party level.

G Robustness of baseline estimates

In the following, we deal with two threats to the robustness of our design. First, while we limit our researcher discretion by using an algorithm to choose our bandwidths, our results might be vulnerable to the choice of bandwidths. In Figure G.2, we guard against this by plotting estimates corresponding to our results in Table 1 for all bandwidths from the 1% closest elections to the 50% closest elections. In the baseline results, we use the bandwidth of 18 to estimate our effects in the main specifications. From Figure G.2, we can see that: i) for every outcome, the average optimal bandwidth resembles the outcome's optimal bandwidth, and ii) the estimates are stable around these bandwidths. Hence, our results are robust to the choice of bandwidth. In the most narrow bandwidths, the estimates lose statistical significance and are more volatile, which is logical given the few observations we rely on in those models. To better understand this pattern, in Appendix H, we simulate the power of our tests to detect our baseline effect.

Second, if the men and women who compose the marginal candidates differ on personal characteristics other than gender, the treatment is bundled, and if these other characteristics are what causes the party to perform better, the excludability assumption is violated (Gerber and Green, 2012; Marshall, 2022). A concern could be that the marginal woman candidate is more competent than the marginal man as found in previous research (Anzia and Berry, 2011; Besley et al., 2017). In our sample, compared to men candidates, the average woman candidate is a) almost 7 percentage points more likely to be college educated, b) 1.7 years younger, c) has an annual income of more than $8,400 lower. Further, like previous studies, we find that the marginal woman candidate to be more competent than the marginal man candidate, when we measure competence as candidates' pre-office earnings *compared to the earnings we would expect them to earn given their background characteristics*, which is prevalent in studies of politicians' abilities (Besley et al., 2017; Dal Bó

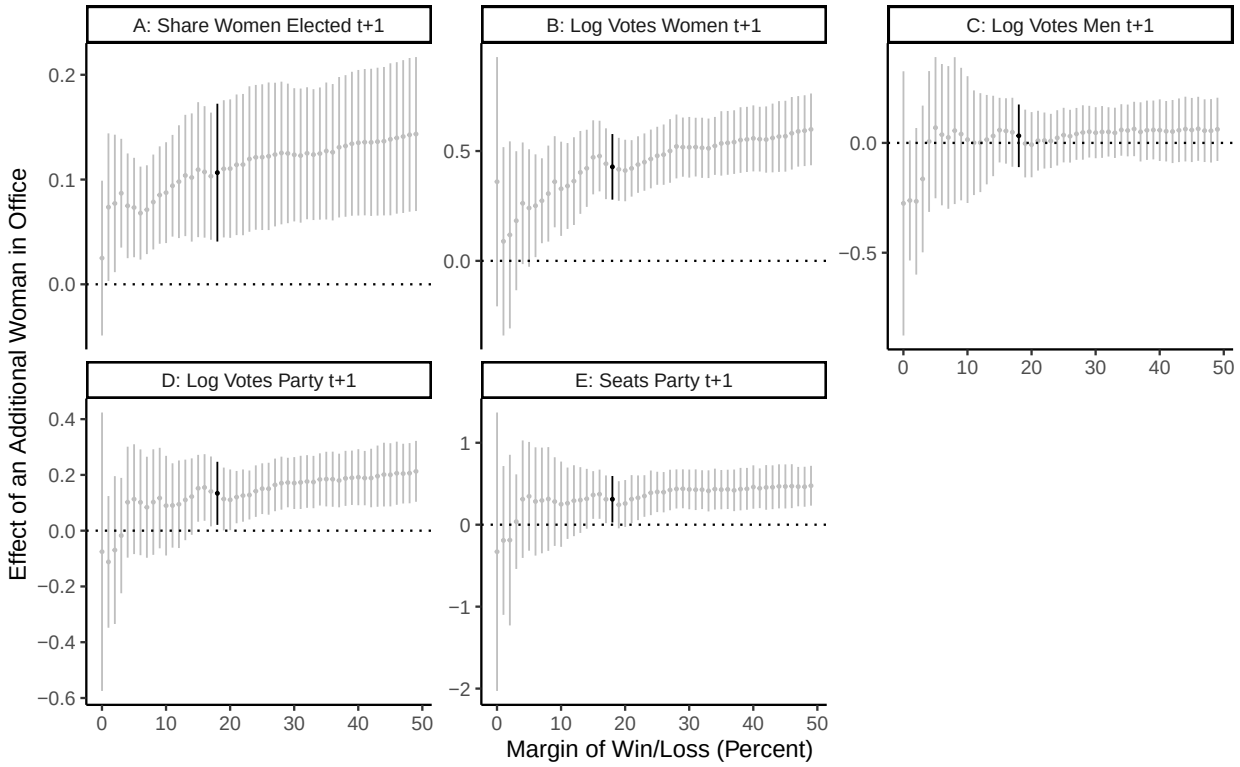

Figure G.2: Estimates of five outcomes over increasing bandwidths. *Note:* Selected bandwidth of 18 used for the main specifications are shown in black. Confidence intervals are 95% from robust standard errors clustered on municipality and party.

et al., 2017; Gulzar, 2021; Meriläinen, 2022).[1] In Appendix A, we elaborate on the measure of competence and describe the steps made to create the measure.

Our concern is that some of these characteristics, education, earnings, age and competence, compose an additional shock to the candidate pool beyond the change in the gender composition. To guard against this, we conduct placebo tests based on the control group; i.e., the party lists where men candidates won the marginal seat over a woman. The logic is to examine a group with untreated potential outcomes, as our placebos otherwise might pick up effects of electing women. We then create binary indicators of whether the marginal winner was a) earning above the median

[1]Of course, it would be naive to expect that measuring competence as pre-office earnings given background characteristics captured all relevant facets of politicians' quality. However, Besley et al. (2017) find that among Swedish political candidates the measure correlates strongly with other measures of competence like with political success, cognitive ability and leadership tests.

candidate annual income, b) above the median candidate age, c) above the median competence, and d) a college graduate.

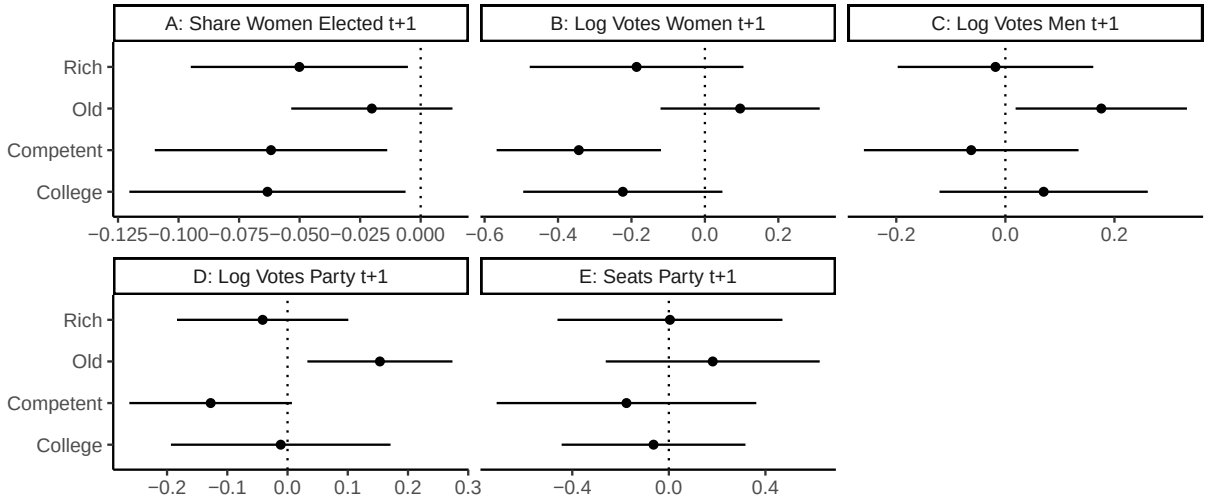

Figure G.3: Placebo estimates for the five electoral outcomes. *Note:* Placebo estimates of the effect of marginally electing: (1) above median earning men candidate, (2) above median aged, (3) above the median competence, and (4) a college graduate at the bandwidth of 18 used in the main specifications. 95% confidence intervals from robust standard errors are clustered by municipality and party.

In Figure G.3, we use each of these placebos on our five main outcomes from Table 1. Panel C and Panel E show that electing richer, older, more competent or more college-educated men candidates into office does not affect the prospective votes for men nor seats held by the party. For Panel A, B, and D, which concern share of women elected, votes for women and seats for the party, the placebo estimates tend to be insignificant and somewhat negative. This suggests that some of the additional elements carried by our treatment might have a negative effect on our outcomes. The estimates for all outcomes in Table 1 are positive and tend to be significant, and therefore, if anything, the other characteristics of our treatment seem to lower our estimates and thus make them more conservative.

H Power Across Bandwidths

To better understand why the estimates change when restricting our data to include only the closest elections, we simulate the power of our statistical tests. To do this, we follow Black et al. (2022) and simulate interventions on the data we use to conduct our analyses. This ensures that our power simulations are done in settings that include all the noise, messiness, non-normality, autocorrelation and heteroskedasticity inherent in our baseline analyses, and which we would never be able to capture in fully simulated data. Specifically, we iteratively subset our data to include data within variying bandwidths. We then simulate 1,000 random interventions, which ensures that they are uncorrelated in expectation with our actual treatment. We examine all five of our dependent variables, and, on average, the intervention has the same effect on the dependent variables as we estimate in Table 1. That is, the simulated intervention on average increases the proportion of women elected by 0.114, percent votes for women by 43.8, percent votes for men by 0.9, percent votes for the party by 12.6, and seats for the party 0.328. We follow exactly our model specifications in our main results – that is, we estimate linear regressions with a binary treatment variable capturing whether a woman is elected over a man. We cluster standard errors on municipality and party. This will practically ensure that the tests conducted within the baseline bandwidth are well-powered, as they have already detected the effects. However, it will help us understand the lack of robustness in the small bandwidths.

Figure H.1 shows the results up to when they become well-powered. The sample size of each analysis is presented in the top of the graph, and we color the lines to distinguish between the results for each dependent variable. As we can see, the tests seeking to detect the effect on the proportion of women elected very quickly approach high levels of power. This is less surprising, as these estimates were very robust across bandwidths. However, the tests detecting effects on the other dependent variables are very slow to gain power. We have power to detect the baseline effect on votes for women at a bandwidth of 12. For votes and seats for the party, respectively, we are powered at a bandwidth of 14. This is after the observe curvature in the estimates in Figure G.2. The test we use to detect an effect on votes for men never reaches any meaningful level of power,

which is unsurprising given the very small effect of the intervention we are simulated (less than 1%).

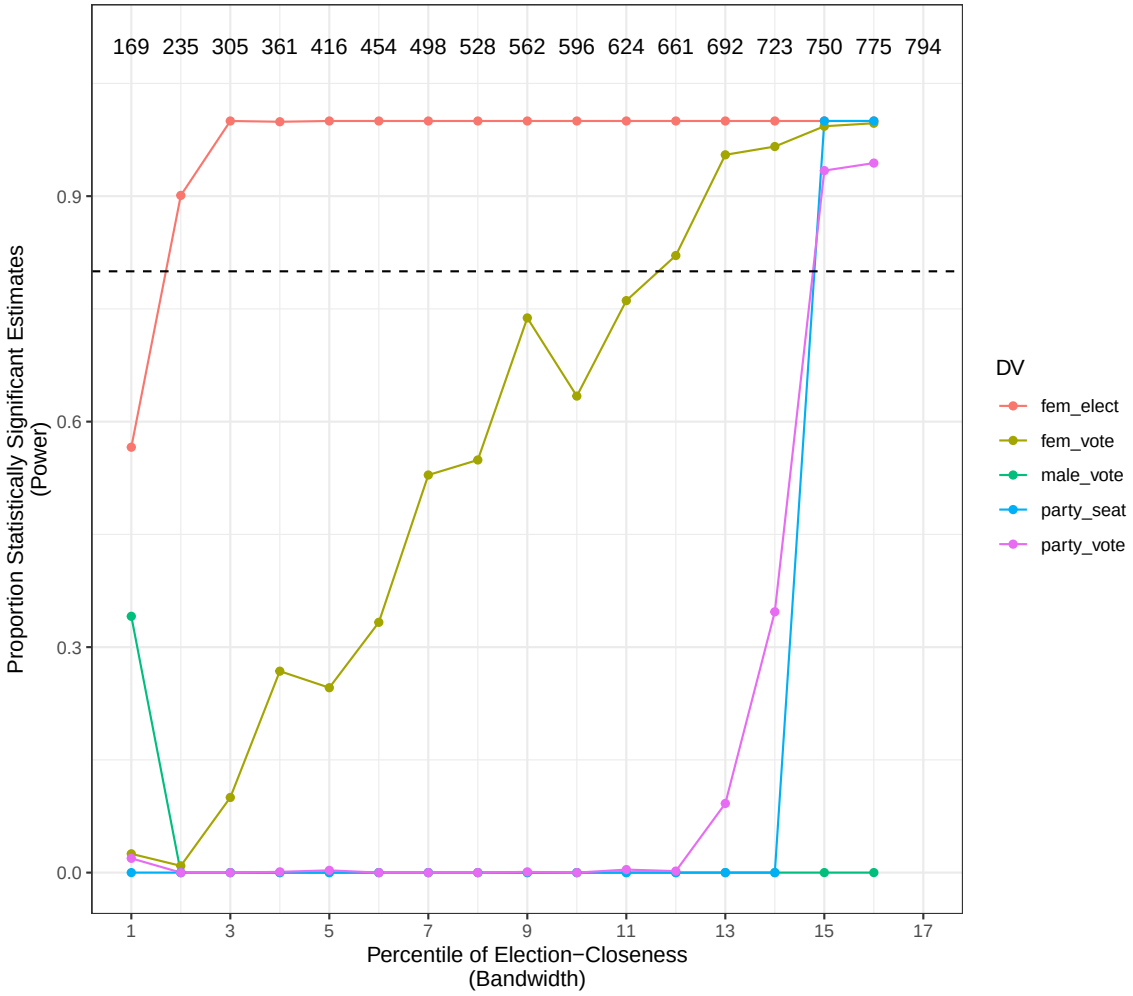

Figure H.1: Simulated Power to Detect the Baseline Effect Estimates. *Note:* The graph shows the simulated power of our statistical tests when detecting an intervention with the same effect on the outcome as the baseline effect estimated in Table 1. The power of the estimate is the proportion of times a statistically significant estimate is retrieved (at the five percent level). We run 1,000 iterations within each bandwidth, and retrieve estimates for all five dependent variables used in the models presented in Table 1. The number of observations within every bandwidth is printed in the top of the graph. The horizontal dashed line shows the point of 80% power.

This provides a reasonable explanation for the observed curvature, and the limited robustness of the estimates in the most narrow elections. Because the samples are small, and the data is noisy, the statistical tests we employ are not at all powered to detect even relatively large effects. This implies that there should not be put too much stock in the curvature of the estimates in Figure G.2, as the tests employed in such low-power settings could produce any estimate.

Next, to get a better idea about how close the elections are in each bandwidth, Figure H.2 shows the differences in estimated votes across bandwidth 1 through 50%. As we can see, the closest elections are, indeed, very close. They increase from approximately 5 votes in the most narrow bandwidths to 47 votes in our main bandwidth. This shows the trade-off between bias and variance quite well: The small bandwidths are very close, but relying on them, we would not be able to estimate well-powered models.

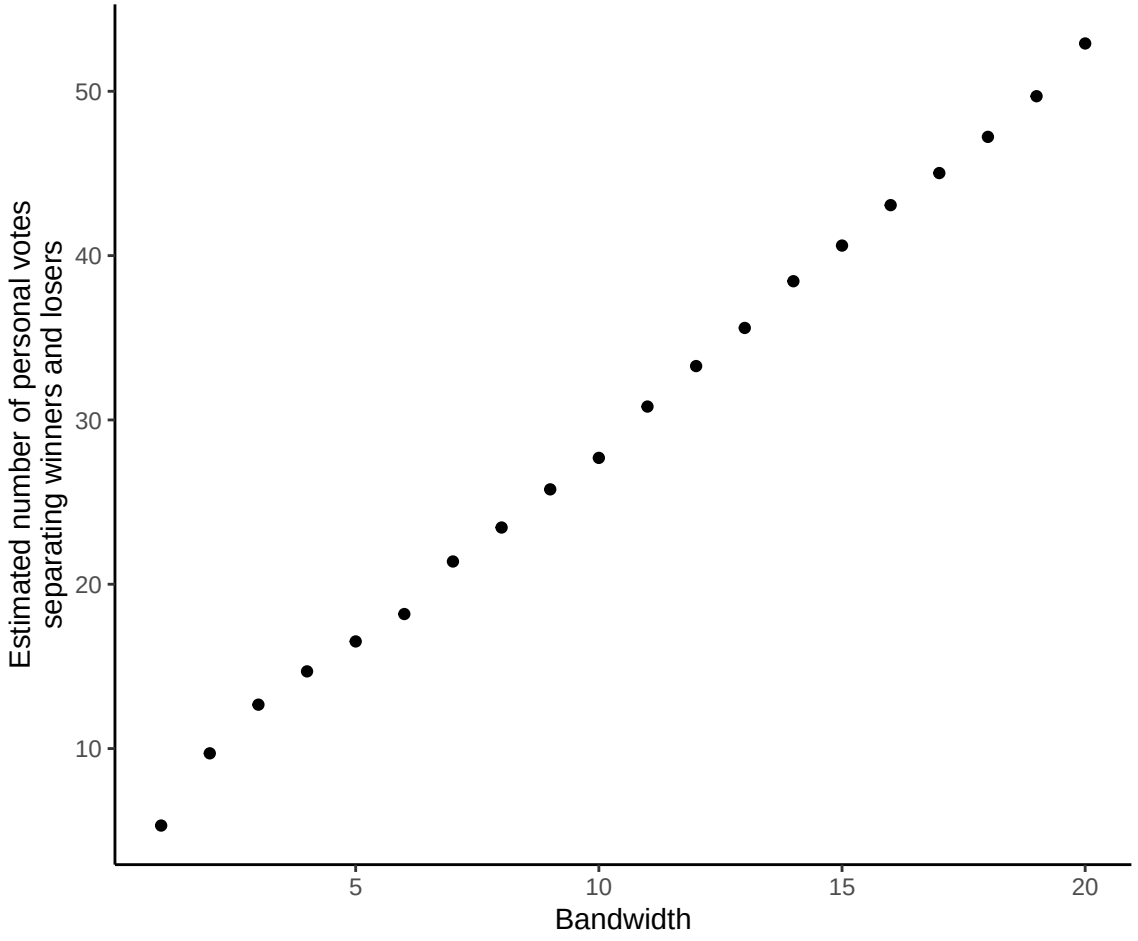

Figure H.2: Vote Differences between marginal candidates across bandwidths.

I Gendered Incumbency Advantage

So far our results reveal, that the additional election of a woman improves a party's electoral performance in the succeeding election and that this result is robust. The gendered incumbency advantage hypothesis suggests that if the results are driven by women servicing their constituents better than men – and the voters are aware of this – then women incumbents should see a strong incumbency advantage in the next election.

In Table I.1, we test for such gendered differences in incumbency advantages. To do so, we move the unit of analysis down to the candidate level. In particular, we consider the marginal election of a candidate, and whether this affects the likelihood of that candidate *1) rerunning* and *2) rerunning and winning* office in the next election (Dahlgaard, 2016; Sevi, 2022).[1] The results are presented in Table I.1.

Table I.1: No Gender Difference in Incumbency Advantage

	Dependent variable:	
	Rerunning t+1	Rerun and Win t+1
	(1)	(2)
Candidate Wins t=0	0.270^{***}	0.327^{***}
	(0.021)	(0.020)
Candidate Wins t=0 X Female	-0.039	-0.053^{*}
	(0.036)	(0.032)
Constant	0.471^{***}	0.156^{***}
	(0.015)	(0.010)
Bandwidth	18	18
Observations	3,268	3,268
Note:		*p<0.1; **p<0.05; ***p<0.01

[1] Since winning is conditional on running, we need to look at winning *and* running instead of winning conditional on running, as the latter would be prone to post-treatment bias if running is an effect of incumbency.

Within our bandwidth of 18, we pool the candidates and estimate an interaction between marginally winning and the candidate's gender. We cluster the standard errors at the municipality-party-year level. The baseline estimate for *Candidate Wins* indicates the incumbency advantage among men, and the interaction term gives us the difference in the incumbency effect between men and women. For men who are marginally elected, there is a considerable incumbency advantage for rerunning and for rerunning and winning. The incumbency advantage seems to be smaller for women, however it is not statistically significant for rerunning. It is marginally significant at the 10% for rerunning and winning. This speaks against the gendered incumbency advantage and the competence mechanism, whic would lead women to gain more in future elections.[2] This suggests that the main effects observed above are not just driven by the marginally elected women being more likely to rerun on the list compared to marginally elected men.

Second, the marginal election of woman might affect the rerunning rates of incumbents differently from the marginal election of a man. Similarly to the previous analysis, this would mean that the additional woman changes the composition of incumbents, which in turn would affect the party's electoral performance indirectly. In Table I.2, we estimate if the marginally elected woman makes other candidates elected in t=0 rerun in t+1, i.e., make incumbents of t=0 rerun in t+1. These incumbents are by definition elected by a larger margin than the marginal woman, and hence elected more comfortably. We estimate this at the candidate pool level and individual level, i.e., incumbent level, for men and women, respectively. We use the bandwidth of 18 as in our main models and cluster the standard errors at the party-municipality level. None of the four estimates are even near significant, and hence we do not find evidence that electing a woman increases the

[2]Our estimate of the incumbency advantage is larger than that reported from Denmark in a previous paper (Dahlgaard, 2016). We point to some differences between our study and the previous paper: the previous paper only studied elections between 2005 and 2013, whereas we have data on close elections from 1997 to 2017. Our data includes elections before a large municipality merger in 2005, which reduced the number of municipalities by more than 60%, making elections more competitive and potentially reduced the incumbency advantage. Dahlgaard (2016) also has data from a different source, which allowed the author to use a bootstrapping method and study parties running on both open- and semi-open lists.

rerunning of other incumbents. Thus, altogether, we find no evidence in favour of the gendered incumbency advantage hypothesis.

Table I.2: Electing women does not make other incumbents rerun more

	Incumbents Rerunning t+1			
	Share Women Individual level	Share Women Candidate pool	Share Men Individual level	Share Men Candidate pool
	(1)	(2)	(3)	(4)
Woman Win t=0	0.016	−0.002	−0.002	−0.011
	(0.037)	(0.026)	(0.019)	(0.016)
Constant	0.717***	0.719***	0.772***	0.785***
	(0.027)	(0.016)	(0.013)	(0.015)
Bandwidth	18	18	18	18
Observations	672	403	1,877	641

Note: The effect of electing women on the share of comfortably elected women and men, respectively, who run again in the next election. The average optimal bandwidth of 16 is used. Robust standard errors are clustered by municipality and party in parentheses. *, **, and *** indicate p $<$ 0.1, $<$ 0.05, and $<$ 0.01, respectively.

J Role-Model Effects Do Not Drive the Results

An alternative explanation to the ones we pursue in this paper is that new candidates might be mobilized differently by the marginal election of a woman or man, if increased women representation triggers a role-model effect, which would lead to mobilization of more women candidates. In this case, the marginal election of a woman or man would also change the party's composition of candidates differently, which could affect the party's electoral performance.

In Table J.1 we test for this by estimating whether a marginally elected woman affects the number of new candidates running for the party. We construct three outcomes capturing the number of: *1) new candidates*, *2) new women candidates*, and *3) new men candidates* in the party.[1]

[1]Contrary to most other models, we do not consider a share in this specification. The share could be endogenous to the number of candidates choosing to rerun as an effect of an additional woman winning a seat.

We use the bandwidth of 18 like in our main specifications and cluster the standard errors at the municipality-party level. All three outcomes are insignificant, and we hence find no indication that the marginal woman increases the number of new candidates in general nor men and women candidates specifically.

Table J.1: The Effect of Electing Women on Future Candidate Emergence

	New Candidtes t+1		
	In total	Women	Men
	(1)	(2)	(3)
Woman Win t=0	−0.012	−0.035	0.023
	(0.148)	(0.051)	(0.129)
Constant	5.657***	1.770***	3.887***
	(0.504)	(0.125)	(0.403)
Bandwidth	18	18	18
Observations	806	806	806

Note: The effect of electing women on the number of new women candidates and men candidates and candidates in general in the next election at the bandwidth of 22. Robust standard errors are clustered by municipality and party in parentheses. *, **, and *** indicate $p < 0.1$, < 0.05, and < 0.01, respectively.

K Representation of women within the party's ideological bloc

Next, we examine whether the mechanism producing parties' electoral advantage of increased women representation stems from under-representation of women. This would be consistent with two of our proposed mechanisms. First, if the effect arises because voters prefer more gender-equal parties, we would expect the largest effects among parties with relatively few women represented. This could happen even if the newly elected woman did not behave differently than the counterfactual man. Second, we also propose the women as party coherence mechanism, where the newly elected woman improves the internal functioning of the party. If a woman is elected into a party with a relatively under-representation of women, this would give her the room to improve the party's functioning even more.

Importantly, voters mostly cast their vote according to what policy they prefer rather than the descriptive representation of officeholders (Pedersen et al., 2019). Therefore, we do not expect electing women to move voters across the ideological aisles. Rather, we expect parties' share of women officeholders to matter, when voters decide between two parties with approximately similar ideology. To fully grasp this notion, it is necessary to provide additional information on the Danish party system. Danish political parties are highly fragmented both at the national and local level. At the national level, the Danish parliament typically has between 7 and 11 parties represented. The situation is similar in the municipalities. However, there are local parties formed around issues arising the municipalities. Parties are highly cemented into ideological blocs at the national level. The situation is more fluid at the municipal level, where parties sometimes cooperate across ideological lines. However, in general parties tend to form coalitions along the same ideological lines as in parliament, and local politicians tend to perceive the ideological space of local politics as similar to the national one (for a discussion of this, see Heide-Jørgensen (2021)). Hence, Danish parties – similar to most other advanced democracies – are typically divided into a left and right bloc, and they tend to be present at both the national and municipal level.

To investigate this, we consider each party's percentage of women officeholders relative to the average percentage for ideologically similar parties in the same municipality. We capture this by

comparing each party's representation of women to the representation within its ideological bloc. We measure the bloc through national belonging, and have to exclude local parties, where ideology tends to be much more fluid. For each municipality-year, we calculate the average percentage of elected women representing left-wing and right-wing parties, respectively, and then subtract this from each particular (left/right-wing) party's percentage of women. This measure is negative (positive), when a party has a lower (higher) percentage of elected women relative to its ideological bloc in the municipality-year. We construct the measure at the time of election (t_0), implying that it captures a party's gender parity during the treatment assignment. We interact the measure with the treatment of a woman marginally winning. Again, we use the bandwidth of 18 where the treatment and control group are balanced. Figure K.1 presents the results from a linear interaction model and from a model allowing for non-linear interaction effects using the Hainmueller et al., 2019 binning estimator.

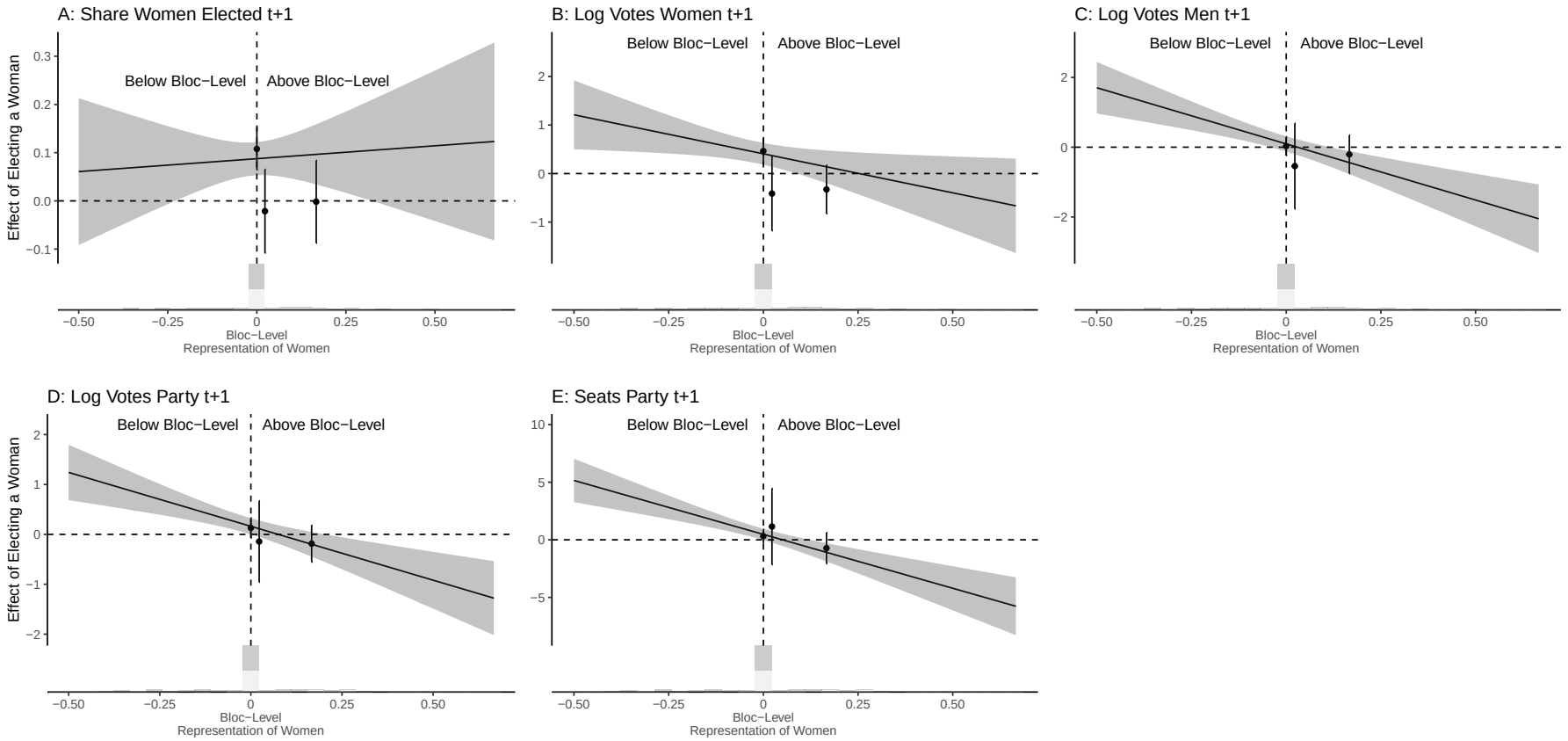

Figure K.1: The Effect of Electing Women is Moderated by the Prior Representation of Women in its ideological bloc. *Note:* The narrow election of a woman interacted with the party's deviation in women representation compared to its ideological bloc in the municipality. Below (above) the dashed, vertical line are parties with lower (higher) representation of women compared to other parties in their ideological bloc. Straight line is a linear interaction. Dots-and-whiskers are from the binning estimator proposed by Hainmueller et al., 2019. Bandwidth of 18 is used. Robust standard errors are clustered by municipality and party in parentheses.

The results reveal that the positive electoral effects of electing a woman are driven by parties with a low representation of women officeholders–compared to their ideological bloc–before they had an additional woman elected (received the treatment). Among parties with less women representation compared to ideologically similar parties, electing a woman improves the party's electoral performance in the following election measured as votes for women, votes for the party, and even when looking at seats won by the party. The linear interaction suggests that electing women also leads to more votes for the party's men candidates. However, the binned estimate is not statistically significant. It is important to note that the non-linear specification is more demanding.

Overall, the results reveal, that parties gain electorally from improving their representation of women toward the point of parity with their ideological bloc. After the point of parity, there is no effect. This suggests that voters avoid parties where women are under-represented. This is consistent with both the legitimacy and party coherence mechanisms.

Table K.1 presents the results in table-form with a linear interaction term only.

Table K.1: The Effect of Electing Women is Moderated by the Prior Representation of Women in the party's ideological bloc.

	Dependent variable:				
	Share Women Elected	Log Votes Women	Log Votes Men	Log Votes Party	Seats Party
	(1)	(2)	(3)	(4)	(5)
Woman win t0	0.088^{***}	0.407^{***}	0.094	0.162^{***}	0.473^{**}
	(0.028)	(0.078)	(0.078)	(0.061)	(0.194)
Women relative to bloc t0	0.002^{**}	0.012^{***}	0.010^{***}	0.009^{***}	0.032^{**}
	(0.001)	(0.003)	(0.002)	(0.003)	(0.015)
Woman win t0 X Women relative to bloc t0	0.001	-0.016^{***}	-0.032^{***}	-0.022^{***}	-0.094^{***}
	(0.001)	(0.005)	(0.006)	(0.005)	(0.015)
Constant	0.245^{***}	5.834^{***}	7.358^{***}	7.519^{***}	4.673^{***}
	(0.016)	(0.244)	(0.257)	(0.199)	(0.973)
Bandwidth	18	18	18	18	18
Observations	819	819	819	819	819
Note:					*p<0.1; **p<0.05; ***p<0.01

L Is the effect concentrated in urban municipalities?

One might suspect that our results were driven by effects from large, urban, progressive municipalities. To test this, we examine whether the results are different in the four largests Danish municipalities; Copenhagen, Frederiksberg, Aarhus, Odense, Aalborg – which are the municipalities of the four largest Danish cities and are the cities in which the vast majority of university education is located. Frederiksberg is a seperate borough municipality surrounded by Copenhagen, and in practice is a part of Copenhagen. Therefore we have included it.

Table L.1 shows our results when we interact the marginal election of a woman with a dummy for being a large municipality. As the table shows, there in fact are larger effects in the bigger cities (the significant interaction term). However, the main term for our treatment – which express the treatment effects when the large city dummy equals 0 – also yield significant effects. Hence, the effects *are* largest in larger municipalities, but do exist in both larger and smaller municipalities.

Table L.1: Estimates when interacting for big cities

	Dependent variable:				
	Share Women Elected	Log Votes Women	Log Votes Men	Log Votes Party	Seats Party
	(1)	(2)	(3)	(4)	(5)
Woman win t=0	0.105***	0.402***	0.030	0.121**	0.245
	(0.033)	(0.075)	(0.089)	(0.060)	(0.151)
big_city	0.002	1.813***	1.558***	1.454***	−0.066
	(0.050)	(0.230)	(0.486)	(0.246)	(0.852)
female_win:big_city	0.032***	0.804***	0.232	0.475***	1.421
	(0.007)	(0.122)	(0.345)	(0.170)	(0.872)
Constant	0.238***	5.699***	7.242***	7.414***	4.566***
	(0.019)	(0.268)	(0.286)	(0.223)	(1.025)
Bandwidth	18	18	18	18	18
Observations	819	819	819	819	819
Note:					*p<0.1; **p<0.05; ***p<0.01

M Vote shares

In this appendix, we consider vote and seat shares instead of the logged counts. We compute the proportion of personal votes for the party's women and men candidates, respectively, as a proportion of all personal votes cast in the municipality. We also calculate the proportion of personal votes cast for the party out of all personal votes cast in the municipality, and the proportion of seats held by the party of the total seats in the municipal council. Overall, this provides substantively similar results to the main findings. The proportion of votes for women is estimated to increase by 1.4 percentage points. This should be contrasted with the average proportion of personal votes received by women which is 3.6%. Hence, the estimated increase amounts to 38.5% of the baseline. This corresponds to 38.9% additional votes. This is not too different from the baseline of 43% extra votes for women. The proportion of votes for the party increases by 0.6 percentage points, which seems small, but translated into extra votes it is 0.11%, which is quite close to our main estimate of 12.6%. Finally, the proportion of seats held by the party increases by 1 percentage point. In the average municipality, this corresponds to approximately 0.22 extra seat. For comparison, in the main text we estimate an increase amounting to a little more than 0.3 seat. It is worth noting that these estimates are all more noisy than our baseline estimates, and the estimated impact on votes for the party and seats for the party are not statistically significant. That is, they are estimated with more noise, but are quite similar in terms of substantive effect size.

There is also a finding that is very different from our main results, where we do not estimate a substantial increase in votes for men. When examining the proportion of votes for men, we find a statistically significant increase of 2.3 percentage points. Which is difficult to consolidate with our main results

Second, to understand better the differences in results (i.e. the higher level of noise and the positive result regarding votes for men), we apply a different strategy that ensures that similar municipalities are examined. In particular, we control for, respectively, the total number of personal votes cast and seats available in the municipality. This is included in the same Appendix as the

Table M.1: Effects on Vote and Seat Shares in Municipality

	Dependent variable:			
	Vote Share Women	Vote Share Men	Vote Share Party	Seat Share Party
	(1)	(2)	(3)	(4)
Woman win t=0	0.014*	0.023*	0.006	0.010
	(0.007)	(0.012)	(0.008)	(0.007)
Constant	0.045***	0.179***	0.156***	0.203***
	(0.009)	(0.056)	(0.038)	(0.049)
Bandwidth	18	18	18	18
Observations	890	890	890	890
Note:				*p<0.1; **p<0.05; ***p<0.01

analysis with vote and seat shares. This produces almost exactly the same results as we present as our main findings.

Overall, our take on this robustness analysis is that it generally confirms our main findings, but that the estimates are associated with more noise. It is also worth noting that the variables capturing vote and seat proportions measure something slightly different. On one hand, the (logged) counts can increase if turnout does, while the relative allocation between parties remains constant. The proportion variables, on the other hand, cannot do so, and hence capture whether parties are successful in their competition. Since the results obtained with the different types of outcome variables are similar, this point is less relevant.

Table M.2: Controlling for the Total Numbers of Votes and Seats in Municipality

	Dependent variable:			
	log(Vote Women+1)	log(Vote Share Men+1)	log(Vote Party+1)	Party Seats
	(1)	(2)	(3)	(4)
Woman win t=0	0.458***	0.028	0.142**	0.343**
	(0.089)	(0.057)	(0.062)	(0.144)
log(Total Votes in Municipality)	0.665***	0.606***	0.552***	
	(0.053)	(0.075)	(0.054)	
Total Seats in Municipality				0.088
				(0.056)
Constant	−0.783*	1.289**	2.006***	2.325***
	(0.439)	(0.605)	(0.435)	(0.567)
Bandwidth	18	18	18	18
Observations	890	890	890	890
Note:				*p<0.1; **p<0.05; ***p<0.01

N Competitor parties' reaction to increased representation

How do other parties react to the increased representation of women in one party? In other words, does the election of an additional woman make other parties run more women? To analyze this, we calculate the proportion of women in parties in the municipality other than the party where a candidate was marginally elected. Figure N.1 displays these estimated spill-over effects. First, in the following election t+1, in which parties with an additional woman elected improve their electoral performance (as shown in Table 1). This is the case in the next election as well, t+2, which is the election where competitor parties have had a chance to observe improved electoral performance of parties with increased representation of women and react to this. Estimates are made from all bandwidths from the 0% closest elections to the 50% closest elections. Confidence intervals are 95% from robust standard errors clustered on municipality and party. In both t+1 and t+2, the estimates indicate that other parties on average increase their proportion of women by around 1 percentage point. The estimate sizes are similar for t+1 and t+2, and the main difference is the statistical uncertainty, which logically arises because looking two elections ahead means that fewer observations are included. This indicate that competitor parties react to the improved electoral performance of parties with increased representation of women, however, the reaction is quite small. The small effect size (1.8% increase) is part of the reason why women remain underrepresented as officeholders. Another reason might arise from women facing higher personal costs of serving, for example, by having higher probability of divorce (Folke and Rickne, 2020) or more exposure to sexual harassment in the political work environment which in turn make them quit politic faster and hence having shorter political careers than men.

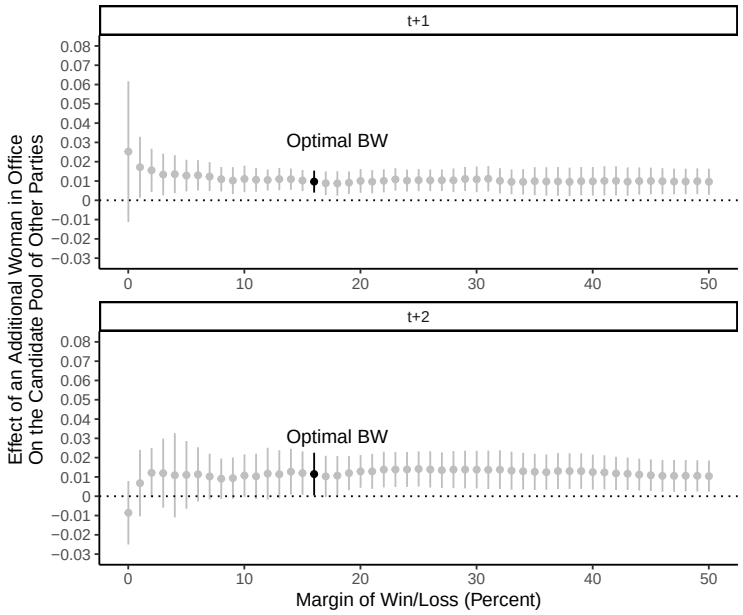

Figure N.1: The effect of electing a Woman on the Proportion of Women in the Candidate Pool of Other Parties.

Note: The figure shows estimates from models where the outcome is calculated by iterating over all party-municipality observations, excluding the current party, and calculating the municipality's proportion of women candidates among the other parties. This is done in the prospective election, t+1, and two elections ahead, t+2. Estimates are made from all bandwidths from the 0% closest elections to the 50% closest elections. Confidence intervals are 95% from robust standard errors clustered on municipality and party.

O Additional Information on Personal Votes

In this appendix, we provide additional information on the role of personal votes in the context of Danish municipal elections. In the elections included in our dataset, personal votes make up between 70% and 80% of the total votes (Nørtoft, 2019). Many of the personal votes go to the top placed candidates, who generally receive by far the most personal votes. However, conversely to local elections in other countries with less open lists, candidates further down the party lists receive large numbers of personal votes, too. Taking the latest municipal election in 2021 as an example, the candidate that received most personal votes in the Municipality of Copenhagen (Line Barfod from the Unity List) received approximately 20% of her party's vote. The pattern is similar among the other big parties in Copenhagen: The social democrat with most votes received 20% of her party's vote, while the top conservative candidate received 25% and the liberal party's candidate received 10%. While the candidate at the top of the list almost always receives a very large share of the personal votes, other candidates in combination receive more. At the same time it is also the case that the marginal candidates – the ones that are elected as the last people on the party list – generally receive much fewer personal votes than the people at the top of the list. However, some of the marginal candidates (both winners and losers) are still quite well-known. This is because the municipal councils are generally quite small, hence most party delegations will be small, and all of the party's strong candidates will not necessarily get elected.

To investigate this, we have examined the vote counts of the marginal candidates. One complication is that we do not have the exact number of personal votes of the candidates, as this would violate Statistics Denmark's privacy rules. However, we have a range within which each candidate's votes will be within. Utilizing the mid-point in that range as the best estimate of personal votes, we can see that on average approximately 163 votes are cast for the marginal candidates that participated in our close elections. This corresponds to 7.6 percent of the average party's total vote count. The median is – as we note in the main paper – 143.5. This resembles the vote counts found in other studies of marginal Danish candidates that do not rely on Statistics Denmark's servers (Dahlgaard, 2016).

This hints at a relatively skewed distribution of votes among marginal candidates. In Figure O.1 we show the distribution of estimated personal votes among marginal candidates, and split it by winning and losing candidates. As we can see, both distributions are, indeed, skewed. But we can also see that the tails follow each other quite well: because all the elections are close, when the winner gains many personal votes, so will the loser.

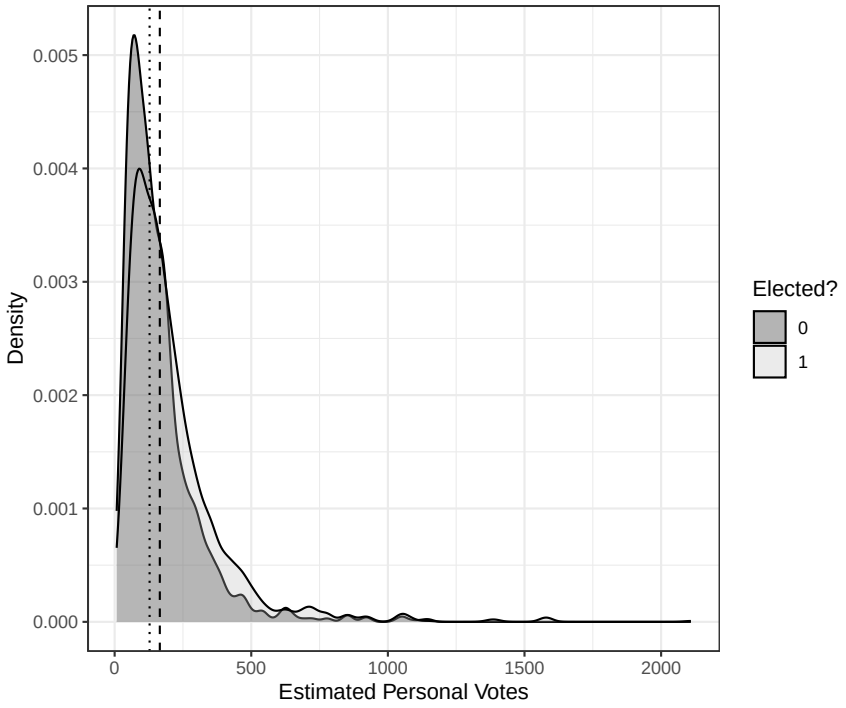

Figure O.1: Distribution of Estimated Personal Votes Among Winners and Losers. *Note: The figure shows the distributions of estimated personal votes among marginal winners and losers, respectively, at the bandwidth of 18% closest elections. The median (dotted line) is 34, the average (dashed line) is 47.*

P Effects on Women Party Leadership

We could imagine that electing a woman would improve the list placement of women in future elections. However, we are not allowed to obtain data on that due to privacy concerns – candidates can be identified if their list placement is known. However, a different – but related – measure is whether women are placed in the top of their list. While we cannot obtain *exact* data on this, we can use personal votes to capture it, because the candidate receiving most personal votes will almost always be the one with the highest list placement.

Hence, we run our main specification, but use whether a woman receives most votes (i.e. is the local party leader) in the upcoming election (t+1). As we can see, we estimate a dramatic 10 percentage point increase in the probability of seeing a woman as the party leader (from a low baseline of 15.4%). However, the estimate is far from statistically significant, and we cannot conclude with any certainty that it is not driven by noise. It does, however, provide suggestive evidence that future research could probe.

Table P.1: Effects on Parties Gaining Women as Leaders

	Dependent variable:
	Woman as Party Leader
Woman win t=0	0.104
	(0.070)
Constant	0.154^{***}
	(0.027)
Bandwidth	18
Observations	815
Note:	*p<0.1; **p<0.05; ***p<0.01

Q Results with Estimated Personal Votes as Running Variable

We have not been able to obtain data on the exact personal votes of the candidates, which we would need to conduct a regression discontinuity analyses, where we control for some polynomial of the running variable. However, we can obtain an estimate of the personal vote, as Statistics Denmark has provided us with a range within which each candidate's personal vote is located. We can obtain a reasonable estimate of the candidate's personal vote count as the midpoint between the limits of the range. This can be used as a running variable by calculating the differences in estimated votes between winners and losers. As shown in Dong and Kolesár (2023), as long as the running variable separates winners and losers, the RD procedure can still retrieve an unbiased estimate of the local average treatment effect at the cut-off, despite this measurement error. However, we cannot distinguish between winners and losers when the vote differences is zero. Therefore, we follow the recommendation in Dong and Kolesár (2023) and exclude all those cases. We then conduct an RD analysis using the estimator proposed by Calonico et al. (2014), using a local linear regression model, a control polynomial of 1, and a triangular kernel weight.

Figure Q.1 shows the visual regression discontinuity analyses with all five outcome variables used in our main analyses. That is, the proportion of women elected, the logged votes for women and men, the logged votes for the party and the number of seats for the party. All measured at the next upcoming election (t+1). In these graphs, we include all races where the marginal candidates were separated by 200 votes. As we can see, there is a clear discontinuity in all cases, where the parties with women as winners see improved performance on all five outcome variables.

Next, we examine the results at the each dependent variable's optimal bandwidth. This is presented in Figure Q.2. In all cases, we estimate statistically significant improvements in the party's future performance. In terms of magnitude, all estimates are larger than the main findings we present.

To ensure that this is not a result of the bandwidth we used, we present results from a wide range of different bandwidths in Figure Q.3. We show estimates from races decided by five votes (the lowest bandwidth where the models are estimable) through marginal races decided by 200

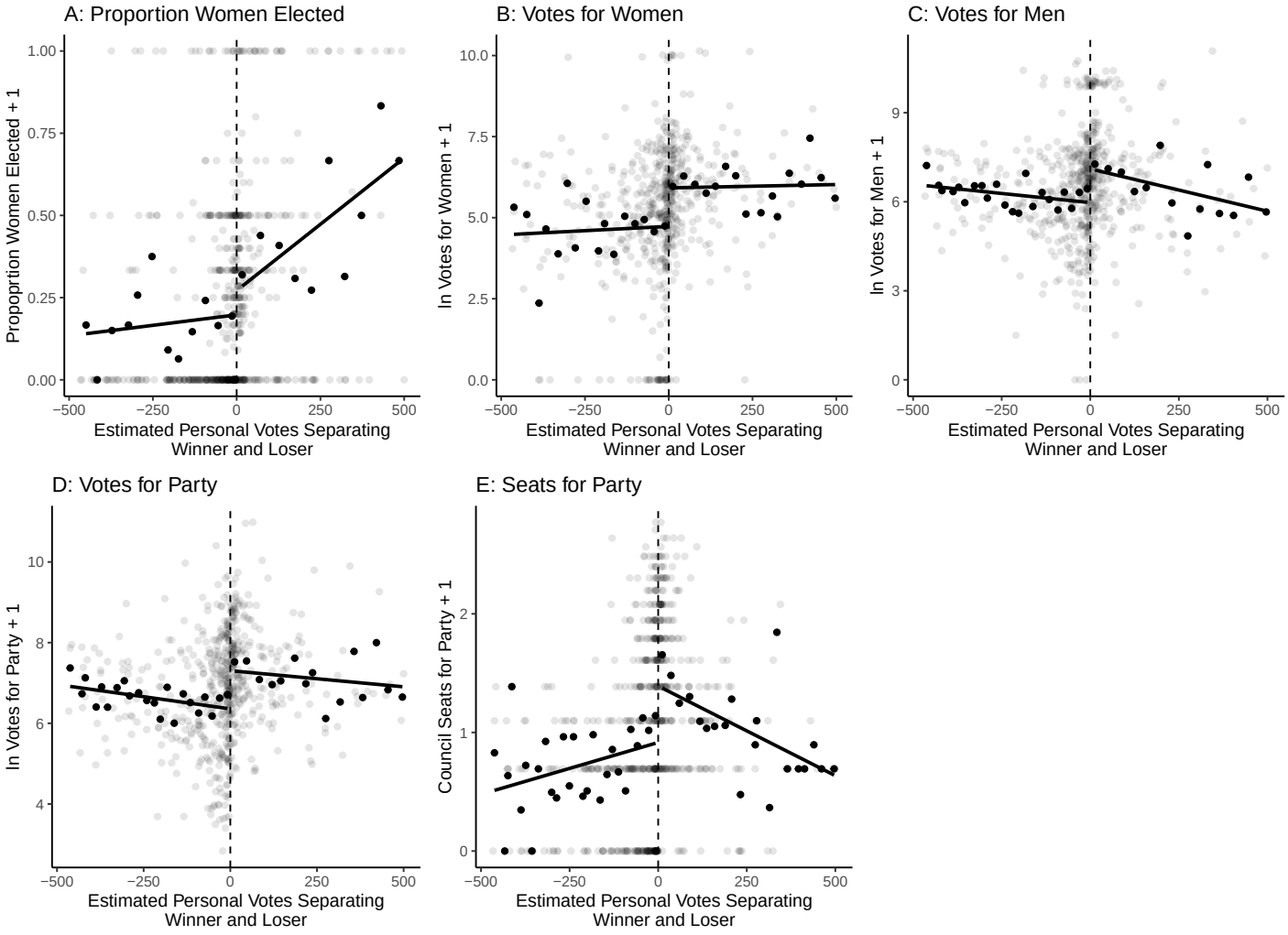

Figure Q.1: Visual Regression Discontinuity Results. *Note: Fully colored dots and lines are the bins and fitted lines from the Calonico et al. (2014) procedure. Polynomial is set to 1. Shaded dots are the underlying data. We present a separate graph for each of our five outcome variables. Races where there is an estimated 0 vote difference between the marginal winner and loser are excluded as recommended by Dong and Kolesár (2023).*

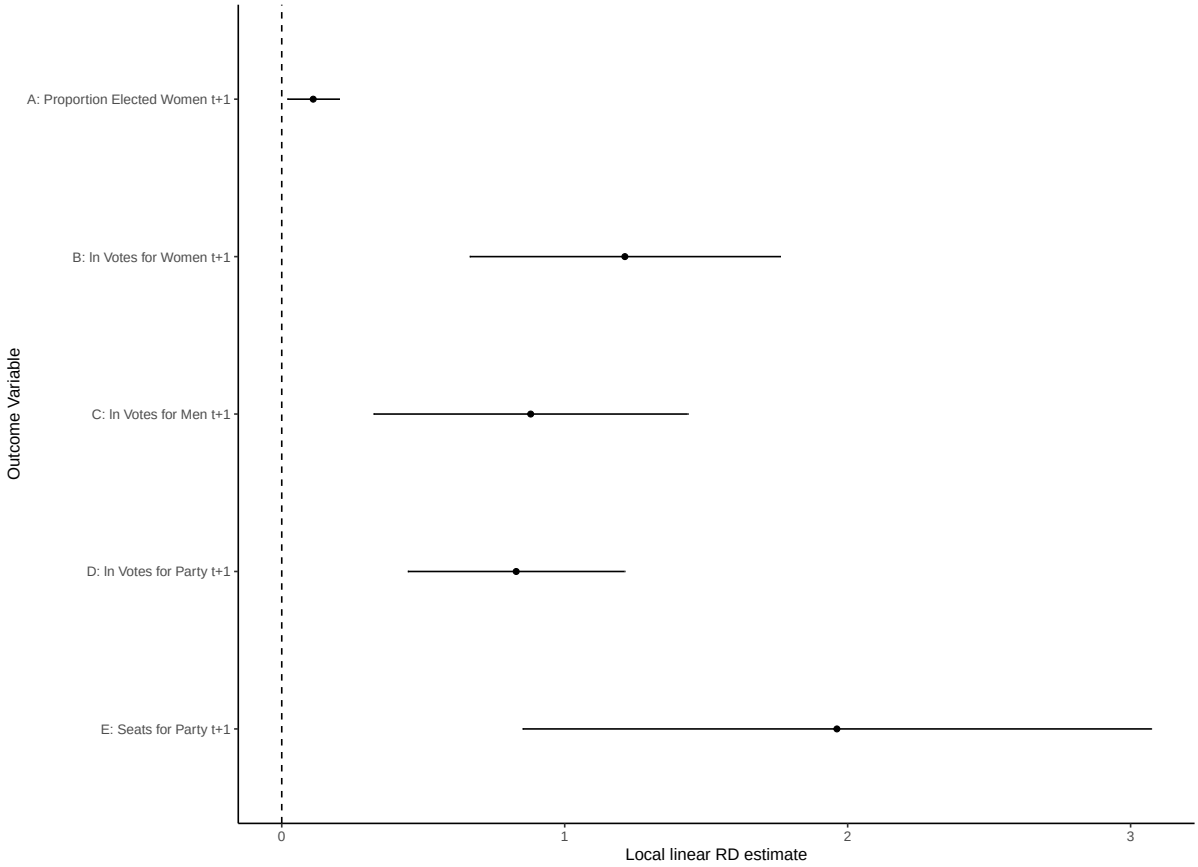

Figure Q.2: Local Linear Regression Discontinuity Results at Optimal Bandwidth. *Note: The figure presents results for each outcome variable at the bandwidth that is individually optimal for that variable selected using the Calonico et al. (2014) optimal bandwidth selector. The following bandwidths are used. Proportion women: 65.9. Votes for women: 102.2. Votes for men: 85. Votes for party: 97.7. Seats for party: 83.7. Estimates are local linear RD models with triangular kernel weights. Solid lines are 95% Calonico et al. (2014) confidence intervals with municipality-party clustering. Races where there is an estimated 0 vote difference between the marginal winner and loser are excluded as recommended by Dong and Kolesár (2023).*

votes. We increase the bandwidth in increments of 10 votes. As we can see, the RD results are quite robust: They are mostly stable across the different bandwidths and only drop off in the narrowest bandwidth for two of the outcome variables. The models generally need between 15 and 20 vote bandwidths for statistical significance. These bandwidths still provide very close elections. Overall, these results provide reassuring additional evidence in support of the main finding and main design.

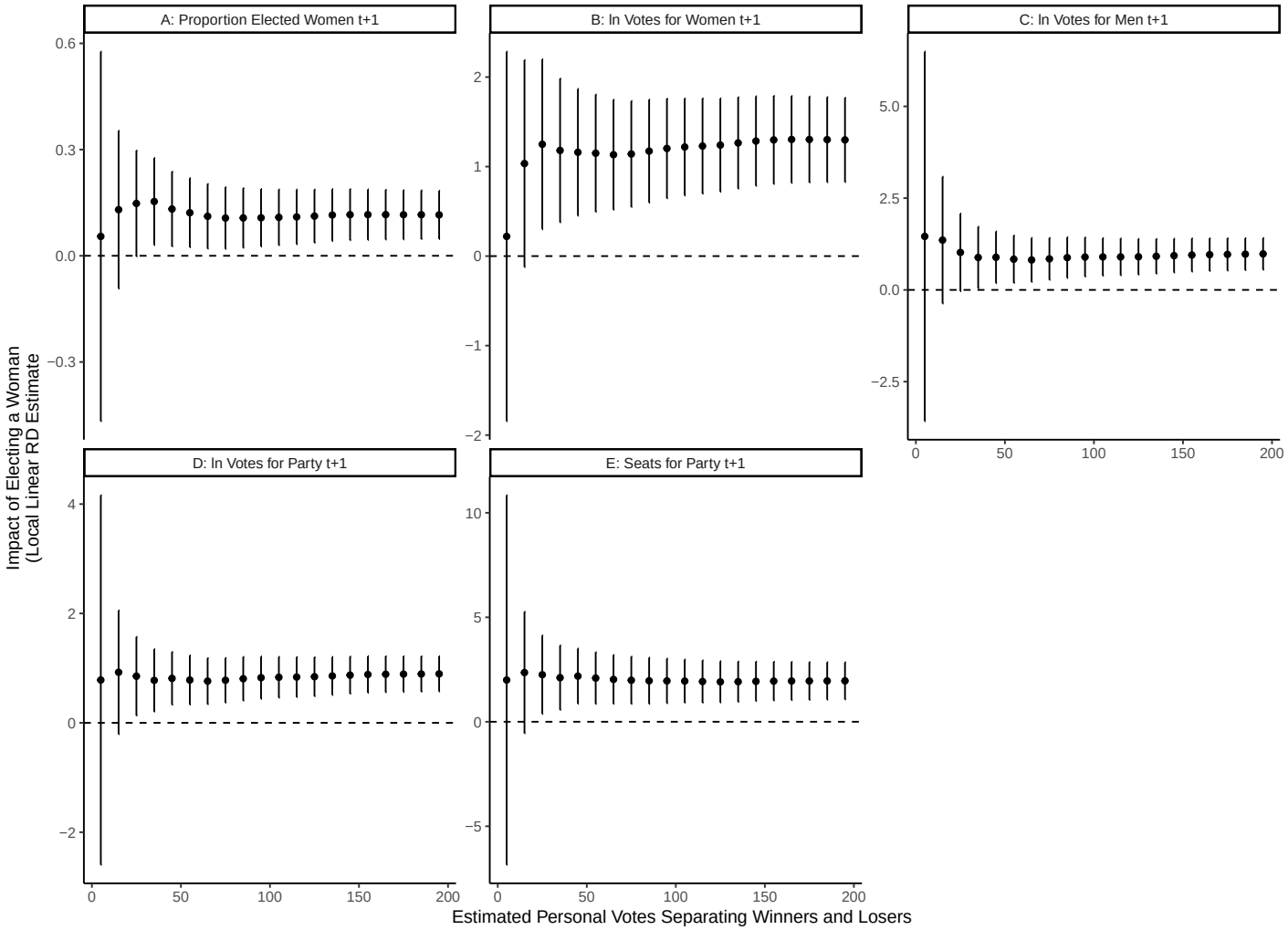

Figure Q.3: Local Linear Regression Discontinuity Results at Many Bandwidths. *Note: The figure presents the results from all bandwidths from the 5 vote through the 200 vote bandwidths with 10 vote intervals. Solid lines are 95% Calonico et al. (2014) confidence intervals with municipality-party clustering. Races where there is an estimated 0 vote difference between the marginal winner and loser are excluded as recommended by Dong and Kolesár (2023).*

R Leaving Out Observations

We might be concerned that individual municipalities or parties drive the results. To examine whether this is a worry, we run a series of models, where we in turn exclude each party or municipality, respectively. Figure R.1 shows the results from excluding municipalities, Figure R.2 shows the results from excluding parties. Overall, the results are very robust. The only exception is when the outcome variable is seats for the party, which becomes statistically insignificant (but still has a strong estimated coefficient) when one of three parties are excluded. We are not too concerned about this, because a) the results from all other variables are very stable, and b) the coefficients are still quite large – the lost significance comes mostly from additional noise.

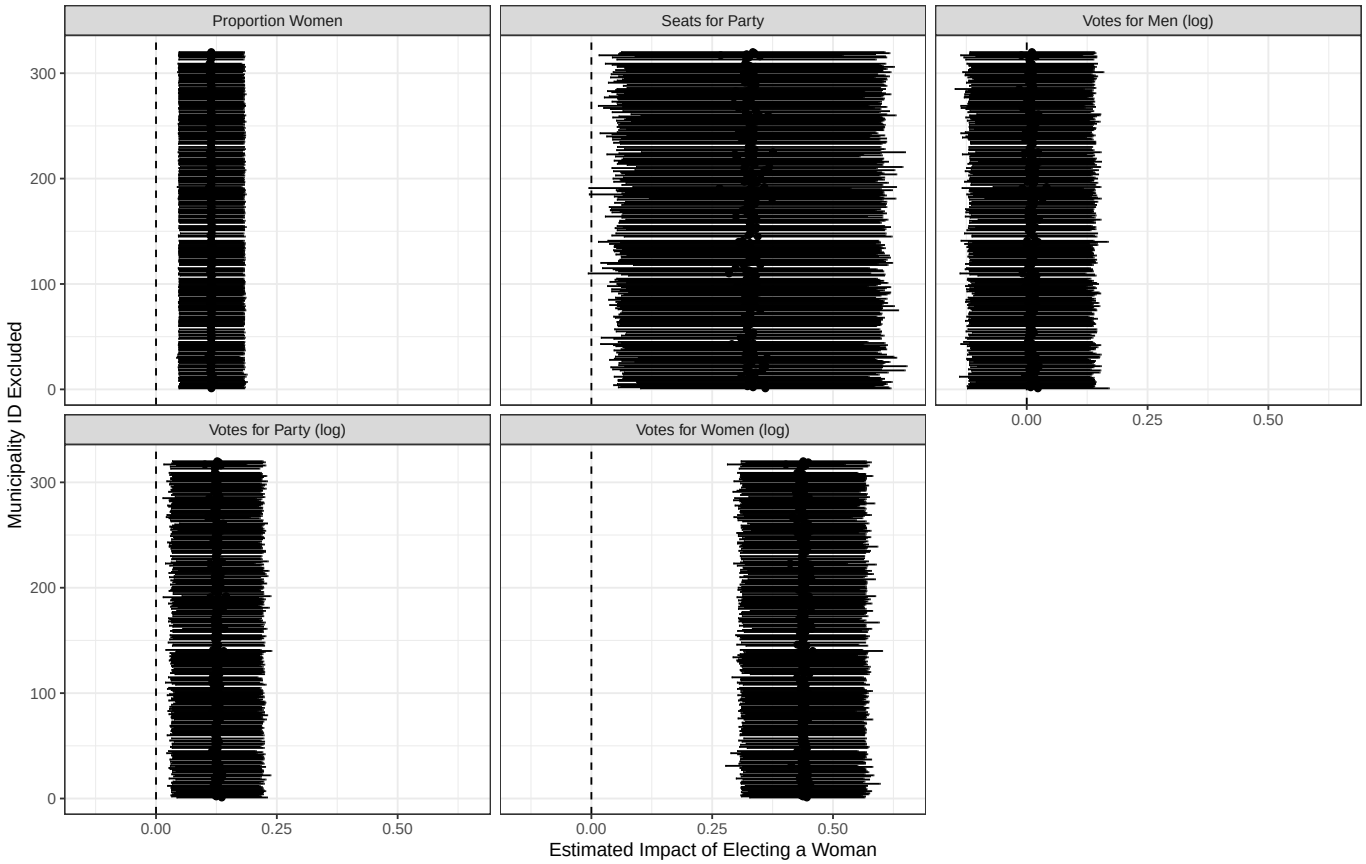

Figure R.1: Leaving Out Municipalities. *Note: The figure presents the results from leaving out each municipality iteratively and re-estimating the models. Point estimates are obtained from the same regression procedure as we used in the main text. Confidence intervals are 95% robust with clustering at party-municipality level.*

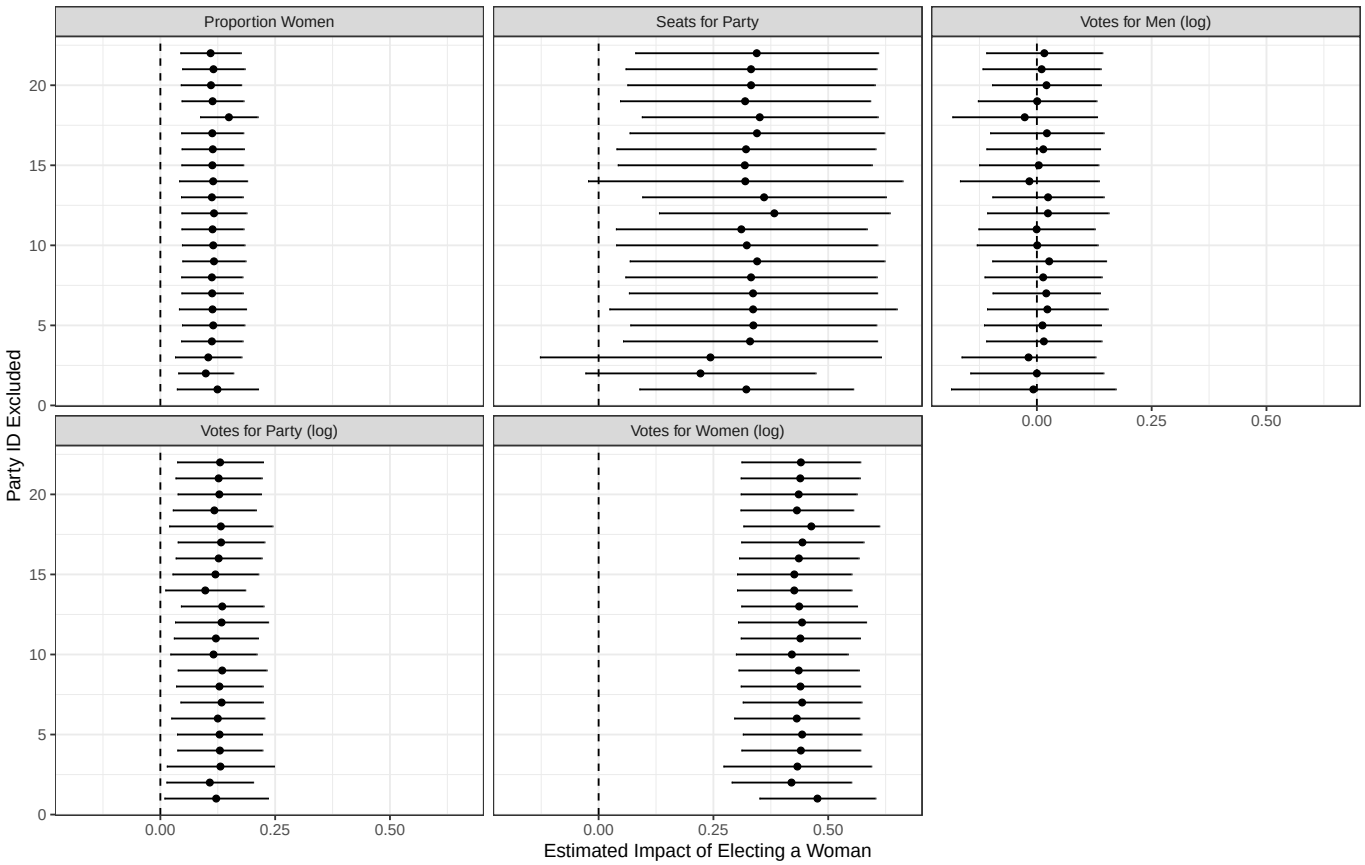

Figure R.2: Leaving Out Parties *Note: The figure presents the results from leaving out each party iteratively and re-estimating the models. Point estimates are obtained from the same regression procedure as we used in the main text. Confidence intervals are 95% robust with clustering at party-municipality level.*

S Personal Votes as a Moderator

If the legitimacy mechanism is what produces our results, we would expect the election of more high-profiled women to produce stronger effects. We test this in Table S.1, where we interact the number of personal votes of a candidate (standardized to make it interpretable) with the election of a woman over a man. This captures how well-known the candidate is in the local community. We would expect the most well-known candidates to produce the largest effect, because the public will be more aware of their election. As we are still conditioning on very close elections, a large number of personal votes for the winner will entail that the loser also received very substantial backing from voters.

Interestingly, we find that the effect on the proportion of women and votes for women are not moderated by personal votes. This makes sense, because the effect on those variables can, in part, be attributed to the incumbency advantage.

However, we do find very strong moderation effects on the votes for men as well as votes and seats for the party. As the number of personal votes increase by a standard deviation, the effect of electing women on votes for men and the party increase by, respectively, 15.3 and 17.7 percentage points. The effect of seats for the party increases by 0.51. Importantly, there only is an effect on votes for men, when the number of personal votes is sufficiently large.

This provides support for the legitimacy mechanism.

Table S.1: The Moderating Effect of Votes for the Marginal Candidate

	Dependent variable:				
	Share Women Elected	Log Votes Women	Log Votes Men	Log Votes Party	Seats Party
	(1)	(2)	(3)	(4)	(5)
Woman win t=0	0.107***	0.413***	0.020	0.123***	0.316***
	(0.032)	(0.079)	(0.067)	(0.046)	(0.112)
Votes for Candidate	−0.014	0.397**	0.298**	0.283**	−0.455**
	(0.013)	(0.182)	(0.120)	(0.113)	(0.179)
Votes X Woman win	0.025	0.202	0.153***	0.177***	0.511***
	(0.019)	(0.131)	(0.038)	(0.054)	(0.144)
Constant	0.238***	5.799***	7.327***	7.494***	4.556***
	(0.018)	(0.291)	(0.297)	(0.240)	(0.971)
Bandwidth Selector	Optimal	Optimal	Optimal	Optimal	Optimal
Bandwidth	18	18	18	18	18
Observations	819	819	819	819	819

Note: *p<0.1; **p<0.05; ***p<0.01

T Is the Likelihood of Treatment Equal?

In Figure T.3, we examine the proportion of elections that are won by women across bandwidths 1% through 50% of election closeness. As we can see, it is very close to 50% in the most narrow bandwidths. In our main bandwidth, the probability that a woman beats a man is approximately 48%.

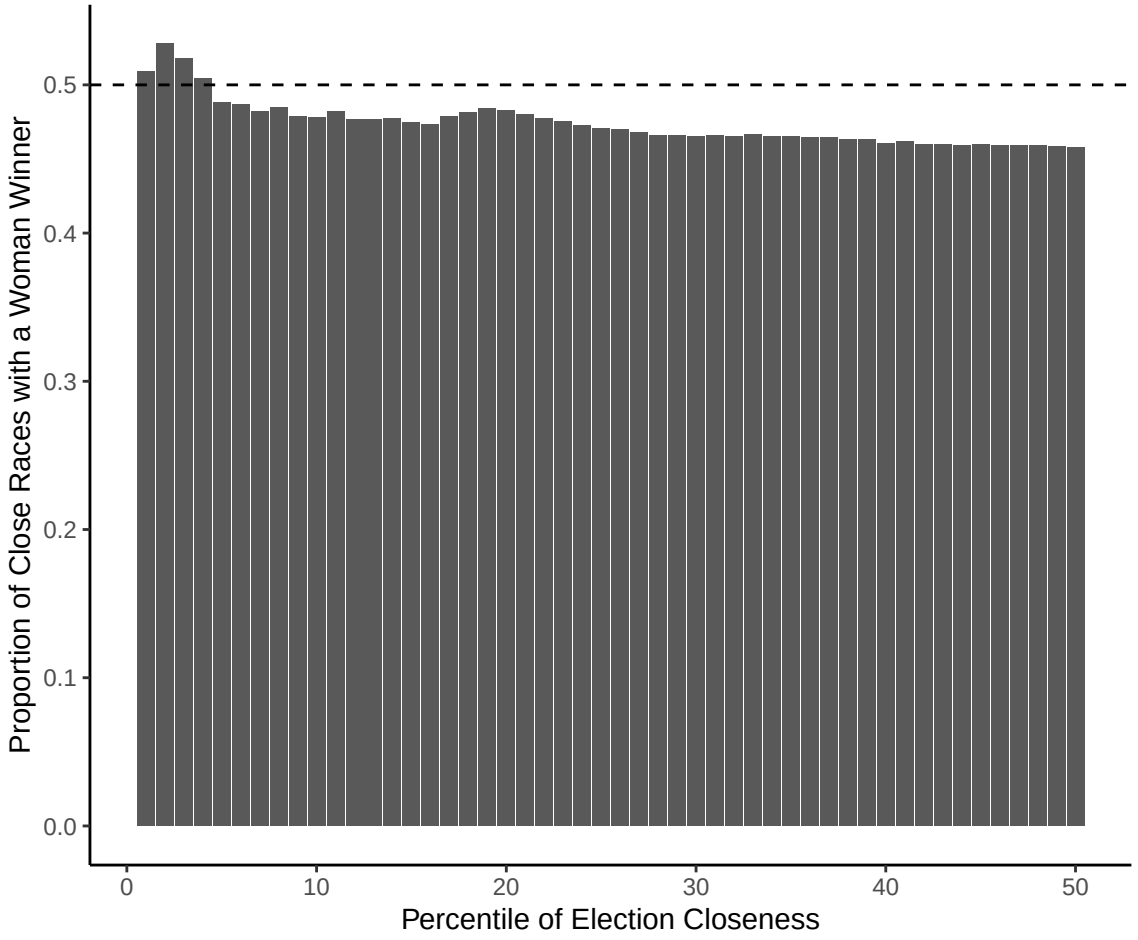

Figure T.3: The distribution of the likelihood that a woman wins the marginal election

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