**Editors:**

The review of your revised manuscript, "Tea Leaf Elections: Inferring Purpose for Authoritarian Elections from Post-election Responses to Defeats," is now complete. The referee reports are included in this message. As you can see, while one of the reviewers is fully satisfied with the revisions, two are not. More importantly, the specific question upon which one reviewer conditioned the original revise and resubmit decision has not been answered satisfactorily. The reasons behind the reviewer's thinking are clearly laid out and, based on our own reading, we share the concern. As a result, we unfortunately cannot accept the paper for publication in the American Journal of Political Science.  
  
We understand that you will might find this decision frustrating, especially since we had all hoped to see this article to publication. Nevertheless, we hope you will find the additional comments helpful.  
  
Thank you for considering the American Journal of Political Science as an outlet for your work. We hope this decision does not discourage you from submitting to us in the future.

**Reviewer #1:** As mentioned in my previous review, this paper is well written and well executed. I based my support of the paper on the condition that it would be able to provide further convincing evidence that the finding was not an artifact of the specific coding decisions made in 2016 combined with the possible confound of the 2015 Budget Law. In reading the revision, I am impressed by the prodigious effort the paper makes to address these issues. Additionally, I am impressed by the methodological sophistication. However, unfortunately I am not suitably convinced that the evidence offered is strong enough to allay my original concerns. I deeply regret this decision as I am always reluctant to be critical of an RR, particularly when so much effort clearly went into the revision. However, for the reasons I offer below, I am not suitably convinced that the data offered in the paper supports the conclusions. While the evidence in support of the theory is suggestive, it is not nearly convincing enough in favor of the theory.  
  
The key problem is that the main finding from the 2016 results already rests on a thin slice of data. Based on my own data from Vietnam (which may be slightly different than the data in the paper) in 2016 only seven provinces had central nominees losing. Of these, two were Hanoi and HCMC, which were dropped. This leaves 5 remaining provinces (again the author's data may differ slightly given some ambiguity in my data as to who a central candidate was, but I am confident my figures are roughly correct) with central losers. Of those, based on my numbers, one province had a central nominee losing by a large margin, thus warranting a drop. This leaves only four provinces in the analysis with a central nominee losing. This is an extremely thin reed on which to base the finding.  
  
However, as mentioned in the original review, I was willing to be convinced if there was support from the previous years. Unfortunately, the analysis in the paper does not provide convincing support for the theory. The revision stresses that this is due to the inability to find close matches due to the lack of data on the winning percentages of the losers. However, based on the 2016 results, I am not convinced on this point. In 2016, only one province outside of HCMC and Hanoi experienced a central nominee losing outside the band. Given this, I think it would be safe to assume that virtually all of the central losses outside of Hanoi and HCMC in previous years are "surprises" and treat them as such. Even if one of the losing candidates did experience a shockingly low vote share, theoretically I would think that this should still be a sufficiently "surprising" piece of information for the regime that based on the theory offered in this paper the regime should respond by increasing spending in that province. Where the "close election" strategy is doing more work is in dropping those provinces where the central nominees wins by a large amount, which the paper can still conduct given that vote percentages are available for the winning candidates in the previous years. This to me obviates the need for the impressive simulation analysis the paper conducts, which in any case still does not provide conclusive evidence in favor of the theory. Given the lack of support in 2007 and 2011 and the small sample size from 2016, I am afraid this is the basis for my concern with this revision.  Again, I want to emphasize that I regret getting hung up on this point. Given my positive previous review, this conclusion will obviously be frustrating to the authors. However, my previous support for the paper was contingent on this crucial piece of evidence, and unfortunately I am not convinced that the additional evidence supports the theory.  
  
More broadly, I am a bit confused as to why the 2016 Budget Law punishes Hanoi and HCMC given the large amounts of central nominees losing in those provinces. In fact, in 2016 HCMC and Hanoi had more central losers than they had in 2011. Shouldn't this shift indicate declining support in those provinces and therefore the need for more concessions? The decision to drop them from the analysis is not particularly well justified from my perspective, especially given the a) low number of provinces that have central losers and b) the fact that more than half of the central losers in 2016 were in these two provinces. If a theory about central losers cannot explain what occurred in the provinces that had the most central losers, the conditions where the theory applies seems quite narrow.  
  
Additionally, with regard to the decision to drop provinces outside the band, I understand the logic based on the identification strategy. However, shouldn't it still be the case that provinces with central candidates winning with large majorities should receive fewer transfers given that their citizens clearly do not pose a threat? Again, as I mentioned in the previous review, a more convincing strategy to me for measuring "surprise" is not whether the candidate won or lost a close election, but rather how well the central nominee did relative to previous years.  
  
As the paper moves forward, while this was not part of the reason for my overall conclusion on the paper, I would encourage the paper to consider a bit more deeply the alternative theory that the center is trying to appease local officials rather than voters with the increased spending. Section 7.2 uses evidence that the money goes to development spending rather than salaries to suggest that the regime is trying to placate voters. I am not convinced. My supposition is that salary increases would likely have to go through a different legal procedure and would be difficult for the regime to parcel out on a province-by-province basis. However, even if this isn't the case, it seems quite possible that local officials could profit from additional development resources from the center for several reasons. First, it could enable them to win support of officials further down the chain (district and commune officials important for their local support base) through the ability to dole out additional patronage. Second, local officials could engage in outright corruption or rent extraction. Finally, local officials could win public support by being able to promote pork barrel projects they brought in.  
  
The reason why I think it is important to consider this theory is because central candidates losing in Vietnam necessarily means that an additional locally nominated candidate wins. Therefore, it is difficult to interpret whether a central loss is dissatisfaction with the center or the unwillingness of the provincial level to nominate sacrificial lamb locally nominated candidates. Again, this is merely a suggestion for future revisions.

**Reviewer #2:** The author has done a terrific job satisfying my original concerns and comments. He/she is to be commended for a much-improved manuscript now close to being ready for acceptance. After a second reading, I have but minor comments in no particular order:  
  
1.      A purely stylistic requirement is a brief statement on the structure of the paper, which belongs at the end of the introduction. This is a standard feature of all journal articles, but it is oddly missing at present.  
  
2.      On several occasions the author mentions the role of electoral uncertainty in authoritarian regimes. It would be great to reconcile the work of Andreas Schedler (2013), who makes a fine distinction between how this plays out in competitive and hegemonic authoritarian regimes (like Vietnam). The top and bottom paragraphs on page 3, top paragraph on page 11, and the middle paragraph on page 31 are obvious places.  
  
3.      The figure on page 15 is missing two functions of authoritarian elections: legitimation (where autocratic regimes feign conformity to establishes rules and norms) and succession (where autocratic regimes use the window or opportunity to change dictators).  
  
4.      The author is encouraged to consider whether turnout in Vietnam - or invalid voting numbers for that matter - matters in combination with the popular vote. In other words, does the regime take any information from variation in turnout rates at the sub-national level? If there is "near-universal" turnout (page 10) in one region, but lower turnout in a neighboring region, for example, is this valuable information? The work of Ferran Martinez i Coma (2016) might be helpful in this regard.  
  
5.      The author constantly alternatives between autocratic regimes, dictators, and regime leaders throughout the paper. Some more consistently is required here; presumably the referent object is autocratic regimes (or the ruling coalition within it?)  
  
6.      I appreciated the effort to reconcile my concerns around the scope conditions of the theory. Another reading, however, has produced further questions:  
  
a.      Does the need to "exercise a high degree of control over its agents through a disciplined party apparatus" (page 4 and others) reveal the need for not only a dominant party, but a disciplined dominant party. In addition to being hegemonic regimes like Vietnam, the cases of Mexico, Malaysia and Singapore show that discipline is a product of time. So is the opportunity to gain information via elections (which requires constraint) only available to authoritarian regimes with a long track record of holding flawed elections, as opposed to newly institutionalized authoritarian regimes?  
  
b.      The paper is concerned with legislative elections (which is good and fine), but a brief footnote saying whether information can or cannot be gained via presidential elections in authoritarian regimes would be helpful.  
  
c.      The author downplays the role of mass organizations in Vietnam, which are important for mobilizing and securing votes (this is clear at the top of page 31). It begs the question of whether they are a required intervening variable for both the successful manipulation and the high level of popular vote. This is a very minor concern for this reviewer though.  
  
7.      References are required for the claims on page four regarding Singapore (suggest Netina Tan's work) and China (no suggestion).

**Reviewer #3:** I find the author's responses satisfactory and recommend publishing the article as it is.