Dear Audinga,

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

I believe I was able to address all your comments.

Below are the detailed answers to each of the suggestions.

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

1. As women represent half of the population, equal participation in political decision-making contributes to legitimizing the democratic body (Stevens, 2007). This may be worth mentioning among the major reasons to achieve the gender balance in politics.

Thank you, the reference to Stevens (2007) definitely enriches the motivation for the paper. The reference has been added.

2. Using data on German local elections, Baskaran and Hessami (2018) show that a quasi-random election of a female mayor leads to more female councilors elected in the future. The authors also show that female candidates receive more preference votes pushing them to higher positions of the initial (party-composed) ranking, suggesting that exposure to a female mayor helps removing negative voters bias against females. The author should introduce a reference to this paper and clearly explain own contribution and differences with respect to Baskaran and Hessami (2018).

Thank you for the very relevant reference. I have added it to the Introduction section.

While voter bias is the main focus of Baskaran and Hessami (2018), my paper is about political participation of female politicians. One of my side results shows that the main effect of fewer women among candidates is stronger in the municipalities where several other women were elected alongside the marginally elected. This suggests that if there is any voter bias in the Czech municipalities, it is likely to go the opposite way than in Baskaran and Hessami (2018) and is ex-ante incorporated in the decision by potential candidates to not participate in elections, or in the decision of parties to not nominate additional women.

3. The reference to Campa (2011) should be updated to Campa and Bagues (2017).

My apologies, thank you for noticing and pointing out. All has been changed.

4. It would be interesting to know more about the size of the effect on the participation of new female candidates. How large it is? To how many women does it correspond?

The table with the main results presents the effect of an additionally elected woman on the number of women who participate in the following elections, it is thus not in shares. Therefore, the point estimate equal to 1 corresponds to 1 additional female candidate in the next elections.

5. The author argues that the competitive elections setting studied in her paper is substantially *different from settings in which the imposition of gender quotas acts as a shock to female political representation. It is important to clarify that in many of these settings, e.g., Italy or Spain, elections are in fact competitive, as the quotas only apply on the candidate lists and not on the share of elected politicians, i.e., all candidates on the lists compete for the votes in a regular competitive election and the outcome of electing more females is not guaranteed. Therefore, the sharpest distinction may be drawn between settings with seat reservation as India and other competitive settings in which female candidates compete for electorate votes (with or without quotas).*

I agree that the election is still competitive if a gender quota is applied to the candidate pool. However, I do also believe that the pool of candidates the electorate are choosing from is slightly skewed. Also, I am citing the evidence from the literature Clayton (2015) that documents potential negative attitude of electorate to an unnatural pool of candidates.

I have added a sentence to stress the fact that elections remain competitive in case a quota is applied to the pool of candidates.

6. The author proposes that a sufficient female representation may be the mechanism explaining why less female candidates choose to run for office. I believe this concept is imprecisely defined. Given that women represent half of the population, the sufficient democratic representation would amount to roughly a half of elected politicians being female (Stevens, 2007). This does not seem to be the case: overall, women seem to correspond to roughly 30% of candidates and to around 20% of elected politicians in Czech Republic (as a side comment - I would suggest introducing a table with the basic descriptive statistics for the overall sample, something like a combination of Table 1.2, Panel A and Table 1.5). The analysis then compares EDs with at least 3 women elected with EDs in which less women were elected to compare localities with and without sufficient female representation. From the descriptive statistics, one may gather that this indeed may correspond to about half female elected politicians in many small municipalities (3 out of 7 or 8 council members, on average), but most likely is far from the balanced representation in larger municipalities. I would suggest framing this part as heterogeneous effects section and comparing the effect for municipalities with close to 50% of elected female politicians and those with far from 50% of elected politicians. If the author instead prefers to keep the analysis as is, then she should provide a more precise definition of what is considered to be a sufficient representation.

That is a fair remark. I changed the text to explain that I use the term "sufficient" relating it to the side result of the paper that the effect is stronger in the municipalities where more women were elected together with the additional woman. I use the term "sufficient" to refer to the revealed preferences of the community - candidates themselves, parties or electorate, that resulted in fewer new female candidates participating in the elections. The representation is thus likely sufficient from the community point of view, not from the economic theory point of view. I made this point clear in the text.

I have added average age of candidates and number of elected female candidates to the Table 1.2 such that descriptive statistics for the whole sample are complete.

7. The RDD estimates are done using the close elections sample, i.e. using races in which the vote difference between marginal candidates is smaller than 5 or 2 percentage points. My feeling is, however, the typical RDD methodology prescribes using several different methods to establish the relationship (e.g., see Baltrunaite et al., 2016; Bagues and Campa, 2017). Therefore, I would suggest the author to make better use of these methods. In particular, the author could try doing at least some of the following: 1) include the discontinuity plots for the main outcome variables (not only the share of new female candidates); 2) implement global regressions using a wider range of victory margins and including different degree polynomials in the victory margin (not only the quadratic form); 3) resort to optimal bandwidth selectors proposed in Calonico et al. (2014) to determine what should be considered a close election (and perhaps show local linear regressions results within these bandwidths) 4) show that the estimates are stable in models with and without controls.

Thank you for the very useful suggestion.

1) The graphs for the total number of female candidates and for the number of female candidates excluding the marginal ones were added to the Appendix B.

2) Gelman & Imbens (2017) do not advise using higher order polynomials in regression discontinuity design for 3 reasons: first, one faces the weighting issue; second, there is an issue of choosing the proper order; third, higher order polynomials result in poor inference. For these reasons I prefer not to conduct the analysis using higher order polynomials.

3) I have estimated the model using the optimal bandwidth proposed by Calonico et al (2014). The results remain valid and are presented in Table 1.A.9.

4) I have estimated the model with optimal bandwidth with such controls as average age and education of candidates, as well as budgetary indicators. The point estimate of the treatment indicator remains stable and statistically significant.

8. In determining future female political participation outcomes, the performance of female politicians while in office may matter. On the one hand, if successful, they may act as role models. On the other hand, if females perform poorly as policy makers due to reasons which may not be observable for the electorate other women may be discouraged from entering into politics. It would be very interesting to see some analysis in this direction, as this may potentially explain the findings.

Thank you for an interesting insight. The data only allows me to study the response from electorate and the response from parties' leaders. I mention in the Main results section that the main effect of electing an additional female candidate is not stronger neither in the municipalities where the marginally elected female candidate was successful (i.e. was elected again with non-trivial margin), nor in the municipalities where the marginally elected female candidates were not successful (i.e. were behind the marginally elected candidate with a non-trivial loss margin). I conclude that I do not observe any confirmation that the performance of the marginally elected female candidates could be driving the main result.

Separately I explored the response from the parties by analysing the placement of female candidates on the slates. I did not find any differences in the initial position the parties assigned to the female candidates in the treated and control municipalities. The analysis is not reported in the paper but is available upon request.

9. Descriptive statistics tables would be more informative if they showed shares than numbers.

I do agree. However, since the main analysis is done using levels and not shares I prefer to keep the descriptive statistics the way they are. Moreover, the paper was published this way and I would not want to make major changes at this stage.

10. The author could better exploit the rich data on party and voters decisions. For example, instead of focusing on the median position of female candidates on slates, the author could better study transitions from initial party-decided ranking to final preference-votes-based ranking of candidates to shed light on voters or partys preferences for female candidates (in a similar vein as Baskaran and Hessami, 2018; Baltrunaite et al., 2016). Disentangling the role of the party preferences and the role of voters preferences would also be very interesting.

It would indeed be interesting to understand the input of parties' and voters' preferences. However, I was not able to extract a meaningful insight from the data. I have tried as an outcome the change in the initial and final rankings of female candidates. I do not observe any significant difference between this change in the treated municipalities as opposed to the control ones.

I have additionally estimated the model for the number and share of female candidates who have climbed significantly from their initial ranking due to extensive voting for them. I find that there are fewer female candidates like that in the treated municipalities, which is likely a mechanical difference since there are fewer female candidates in general.

11. Bagues et al. (2017) show that female presence on selection committees does not have a positive impact on female promotion, if anything the effects is actually negative. This could be a useful reference to corroborate the idea that female presence on decision making bodies may indeed harm future outcomes for females.

Thank you, the citation has been added.

12. The discussion in the robustness section is somewhat vague: the author discusses several problematic issues, which in principal should have been already addressed by the RDD methodology (such as the manipulation of the forcing variable which determines the victory chances and the identity of marginal candidates). This section should be revised (alternatively, if the author meant *something else, this section should just be made clearer).*

Thank you for raising the concern. However, I want to stress out that this section is not about manipulation of the forcing variable. In the data I do not observe any manipulation of the victory margin, that is confirmed by the absence of any jump on the histogram demonstrating density of cases around the cut off point (Figure 1.1).

This section is about how the actions of parties and electorate could influence which candidates become marginal. Again, from the distribution of the forcing variable we see that there is no manipulation. However, since the data allows, I am checking this assumption in these additional ways - by testing that parties' decisions did not influence which candidates become marginal, and by testing that electorate did not influence that either.

I have added this clarification to the section.

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

1. In the first paragraph, the paper refers to the implications of the median voter theorem. The author could also mention the citizen candidate model (e.g., Osborne and Slivinski, 1996), which delivers substantially different implications on the relationship between elected politicians identities and public policies.

Thank you, I have added the citation.

2. Using data on Italian municipal elections, Baltrunaite, Casarico, Profeta and Savio (2017) show that gender-quotas-induced increase in the share of female councilors results in changes in the composition of local public spending. I would suggest introducing a reference to this paper and discussing how and why the findings differ. Perhaps most important, the author exploits an arguably small shock to female composition due to one additional marginal councilor, while the effects documented in Baltrunaite et al. (2017) are much more sizable. Can the author exclude that the null results are the artifact of the small effect size which then is not statistically detectable with a small shock? Otherwise, the conclusions should be more cautious, as it is not crystal clear that women policy makers do not make different decisions; it may as well be that the levers exploited in the paper are not strong enough. In order to shed light on this point, the author could resort to the heterogeneous effects analysis and compare the effect size for municipalities along the size of the shock induced by a random election of a given politician to the composition of the municipal council.

Thank you for pointing out this gap in my literature review. I have added the reference to the paper.

Regarding the possibility of the small size of the shock after electing just one additional candidate with certain characteristic, I explain that this is unlikely in the last paragraph on the results section.

I can reject this potential explanation in case of women and educated as additional candidates. I estimated the model for the municipalities with 7 or fewer candidates, the additionally elected candidate constitutes approximately 15% of the resulting council, creating a large enough shock to the council composition. I do not observe any effect of electing additional women or educated candidates on the main spending categories.

In case of entrepreneurs as marginally elected candidates, I find that in the municipalities with 7 or fewer councilors capital spending on administration has decreased significantly after electing an additional entrepreneur. At the same time, the effect on the current spending on fire fighters is not statistically significant estimating the model using the optimal bandwidth. It seems plausible that councilors in small municipalities could have different preferences over municipal spending and the larger shock to the council composition is not the only difference of small municipalities compared to all municipalities. I thus prefer to keep this analysis for future at this point as it requires further investigation.

3. Using data on Spanish local elections, Curto and Gallego (2018) show that educated politicians are more fiscally conservative, spend less in capital investment, and prioritize different spending areas. They also propose that more education may rather be a proxy of political preferences, rather than quality. Also this paper would seem as a useful reference to me.

Thank you, the reference has been added.

I have contacted one of the authors of the paper and learned that their results regarding the likely different preferences of educated politicians are not entirely robust at this point. I am thus only citing the result on the quality of educated politicians which they are certain about.

4. What is the role of the municipality x time fixed effects in equation (2)? They may absorb substantial variation in the data and serve to reduce standard errors, but it would be comforting to see that the estimated coefficient from the model with and without these fixed effects are comparable.

In the main specification I include *municipality * elections* fixed effect among others. I am following Cellini et al (2010) in that. Also, from the co-variate balance we know that treated and control municipalities remain not perfectly balanced in certain co-variates, which is not unusual even for experimental designs. I thus prefer the specification with *municipality * elections* fixed effects.

Nevertheless, I have estimated the model without *municipality * elections* fixed effects. What I observe is that some coefficients remain fairly stable, whereas others change. For example, the statistically significant coefficients that show the effect of electing an additional female candidate on debt per inhabitant in the global RD specification remain stable when estimating the model without *municipality * elections* fixed effects. On the other hand, the coefficients that show the effect of electing an additional entrepreneur on current spending on fire fighters changes substantially. The underlying reason is that the municipalities that eventually elected entrepreneurs marginally happened to have different level of current spending on fire fighting before elections. In this regression it is thus important to control for the *municipality * elections* fixed effects.

5. I would be more cautious regarding the interpretation of a non-significant effect for the entrepreneurs, in particular, because the coding of the variable seems to be particularly noisy (there is no obligation for candidates to report their business background). If the author wants to push this result further, the measurement should either be improved or better defended. In alternative, the direction of the bias stemming from the measurement error could be discussed.

Thank you for raising this issue. It is indeed possible that the self-employment status is under reported. I have added to the paper the discussion that under reporting of the self-employment status could lead to downward bias in coefficient estimate and to a failure to identify the effect of electing additional entrepreneurs to local councils.

6. Is the author doing something particularly innovative regarding the voters negative bias towards women and a positive bias towards educated candidates, already documented in Juradja and Munich (2016). If not, these results could be discussed less extensively. If yes, the contribution could be better explained.

I removed this part of analysis as it indeed does not bring new insight in addition to that documented by Jurajda & Munich (2016).

7. I would suggest trying a log specification for all monetary variables in regression specifications (as opposed to entering them in levels, as is done in the current version of the paper). This would also make the interpretation of the findings more straightforward. Relatedly, the composition of the spending may depend on the total spending available to the municipal council. Since there is some indication that totals change, it would be useful to consider each spending item relative to the total (i.e., define them as shares, in alternative to levels or logs).

In using levels I was following Cellini et al (2010). However, I do agree that expressing the monetary outcomes in natural logarithms is preferable. I am only reporting the output for the outcomes expressed in logs in the new version of the paper.

In my point of view, the indication of changes in overall revenues or spending is rather small. I do, however, test the only consistent result I observe - the negative effect of electing an additional entrepreneur on the current spending on fire fighting. I express this category of spending as a share of total spending. The point estimate is economically small at approximately 0.2% and statistically significant in the specification where I pool the years after together, and in the years 1 and 3 in the specification where I split the years after the election. The respective output is available upon request.

8. How much budgeting decisions in the municipality may be affected by a single councilor? This *needs to be discussed, referring to the institutional setting. If the major decision making power is concentrated in the hands of mayors, then it is not surprising that councilors characteristics do not matter; this does not mean though that personal characteristics of politicians do not matter. The conclusions of the paper may thus change substantially.*

Thank you for raising this important concern. I have added the explanation to the institutional background that it is indeed the case that in Czech municipalities the majority of work is performed by the mayor and the deputy, while the other councilors mainly participate in discussion of issues and voting. Nevertheless, especially in the small municipalities, the marginally elected candidates are likely to have the opportunity to actively participate in the discussion, express their opinions and thus have a sizable impact on the actual decisions of the council.

9. The point regarding the distinction between competitive and non-competitive elections (point 5 in the previous paper) also applies here.

Thank you, I have stressed in the text that elections with gender quotas are competitive. However, I have also noted that the side effect the quotas might have on electorate is that the latter have to choose from among a not natural pool of candidates, which might affect their attitudes towards candidates (Clayton 2015).

10. For the further development of this paper I believe it would be interesting to exploit random elections of councilors with different personal characteristics to construct shocks to council diversity and study how diversity of the decision-making body affects the size or the distribution of public resources, or even the fiscal discipline.

Thank you for an interesting suggestion. I will think on how I could develop such a measure of diversity and will attempt to perform the proposed analysis in the future.

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

1. The research question of the paper could be framed in a more general manner. For example, the event of a natural disaster may be used as a natural experiment to study more general phenomenon, such as expanding the decision making power of local governments, or increasing decentralization (this is in part mentioned by the author on page 97).

I agree that generalizing the research question helps the interpretation of the results and helps to better fit the paper in the literature. However, I want to be careful not to generalize the research question too much in order to avoid false claims. To generalize my research question more than it was, I stressed in the introduction and conclusion that I am studying the effect of giving larger power to councils, which resulted from the additional responsibilities laid on the councils after the flooding, on the local candidacy.

2. It is likely that the flooded municipalities experienced not only a political shock increasing local governments involvement in managing the municipality, but also suffered from a negative *economic shock due to, for example, temporary or prolonged disruption of the economic activity, etc. Then, the economic voting hypothesis (Downs, 1957) would suggest that the documented changes in candidacy strategies may be explained by candidates adjustment to anticipated changes in voting patterns (e.g., if the incumbent nation-wide party is held accountable by the electorate for the worsening of economic conditions due to flooding). The author should discuss whether this is a likely scenario and whether it is possible to exclude that local independent candidates changed the slates on which they want to run as a response to a negative flooding-induced economic shock. It seems to me that it is another plausible explanation, on top of other proposed ones on independent candidates gaining experience or developing a closer link to the local electorate.*

Thank you for drawing my attention to the economic voting hypothesis. I argue that this hypothesis is not a likely alternative explanation to the effect I observe one electoral cycle later. Before the independent candidates had adjusted their behaviour they had the chance to observe the voters' behaviour in the elections three months after the flooding. Three months is a sufficient time for the incumbents to show their response to the disaster and for the electorate to form their attitude towards the incumbents and local political entities and to reflect their preferences in their voting behaviour. However, I do not observe any change in the voting behaviour in these elections immediately after the flooding. I thus feel that I can reject the economic voting theory as a potential explanation behind the independence of the local candidates.

3. Geographic location of the municipalities should be correlated with the probability of flooding all flooded municipalities should be very close to a river, etc. It would be interesting to check the robustness of the results to comparing treated and control municipalities with a similar geography.

Taking into account the geographic component and comparing the flooded municipalities to those that are also close to the river but maybe just marginally further such that they did not get flooded was under consideration as a empirical strategy for this paper. There are, however, two following limitations. First of all, there is no easily available data on the location specifics of each municipality. Secondly, it is possible that the unaffected municipalities that are located nearby those flooded could be considered as partly treated as well. It is likely that the municipalities that are neighboring those flooded participated in helping their neighbours to solve the flooding aftermath both financially by donating part of their budget, and in terms of actual effort. It is thus possible that the local candidates in the unaffected neighbouring municipalities can be considered treated as well, since they likely also participated in rebuilding the (neighbouring) community, potentially together with their own community members and also received similar experience as their colleagues in the affected municipality. That being said, I am keeping this analysis in mind as a potential future extension to the paper.

4. It seems to me that observing any effects on candidacy in the 2002 elections is extremely unlikely as there was virtually no time to form new slates for independent candidates and meet the *legal requirements to present these slates in the election. Therefore, in my opinion, this reason could be used to dismiss all the lengthy discussion of why there is no effect in 2002.*

Thank you for this point. Although I did mention the idea that there was likely not enough time for the local candidates to create their slates on time, I do agree it must be stressed more. I rephrased the discussion of no effect in 2002 elections into the following argument. In my setting I cannot test whether the local candidates had the incentive to be more independent in the council in order to help the municipality with the rebuilding works due to the short time span between the flooding and the elections in 2002. However, if that was the only motivation of the independent candidates, I would not observe the effect in the later elections.

5. The author argues that having to deal with the aftermath of the flooding raised the activity of independent candidates. However, this rather looks like an increase in the number of tasks they have to deal, not their effort level. Does that change the story somehow?

Indeed, the flooding first of all increased the number of tasks that the councils had to perform. In my point of view, solving more tasks requires putting more effort. I thus see the two as synonyms. If we were to think of the effort as a separate dimension from the number of tasks, I argue that the level of effort likely also increased since the issues related to the flooding aftermath had to be addressed without any delay and the local politicians had to work under pressure. I therefore conclude that both the number of tasks and the effort were higher in the affected municipalities after the flooding. In the paper I do mention that the number of tasks has increased after the flooding.

6. It would be interesting to compare the effect in municipalities which experienced a large vs. small shock to their public finances, as this would be indicative of the shock to the workload or activity of local governments (personally, I would find that more convincing than comparing small and large municipalities with an argument that the shock was larger in small municipalities).

Thank you for this insightful suggestion.

First, I plot the subsidy per inhabitant received from the central government budget before and after the flooding (Figure 3.7). Second, I estimate the model dividing the treatment indicator into two depending on the subsidy received from the central government - treated and received higher additional subsidy than median level of additional subsidy per inhabitant, and treated and lower additional subsidy than median additional level of subsidy per inhabitant (Table 3.4). The results indicate that the main findings hold in the small municipalities where the shock to the received subsidy was higher, i.e. which were damaged more. In the small municipalities that received lower than median additional subsidy, the major parties were more likely to not submit their slates, however the independent candidates from those slates seemed to simply not participate in elections.

In the large municipalities, the subsidies per inhabitant increased to a much smaller extent. I do not observe the same effect in the large municipalities, irrespective of the amount of additional subsidies they received. The likely reason is that larger municipalities must have similar in terms of size part of the municipality damaged by the flood as in the small municipalities. Thus, relative to the entire municipality, the shock of flooding is smaller.

7. I do not find the argument on page 117 that preferences of electorate did not change in elections following the flooding fully convincing, because the composition of candidates running with a given slate changed, as the paper documents itself. The argument regarding voters preferences should therefore be better developed.

This is a very fair point, thank you. Indeed, the composition of slates changed. The data not only allows me to see on which party slate a given candidate is running, but also the candidate's affiliation. I added to the Figure 3.9 vote shares for the nation-wide party representatives and for the representatives of independent slates. The main message is not affected - the electorate did not change their preferences towards nation-wide party candidates, neither towards independent candidates.

8. Given that the paper exploits a very particular setting (not everywhere party lists may include independent candidates) to document a rather subtle change in candidacy, it would be useful to discuss the external validity and generalizability of its results.

Generalizability of the results is indeed a relevant concern. I argue in the paper that even though the Czech local elections system is unique in that candidates can run on different slates, the main result of my analysis is that empowering local politicians can make them more involved. Involvement of locals in political life is a goal in many countries. Therefore, if my analysis suffers from the lack of generalizability, it suffers only marginally.

Minor points

1. The definition of independent candidates should be made clear and uniform throughout the paper. As far as I understand, the author to refers to independent candidates as candidates without a nation-wide party affiliation on a candidate list without a nation-wide party affiliation in the third paper.

The definition of "independent candidates" is the following: these are the candidates who do not identify with any political party - nation-wide or any other party. The definition remains consistent across all three chapters.

In the third chapter I refer to independent candidates as those without not only nation-wide party affiliation, but also without any other party affiliation. I refer to independent candidates who are represented on a slate that does not belong to any party - nation-wide or any other party - as independent candidates on independent slates. This is described in paragraph 4 of section institutional background.

I have clarified the definition in all three chapters.

2. The exposition style could be improved. The identification strategy could be presented more schematically, in particular, regarding the identifying assumptions. The discussion of the results could be made more concise, giving a clear distinction also in terms of the space dedicated between the main outcomes and auxiliary results.

Thank you for this advice. I have advanced the discussion of the identifying assumptions in all three chapters. I believe the discussion of the results is more concise after having addressed other comments.

3. There are some tables missing from the paper and some references are mixed up (e.g., the first chapter refers to Table 3.2, which only appears in the third chapter and includes different analysis). This should be corrected for the final version of the thesis.

Thank you for point that out and apologies for the inconvenience. I believe I have now corrected all the inconsistencies.

References:

Calonico S., Cattaneo M.D., Titiunik R. (2014), 'Robust nonparametric confidence intervals for regression-discontinuity design', *Econometrica*, 82(6): 2295-2326.

Cellini, S.R., Ferreira, F., Rothstein, J. (2010), 'The value of school facility investments: Evidence from a Dynamic Regression Discontinuity Design', *The Quarterly Journal of Economics*, 125(1): 215-261.

Clayton, A. (2015), 'Women's political engagement under quota-mandated female representation: Evidence from a randomized policy experiment', *Comparative Political Studies*, 48 (3), 333-369.

Gelman A., Imbens G. (2017), 'Why high order polynomials should not be used in regression discontinuity designs', *Journal of Business & Economic Statistics*, DOI: 10.1080/07350015.2017.1366909.

Jurajda, S., Munich, D. (2016), 'Candidate ballot information and election outcomes: the Czech case', *Post-Soviet Affairs*, 31(5): 448-469.