I would like to thank the three reviewers and the editors for your very valuable feedback. I have addressed your questions and implemented your recommendations to the best of my ability, and have found the quality of the manuscript to have greatly improved as the result. Please find my response to each individual reviewer, organized by each point they raised, below. I hope you find them satisfactory.

**Reviewer 1**

*On framing and theory*, the reviewer suggests I focus more on the response to the information that the regime thinks it is getting, instead of the informational tradeoffs. The reviewer raises the issue that, given high level of manipulations, information from elections here should not be useful.

I thank the reviewer for pointing out the ambiguity in my original argument. I would like to clarify that my paper rests on the premise that manipulation, if consciously designed, may actually *increase* the informational value of authoritarian elections. Even though it is not the theoretical thrust of the paper, this premise is important to understand how authoritarian regimes are fully expecting to receive (and thus react to) information from manipulated elections’ results, instead of just discarding the signal.

To make this point clear, I choose to engage with the manipulation-information tradeoff more thoroughly instead of less. In particular, I have added a new Section 2 to discuss the conditions in which manipulation can lead to better information. Specifically, I argue that when authoritarian regimes selectively manipulate certain aspects of the process but leave others free, the selected manipulation strategies can act as noise filters while the unmanaged aspects generate information without threatening the regime. Further on in Section 3.2 of the paper, I argue that the CPV’s electioneering exemplifies this selective manipulation logic. In other words, the CPV was plausibly doing its best to ensure elections generate some information. The rest of the paper then follows: we cannot know what specific information this could be, but post-election responses may lead us to a definite answer.

This new section acknowledges the Rozenas (2016) story but explains why it does not contradict my paper’s premises. Specifically, the Rozenas story is that manipulation deteriorates information when it is not intentionally limited in scope. In my and the others’ accounts, manipulation facilitates information when it is used with high intensity but limited scope.

*On the distinction between Mexico’s punishment regime and Vietnam’s placation strategy in footnote 17*, the reviewer asks why voters rebelling against the incumbent instead of supporting the opposition matters. The reviewer asks if the key distinction is that one regime is a single-party regime whereas the other is a hybrid regime. The reviewer recommends more development on this theory about regime types.

A point of clarification: districts with central candidate defeats did not “go to the opposition,” but only to other regime agents who work at the local level (see page 8 of the manuscript). In light of this, the distinction is that in Vietnam the regime only faces individualized unhappy citizens who are not acting collectively and will not be mobilized into an organized movement. In Mexico, punishment works not only because it deters citizens from voting the “wrong” way, but also because it weakens party-building efforts of the opposition party. In Vietnam, there is no need to weaken party-building efforts of the opposition, because there is no opposition. Placation addresses the deeper problem of citizens feeling dissatisfied.

This difference is mostly about the possibility of mobilized and organized opposition, but this is likely to correlate with regime types. Some exceptions exist – groups that can mobilize displeased citizens into an organized threat in single-party regimes (e.g. the Falun Gong movement in China), or opposition parties that are too disorganized to have any mobilization capability in hybrid regimes (e.g.in Tanzania before 2015) – but these are rare. Ultimately, the reviewer’s suggestion to elaborate on the hybrid vs. single-party regime story is an excellent advice, and I have followed it by elevating footnote 17 to a paragraph in the new manuscript’s Section 8, which discusses my empirical test’s generalizability.

*On the budget process*, the reviewer requests for a more detailed description of the budget negotiation process. In particular, the reviewer requires evidence that the revised 2015 State Budget Law does not confound the result. Also on the budget process, the reviewer asks whether the fixed budget cycles of 3-5 years should produce no long-term shift in provincial budgets.

The reviewer’s suggestion is extremely helpful; I have followed it by adding a description of the process in Section 4.1. In this section, I also explain that despite some components of the budget (e.g. the sharing rates for revenues that provinces must share with the central government) being fixed for cycles of 3 to 5 years, the cycle has always begun right after one election and ended before the next. This means that the outcome of the negotiation for these components could be part of the post-election responses i.e. it could be part of the treatment effect. If this is true, a long-term effect is entirely possible.

In regard to the revised Budget Law, the reviewer is absolutely correct in pointing out that it could lead to a spurious relationship. (Although the confounding is the same, its mechanism is opposite of what the reviewer suggests: the new law actually made richer provinces retain *less,* not more*,* revenues. Yet, Table 1 in the paper also shows that these provinces are also *less* likely to suffer central candidate losses.)

At the same time, the bias is unlikely to be detrimental. I have added a new section in Appendix B (pages 5-6) detailing the effect of the Budget Law on provincial budgets. I show that, among the law’s targets, only Hanoi, Ho Chi Minh City and Binh Duong also experienced close central candidate defeats or victories. With the first two already excluded from the analysis, Binh Duong remains the only province where confounding could manifest. Given that it is also a highly influential outlier, I have decided to exclude it. The results in Sections 6 and 7 of the new manuscript are still large and statistically significant.

*On empirics*, the reviewer suggests that finding similar results for the 2011 and 2007 elections would strengthen the paper and allay concerns about potential confounding.

I follow the reviewer’s recommendation and conduct similar analysis for the 2011 and 2007 elections. For both elections, absence of vote percentage data leads to serious imbalance in the data. However, I find some evidence that the treatment effect is most likely positive for 2011 and 2007. I have presented this analysis in Appendix C (pages 7-17), and also preview it in Section 7.1 of the new manuscript.

*On the operationalization of poor performances*, the reviewer inquires whether changes in central candidate performances from previous elections would capture better the surprisingly poor results.

I find that there are several potential issues with using the changes in central candidate vote shares from previous elections. Most importantly, because vote share data is not available for defeated candidates in 2011, there would be non-classical measurement error. In addition, incorporating election results from 2011 into the treatment variable for 2016 would cause mechanical dependency between present treatment and past treatment. Finally, because negative and positive vote swings are conceptually different, any analysis must allow for non-linear effect.

Even in light of these problems, I have included in Appendix G (pages 23-24) an analysis of vote share changes’ effect on central transfers, using quadratic terms to account for non-linearity. I find that the effect estimates are small but tend towards positive values for big vote swings. The estimates shrink when vote share changes and central candidate defeats are in the same model, suggesting that both operationalizations are capturing the same pattern. Given the measurement problem, this evidence does not lead to any definite conclusion, but still aligns with the idea that poor performances drive transfers.

**Reviewer 2**

*On scope condition*, the reviewer suggests that I discuss scope condition clearly and early, in particular the fact that elections in Vietnam are uncompetitive, and that the literature have often focused on hegemonic regimes where manipulation is rampant (and potentially weakens the informational values of elections).

In describing the election in the original manuscript, I have explored in details how the CPV’s electioneering has helped deliver results nearly identical to what the regime had planned before the elections. I also described how it specifically ban some independent candidates from running, and how nearly all candidates are still regime agents, even when they compete against each other.

To connect these features of the VNA elections to larger questions about scope condition, I have added a new Section 1 to the new manuscript to discuss the conditions in which authoritarian elections may be informative. I argue that authoritarian elections may deliver useful information when they are held by strong hegemonic regimes who can vary their level of manipulation across different strategies. This argument clarifies the scope condition not only for the paper, but also for the literature on informational theories of authoritarian elections as a whole.

*On the literature review*, the reviewer recommends a number of key scholarship that should be included. In discussing this scholarship, the reviewer asks whether the legitimation function it raises is compatible with the case of Vietnam.

I thank the reviewer for the helpful suggestion. Where space permitted I have included reference to this scholarship, most important of which is Morgenbesser (2016). In the newly added Section 1, I build on this work to argue that information gathering is possible if the regime exercise restraint in terms of which manipulation strategies to use (instead of just the level of manipulation). This strategy, however, is feasible only in hegemonic regimes, especially those backed by a strong and efficient ruling party.

Regarding the legitimation function, I added to Section 3 a discussion about how other functions of authoritarian elections may reduce the range of information that autocrats can seek. In regimes like Vietnam, securing legitimacy through overwhelming victories is one of these other functions. My argument then follows: it is because of the restriction placed by other functions of elections that autocrats cannot make elections reveal all the possible information.

*On the impact of central candidate defeats to the regime*, the reviewer raises the possibility that some defeats may even be desirable for the regime. The reviewer suggests that these defeats may serve a legitimation function by demonstrating to the public that the election is competitive. In addition, they may help the regime get rid of some undesirable elites. If this is true, increases in central transfers should be seen as reward for provincial officials, rather than a placation strategy towards citizens.

I thank the reviewer for raising this possibility, but believe it is unlikely to hold in Vietnam. First, the CPV has never sought to convey that the election is “competitive.” In official narratives the elections serve only to identify those who with competence and ethics (*có tài có đức*); they were not intended to provide a platform for comparison. In terms of legitimation, the legitimacy that the CPV can derive from VNA elections is more likely to come from lopsided results that signal high levels of consensus. Second, if it were to seek removal of some undesirable candidates, the CPV can easily do so with the Party’s internal bureaucracy, or in the pre-election negotiation rounds. Both options are much easier and less risky.

Empirically, if some central candidates are positioned to fail, we should see them placed in more difficult districts, facing more capable local candidates. There is no evidence to support this.

In any case, if there are such undesirable candidates, their defeats are more likely to be heavy, because it is incentive-compatible for provincial officials to engineer their losses. All the analyses in my main paper, however, focuses only on close central candidate defeats.

*On local elections*, the reviewer requests clarification on the function of local elections, especially on whether it already serves the CPV’s information needs.

The reviewer’s feedback brings up a very important question. In short, local elections do not provide useful information *for the central government* because the latter cannot directly manipulate local elections as much as they do national elections. For example, the central government does not send candidates representing them to contest in local elections. In addition, local elections are prone to capture by local elites (see Malesky and Schuler 2011, and also Malesky, Nguyen and Tran 2014). It is entirely possible, however, that lower-level party organizations are deriving good information from these elections, but this information is unlikely to be faithfully relayed upwards. In other words, the value of local elections is limited by center-periphery conflict and the local levels’ control over local elections. It is telling that the CPV is experimenting with abolishing elections at the lowest levels of government.

*On the secret ballot*, the reviewer asks for more evidence that the secret ballot does allow citizens to vote the way they want to.

The reviewer is right to suspect my original claim about the secret ballot. My original intention was only to show that some citizens feel that individual ballots are anonymous. I have thus modified the language in footnote 12, and added a new footnote 13 citing data from the Perception of Electoral Integrity survey showing that violation of ballot secrecy is a far lesser problem compared to other forms of violation. There is no pretension that elections in Vietnam are far from free and fair, but the public and experts alike agree that the most serious violations take place well before election day.

**Reviewer 3**

*On the empirical evidence*, the reviewer is concerned that the analysis in the paper focuses only on localized defeats in 2016, and that the sample size too small for any strong conclusion.

In the original manuscript, I restrict my analysis to 2016 because this election offers significantly more valuable data that is indispensable for an internally valid analysis. I thank the reviewer for pushing me to consider whether evidence from only this election is not sufficient for generalizability.

Following the reviewer’s feedback, I have conducted analyses for two previous elections in 2011 and 2007 in Appendix C (pages 7-17), and previewed its results in Section 7.1 of the new manuscript. In Appendix C (pages 7-17), I explain in details why the focus on the 2016 election was necessary, and then show that data problems in the 2011 and 2007 elections can prevent reliable estimation of the treatment effect. Specifically, because data on vote shares for the defeated candidates is only available for the 2016 election, it is not possible to identify truly close victories and defeats in the 2011 and 2007 elections. This results in imperfect measurement of the treatment variable. However, using the best alternative possible to get around this problem for 2011, as well as an exercise to identify the most likely treatment effect estimates that would be estimated if data was fully available, I show that the treatment effect is most likely to be positive for 2011 and 2007, consistent with the 2016 evidence.

Regarding the small sample size, I believe the issue is less serious because the sample of treated units in my analysis is very close to the universe of all treated provinces in Vietnam. For this finite sample, methods using exact tests through randomization inference should be adequate because they do not depend on asymptotic properties that only work with larger samples.

*On the theoretical argument*, the reviewer points out that voters may take advantage of the regime’s strategy by continuing to vote against the regime in future elections. The reviewer suggests that this makes the regime’s strategy risky and unsustainable, and recommends that I address it by finding out whether voters do learn to exploit the regime’s strategy.

I thank the reviewer for pointing out this potentially contradictory logic in my hypothesis about the CPV’s strategy. I have added a discussion of this issue in Appendix H. As data from previous elections is available, I was able to analyze voters’ behavior in subsequent elections, and found that, all else being equal, central candidate defeats are *unlikely* to repeat in the same province, even after accounting for the effect of increased central transfers. This suggests that the placation strategy is still sustainable, either because strategic voting by citizens is not happening, or is happening but not at a significant level. It is likely that strategic voting is difficult in practice, as it would require a large number of voters to coordinate, not just to vote against the central candidate, but also to agree on which same set of local candidates to vote for to avoid spreading their votes too thinly.

*On the promotion record of past provincial leaders*, the reviewer questions why evidence pertaining to past elections is relevant for this current election.

The reviewer is correct to say that promotion records of provincial officials who served in 2007 and 2011 say nothing about punishment for central candidate defeats in 2017. Indeed, they only show that central candidate defeats did not result in punishment for provincial leaders in 2007 and 2011. The analysis is only an indirect test: it shows that the CPV does not have a *history* of punishing provincial leaders for failing to secure central candidate victories. I am restricted to this indirect test because data on the career paths of officials after 2017 is not yet available, as most major personnel decisions that will affect these officials will only be announced at the upcoming Party Congress in 2020.

Overall, this indirect evidence is suggestive at best, but is helpful because it corroborates my observation that the CPV rarely resorts to punishing its agents, except for serious transgressions. Acknowledging that this is neither a strong evidence nor a central claim, however, I have modified the language in the manuscript and relegated most of the analysis to Appendix F (pages 21-22).

*On other issues*, the reviewer notes a typo on page 11.

I thank the reviewer for pointing out this error, and have fixed it accordingly.