Reviewer #1: 'Tea Leaf Elections: Inferring Purpose for Authoritarian Elections from Post-election Responses to Defeats' nuances how we understand authoritarian elections. The author(s) reviews the informational benefits of these elections elucidated in existing scholarship. They argue that the same election result can signal different information and so authoritarian elections are less of a clear source of information that we might otherwise think. The author(s) contend that autocrats must control the electoral environment (how much manipulation, what type) in order to make the election as informative possible in terms of the type of information they are looking for. They focus on the trade-off between information on performance of local agents and discontent with the regime. The main contribution the article tries to make is providing a test to allow scholars to infer what type of information the autocrat is interested from autocrats' responses to electoral defeats. They do so using a range of sophisticated methods on a small dataset from a single-party election in Vietnam.

Carefully thinking about what autocrats learn from election results is a promising research direction. While the idea is interesting and the paper is very ambitious methodologically, I have major concerns about the logic of the test the author(s) propose. Given that this is the main contribution of the paper, I cannot recommend publication of this paper at the APSR.

**Theory and test:**

The author(s) argue that autocrats are either interested in information about subnational variation in opposition to the regime or they are interested in performance of local officials. However, an electoral defeat in a district caused by general dissent and local poor performance may be observationally equivalent. To ameliorate this problem, the autocrat controls the electoral process to make sure they get the information they want. If the autocrat cares more about understanding the electorate's approval of the regime, they will not encourage local agents to extensively manipulate the results to guarantee the victory of centrally backed candidates. If central candidates lose in these elections, then the regime knows they are unpopular in that area. If the autocrat cares more about understanding the performance of local officials, they will encourage selective manipulation. If central candidates lose in these elections, then the regime learns that the local agents in that constituency are shirking or incompetent. This argument serves as the backdrop for the test.

This argument is plausible and not overly complex but the exposition of it was somewhat hard to follow and made it seem much more complex than it actually is. The exposition of the argument could be improved by making it less abstract and more grounded in examples and specifics of what this selective manipulation actually looks like. This would also make the link between selective manipulation and better information about local performance clearer.

The test does not hinge on observing the level of manipulation because this is difficult data to gather. Instead, the author(s) propose we can learn what strategy the autocrat is using by observing their response to these electoral defeats. The logic of the test, as the author(s) propose it, is as follow. If the autocrat cares about general discontent, an electoral defeat signals to the regime that citizens in that area are unhappy with the regime. Fearing mass dissent, the regime is likely to increase central transfers to these areas in order to appease citizens. However, if the autocrat cares about performance of local officials, a defeat signals that they are performing badly so the regime will cut central transfers to punish these officials. Officials benefit from corruption so cutting the province's budget will reduce what they can expropriate.

The logic of this test relies on this separating equilibrium strategy. I have two main concerns about how plausible this is. First, what is local performance and why does it matter? The paper draws on Malesky and Schuler (2011)'s paper on local performance. Indeed, it uses the same quirk of central candidates in VNA candidates to contrast regime support and local performance. Malesky and Schuler argue that loss of central candidates provides useful information about local performance. They contend that local performance in delivering the vote to central candidates matters because it tells the regime about bureaucratic compliance at elections but also because it tells the regime about how likely it is that these officials are incompetent or corrupt in their other duties. If the local agents are not delivering services properly to citizens, this is also likely to generate dissent against the regime as time goes on. Given that, you might expect the regime to redistribute to areas where there are losses even if they primarily care about learning about local performance. That is a major problem for the author(s)' empirical strategy. If the responses to dissatisfaction with different levels of government are the same, we actually have a pooling strategy. The empirical result then only tells us that autocrats government distributes to places they have fared worse, the kind of swing voter strategy that is well documented.

Second and given my last point about dissatisfaction with the regime caused by poor local performance, why would the regime punish poorly performing local agents in this indirect way which also hurts citizens? The regime could fire them, dock their pay, cycle them to less desirable postings and so on. Cutting central transfers to hurt individual officials seems like a very roundabout way of punishing them and one that is likely to backfire given the broader understanding of why local performance matters I discuss. The punishment strategy the author proposes is likely to foster further discontent if the local citizens really have been suffering the consequences of incompetent local state officials. Given that, I do not necessarily believe that the autocrat would respond to information about poor local performance by cutting transfers. Again, this creates a pooling rather than separating equilibrium.

I also worry about the generalizability of the test to other cases. First, I worry about the specificity of the role of these Assembly members. Centrally affiliated candidates parachuted in and without any history of constituency service in that area certainly makes the test cleaner as it is far less likely that local performance in service delivery will influence vote choice. However, how common are these kinds of roles in other electoral autocracies? How often do Assembly members/legislators in other cases not have ties to the constituency they work in? How often are they parachuted into a different district from one election to the next? To my knowledge, this is fairly rare. That is an issue because once the Assembly member has more of a local record to stand on, the test becomes much less clean as a way of separating information about regime dissatisfaction and local performance. If the Assembly member has a local mandate and central mandate then voters might punish an incumbent Assembly member for either or both of those.

Second, I worry about how informative defeats are in the broader authoritarian context. The author says that localized defeats are very rare and so these defeats are highly informative. I can see that this is certainly true in Vietnam but I think that argument holds less water elsewhere. In electoral authoritarian regimes, regimes often lose seats. A standard authoritarian legislature is likely to have 15-30% or more opposition party legislators. Some of these are in opposition strongholds and some are not. Not all electoral defeats in these contexts actually provide any new information about regime satisfaction. That means the author(s)' test would pool these cases in electoral autocracies making it less likely to tell us anything about how the regime responds to regime dissatisfaction.

This is an important point in itself but also gets at a broader issue with scope conditions. What does Vietnam tell us about other places? Vietnam is a one-party state. How would this test work in an electoral autocracy? I would like for the author to do more work to contextualize their case to convince the reader of the broader contribution of the test. The author could make a bigger contribution by more clearly spelling out how we can perform such a test in a range of authoritarian contexts, including and especially electoral autocracies.

**Case:**

The author(s) clearly has good case knowledge but I think that could be put to better use to understand the test. More details about the role of VNAs would be helpful to assess its plausibility. I would have liked to know more about what these MPs do if they are not supposed to be involved in central party activities? Do they do constituency service? Given that, is it important if the central candidate is competing against an incumbent candidate? Might your cases of 'discontent' just be cases of coming up against a strong, well-liked local guy? What problems would that pose for inference?

I would also like more discussion of why these particular candidates may have lost. Were their remits in the central party very different from the ones who won with no problem? You have a small sample size and do admirable quantitative work to overcome the problems of that. The paper could be greatly improved by including more information about the individual cases (the defeats) themselves to convince the reader that these defeats were because of broad regime discontent rather than something to do with the individuals in question. For example, the synthetic control robustness test focusing on Can Tho would be more convincing with more details about the case and/or how it compares to a representative case that does operate by the proposed mechanism. The paper in general had a problem of reading as very abstract and therefore hard to follow. More grounding in what actually happened and the cases would certainly help.

**Empirics:**

The empirics are very well done. The author convinces the reader very thoroughly that more resources are indeed sent to these areas of electoral defeat. The paper could be improved by making the exposition of the methods clearer. At the start of the empirical strategy section, please be explicit about the number of units you are dealing with. How many provinces? How many observations in the panel? That only came up in a later table. The author(s) use sophisticated methods to overcome small samples and this is impressive. The methods used were well explained. However, I found that some of the exposition was lacking a final 'and this is what that method gets me here' sentence/punchline. Readers of authoritarian politics work have varying levels of competency in these methods and spelling out why you are using a given method and what exactly it gives us will make your work more accessible.

However, the significance of the relationship between defeats and transfers really comes down to the theory and the extent to which the test really does separate autocrats who care about subnational variation in dissatisfaction from those who care about local performance. I ultimately do not think the author(s) show this to be the case.