

Dear Manuel,

Thank you for the detailed comments you made. I appreciate your effort and valuable input.

I believe I was able to address all of the concerns you have raised. I decided not to follow some of your suggestions that would contribute to making the chapter 1 appear more user friendly because this work has been already published. Nevertheless, I am taking all of your suggestions and advice into account and will apply them in the future papers.

Below are the detailed answers to all the comments.

Chapter 1: Does the Election of an Additional Female Councilor Increase Women's Candidacy in the Future?

1. *I would encourage the author to present from the very beginning the analysis for the whole sample, and, perhaps, later on she can report the analysis separately for small and large municipalities (or any other dimensions). Incidentally, the author may want to consider adjusting standard errors for multiple testing in these additional analyses.*

Thank you for the suggestion. I decided not to present the analysis for the entire sample because I do not observe any useful insight in those regressions. I thus present the analysis for the small municipalities, and mention the results for the larger ones, straight away in order to save space.

I have performed the Holm-Bonferroni correction of errors for the optimal bandwidth sample. After the correction, the main result for all small municipalities loses its statistical significance ($p=0.132$). However, point estimate for the small municipalities with two or more other female candidates elected remains statistically significant. The results are in the Table 1.5.

2. *RDD should be performed using state-of-the-art methods. In particular, I would encourage the author to select the bandwidth according to some criteria of optimality and to use local linear methods (e.g., a la Calonico, Cattaneo and Titiunik).*

Thank you for the suggestion. I have used the Calonico et al (2014) optimal bandwidth selection procedure and estimated the model using the optimal bandwidth and linear assignment to treatment function. The main results hold: I observe fewer newly participating female candidates in the small municipalities after the marginal election of an additional female candidate; this result is driven by the municipalities where two or more other female candidates were elected at the same time. The results are presented in Table 1.A.9.

3. *The author may want to relate her findings more closely to Bhalotra et al (2016) who, using data from India, also find that the victory of a female candidate leads to a decline in the entry of new female candidates.*

Thank you for correcting this mistake. I was erroneously citing the earlier version of the paper. I have now corrected the references and related my findings to those in Bhalotra, Clots-Figueras &

Iyer (2018).

The findings of the authors are indeed very similar to mine. They find that electing female candidates in state elections in India results in lower entry of new female candidates in the future, especially in the constituencies with gender bias. I observe a reduction in the entry of new female politicians as well, however, on the municipal level. I also observe a suggestive evidence that the reduction happens in the municipalities where the society likely thinks that their optimal level of female political participation has been reached.

4. The paper sometimes provides too many details about the creation of the database that are irrelevant for the average reader and should probably be moved to the appendix (e.g. second paragraph of page 18). Similarly, some tables are not reader-friendly. They should more focused on the relevant information that the author is trying to deliver and they should display information in a way that it is easier to read. For instance, if the author wants to show in Table 1.2 the differences in the characteristics of the different samples, perhaps she can just report the mean of each variable for each group in a different column. (And probably much of this information could be moved to an online appendix without much loss for the reader.)

I have moved the description of data creation to Appendix C, leaving the more important parts in the main text.

Thank you for the suggestion that some tables could be simplified. However, I prefer to keep the tables unchanged since the paper has been published already.

5. The calculation of the victory margin is crucial and, in this context, is non trivial. I would perhaps try to explain more clearly how it is calculated and perhaps verify how other authors in the literature have addressed this problem (e.g Kotakorpi, K., Poutvaara, P. and Tervi, M. 2017). For instance, my understanding is that the author is only contemplating within party competitors, but not across party ones, but I may be wrong. (Furthermore, apparently it does not seem to take into account the implications of the dHondt method for the calculation of this margin.)

Calculation of victory margin is indeed very important, I appreciate you for raising this point.

In Kotakorpi, K., Poutvaara, P. and Tervi, M. 2017 the authors resort to bootstrapping because the Finnish election system is more complicated, elections are held across multiple districts and the researchers face multiple margins. As a result, it is hard to measure closeness of election directly.

In the case of the Czech Republic, as in the cases of other papers in the literature (Baskaran & Hessami 2018, Bhalotra et al 2017, Ferreira & Gyourko 2014, Brollo & Troiano 2013, Broockman 2014), the victory margin is easy to express in terms of vote share. In the papers mentioned above the victory margin is expressed as a difference in vote share received by the winning and the losing candidate. In my analysis I am following the same approach with one modification. Because I am not working with mayoral elections or elections where candidates receive certain share of votes

and where that share is the sole identifier of who is elected, I need to perform a more elaborate calculation in order to arrive to the vote share. In addition, because each voter has not one vote but as many votes as there are seats to be allocated, I express victory margin not in term of vote share, but in terms of share of voters.

I start the victory margin calculation by repeating the d'Hondt's method, i.e. I entirely replicate it in order to identify the candidates who were at the margin of being elected. The outcomes of this exercise are the numbers that are assigned to each candidate, no matter whether the two marginal candidates are from the same party or not, that do not carry any meaning as they are. I thus have to transform them into the dimension of share of voters. I provide a detailed explanation of how the transformation is performed in Appendix 1.A.

6. I do appreciate the attempts of the author to disentangle the potential mechanisms, but I wonder whether the differences between the two groups (at least 2 women vs. less than 2 women) are statistically significant. A proper test should be provided. Moreover, multiple testing might again be an issue.

I believe I provide the evidence that the differences between the two groups of municipalities - those where two or more other women were elected and those where less than two other women were elected - is statistically significant in Panel C of the Table 1.4. In the respective regression I use both types of municipalities and report the point estimate of the interaction between being treated and being the municipality where two or more other women were elected. The statistically significant and economically sizable point estimate indicates that the effect in the two groups is indeed different.

I also performed the Holm-Bonferroni error correction for multiple testing on the sample of optimal bandwidth and presented the results in Table 1.5. The main effect for the small municipalities with two or more other women elected remains statistically significant.

Other minor points: (i) I missed in Table 1.2 information on the number (or share) of women elected, which might be also an interesting outcome variable.

Number of elected female candidates was added to the Table 1.2.

(ii) Appendix tables should be numbered appropriately (e.g Table A1 instead of Table 10).

Thank you, addressed.

(iii) Typo: In the text the author refers to Table 3.2 when she is describing Table 11.

Thank you, corrected.

(iv) Balance check: probably there is no need of doing the balance check for all samples (at least not in the main text) just report the balance check for the optimal bandwidth sample.

I appreciate your suggestion. I will keep this advice in mind for future papers. However, at this

stage of the paper I prefer to keep the outputs as they are.

(v) No need to report the dirty laundry. Do not provide in the text descriptions of how exactly you arranged the database, merging datasets and so on. Just explain clearly the regression you run and the definition of each variables. (e.g. page 38, 1st paragraph: To test the long-term...)

I have moved the mentioned paragraph to Appendix C.

(vi) Most scholars in the field should be familiar with the dHondt method. Perhaps the description in Appendix 1A could be reduced to a few sentences explaining the method.

Thank you for the suggestion. However, I prefer to keep it the way it is since it has been already published.

Chapter 2: Do Personal Characteristics of Councilors Affect Municipal Budget Allocation?

1. In the theoretical framework, in addition to the median voter model, it would be useful to consider citizen-candidate models (e.g. Osborne and Slivinski 1996), which may rationalize why politician characteristics may matter, specially in a context such as local elections.

Thank you, I have added the citation.

2. Let me add a methodological note. As the title of Gelman and Imbens (2018) article indicates, high-order polynomials should not be used in regression discontinuity designs. Instead, the author may want to use local linear methods and, as I also pointed out in my comments to the first chapter, she may want to use the ‘optimal bandwidth instead of an arbitrary one.

Thank you for bringing to my attention the work by Gelman and Imbens.

Since the Dynamic RDD was introduced by Cellini et al (2010) and has not been extensively used otherwise, I decided to keep the estimation using higher order polynomials. In addition, following your suggestion, I also estimate the model using the optimal bandwidth. Interestingly enough, the coefficient of the only consistent result (election of an additional entrepreneur on current spending on fire fighters) has the same magnitude in both specifications - global RD with higher order polynomial and local RD with optimal bandwidth.

3. Given that the power of the analysis may be limited, it would be useful to report explicitly the magnitudes that can be rejected.

Thank you for this useful suggestion. I have added the discussion on the magnitude of the effect I reject to the Results section.

4. I wonder whether it would be possible to guide the analysis using information on the individual preferences, as in Bagues and Campa (2017). For instance, which type of expenditure do female voters care about in this particular context?

Unfortunately, I am not aware of any study or survey performed on the Czech municipalities

that would aim to determine the preferences of local politicians depending on their characteristics. Testing candidate-specific spending categories that would be characteristic specifically for the Czech Republic is thus not feasible at this point.

5. Let me note that this empirical strategy helps to compare municipalities that are similar, but there is no reason to expect that marginal candidates with different characteristics should be similar in other dimensions. For instance, if we observe two candidates, one who is college-educated candidate and another who is a high-school dropout, who receive the same amount of votes (e.g. one vote above the threshold), we should probably expect these two candidates to differ in other dimensions that are observable to voters but not to the econometrician. In sum, without introducing more structure, it is not clear what close elections are identifying in this context. A related point is that I would not consider Panel B in Table 2.3 as a check of covariate balance. There is no reason why we should expect male/female candidates to be similar in other dimensions.

Thank you for this important point.

First, I want to stress why I find the Panel B of Tables 2.3 (global estimation with victory margin polynomial) and 2.4 (local liner regression with optimal bandwidth) an important part of co-variate balance check. By comparing the composition of all candidates and elected candidates other than those elected marginally I want to show that the municipalities that did and did not marginally elect candidates with certain characteristics were similar in other elections indicators and the resulting councils were balanced.

Following your advice, I have added comparison of other characteristics of the marginally elected candidates to the co-variate balance. As you correctly expected, the marginally elected candidates are indeed different in other characteristics. Marginally elected women are less likely to be entrepreneurs, and vice versa. Educated candidates are more likely to be women and less likely to be entrepreneurs. To make sure I avoid contamination of results, I control for other characteristics in the outcome regressions. The results are not affected significantly.

Chapter 3: Can a Natural Disaster Change Local Political Candidacy?

1. It would be useful to link more closely the paper with the previous literature on the impact of natural disasters. For instance, the main electoral variable that has been considered previously is the vote share received by the incumbent. The author reports that, in this case, there is a small increase in the support for the incumbent (at the local level) which is not statistically significant. This is an important result in itself which should probably be stressed (the scientific importance of findings does not depend on whether they are significantly different from zero or not).

Thank you for pointing it out. Indeed, I do not find any effect of the flooding on the incumbent support, whereas there are studies that do. I added the discussion of this stream of literature to the introduction.

2. *The findings are very suggestive but, as Maniadis, Tufano and List (2014) remind us, ‘one swallow doesnt make a summer. I wonder whether it would be possible either to think about a more general theoretical framework that allows to examine additional testable implications (somehow in the spirit of Fowler and Montagnes 2015) or whether in would be possible to verify whether these results also hold in some other similar context (e.g. other natural disasters in the Czech Republic that affect similarly local councilors). That type of evidence would make the evidence even more convincing and would help to mitigate the concerns with multiple hypotheses testing.*

I appreciate you raising this discussion. I agree that having at least one more similar ”experiment” that would show the same result would reinforce my findings. The flooding in Moravia that happened earlier could be a candidate event. I have included finding the necessary data and other potential events on my agenda.

3. *The literature review could be more comprehensive. The author may want to check the recent work by Ramos and Sanz (2018). Their paper is very related to this chapter and its cite several useful references that are missing here.*

Thank you for the reference. I have added it to my literature review, together with other references from the paper.

References:

- Baskaran, T., Hessami, Z. (2018), ‘Does the election of a female leader clear the way for more women in politics?’, *American Economic Journal: Economic Policy*, forthcoming.
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- Brollo F., Troiano U. (2013), ‘What happens when a woman wins an election? Evidence from close races in Brazil’, *The University of Warwick Working Paper Series*, 161.
- Broockman D.E. (2014), ‘Can the presence of additional female politicians and candidates empower women to vote or run for office? A Regression Discontinuity approach’, *Electoral Studies*, 34, 190-204.
- Calonico S., Cattaneo M.D., Titiunik R. (2014), ‘Robust nonparametric confidence intervals for regression-discontinuity design’, *Econometrica*, 82(6): 2295-2326.
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- Clots-Figueras I. (2011), ‘Women in politics. Evidence from the Indian states’, *Journal of Public Economics*, 95, 664-690.
- Ferreira F., Gyourko J. (2014), ‘Does gender matter for political leadership? The case of U.S. Mayors’, *Journal of Public Economics*, 112, 24-39.