**Editor:**

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 some new and lingering important concerns and offer several useful suggestions. Please pay particular attention to the issues raised by R2 in your next draft and response memo. Based on the reviews and my own careful reading of the manuscript, I invite you to revise and resubmit your manuscript for further review.

**Reviewer 1:**

The author has responded satisfactorily to all of my criticisms.  I was already positively disposed toward this article when I reviewed it, but now I am even more so.

This is an excellent paper that makes three important contributions.  First, it synthesizes and presents the informational arguments about elections in a super clear manner that will allow analysts to make clear predictions about the validity and implicit trade-offs inherent in each theory.  Second, it highlights a fascinating puzzle in how single-party regimes should respond to localized electoral defeats through punishment or reward.  Third, it offers an incredibly rigorous approach to testing these theories reliably in a setting with tremendous data limitations including necessarily limited sample sizes and incomplete information on election totals in previous elections.  The author offers an array of creative persuasive solutions to these problems with findings that all point in very similar directions - the regime transfers substantial resources to provinces where central nominees lost in previous elections.

I think the article be well-cited by scholars of authoritarian elections, but will also be extremely influential in the field of Southeast Asian politics, where rigorous work on how political institutions operate remains relatively rare.

I have just a few minor issues.  First, there are still a few typos and grammatical issues that need to be cleaned up.

Second, I was curious about a point on page 6 about existing sources of information for the authoritarian regime in Vietnam.  The author cites international surveys as having too small a sample size and internal surveys as being low quality and qualitative.  What about surveys performed by non-state actors in Vietnam that have quite large sample sizes and receive a great deal of publicity? I am thinking of the Youth Integrity Survey of Transparency International (<https://towardstransparency.vn/wp-content/uploads/2019/09/YIS-2019_Executive-Summary_EN.pdf>) , the panel business survey by UNU-WIDER (<https://www.wider.unu.edu/database/viet-nam-data>), the Vietnam Provincial Competitiveness Index Survey (PCI) by VCCI and USAID (<https://pcivietnam.vn/>) and the UNDP and CECODES PAPI Survey of citizens (<https://papi.org.vn/eng/>).

The PCI survey has covered about 10,000 small businesses in all 63 provinces since 2006 and the PAPI survey has covered 14,000 citizens a year in all 63 provinces since 2009.  Do these play any role in providing information on citizen satisfaction with the regime?

And one final citation -  Other reviewers asked the author to respond to the work of Malesky and Schuler on the VNA.  And the author has done so in spades. However, there is one piece by Malesky and co-authors that particularly relevant for the focus on equalizing transfers across provinces and political business cycles related to these transfers in Vietnam. It might be worth mentioning the consistency of these findings.

Malesky, E., Abrami, R., & Zheng, Y. (2011). Institutions and inequality in single-party regimes: A comparative analysis of Vietnam and China. Comparative Politics, 43(4), 409-427.

**Reviewer 2:**

I previously evaluated this paper as having some promising ideas, but with serious question marks and need for greater depth. Although the authors have made several responsive revisions, a better understanding of what's being tested here and remaining weaknesses leave me less supportive of the paper. I note several outstanding issues.

I previously misunderstood the authors' description of how these elections work in Vietnam. With a better understanding of this, I don't think this is testing what the authors claim. As I now gather, central national candidates are running alongside local officials and elites in multi-seat elections. In a handful of cases, these national candidates lose to these local candidates, all of whom appear to be allies of the party. I agree that this might be embarrassing to the party and signal local opposition, which could plausibly be tied to the increased local funding the authors find.

However, the authors go further and claim that the lack of resulting punishment of local elites is testing the theory of elections as elite monitoring/recruitment devices. I don't see how this follows. That theory suggests that underperformance by local candidates will lead to their punishment (e.g., lack of promotions). Here, the local candidates are overperforming. Why punish them? There's a vague suggestion that they're supposed to be engineering votes in just the right way, but I see no backing for this and it's not clear the candidates themselves would be in charge of that. In sum, this specific setting is not well-suited to testing the candidate-monitoring mechanism, which is set up as a major contribution. Rather, we're left solely with testing the signaling/funding mechanism, which has been done quite a bit in single-country settings.

There are other remaining issues. I still think the idea of informational tradeoffs, complexity, etc., is intriguing, but it's just not dealt with sufficient depth here to make a theoretical contribution. This is begging for a formal treatment in my eyes, as well as some specific examples of how the tradeoffs work. I encourage the authors to pursue this idea, but to really give it room, maybe in a restructured separate paper.

As another reviewer noted, I don't think the RDD empirical setup makes much sense here. It's not clear why defeats for the central candidate carry such distinct weight if the mechanism is signaling. Why doesn't general underperformance matter?

A remaining problem, which the authors can't do too much about but is a problem nonetheless, is the limited data. There are 4 (or 5?) cases of election loss being tested here, and I'm dubious that some methods like generalized synthetic control can credibly be run on such a sample. When looking at other elections (Appendix E), the results don't seem to hold up. Granted, data limitations mean these tests can't be limited to close elections, but it's odd that there's no effect at all.

Another issue that would need to be confronted is the pattern in Figure 2 showing that these districts with losses (and resulting increases in funding) also displayed reductions in funding of the same magnitude in 2014, two years prior to the election. This is not statistically significant, but the magnitude seems striking, and I interpret this as meaning these districts were actually just restored to their expected level of funding pre-2014. And perhaps the votes were a reaction to what was happening in 2014?

To bring this together, the authors have a suggestion of a result (analyzed well given the sample and data problems, to be sure) that electoral repudiations of the central party are met with funding shifts. This offers some support for the signaling model of autocratic elections, but this type of analysis has been done before. The authors are claiming the results can do more by also testing the candidate recruitment model of elections, but I don't think that holds up. Given the lack of much theoretical novelty, I personally don't see the resulting contribution to be sufficient for JOP.

**Reviewer 3:**

This manuscript is much improved generally, and I find the author's revisions, additions, and counterarguments generally persuasive. Specifically, the author's discussion of and response to my concerns regarding (1) the possibility of increased transfers in the event of incompetence, (2) punishment of lower-level officials, and (3) repeated offender provinces do in fact assuage those concerns. The minor points have also been handled.

My final outstanding concern was the seeming incompatibility between the assertion of central nominee electoral defeats as informative events and the motivating logic of the RDD that close elections are as-random coin tosses. Here, too, I find the author's discussion persuasive. In particular, I tend to agree that the closeness of such elections is complicated (by multimember districts) and noisy, and that the primary signal is a binary one.

I now recommend this manuscript be accepted and published.