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 find the quality of the manuscript to have greatly improved as the result.

The reviewers and the editors may notice that I have made a few major changes to the original manuscript. First, in the theory section, I have moved the arguments about the informational limits of authoritarian elections upfront and have merged the discussion of the selective manipulation strategy into this discussion. Second, in the empirical section, I have moved the discussion of the sample selection strategies ahead of the discussion of the empirical techniques. Third, I have consolidated the auxiliary evidence into two subsections of the results section. Fourth, I have rewritten the paragraphs on pages 23-24 on the magnitude of the treatment effects, fixing a mistake I have made in the original manuscript when interpreting the regression coefficients. I hope that these changes would allow me to address the reviewers’ primary concerns more directly.

In response to the reviewers’ comments, I have also made other specific changes to the manuscript. Please find my response to each individual reviewer, organized by each point they raised, below.

**Reviewer 1**

On the *theory*:

The reviewer suggests that I engage with arguments by Andrew Little and Jennifer Gandhi. Where it is appropriate, I have added citations to Little (2015, 2017) and Gandhi (2015) to the manuscript.

The reviewer also recommends that I address the theory that authoritarian regimes use elections to identify leaders for promotion. I have added this argument to Figure 1 on page 15 of the revised manuscript. In addition, I have added footnote 10 to page 12 to bring up Malesky and Schuler’s (2013) finding that the CPV did *not* use the results of the 2007 VNA election to identify cadres for promotion, which highlights an instance in which it has forfeited a possible function of authoritarian elections.

The reviewer further requests that I explore the regime’s rationale for interpreting the election results the way it did. I do this by providing additional argument and qualitative evidence on pages 12-13 of the revised manuscript. I argue that the CPV knows that central candidate defeats could be indicative of public disenchantment because a) it has received a large amount of petitions and complaints from the public prior to the election, and b) it knows from previous elections that many voters have expressed their dissatisfaction by casting abortive votes against even the strongest candidates.

At the same time, on pages 12-13 of the revised manuscript I note that the regime would have had reasons to believe in the alternative story if it had chosen to do so. This highlights the problem of any *post hoc* attribution of cause to electoral defeats. Instead, I argue that the regime must have made the decision about how to interpret the election results before the election even took place i.e. it must already expect to see the defeats as evidence of local dissatisfaction before they even happen, and move to placate as if it is following standard operating procedures. Later, on page 14, I restate this argument in general terms: “*regime leaders see in elections what they look for*.”

This argument is consistent with the idea that information collection using elections is a coherent and premediated strategy. It is also consistent with the fact that official narratives about central candidate defeats have always ambiguously discuss multiple and over-determined causes of defeats rather than focusing on identifying one, because being transparent about what it seeks from elections would only expose the CPV’s strategy and allow other actors to behave strategically.

On the *qualitative evidence*:

The reviewer asks for a table summarizing central candidate defeats in each of the three recent elections to check whether there is learning over time. I have included this table in Appendix I (pages 48-52), as part of a larger discussion on the effectiveness of the CPV’s placation strategy. Using this table and additional analysis, I show that defeats tend to happen to a small group of provinces (indicating underlying problems) but also that defeats tend not to happen repeatedly to a province within this group (indicating some learning over time). I also mention this discussion on page 31 of the revised manuscript.

The reviewer also requests for more details about the central nominees, and I have added this information to page 8 of the revised manuscript. In particular, I note that there are three subgroups of central candidates: a) the regime’s very top leaders, b) current VNA leaders and other officials who are expected to assume VNA leadership positions once elected, and c) leaders of state-sponsored mass organizations under the Vietnam Fatherland Front. Candidates from the VNA bloc are slated to hold VNA leadership positions, which in addition to committee chairs also include deputy chairs and ordinary membership.

Regarding the defeated candidates, I note on page 10 of the revised manuscript that they belong only the latter two groups, which comprises the majority (nearly three quarters) of the central candidates, and that they are also expected to hold leadership positions in the VNA like most other central candidates. Finally, I also add an Appendix G (pages 27-41) which provides detailed accounts of what happened in each province that experienced central candidate defeats (excluding Hanoi and Ho Chi Minh City). This new Appendix G provides detailed information about the defeated central candidates’ backgrounds as well as those of their local competitors.

The reviewer recommends that I provide examples of how provinces used the additional state transfers. I thank the reviewer for this recommendation, as this is a very important piece of evidence that would help enhance my quantitative results. I have thus added to Appendix G (pages 27-41) a detailed list of public projects being implemented in 2017 for every province for which data is available. Highlights of these projects, which include new hospitals beginning construction in Can Tho, Soc Trang, and Tra Vinh, major road constructions in Can Tho and Soc Trang, and inland waterway improvement in Tra Vinh – all of which are highly demonstrable – have also been added to page 29 of the main manuscript, as part of an expanded section on the downstream effect of central transfer increases.

Finally, the reviewer suggests I provide accounts of the central candidate defeats in the state media. After a careful review of news coverage from different media outlets, I found that most coverage of central candidate defeats tend to be light on the analysis, such that they only report the defeats without attempting to interpret them. In one rare instance, the media mentioned that a defeated candidate has made controversial public statements, but even then, it does not explicitly attribute these statements to the candidate’s defeat. On the other hand, writings by an VNA/CPV intellectual suggest that the CPV is aware of multiple different interpretations of any negative result but does not reveal in which interpretation it believes more. I interpret this overall ambiguity in official messaging as indicative of the regime’s unwillingness to reveal its internal motivation. Indeed, if the regime were to even imply that it uses central candidate defeats to decide which province should receive increased funding, it would only create perverse incentives in the following election. A summary of the above discussion has been added to pages 12-13 of the revised manuscript.

On the *quantitative evidence*:

The reviewer suggests that I make room for the drop one analysis in Appendix C (pages 7-9) in the main text. I have heeded this advice and added a discussion of this analysis to page 27 of the revised manuscript.

The reviewer also suggests that I make room for the imputation analysis in the original manuscript’s Appendix D (now Appendix E, pages 13-23, in the revised manuscript) in the main text. I follow this suggestion by adding a discussion of this analysis to pages 28-29 of the revised manuscript.

The reviewer further raises an important concern about the RDD’s lack of external validity, particularly because candidates who are in close elections may be different from those who are not. To probe the seriousness of this problem, in Appendix D (pages 10-12), specifically Table D.1 (page 10), I have analyzed the distribution of candidate-level characteristics across different ranges of vote margins. Fortunately, I find that generally the central candidates are quite homogeneous: candidates who win convincingly are nearly identical to those who are in close elections. The only difference is that those who win convincingly have slightly stronger backgrounds, but this difference is substantively small. To confirm that this difference does not compromise the key findings, later in Appendix D (pages 10-12) I provide evidence from all three methods showing that these findings hold even for the entire sample without any restriction.

Finally, on *typos and grammatical mistakes*, I have reviewed the manuscript to fix many serious errors. I hope the revised manuscript has become more pleasant to follow.

**Reviewer 2**

On *the paper’s primary argument*, the reviewer is interested in the idea that the gathering of some information using elections may lead to conflict with other goals of authoritarian elections and with the collection of other information as well, and suggests that I expand on this idea.

I follow this advice by restructuring the theory section to move this argument upfront and by adding specific examples throughout the section. Specifically, the section on the informational limits of authoritarian elections now begins on page 2 of the revised manuscript. In it, I specify my argument to say that different types of information *can* coexist and may simultaneously emerge from the same election, but autocrats are only able to use a small subset of them before the information becomes useless to them. As I summarize on page 5 of the revised manuscript, “*the limits to authoritarian elections’ informational value pertain not to how many different signals an election may emit simultaneously, but rather to how many of these signals a regime can digest*.”

To make the argument clear, on pages 3-5 of the revised manuscript I present four related reasons autocrats cannot take in all the information that an authoritarian election may provide:

* First, because so many variables may influence election results, it is not easy to tell on which of these variables a certain result is really providing information.
* Second, autocrats who seek to reduce this informational complexity must refrain from manipulating some variables in the electoral process while selectively manipulating some others to ensure that information comes only from those variables that were not manipulated; when doing so, however, autocrats must forfeit all the information about the variables that were manipulated.
* Third, this strategy of selective manipulation is difficult and may prevent autocrats from achieving other goals with elections, such as winning with overwhelming margins. This prevents many authoritarian regimes from pursuing it fully.
* Finally, if a regime takes in multiple types of information from an election, the appropriate policy response to one type of information may contradict with the appropriate policy response to another, preventing the regime from making use of both types of information.

On *the conflict between reacting to both information about popular discontent and information about incompetent or disloyal officials*, the reviewer requests for more clarity on why regimes cannot do both.

I would like to clarify that I made this argument specifically for the case of Vietnam (the discussion on page 31 of the revised manuscript suggests that scholars of other regimes may encounter different conflicts between different informational goals, depending on the local context). The reason it applies to Vietnam is that the high level of institutionalization here has constrained the policy space so much that budget adjustment is the only tool that the CPV could use to immediately respond to information from elections. Specifically, I clarify on page 17 of the revised manuscript that “*explicit methods of punishment, like firing, demotion, or withholding promotion require several institutional hurdles and broadcast negative signals about the regime's internal unity*,” which makes them unattractive to the regime.

Expanding on this argument, Appendix H (pages 42-48) provides further details on the formal process behind Vietnam’s personnel decisions. First, decisions to promote (or not to promote) officials are made around the national Party Congresses, which take place before and thus cannot make use of information from VNA elections. Second, decisions to punish require a formal disciplinary process, in which not only must the officials be found guilty of some transgression that is *not* failure to manipulate elections – as the latter is not legally a punishable crime – but the party leadership must also debate and vote on the appropriate punishment. (The reviewer may note that this process bears much similarity to the impeachment process in the U.S.) This process is observable to the public, so the regime must be careful whether it is broadcasting signs of internal weakness or disunity too broadly. Third, each level of government only has disciplinary authority over its own level or the one immediately below it, meaning that the CPV leadership has no power to punish officials below provincial leaders.

The institutionalization of the disciplinary process does not mean that the regime can never punish officials somewhat arbitrarily, but it does suggest that the regime is not able to simply issue straightforward punishments to officials it find incompetent at manipulate elections. For this reason, it is not surprising that the regime only reserves formal forms of punishment for much more serious transgressions. To provide additional evidence to support this point, I have added a section to Appendix H (pages 42-48), which lists out all incidences of high-level punishment that have happened in the period after the 2016 election, as well as the career trajectories of all the provincial leaders in provinces that suffered central candidate defeats. The evidence suggests that officials in these provinces received no formal punishment and still received promotions.

On *the official effect*, the reviewer questions whether the VNA elections could be testing the officials at all if the candidates are not from the provinces.

I would like to clarify that the test would still be valid, because the regime is not testing the candidates but rather the provincial executives who are in charge of managing (and thus manipulating) the elections. Each electoral district typically has one central candidate who is often a non-native and three to five local candidates who are always natives. The local candidates are tied to the provinces, so the provincial executives are interested in getting some of them elected, but these executives are also told to get the non-native central candidate elected as well. Each district can have two or three winners, so the provincial executives may try to secure one seat for the central candidate while keeping the remaining one(s) for his allies, or give all the seats to local candidates instead. The fact that central candidates are not from the provinces presents a clear dilemma to the provincial executives. The regime’s test is thus about how these executives solve this dilemma, specifically whether they are willing to or able to deliver a seat to the central candidate at the expense of one (or more) of the local elites.

On *engagement with the literature*, the reviewer recommends more engagement with Malesky and Schuler’s work.

Given the importance of several works by the two authors (especially Malesky and Schuler 2009, 2011, and 2013) to the understanding of authoritarian elections in Vietnam, I have added multiple references to these works throughout the revised manuscript. Most importantly, on page 10 I highlight that Malesky and Schuler (2011) were the first to propose that the CPV has used electoral manipulation to maximize the informational value of authoritarian elections. In addition, on page 12 I note that although Malesky and Schuler did not explicitly acknowledge the informational limits of VNA elections in an earlier work (2011), in a different paper (2013) they have found at least one function of authoritarian elections that the CPV has chosen not to pursue. This is consistent with my overarching argument that authoritarian elections cannot accomplish all the goals that the literature says they could.

On *the empirics*, the reviewer proposes several addition to the paper. Specifically, the reviewer requests more details on the empirical technique, especially on the RDD and the generalized synthetic control. The reviewer also recommends a simple randomization inference analysis, as well as a qualitative focus on each individual case.

I have added several details to both the Empirical Methods section and the Results section in response to the reviewer’s very important suggestion. Specifically, on page 25 of the revised manuscript, I specify that the local randomization RDD re-randomizes the election outcomes of 17 different central candidates who were in close elections (for a total of 2^17 different possible permutations). Earlier, on page 22 I note that estimates of the treatment effects in the local randomization RDD analysis also make use of the panel data structure to improve precision, further reducing the demand on sample size.

On page 23 of the revised manuscript, I have rewritten the description of the generalized synthetic control method, noting that it uses an interactive fixed effects model and data from control provinces to estimate for each treated province a synthetic control that has nearly identical values for the pre-treatment outcomes. This is equivalent to predicting the counterfactual values for the post-treatment outcomes for each treated unit.

It is important to note here that because it predicts counterfactual outcomes for each treated unit, the generalized synthetic control method allows analysts to identify and estimate individual treatment effects for each treated unit by taking the difference in post-treatment outcomes between each treated unit and its respective synthetic control. This makes the method particularly appropriate for situations in which the number of treated units are small but finite, as in the case of my paper. Indeed, its “cousin” the synthetic control method has been proposed as a solution to conduct *quantitative case studies*, with each of the original papers (Abadie, Diamond, and Hainmueller 2010, 2015) explicitly working with only one treated unit.

The utility of the generalized synthetic control method notwithstanding, I have also conducted a simple randomization inference analysis using only 2017 transfers and data from every province in the sample. The result, presented in brief on page 23 of the revised manuscript, shows that the difference between provinces that saw central candidate defeats and provinces that did not averages to about 2 trillion VND or 86.7 million USD for each of the treated provinces. This effect is larger than 69% of the hypothetical effects estimated by randomly permuting the treatment indicator. The main RDD analysis improves on this simple analysis by excluding incomparable control provinces and making use of previous transfers to each province to eliminate pre-treatment differences; it finds similar but statistically significant effects.

More importantly, following the reviewer’s excellent advice, I have added a new Appendix G (pages 27-41) which provides extensive qualitative details on every province other than Hanoi or Ho Chi Minh City that experienced central candidate defeats. This new appendix presents for each province very specific information on a) the electoral contest – including the profiles of the defeated central candidates and all their competitors, b) the patterns of funding before and after the election, c) lists of new public projects that may have benefited from the increased transfers, and d) the career trajectories of both provincial leaders who were in charge during the 2016 election. The findings from the qualitative data completely align with those from the quantitative analysis in the main manuscript.

Finally, I would like to note that elsewhere in the revised manuscript, specifically on page 27 of the main text and in Appendix C (pages 7-9) I have provided evidence to show that the small sample size does not present a validity problem to the findings. I hope this evidence would help in assuaging the reviewer’s concern about my sample.

**Reviewer 3**

On *the implication of the incoming information*, the reviewer raises the concern that the CPV could have responded to information about local agents’ incompetence by providing these agents with some form of assistance instead of punishment, which would also be consistent with the evidence of increased transfers found by the paper.

Because this concern is particularly serious, I have made every effort to empirically probe the reviewer’s alternative hypothesis. I do so by looking at detailed budget breakdowns at the provincial level for all provinces for which data is available. If the transfer increases had been intended to assist local officials, there would have been evidence of spending on some form of capacity building programs. However, I find no evidence of training or similar programs in the budget documents. Instead, according to detail qualitative data from the newly-added Appendix G (pages 27-41), most of the public spending in all the provinces that saw central candidate defeats have been on visible construction projects, which is more consistent with the paper’s placation story.

This evidence is consistent with contextual features of Vietnamese politics. Generally, it is unlikely that the CPV would have given assistance to local officials it finds to be incompetent or disloyal. As I note in an additional discussion on page 18 of the revised manuscript, provincial officials have *de facto* control over how central funds are allocated. Thus, if these officials are truly incompetent, corrupt or disloyal, any funding for assistance purposes is only going to be used inefficiently or outright embezzled. In the worst case, it may create perverse incentives for the officials.

Furthermore, in the long run, patiently propping up incompetent officials is also inefficient from an elite management perspective. Because for each leadership position there can be a large number of competent officials from the same level or the one below it who are vying for competition, it is easier for the regime to improve competency by replacing incompetent officials with more competent alternatives.

On *the results on the punishment of provincial-level executives*, the reviewer suggests that the province level is not where punishment would be expected if the CPV were to punish local agents for incompetent or disloyalty. Instead, the reviewer proposes that provincial executives should also delegate electoral management down to officials at lower levels, and that it is these officials who should receive punishment. Thus the reviewer requests additional evidence for whether lower-level officials have been punished.

I thank the reviewer for this important point but would like to offer a few counter-arguments. First, in a new paragraph added to page 44 of Appendix H (pages 42-47) of the revised manuscript (which was Appendix G in the original manuscript), I note that the accountability chain in Vietnam follows a pattern of “double subordination” (Kerkvliet 2004). In particular, each level of government only has disciplinary power over officials at the same level and those at the level directly below. This means that the central party leadership’s authority to punish only extends to the provincial leadership but not to any official below them.

Second, if the story about local agent disloyalty is true, provincial leaders have no reason to punish district-level and lower-level officials because central candidate defeats are desirable to these leaders. Rather, they may even reward these subordinates for delivering these results.

It may be possible, however, that the central government may be able to find creative ways to issue targeted punishments of officials at lower levels, for example by getting them implicated in high-level corruption investigations that have become a frequent occurrence in Vietnamese politics. Additionally, provincial leaders who fail to secure central candidate victories out of incompetence may look to shift the blame downward to their subordinates. In both cases, we should expect punishment events that are highly visible to the public.

In Appendix H (pages 42-47) of the revised manuscript, I probe this hypothesis by exploring new qualitative data about punishment events that have happened in the 18 months following the 2016 election; this data was obtained by reviewing records from the CPV’s Central Inspection Commission and through the media. Based on this data, I note on page 46 of Appendix H that, outside of Hanoi and Ho Chi Minh City, “*no district in any of the provinces that have experienced central candidate defeats has had their official punished during this period*.” Even though I was not able to compile punishment data for every province to conduct a proper comparison, the complete absence of any punishment of district-level official still confirms that the CPV’s punishment did not extend to lower levels of government.

Noting the reviewer’s concerns about the small sample for the province-level analysis of 2007 and 2011 provincial officials’ career trajectories, I have also made sure that the above analysis also considers provincial officials. I find that a) none of the top officials who were governing in the five provinces that experienced central candidate defeats (excluding Hanoi and Ho Chi Minh City) were implicated in the anti-corruption campaign, b) everyone among these officials who is eligible for promotion has received one by 2020, and c) the remaining officials get to finish their term in office and retire without encountering any trouble. The complete absence of punishment and the perfect record of promotion for these officials offer strong evidence that should be less severely subjected to sample size concerns.

On *the validity of the RDD strategy*, the reviewer questions whether the RDD logic and the manuscript’s theoretical motivation are at odds with each other. Specifically, whereas the manuscript argues that central candidate defeats are high-information events, the RDD focuses only on defeats and victories that are so narrow that they can be assumed to be as-if random.

To be fully transparent, I note that this concern should also apply (albeit to a lesser extent) to my linear fixed effects and generalized synthetic control analyses, because they also rely on a sample of close elections to improve internal validity.

In light of this, I argue that even close elections still generate a significant amount of information, and have added a discussion of this argument to page 20 of the revised manuscript. Specifically, I argue that most of the information of an election results comes from the binary signal of a defeat or a victory. Whether an election is close or not, on the other hand, is not apparent to the regime, because vote margins are not straightforward when there are multiple candidates in a race. (This is because each candidate’s margin depends on not only his or her performance vs. the closest competitor, but also this competitor’s performance vs. other candidates.) Similarly, to identify close races for the RDD sample, I had to use a grid-search algorithm that takes into account a wide range of candidate-level and district-level covariates (this algorithm is detailed in footnote 13 on page 19 of the revised manuscript). The CPV is unlikely to have done anything similar.

For these reasons, I believe that the Vietnamese regime is not always aware of how close an election has been. They may have inkling that some results are narrower than others are just by reading the vote shares, but this information is noisy. In contrast, the binary signal of a defeat is clear and unambiguous. At the very least, a defeat no matter how close still disrupts plans for the incoming VNA leadership. It is also a visible event attracting the public’s attention. It is worth noting that accounts of the central candidate defeats (e.g. Le Son 2016, cited on page 10 of the manuscript) only mention the fact that these defeats happened without specifying their margins.

In other words, the CPV should get most of its information from whether a defeat has happened at all. The informational value of a defeat may still be weaker when compared against a close victory than when compared against a convincing one, but this difference is small, such that each defeat still offers a high dose of new information. Thus, the RDD analysis in particular and the sample selection strategy based on close elections in general should not contradict the theoretical motivation.

If anything, the sample selection strategy mostly removes cases of convincing victories but excludes only one heavy defeat from the final sample. Compared to an analysis that uses the entire sample, my final effect estimate thus ignores the effect of defeats for which the counterfactual is a convincing victory. This omission should only lead to bias against my findings.

On *the analysis of repeated defeats*, the reviewer finds it surprising that previous defeats do not predict future ones. The reviewer is concerned that the evidence in this paper suggests that regime support are not persistent over time, and recommends that I engage with this implication in the revised manuscript.

I thank the reviewer for this important point, as it suggests an additional contribution that my paper could be making. I have thus added a discussion of the lack of repeated defeats to pages 30-31 in the Discussion and Conclusion section of the revised manuscript. In addition, I have greatly expanded the original Appendix H on the analysis of repeated defeats, which has become Appendix I (pages 48-52) in the revised manuscript, to discuss more broadly what my findings imply about the CPV’s placation strategy.

In these discussions, I note three separate facts about central candidate defeats. First, central candidate defeats do repeat in few provinces. Second, within provinces that have ever seen a defeat, once a defeat has occurred in an election – and the province has received increased funding as the result of that defeat – the defeat is unlikely to repeat in the next election. Third, roughly half of each election’s defeats happened to provinces that have never seen them before.

The first fact does concur with the reviewer’s impression that public opinion about the regime within certain regions should be persistent over time. The second fact, however, suggests that the CPV’s placation strategy may have succeeded in temporarily mollifying public discontent in some provinces. Nevertheless, because the probability of repeated elections is still greater than zero, this strategy cannot be considered as 100% successful. Finally, the third fact suggests that public opinion can sour unexpectedly, especially in areas that did not enjoy increased funding.

Altogether, these implications suggest that public opinion may have both a persistent and a volatile component. The regime’s placation strategy may have succeeded in tackling discontent that emerged from the volatile component of public opinion, but it is less successful at addressing more systematic problems. It is also unable to pre-empt new discontent from emerging. As a result, the CPV’s strategy of using elections to gather information about geographic distribution of regime support to identify which provinces should receive more funding only works partially, and may not be sustainable in the long run. This has important implication for models of accountability under authoritarian regimes, specifically for whether most authoritarian regimes can sustainably identify and mollify emerging discontent before they become serious problems.

On *additional points* made by the reviewer:

The reviewer is interested in the vote simulations in Appendix D of the original manuscript, which has become Appendix E (pages 13-23) in the revised manuscript. I have made some more room for them on pages 28-29 of the revised manuscript

The reviewer notes an error on page 11 of the original manuscript. I have fixed it accordingly

The reviewer finds the description of the synthetic control confusing. I have rewritten it using more specific language and presented the new description on page 23 of the revised manuscript.

The reviewer notes a copy-paste error with Table 1. I have fixed it accordingly.

The reviewer requests more specificity for the y-axes for Figures 2-4 and spots an error in Figure 4. I have modified the figures accordingly.

The reviewer questions my use of only one case to “rule out” an alternative hypothesis in the subsection titled "Alternative Mechanisms for Increased Central Transfers". Although I have had to relegate this discussion to the Appendix, I acknowledge that my original language is inappropriate and have adopted more tentative language when discussing the relevant findings, which is now in section G.1 of Appendix G (pages 28-32) of the revised manuscript.