

Review of NHESS-2023-192

Title: Investigation of an extreme rainfall event during 8-12 December 2018 over central Viet Nam – Part 2: An evaluation of predictability using a time-lagged cloud-resolving ensemble system

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Summary: After the first round of revisions, the authors have done a deeper analysis of the results. While I do find the manuscript more cohesive, some of my initial concerns from the first review are still present. These include the lack of presented hypotheses and what this paper adds to the body of literature, chiefly. The additional analysis allows me to see a path to deal with these concerns, compared to the last version of the manuscript. For these reasons, I inform a decision of *Major Revisions*.

Recommendation: Major Revisions

Major Comments (Comments are not listed in order of importance):

1. No hypotheses are presented in this work, this comment remains the same from the previous review. Please include specific hypotheses. This will also help guide the reader into making clear what the manuscript adds to the body of literature. As I have said before, having model simulations are not alone publishable unless used to evaluate a scientific question/hypothesis. It is thus important to outline scientific based hypotheses in which the experiments in the manuscript are designed to evaluate, which will then make it clearer how the work adds to the body of literature. Please state them.
2. From the last round of revision: Some discussion and framing of the work here from a context of intrinsic versus practical predictability is needed. Additionally, the scale dependence of predictability. I suggest Melhauser and Zhang (2012), Nielsen and Schumacher (2016), Weyn and Durran (2018), and citations within as starting points. There is also some useful suggestions from an ensemble analysis within these papers. The authors mention in the review responses that there was discussion on intrinsic vs. practical predictability added as well as these references. I could not find where in the manuscript this was done.
3. How was the WRF data mentioned in the methods section used? I did not see this in the new version of the manuscript. It is possible I missed it. If it is not used, please remove from the methods.
4. As I have mentioned above and in the last review, there needs to be some aspect of the conclusions/experiments that add to the body of literature ensemble prediction of extreme precipitation. This is currently missing in my opinion, as we generally know predictability increases as lead-time decreases. The authors claim that this is a novel result for this geographic region in their review responses. I can be convinced of this, if this is clearly demonstrated with correctly posed hypotheses and results discussion (see point 1). One potential avenue could be comparing the spread in the event given by a time lagged ensemble compared to a traditional ensemble initialized at one specific time

leading up to the event. This might speak to more general predictability in the region. The above citations can potentially aide in adding this piece to the paper.

Additional Comments (Comments are not listed in order of importance):

1. Throughout: Please make use of more concise paragraphs to organize the manuscript. Some paragraphs in the current version span the entirety of a page or more. Breaking these up into smaller paragraphs at logical points will help with the flow and readability of the manuscript.
2. Figures 10-13: These are quite hard to read. I cannot make out the light blue contours that are overlaid on these plots. I can barely make out the black contours. Please evaluate the color choices on these. Additionally, the panels are quite small. It might make sense to break these into multiple figures.
3. Lines 336-338: This is true for a well calibrated ensemble, only. I would mention this.
4. Table 1 and lines 266: How many moments is the bulk microphysics scheme?
5. Line 55: Remove “until now,” as it contradicts the rest of the sentence.