**Editor:**

I have received the reviews of your manuscript, Number 201000, "Tea Leaf Elections: Inferring Purpose for Authoritarian Elections from Post-election Responses to Defeats." These are attached at the end of this email.

The reviews, as you can see, are mixed. While the reviewers are generally sympathetic to what you are trying to do and your general approach to the question, they also raise a variety of important concerns and offer several useful suggestions. Based on the reviews and my own careful reading of the manuscript, I invite you to revise and resubmit your manuscript for further review.

I was on the fence on this manuscript, as all three reviewers are both enthusiastic but also indicated that there are major revisions to be made. In the end, I decided that enthusiastic reviewers are a strong signal to proceed in spite of the significant revisions they request.

When I receive the revised manuscript I will send it back to all three Reviewers. Although I do not expect to send it to a wholly new reviewer, I reserve the right to do so, especially if one of the original reviewers is unavailable to read the manuscript a second time.

**Reviewer #1:**

Summary: This paper builds off an extensive literature on authoritarian elections and a large number of articles on the Vietnamese National Assembly (VNA). The author argues that one stream of the authoritarian institutions' literature on the informational advantage of elections is too blunt, suggesting that the same information can be interpreted multiple different ways by authoritarian leaders. Narrowing in on localized losses, the author argues that they can convey either poor implementation by electoral officials or disenchantment with the regime in some constituencies. Taking advantage of the peculiar institution of central nominees in the VNA elections, the author argues that these can be empirically separated in the Vietnamese context by analyzing whether provinces, where central nominees lost, are either punished (deprived of development funds) or placated (provided with additional development funds). Using a range of clever statistical approaches, including a generalized triple difference, an RDD, and generalized synthetic conrol, the author demonstrates that such provinces receive greater amounts of development funds, however, the do not receive greater cyclical administrative expenditures.

General: This paper is extremely well done and should be published in a top outlet like JOP. The statistical analysis is well-handled and the author anticipates a number of reviewer concerns: including the small-n, generalizability to other VNese elections, the 2015 Budget Law, and pre-existing structural differences between provinces. That said, I have a number of smaller concerns that I would like to see addressed before publication.

First, there are some omissions in the theory section that needed to be filled-in.

Theory 1: The idea that information from serve multiple purposes and officials must make difficult choices about how to read election results is not new. Andrew Little and Jennifer Gandhi have made similar points. They should probably be cited.

Theory 2: One source of information from local elections that has some history in Vietnamese studies, but was not explored in Figure 1, is cadre selection - that the regime uses election results to identify talented leaders from promotion (see Boix and Svolik 2013, Soloman 2007, Bui 2020, Malesky and Schuler 2011).

Theory 3: It is not clear from the author's theory how the VCP knows how to read the elections. The author demonstrates persuasively that they see it local dissatisfaction and move to placate, but what exactly is the regime seeing that allows them to make the decision?

Second, the piece could be enhanced by qualitative information that would color and enrich the statistics, allowing readers to understand the processes that the author knows so well.

Qualitative 1: A table of which provinces had central nominees lose for each 2007, 2011, and 2016 would be very helpful for the placation story, if it showed that central nominees lose in different provinces. If they are losing in the same province every year, the theory is more difficult to understand, because it does not seem there is learning over time.

Qualitative 2: Who exactly are the central nominees in general? Provide some description of their background and the expectations that the regime has for them in the VNA and government general. In particular, who are the ones who lose? Do they appear to be different in their backgrounds or prospective roles? Schuler 2020 (cited in the paper), for instance, makes the points that central nominees are slated to chair VNA committees. Is that also true for losing candidates?

Qualitative 3: What did the money used to placate provinces go to? Some examples of how provinces used the additional state transfers and whether those development expenditures were visible to citizens would help make the story. Highly demonstrable projects would really highlight the placation story.

Qualitative 4: National and local news coverage of individual, central nominees' losses. Are those interpreted by the media in similar ways to the author's interpretation?

Third, the empirical work is excellent. However, some really important answers to obvious concerns are relegated to the appendix. In particular, the obvious problem of having such a very small number of central nominee losses per year, and the fact that losers' vote share data is only available in a single election.

Quantitative 1: Appendix C on small size, especially the drop one analysis, is extremely convincing. I would like to see room made for it in the main text.

Quantitative 2: The imputation approach to recover vote shares for losing candidates in Appendix D2 using the mathematical ranges of their losses and delegate information is also really well done. I would move that analysis into the main paper and leave the other 2007 and 2011 approaches in the appendix.

Quantitative 3: The author uses the RDD to help discover unpredicted losses. This is very persuasive. That said, there still remain concerns about selection issues. Central nominees are allowed to select the provinces they would like to run in, and regime authorities then choose to assign them based on their preferences and regime goals. A serious concern is that weaker candidates are sent to location where it is more difficult to win, and therefore we should only see the weakest candidates in close and losing elections. This would make it more difficult to generalize broadly beyond the very small sample.

Copyediting: There are numerous typos and grammatical mistakes in the article. I was keeping track, but a certain point, it became overwhelming. None of these typos distract from the author's argument. It is still quite easy to understand, but the cumulative effect of the errors is very distracting.

Once these important changes are made, I believe this article has the potential to strongly influence bot the theory and methodological analysis of authoritarian elections.

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Little, A.T., 2017. Are non-competitive elections good for citizens?. Journal of Theoretical Politics, 29(2), pp.214-242.

Little, A.T., 2015. Fraud and monitoring in non-competitive elections. Political Science Research and Methods, 3(1), p.21.

Gandhi, J., 2015. Elections and political regimes. Government and Opposition, 50(3), p.446.

Malesky, E. and Schuler, P., 2013. Star Search: Do Elections Help Nondemocratic Regimes Identify New Leaders?. Journal of East Asian Studies, 13(1), pp.35-68.

Bui, T.H., 2020. Governance, the Socialist Market Economy, and the Party-State in Vietnam and China. In The Socialist Market Economy in Asia (pp. 117-140). Palgrave Macmillan, Singapore.

Salomon, M., 2007. Power and Representation at the Vietnamese National Assembly. In Vietnam's new order (pp. 198-216). Palgrave Macmillan, New York.

Boix, C. and Svolik, M.W., 2013. The foundations of limited authoritarian government: Institutions, commitment, and power-sharing in dictatorships. The Journal of Politics, 75(2), pp.300-316.

**Reviewer #2:**

This paper examines how Vietnam's regime reacts to surprise election losses, finding that the provinces receive more spending immediately after. This is a promising paper, but needs an expanded theoretical buildup and better contrast with the literature. Further, the small number of cases makes the empirics somewhat questionable.

The framing fits into a solid literature on the functions of autocratic elections. The authors note the existing idea of using elections to gather information, but argue that this comes at cross purposes with other goals and even other types of information-gathering. I find this an intriguing idea that needs expansion. More specificity on why different types of information cannot coexist, perhaps with examples, would make the theoretical contribution stronger.

The authors then contrast the idea of gathering information on and mollifying popular dissatisfaction with the idea of penalizing lower-level regime officials for underperformance. However, it's never clear to me why regimes cannot do both. I understand that a negative election could indicate either problem (popular opposition or bad officials), so there's a lack of clarity, but I'm not clear why regimes would choose only one of the responses. It's also questionable that this is testing the officials effect, as the candidates are not from these provinces. Moreover, the most straightforward response in punishment would be firing (or worse) the officials themselves, whereas the authors only look at funding levels. If the officials were removed, then funding would be largely immaterial to them so it's not a great test.

A final note on the front end is that I'd like to see more engagement with Malesky and Schuler's work on Vietnam, which directly relates to issues like elite management and information-gathering.

In the empirics, the authors try three appropriate techniques: a panel model, RDD, and general synthetic control. Each (and especially the latter two) need more detail to evaluate. Are there sufficient close cases on either side of the threshold for RDD? How was the synthetic control constructed? A major limitation here is that, from what I can tell, there are only 4 cases of turnover in the analyzed election, a small number indeed. As a result, the authors' emphasis on randomization inference is appropriate. In fact, I'd like to see a really simple first analysis using simple randomization inference to show how large the funding shifts of the treated provinces are compared to all other shifts. Whether the analyses can work with this limited variation is an open question that the authors need to do more to confront.

Further, with such a small number, it's worth considering a more qualitative lens and discussing each of the cases, perhaps why there was a loss, how officials there were dealt with, and the funding patterns.

**Reviewer #3:**

This manuscript engages a frequent claim in the authoritarian institutions literature—namely, that authoritarian elections are organized to furnish the dictator with information—to develop a framework for inferring the information sought by the dictator. In the context of elections to the Vietnamese National Assembly (VNA), the party-state could leverage election results (specifically, the incidence of central nominees' losses) to learn about local-level regime support or to glean evidence of local officials' incompetence or disloyalty. If the former, the author predicts increased budgetary transfers from the center as a means of placating disgruntled citizens; if the latter, the author predicts punishment of provincial officials in the form of reduced transfers. Applying three separate empirical approaches to electoral returns from the 2016 VNA elections, the manuscript consistently finds evidence for increased budgetary transfers to locales in which central nominees failed to win election.

I greatly appreciate the author's careful engagement with the literature on authoritarian elections: the gathering and organizing of the various explanations provides a service to the field. I also applaud the effort to draw out the implications of these potentially competing explanations for the specific case of Vietnam, and to then adjudicate between those deemed most likely to apply. Furthermore, I have a hard time arguing against the primary empirical evidence, which documents increased transfers to those provinces in which central nominees lost. Despite these advantages, however, I also have serious concerns regarding the theory and its linkage to the empirics and remain unsure whether these concerns could be addressed in a timely manner through the R&R process.

PRIMARY POINTS

First, the author is only able to adjudicate between competing strains of the informational theory because they are asserted to lead to divergent responses, yet this may or may not be true. Specifically, the "geographic distribution of regime support" motivation is predicted to prompt placation of the public and thereby lead to increased transfers, while the "competence or loyalty of local agents" motivation is predicted to incur punishment of those agents through decreased transfers. Yet it seems equally conceivable that incompetence on the part of local agents could warrant greater assistance rather than punishment, and therefore produce the increased transfers so diligently documented in the empirics.

Alternatively, and building upon the discussion of Appendix G, even if we assume that incompetence or disloyalty on the part of local agents warrants punishment, it is not clear to me that punishment of provincial-level executives is where such punishment would be meted out. Just as central authorities delegate to the provinces, provincial executives surely delegate tasks such as mobilization and cajoling to officials considerably lower down the food chain (potentially even street-level bureaucrats). If that were the case, there would be plenty of far more targeted forms of punishment than reducing transfers to entire provinces. I believe the examination of punishment of provincial-level executives in Appendix G should be supplemented by analysis of lower-level—and more directly responsible—officials in order to rule out the incompetence/disloyalty explanation altogether. In addition, this would ameliorate the small-n problem as it currently exists in Appendix G. It would, however, necessitate a substantial rewrite to acknowledge that the dictator may still seek—and may even be able to address—multiple kinds of information concurrently.

A second problem lies in the juxtaposition of the theoretical claim that central nominee defeats are informative with an RDD empirical strategy in which victory and defeat are so narrow they "can be assumed to be generated by chance" (21). If victory or defeat amounts to a coin toss, how could defeat possibly convey a strong signal to the regime? The manuscript does not hinge upon these results, as the linear fixed effects and generalized synthetic control analysis still stand. But I think the tension between the manuscript's theoretical motivation and the RDD's empirical strategy is too great for this analysis to remain. It could be that in the absence of any information, even a weak signal is heeded. But that is not quite the story this manuscript tells.

I appreciate the author's analysis in Appendix H of potential 'repeated offender' provinces, although not necessarily to examine voters' strategic behavior. Assuming for the moment that central nominee defeats are reliable signals of localized discontent, I'm honestly surprised that earlier defeats are not predictive of later defeats. In other words, I would assume that the geographic distribution of regime support/discontent would be far 'stickier' over time. The argument and results presented in this manuscript instead verge on the classic 'whack-a-mole' game. I would appreciate greater engagement with this aspect on the part of the author.

OTHER POINTS

The vote simulations in Appendix D are audacious and truly cool.

The sentence on page 11 beginning with "At the same time, the anonymity…" has another sentence embedded within it (an errant cut and paste, most likely): "Central candidates become clear targets…"

The discussion of how the synthetic control is constructed (p. 21) is confusing.

Table 1 does not present the linear fixed effects and RDD results. It presents one or the other, I'm not sure which. But columns 6—10 exactly duplicate columns 1—5.

Figures 2—4 need more informative y-axes—particularly Figure 4. The caption for Fig. 4 also mistakenly describes the "horizontal dashed line" as marking the election year. This should read "vertical…"

The subsection entitled "Alternative Mechanisms for Increased Central Transfers" purports to "rule out" an alternative mechanism on the basis of a single province. That's not how I understand social science to work.