Hi Asako,

This is my take on the paper – let me know what you think…

The manuscript explores the effect of precipitation on voter turnout. Specifically, the authors look to explain county level turnout in presidential elections from 1972-2000. They hypothesize that precipitation leads to a decrease in turnout for non-major party candidates and in competitive election states leads to increased turnout for the major party candidate closest to the leading non-major party candidate.

Let me try to explain the mechanism the authors argue underlies their hypotheses. They posit that precipitation causes individuals to become risk averse, which leads them to change their vote choice. In the case of voters who prefer non-major party candidates, this increase in risk aversion drives them to vote for a major party candidate closest to the leading non-major party candidate. This mechanism is so ridiculous and convoluted as to have only come about as a post hoc explanation of these strange empirical results.

Indeed, the fact that the rain and snow interactions with competitiveness have opposite signed effects should have tipped the authors off to the implausibility of the mechanism and the lack of robustness of the results. Where is the discussion of snowfall? Are snow and rain different in terms of regret aversion?

I don’t see the connection between regret aversion and turnout. The authors simply site a conference paper that attempts to link weather and candidate perception in the lab.

In terms of the model, the positive coefficient on the lower order rain variable indicates that rain had a positive effect in non-competitive states for non-major party candidates. Can the authors explain that? It seems contrary to their theory.

There is no evidence that the matching worked. Asserting it is inadequate; show the balance statistics.

The competitiveness measure seems to be based on the actual margin of victory in the state for the election turnout that is being predicted. Isn’t this an endogeneity problem? Shouldn't we be concerned about the causal loop here (at least for a number of counties and elections)?

The authors do not say how they determine which major party candidate is closest to the non-major party candidates. This would be a non-trivial exercise as non-major party candidates frequently have some policy positions close to Dems and others close to Reps.

The authors seem to assume that non- major party supporters are consistently so. That we should see the same number of them turnout year after year, when we know from decades of political science research that that is not the case.

Moreover, the authors never statistically demonstrate the movement of non major party supporters to major party voters. They merely run separate models for each candidate party subset. We have no way of seeing how voters moved with these aggregate level data and models. All that is provided is a lag turnout measure, which doesn’t help us understand whether new voters turned out or old ones changed preferences…etc. The authors are confused throughout the manuscript about what the aggregate level data can tell us.

The authors provide no evidence of their own nor do they point to any work that shows that non-major party supporters across 30 years of elections “know that their preferred candidate will not win...”

In general, this paper demonstrates a lack of understanding of non-major party voters and the vast literature on them.