Reviewer 1 - TK

**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.

**My response:**

1. *On the theory of elections serving as elite monitoring devices*, the reviewer is unconvinced that the paper can test the candidate-monitoring mechanism.

To my chagrin, I see from the reviewer’s comments that the paper has not clearly communicated this important argument. The reviewer understands that the paper tries to test the candidate-monitoring mechanism, but in fact it does *not* seek to test this mechanism at all. Instead, as pages TK and my initial response to the reviewer explain, it is testing a different elite-monitoring mechanism that focuses on the role of provincial executives (similar to governors and mayors). Specifically, in my initial response to the reviewer, I note that if it is using elections to monitor its own agents, “*the regime is not testing the candidates but rather the provincial executives who are in charge of managing (and thus manipulating) the elections*.” In other words, instead of the candidates contesting in the election, it is the provincial executives managing the electoral process at the province level who are the subject of my theory.

My thanks to the reviewer for highlighting the continued confusion. To address this problem, I have made more edits on pages 11-12 (TK) and on page 17-TK the revised manuscript to describe the theories being tested more clearly. For example, the first sentence of the last paragraph on page 11-TK now reads:

*In regard to province-level executives, specifically the Provincial Party Secretary and the Provincial People's Committee Chairperson, the CPV may monitor these officials' competence and loyalty by evaluating how well they secure victories for the central candidates.*

And the first sentence of the last paragraph on page 17-TK now reads:

*However, if the CPV uses elections to evaluate province-level officials, the defeats will reveal the identity of executives who are so incompetent or independent that they failed to secure victories for all central candidates.*

Given that the manuscript still faces space constraints that may get in the way of an exhaustive description, I present a more verbose summary of the elite-monitoring theory below.

To begin with, the theory involves four relevant (groups of) actors:

**A**: The regime leadership

**B**: Provincial executives, which consists of the Provincial Party Secretary and the Chairman of the People’s Committee

**C**: Central candidates, who are high-ranking CPV elites who operate at the central level of government

**D**: Local candidates, who are CPV elites at various ranks who operate at local levels of government

There is some overlap between A and C, as some cabinet members who are members of A also have to compete in and win seats in the legislature to justify their position. There is some but very minimal overlap between B and D. For simplicity, we can assume these groups of actors to be mutually exclusive.

In each electoral district, one central candidate C would compete against multiple local candidates D. Provincial executives B are in charge of election management efforts, including electioneering e.g. allocating candidates between different districts, deciding district sizes, etc., and voter mobilization/persuasion. Because of this, they have the power to influence election results. The candidates C and D themselves play minimal roles in the electoral process.

The regime leadership A wants all the central candidates C to win elections, and orders provincial executives B to make that happen. Provincial executives B are held accountable to the regime leadership A through the cadre management system and the top-down allocation of resources. Thus, A may reward a successful provincial executive B by promoting them, or by giving them more funding through central transfers. If A wants to punish B, it may choose to do it through demotion or a delay in promotion, or by reducing transfers to B’s province.

At the same time, because both provincial executives B and local candidates D are tied to the same provinces, provincial executives B also want as many local candidates D to win as possible. However, the more efforts provincial executives B invest in helping local candidates D win elections may jeopardize the chance of central candidates C and vice versa. It is also possible to arrive at a sub-optimal outcome in which too many votes go to either C or D that not enough votes are left for the other to pass the minimum threshold, resulting in unfilled seats. Provincial executives B must decide how much they want to help central candidates C vs. local candidates D and allocate their efforts accordingly.

The paper argues that the regime leadership A could be using election results to test the competence or loyalty of provincial executives B. Specifically, if a central candidate C is defeated in B’s province, the regime leadership A could infer that B either did an inadequate job at helping C or intentionally chose to favor D in defiance of A’s order. If this is the case, then the regime leadership A should seek to punish provincial executives B whenever it sees a central candidate defeat.

Evidence of punishment happening to provincial executives B thus constitutes support of the paper’s theory. In other words, the theory does not suggest that “*underperformance by local candidates will lead to their punishment (e.g., lack of promotions)*” as the reviewer has noted in their comment because the performance being assessed here is that of the provincial bureaucrats B and not that of the local candidates C.

In other words, the “elites” in the paper’s elite-monitoring function are not the candidates running in the election. Rather, the “elites” refer to the politicians/bureaucrats who are in charge of manipulating the elections to ensure victories for central candidates preferred by the regime. For the regime, elections thus serve as a test of compliance, specifically of provincial executives’ willingness and ability to comply with an order that conflicts with some of their self-interests. This distinguishes the theory in my paper from others that rely on the candidate-monitoring mechanism.

The idea that elections may help authoritarian regimes monitor the local politicians/bureaucrats in charge of election management instead of the candidates running in the elections has previously been proposed by Malesky and Schuler specifically for the case of Vietnam:

*Nevertheless, rigged elections can also allow regime elites to monitor bureaucratic competence and allegiance, but in a very different way. In these cases, the signal that regime leaders read is not the vote share of a particular bureaucrat. Indeed, the bureaucrat under surveillance may not even be a candidate. Rather, the regime monitors regional compliance by assessing how well the local bureaucrat  
manipulates the election to assure victory and supermajorities for candidates favored by the regime. Such knowledge is most important for countries characterized by severe political and ethnic cleavages that map onto geographic boundaries.* (2011, 496)

Outside of Vietnam, Myagkov, Ordeshook, and Shakin have also described it in the case of Russia:

*Absent the usual signals that a true democracy, imbedded in a market economy, provides, the Kremlin needs ways to judge the loyalty and competence of those outside its walls, and elections  
serve that purpose. A weak showing, relative to the past, on the part of Putin, Medvedev, or United Russia in some oblast, rayon, or precinct signals a governor or local apparatchik who needs replacement if not outright incarceration.* (2009, 136)

Additionally, there is also some similarity between how an authoritarian regime may use election results to monitor the election management performance of its politicians/bureaucrats and how a clientelistic party may use election results to monitor the performance of its brokers. Larreguy, Marshall, and Querubin (2016) were one of the first to look at this function of elections and study its impact.

In sum, previous theoretical and empirical scholarship has started to explore the ability of elections to help authoritarian regimes monitor their politicians/agents through their performance in securing victories for regime-favored candidates even when the former are not candidates themselves. There have not been, however, rigorous tests of this function, both on its own as well as vis-à-vis other functions of authoritarian elections, and my paper seeks to fill in this gap.

In the paper, I find evidence against this theory of elections as a device to monitor local politicians. Specifically, whereas this theory predicts that the regime A would punish provincial executives B in provinces where central candidates D are defeated by reducing transfers to their provinces, I find that it does not do this. Instead, the regime chooses to send more money to these provinces.

To provide additional support for this conclusion, in Appendix H of the revised manuscript, I also look at other forms of punishment that the regime leadership A could use against provincial executives B. I find that provincial executives B did not suffer other forms of punishment such as firing, demotion, or delay of promotion.

I have also followed the advice of the reviewer in the previous round of review and gathered a large body of qualitative evidence in Appendix G, which further confirms that none of the provincial executives in provinces that saw central candidate defeats – as well as their subordinates at the district level – has suffered any punishment in any form.

As a side note, the candidate-monitoring mechanism of elections has already been explored for the Vietnam case by Malesky and Schuler (2013), who find no evidence that the regime is considering the performance of individual local candidates C when deciding whether to promote them to higher positions. In fact, it was the reviewer’s previous suggestion to engage more with Malesky and Schuler’s work that had prompted me to discuss this paper. This discussion is found in footnote 10 on page 12-TK of the manuscript’s previous version, but I have revised it to emphasize the distinction between my theory and that of Malesky and Schuler (2013). The revised discussion is now in footnote 10 on page 13-TK of the newly revised manuscript.

1. *On the idea of informational tradeoffs and informational complexity from election results,* the reviewer finds that the paper has not explored this idea enough for it to be a theoretical contribution. The reviewer recommends that I consider a formal treatment of the idea, as well as to provide specific examples of the informational tradeoffs that authoritarian regimes have to face.

My paper builds on a diverse body of theoretical and empirical scholarship that has explored the tradeoff between autocrats’ use of elections as an information source and their need to win these elections. The paper contributes to this body of theory by arguing that similar tradeoffs also exist between different types of information. I note specifically four related ways such tradeoffs can exist:

* When two types of information are observationally equivalent given a same set of results
* When one type of information can only be made clear if the regime selectively manipulates some component of the election to prevent it from giving any signal about another type of information
* When some types of information can only be made clear if the regime selectively refrains from manipulating some components of the election, but doing so with too many components of the election compromises the regime’s other goals e.g. winning convincingly
* When one type of information requires a policy response that contradicts the rational policy response that would be required by another type of information

To be fully transparent, I have not originally intended for this to be the main theoretical innovation of the paper. Rather, as Reviewer 1 generously notes, the paper seeks to “*synthesize and present the informational arguments about elections*” and identify potential contradictions that exist among these arguments. The arguments about informational tradeoffs and informational complexity serve to explain why such contradiction may exist, but they are not meant to be fully comprehensive.

I am grateful to the reviewer for the suggestion to develop a formal model. Since this would take more space than the current paper allows, I intend to pursue this suggestion in a separate paper with a clearer focus on the formal model. In the current paper, I have added Footnote 2 to page 4 of the revised manuscript to acknowledge the potential that my theory could motivate extensions to current formal treatments of the tradeoff between the informational value of elections and their certainty. For example, Little (2017) – which was suggested to me by Reviewer 1 in the previous round of review – has explicitly modeled the informativeness of an election as where is the incumbent vote share and is the information to be learned, in this case, the incumbent’s strength. An extension of this model to allow for multiple types of information could potentially be very useful.

I am also thankful for the reviewer’s advice to add several specific examples of the tradeoffs to the revised manuscript. Specifically, on page 3, I have brought the example from Kenya by Gandhi (2015) (previously suggested to me by Reviewer 1) to the forefront. Also on page 3, I have added an example of a very similar tradeoff facing clientelistic parties in Mexico from Larreguy, Marshall, and Querubin (2016).

1. *On the RDD setup*, the reviewer questions why defeats and not general underperformance drove most of the effect.

As the reviewer also notes, this concern had also been brought up by Reviewer 3 in the previous round of review. In response to this concern, I have argued that the exact closeness of an election – and hence the degree of underperformance by individual candidates – is difficult to observe in elections with multimember districts. In the paper, I have relied on a non-straightforward algorithm to identify close races for the RDD analysis (this algorithm is detailed in Footnote 13 on page 19-TK of the revised manuscript), and I do not believe the CPV would have resorted to anything similar. For this reason, from the regime’s perspective, any information about the level of performance to be gained from vote shares alone would remain noisy.

In contrast, the binary signal of a defeat is noticeable and cannot be easily mistaken. A central candidate defeat, no matter how close, always requires the regime to adjust its personnel plans for the incoming legislature; it also attracts the public’s attention. As I noted in my previous response to Reviewer 3, official accounts of the central candidate defeats always focus on the defeats themselves without mentioning the margins.

For these reasons, it is more reasonable for the regime to base its reactions on the binary signal of individual central candidate defeats rather than other measures of these candidates’ performance. I hope the reviewer may find it reassuring that Reviewer 3, who also brought up this point in the previous round of review, has found the above explanation persuasive and now agrees that *“the closeness of such elections is complicated (by multimember districts) and noisy, and that the primary signal is a binary one.”*

1. *On the data limitation*, the reviewer remains unconvinced that the paper has successfully addressed its small sample size problem.

To address this concern, the many empirical techniques I have chosen for this paper are those specifically designed to tackle the small sample size problem. As I explain in Appendix C of the revised manuscript, a small sample size may compromise an analysis in two ways: a) undermining inferences that rely on asymptotic properties and b) introducing small sample biases. In terms of inferences, I have applied randomization-based inferences in both my local randomization RDD analysis as well as in the generalized synthetic control analysis. (The fact that the generalized synthetic control method also uses randomization-based inferences was not presented clearly in previous versions of the manuscript; I have therefore added this point to the discussion on page TK of the revised manuscript). In the previous round of review, the reviewer has also recommended a simple randomization inference analysis, which I have done and found results that agree with my main analyses. In terms of biases, Appendix C has demonstrated that my estimates are very robust to perturbations, both to the control and to the treatment sets, which suggests that the main results are not an artifact of the sample size.

In addition, although there a few provinces in the sample, in all analyses I have made use of the long panel data to improve the estimates’ precisions.

Looking at the number of central candidate defeats i.e. the size of the treated pool, I note that my analysis is more robust than the reviewer suggests. Overall, there are 5 cases of election defeats that happened, but I intentionally dropped one from the analysis because its margin was larger than all the rest. Adding back this case does not weaken the results; in fact, it even makes the results stronger.

In particular regard to the generalized synthetic control, I would like to note that the method is very well-equipped to work with very small numbers of treated units. I have previously alluded to this advantage of the generalized synthetic control method in my earlier revision, but in light of the reviewer’s concern I have made this point more transparent in the description of the method on page TK of the revised manuscript. Specifically, I explain more clearly that the method is able to estimate individual treatment effects for each and every treated case and so can perform even when there is only one province with defeat. This is because the method draws only on data from the control units in the model-fitting process. In estimating the treatment effect, because it applies the model separately to each of the treated unit to identify its own outcome under the counterfactual, it can estimate each treated unit’s individual treatment effect without relying on data from other treated units. This means that the method should be reliable even for scenarios with as few as one treated unit.

In particular, to summarize Xu’s (2017) description of the method, it first uses data from the control units to fit a model of pre-treatment outcomes. It then applies this model on each individual treated unit to predict its pre-treatment and post-treatment outcomes. The credibility of the method depends on how well the prediction for each treated unit’s pre-treatment outcomes match with their actual values. If the match is perfect, i.e. the predicted pre-treatment outcomes are the same as the actual pre-treatment outcomes, then the prediction for the post-treatment outcomes can be assumed to be identical to the counterfactual values under control; the set of predicted outcomes for each treated unit is said to this unit’s synthetic control. Individual treatment effects are then estimated by taking the difference between each treated unit’s actual post-treatment outcomes and those of the synthetic controls. Aggregate treatment effects are estimated by averaging the individual treatment effects.

The appropriateness of the generalized synthetic control (as well as its “cousin” the original synthetic control method) for small numbers of treated units is best understood when considering how its logic closely emulates the comparative case study method, specifically the most-similar design. It is analogous to a qualitative approach that pairs each treated unit with a most-similar untreated unit then compare the difference in outcomes within each pair. The quantitative approach by the (generalized) synthetic control improves this process by allowing multiple untreated units to be blended together into a single control that matches the treated unit better than any individual untreated unit could.

The above description suggests that the performance of the generalized synthetic control method depends not on the number of treated cases. In this sense it is similar to the original synthetic control method by Abadie et al (2010, 2015). It is telling that in one of the examples in Xu (2017, Table 3, page 72) there are only 3 treated cases. As I have also mentioned in my response to the reviewer’s previous review, Abadie et al (2010, 2015) who proposed the synthetic control method also focused on studies with only one single treated unit.

Instead, the method’s performance depends on the number of control units instead of the number of treated units. In this regard, my analysis may make use of up to 52 untreated cases. In contrast, Abadie et al (2010) used only 38 control cases to study the effect of California’s tobacco control program (of which only 33 ended up being dropped, leaving only 5 in the final synthetic control), and Abadie et al (2015) used only 16 control cases to study the impact of reunification to West Germany (of which only 5 entered the final synthetic control).

My confidence in the credibility of the generalized synthetic control analyses can be further backed up by evidence from the paper. Specifically, the method is best evaluated by the degree to which the synthetic control’s pre-treatment outcomes match with those of the actual observed treated cases. If there is indeed insufficient data for the method to perform, it would fail to generate a synthetic control that successfully matches the pre-treatment outcomes of the treated units. Instead, the match should be imperfect, and the line showing the treatment effect in the result plots should diverge from the zero line in some periods during the pre-treatment years. Figure 4 on page 27 of the manuscript demonstrates that the match is perfect: it shows that, before the treatment kicks in in 2017, the synthetic controls and the treated observations are balanced in terms of all treatment outcomes.

To provide even further evidence of this method’s credibility, I have also added to the individual provincial accounts in Appendix G-TK different plots that are similar to Figure 4 but show the result for all five individual treatment effects – including that of Can Tho which has been dropped from the main analysis. In all five plots, each treated observation shows remarkable balance when compared with its respective synthetic control. This confirms that the generalized synthetic control method performs well in both the aggregate case as well as for each individual treated unit.

It is also important to note that, besides bias, inferences by the generalized synthetic control method also has excellent finite sample properties, meaning that the correctness of its standard errors has been found to be very insensitive to the sample size. The simulations by Xu (2019, Table 1, page 68) show that halving the number of control units from 80 to 40 and halving the length of pre-treatment periods from 30 to 15 has virtually no impact on the coverage probabilities of the 95 confidence intervals around the treatment effect estimates. This offers strong reasons to believe that the confidence intervals the generalized synthetic control figures throughout the manuscript and the appendix are appropriate.

Finally, after the previous round of revision, I also heeded the reviewer’s previous advice and added significant qualitative evidence for each individual case of central candidate defeats. As the reviewer can see in Appendix G, all individual accounts tell essentially the same story, which I hope can assuage the reviewer’s skepticism of my other results.

1. *On the results from previous elections in Appendix E*, the reviewer believes that they do not agree with my conclusions.

On this point, I respectfully note that the reviewer’s interpretation of Appendix E is not consistent with the content I present in Appendix E. Specifically, Appendix E suggests that the results from previous elections *do* seem to hold up. While it is true that Figures E.1 and E.4 present null results, these null results come from bad data – I intentionally present them to show that bad data would lead to biased estimates, and the two figures show exactly that. In other words, the null results in Figures E.1 and E.4 are incorrect.

The key takeaways from Appendix E are actually found in Figures E.2, E.3, and E.5. In Figure E.2, I show that as soon as I attempt to mitigate some of the biases coming from bad data – through the use of an imperfect synthetic control analysis – the results begin to trend upwards and yield positive and statistically significant results. In particular, the average effect over the entire post-election period, defined as the area under the black curve, is clearly larger than zero, driven by a strong effect manifesting in the year 2014.

Moreover, given that the analysis in Figure E.2 minimizes but still faces data problems, I show in Figures E.3 and E.5 the entire universe of results that I may be able to get if all the data is available. They show that, if I were to observe fully vote shares for every candidate in the 2007 and 2011 elections and proceeded to estimate the treatment effects of central candidate defeats as I did in the main manuscript, the result is overwhelmingly going to be positive. The violin plots in Figures E.3 and E.5 specifically show the distribution of treatment effect estimates that could be produced by considering every possible allocation of the unobserved vote shares among all the candidates. The fact that the mass of the plots is consistently concentrated above zero for all specifications suggests that these estimates are likely to be positive. The results from this analysis serve as a form of sensitivity analysis and show that my conclusions in the main manuscript are relatively insensitive to the data.

1. *Finally, on the pre-treatment imbalance observed in 2014*, the reviewer is concerned that the large pre-treatment difference in funding between provinces with defeats and provinces without defeats in the opposite of the main effect may suggest that the effect only restored defeated provinces’ funding to the pre-2014 level. Additionally, the reviewer questions whether the votes against central candidates were a reaction to this 2014 drop in transfers.

I thank the reviewer for this very important point. In an earlier draft of the paper, I have discussed this pre-treatment imbalance, which is apparent in Figure 2 as well as in Figure 3 of the manuscript, but I had cut the discussion of it due to space constraints. In response to the reviewer’s comment, I have added this discussion back to pages TK of the revised manuscript.

In this discussion, I highlight that the gap in 2014 could result in dynamic causality if pre-treatment outcome could have an impact on post-treatment outcome i.e. if the 2016 effect was just to restore some provinces to its pre-2014 level, or if pre-treatment outcome could have an impact on the probability of treatment i.e. if the drop in funding led to votes against the regime.

Fortunately, although the linear fixed effects model (in Figure 2), as well as the local randomization analysis (in Figure 3), cannot adequately control for dynamic causality, the generalized synthetic control method (in Figure 4) is especially suited for this task. It shows that, even after all the pre-treatment differences in outcomes have been controlled for by perfectly match treated units with synthetic controls that are identical in terms of pre-treatment outcomes, the treatment effect remains strong, positive, and significant. Even when comparing provinces that witnessed losses with other provinces that did not and that had the same level of transfers for every year from 2003 to 2016, provinces that witnessed losses still received additional transfers from 2017 to 2019. This rules out the hypothesis that the treatment effect was only to restore these provinces’ funding to the 2014 level. By controlling for the 2014 difference, it also rules out the effect of 2014 transfers on 2016 vote results.

More intuitively, the individual accounts in Appendix G – which I have added in the last round of revision in response to a suggestion from the reviewer – also show that the provinces with central candidate defeats receive a higher level of transfers in the 2017-2019 period than they did in the pre-2014 period.

Reviewer 3 - TK