Outline for 2007 and 2011 results

In the main paper, I focus on the 2016 elections because focusing on 2016 would provide the most analytical leverage

* First, only in 2016 did the CPV make available vote shares for the defeated candidates. Previously, defeated candidates are listed by names only, and no publicly available records would contain their vote shares. Thus it is impossible to determine the margin by which each loser has lost.
  + Information on margin is important because it helps separate out central candidate defeats that were close and hence truly surprising, and central candidate defeats that seemed too certain to be unexpected.
  + Central candidates who lost by a large margin are likely to be different from those who lost only narrowly, and provinces where central candidates are permitted to lose that heavily are also likely to be different.
    - It is plausible that these candidates would be significantly less popular with both the voting public and the provincial Party apparatus. If this were the case, their defeats would not be surprising, and thus would not necessitate a response on behalf of the CPV. Including these cases would attenuate the estimated effect towards zero, resulting in a bias against the result.
    - Another hypothesis – kindly suggested by an anonymous reviewer – is that these candidates are disloyal, incompetent or ageing elites that the ruling party wants to punish with electoral defeats, and provinces may receive reward in the form of increased transfers if they could ensure such defeats. Including these defeats would then result in a bias in favor of the result.
    - In the Vietnamese context, these candidate-level explanations are tenuous on their own, because the Party already has access to the three negotiation rounds preceding each election, which is a more effective instrument to filter out weak or unwanted elites. In these negotiation rounds, the Party can decide both which elites they want to contest in VNA elections, and which district they would contest in. It serves the Party no purpose to allow unpopular elites to even contest and risk certain defeats when they have a large pool of other elites to draw from, and also a wide range of non-electoral positions within the party apparatus that they could give to these less popular elites. Similarly, punishing disloyal elites with election defeats are excessive and uncertain when it is possible to do that much earlier during the negotiation rounds.
      * A possible explanation – again kindly suggested by an anonymous reviewer – is that the regime needs some defeats by central candidates to show that the elections as competitive. In the case of Vietnam, however, it seems unlikely that elections serve this legitimation function. Official narratives have mostly cast the legislative elections as a technocratic tools – helping select “competent and moral” (tài đức) people to serve rather than a platform for representation or even power-sharing [CITATION]. When central candidates happen, these defeats are discussed negatively by the state media [CITATION].
  + The more plausible difference between close and large central candidate defeats are likely found at the province level. Whereas close defeats can happen out of a miscalculation by provincial officials, landslide defeats are more likely to be intentional. Provinces where such defeats happen may thus tend to be more independent of the central government. And, if they were independent enough to boldly reject central candidates, the central government would already have known this, which means that the election defeat should not provide any extra information and thus should not elicit any special response. Including provinces with heavy defeats in the analysis would therefore lead to an underestimation of the average treatment effects.
    - Compared to candidate-level explanation for large central candidate defeats differ from narrow ones, this province-level explanation is more plausible. Whereas the regime can use the negotiation rounds to exclude unwanted candidates from even contesting, it cannot exclude individual provinces from fielding central candidates. Thus, it has no way to avoid potential noncompliance from independent provinces, and thus has to expect the possibility of central candidate defeats there.
    - Given that the regime expects central candidate defeats to be more likely in certain provinces, it is possible that they would send central candidates of whom defeats are not the most consequential to contest in these provinces. These maybe less popular candidates serving in less important committees, or representatives from less powerful central-level organizations, such as the Veterans Organization or the Writers’ Guild. As argued above, however, they are unlikely to be elites the regime wishes to be purged, because if such punishment is desirable the regime would have had much more reliable methods to do so than to rely on the cooperation of provincial officials already known to be resistant to central commands.
  + In any case, it can be established that central candidate defeats with large margins are different from those with narrow margins in ways that are substantively important. Unlike narrow defeats, large defeats are less likely to be inconvenient for the regime, and provide less information content because they are not unexpected. To truly identify the CPV’s response to information from elections, it is necessary to exclude these cases from the analysis. Failing to do so may bias the estimates, likely towards zero. Because vote shares data are not available for defeated candidates in 2011 and 2007 elections, I focus instead on the 2016 elections.
    - In practice, focusing on 2016 elections enable two analyses
      * First, it allows me to restrict the linear models only to cases of close victories and close defeats.
      * Second, it allows me to conduct candidate-level randomization tests
* Second, focusing on the treatment year of 2016 offers a longer pre-treatment period. Given that budgetary data for Vietnam is only available from 2004 onward, an analysis focused on the 2016 election (and hence designating 2017 as the first post-treatment year) would have 13 pre-treatment periods. This is most critical for the synthetic control method.
  + The long pre-treatment period allows more data to be used to construct the synthetic control. The 13 pre-treatment periods is already close to what Abadie et al (CITATION) recommended.
  + An analysis for the 2011 election would have fewer pre-treatment data, which could affect both estimation and inference. With only 4 pre-treatment periods, synthetic control is simply not possible for the 2007 elections.

Nevertheless, even though focusing on the 2016 election would lead to the most internally valid analysis, external validity requires that I identify whether the Vietnamese government also responded to localized central candidate defeats in 2007 and 2011 with similar increases in central transfers. I thus attempt to replicate the 2016 analyses for 2007 and 2011 elections

* There are several analytical challenges to this.
  + For the linear fixed effects models, inability to filter out provinces with large and thus unsurprising defeats leads to dissimilarity between treated and control provinces. The treated pool now also contains an unknown number of provinces that are fundamentally different from control provinces.
    - Because the effect of defeat on central transfers in these provinces are expected to be zero, the estimated treatment effect is likely to be downward-biased. More importantly, because central transfers dynamics in these provinces are expected to differ even before the elections, their inclusion is likely to induce dynamic effects as well.
    - Figure XX shows that both these concerns hold true.
      * LFE EFFECT FIGURE FOR 2011 GOES HERE
      * The point estimate for the treatment effect is close to zero
      * There is a large placebo effect both at t-1 and at t-2, which should not be expected if treated and control provinces are comparable. Especially at t-2, the placebo effects are large, and although noisily estimated, are significant at the .5 level in one specification. In terms of point estimates alone, they are larger than all the placebo effect estimates in the 2016 analysis. Each placebo effect for 2011 is much larger than the corresponding effect for 2016.
    - It has to be admitted the linear fixed effects models are compromised here and unlikely to yield a correct result
  + For the local randomization interpretation of the regression discontinuity framework, bandwidth selection is not possible when vote shares are not available for defeated candidates. In addition, because vote shares are not available for defeated candidates, it is not possible to calculate the margins by which winders won.
    - I resort to a relaxed version of the bandwidth selection procedure. Specifically, I conduct only one-sided bandwidth selection, and do that on recorded vote shares for winners instead of vote margins for both winners and losers. I essentially search for an upper boundary such that central candidates who won with vote shares smaller than this boundary are statistically indistinguishable from all central candidates who lost.
    - This approach is unlikely to yield a satisfying bandwidth that would meet the requirements of the regression discontinuity framework. Because central candidates who lost with large margins are already different from central candidates who lost with narrow margins, they are even more likely to differ from central candidates who won. Expanding or contracting the upper margin is unlikely to remediate this problem
    - Evidently, the resulting window from this approach (-Inf, 12.5) is not stable. It only takes small perturbation to the upper boundary for the candidates on either side of the zero threshold to be statistically different again. This raises concern that the lack of difference from this threshold may be a statistical artifact.
    - The result, shown in Figure XX, also provides little reason for confidence.
      * RDD EFFECT FIGURE FOR 2011 GOES HERE
      * Similar to the linear fixed effects results, the point estimate of the treatment effect from this analysis is also attenuated to zero, whereas estimates of placebo effects are large, and although statistically indistinguishable from zero, is substantively large.
  + In both previous analyses, the presence of significant placebo effects suggest the presence of serious dynamic causality. This calls for the generalized synthetic control
    - On their own, differences in pre-treatment outcomes are already serious threat to causality because they imply differences in post-treatment counterfactual outcomes. When pre-treatment outcomes can plausibly influence the likelihood of treatment – in this case, the amount of central transfers a province received in the years before an election may make it more or less likely for central candidates to lose – such differences may further compound the problem, resulting in seriously biased estimates
    - Unlike in the 2016 analysis, therefore, addressing this issue is critical. The only method that can plausibly do so is the generalized synthetic control method. In the presence of dynamic causality, it provides the only reliable estimate of the causal effect.
      * In this case, however, estimate is likely to be noisy because there are only a few pre-treatment periods
    - The result, shown in Figure XX, shows that the generalized synthetic control has partially mitigated dynamic causality: the pre-treatment differences in outcomes are much smaller, and tend towards zero around the treatment year
      * GSYNTH FIGURE FOR 2011 GOES HERE
      * Estimates of the treatment effect is found to be positive in all post-treatment years, with magnitude increasing the further away from the election year and becoming statistically significant (at the .1 level) at t+4.
    - The generalized synthetic control is still far from perfect
      * Short pre-treatment period results in imprecision
      * It mitigates but does not eliminate dynamic causality
    - Yet, it offers the most reliable estimate of the treatment effect for 2011 given constraint in the data. The evidence suggests a tentative positive effect i.e. an increase in central transfers to provinces where localized defeats happened.
      * When compared with estimates using linear fixed effects and local randomization, it is possible to see that the magnitude of the estimate increases as dynamic causality is reduced, suggesting that perhaps confounding induced by provinces with large defeats leads to downward bias