NHESS-2023-192

Authors' Responses to Reviewer 1 (RC1, anonymous)

Date: 6 June 2024

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

Authors: C. C. Wang et al.

Firstly, **we thank the reviewer for spending valuable time reviewing the paper and giving us constructive comments that helps to improve the clarity of the paper.**

COMMENTS

In general, I think this is a good follow-up study to evaluate the usefulness of CReSS ensembles in forecasting an extreme rainfall event over Viet Nam. However, I believe significant editorial improvements (texts and figures) are necessary to enhance the manuscript's readability. For example, varies data are descripted in the data section (Section 2.1), but many of them are not used in the subsequent sections (e.g. TIGGE, WRF). Some of the colour bars and their labels are quite small. Overall, I think the current manuscript needs major revision.

Specific comments:

Abstract: The authors should state the full form first before using abbreviations e.g. CReSS, QPF, SSS, and ESA.

Reply: Thank you for your comment. We have made corrections and give the full form in the first use of these abbreviations.

Line 36: Perhaps it would add some clarity to "The observational data …" by adding GPM in the text.

Reply: Thanks for your suggestion. Here, we are referring to the rain-gauge observations, so we mention this explicitly to add some clarity, along the lines as

suggested. The GMP data are also used in this study, so they are mentioned later in the text.

Lines 40-42: Missing reference(s)?

Reply: We agree with you, we have added the reference as suggested.

Lines 97-98: Missing reference(s)?

Reply: Thank you for your concern. The references were cited on line 101 in the previous draft, and we now move some of them here for better fluency and clarity, along the lines as suggested.

Lines 113-119: I am not sure whether these details are relevant to the current study.

Reply: Thank you for your comments. To avoid too much detail, we have deleted the specific numbers in the favorable factors in the revision, as suggested.

Lines 123-139: Given the nature of this study is to demonstrate the CReSS ensemble can produce good forecast of extreme precipitation events. Perhaps it is necessary to have a brief description of other models, which could perform simulation at a cloud-resolving resolution, e.g. MM5 by Son and Tan (2009), WRF by Toan et al. (2018) and Nhu et al. (2017). Information such as resolution and relevant parameterisation schemes (if any) would be relevant in this case. If the resolution and relevant parametrisation schemes of those models would be the same as the current study, the authors might want to emphasis this point.

Reply: Thanks for your suggestion. We will make changes and add the brief description in the revision, as suggested.

Lines 139-147: The authors might want to highlight the fact that these global NWP models do not have “cloud-resolving resolution”.

Reply: Yes, you are right and we will make the highlight as suggested. Besides, we would also like to point out that the current global weather prediction models cannot capture the precipitation at the correct quantity in Vietnam.

Line 173: Not sure why UKMO is mentioned but their forecast outputs were not used. Also “ECMWF of the European Union” is not accurate as the UK is a member state of ECMWF but not in the European Union.

Reply: Thank you for pointing this out. We have removed that information. We used the UKMO data in the first version of the manuscript. However, we had removed it from the submitted version of manuscript. But we were missed to remove the relevant information at line 173.

Lines 175-176: The authors should state the usage of this dataset at the beginning of the paragraph first rather than in the middle of the paragraph.

Reply: Thanks for your suggestion. In the revision, we have mentioned that this dataset is used in the study in the first sentence of the paragraph, as suggested.

Section 2.1.2: It might be worthwhile to highlight the fact that NCEP GFS is a deterministic forecast.

Reply: Thank you for your comments. As suggested, this clarification has been made in the revision and now the sentence reads as "… from the analyses and deterministic forecast runs…"

Section 2.1.3: The authors should highlight the fact that these are the in-situ observation data as GPM data is also a type of "observation" data.

Reply: Thank you for your suggestion. We will make change in the revision, as suggested.

Section 2.1: The authors might want to restructure this section so that the data, which serves as similar purpose would be grouped together. A possible structure could be: "Section 2.1.1" In-situ observation data and "Section 2.1.2" GPM are used to model validation (ground truth); "Section 2.1.3" TIGGE and "Section 2.1.4" WRF are used to demonstrate the added values of CReSS ensemble; "Section 2.1.5" NCEP GFS is used to drive CReSS. This structure would fit nicely into "Section 2.2 Model description …"

Reply: Thank you for your suggestions. We will make change in the revision, as suggested.

Figure 1, Section 2.1: ERA5 was used in Figure 1 but was not mentioned in Section 2.1.

Reply: Thank you for your comments. We will update the information in the revision.

Lines 228-230: “The first members ran at 12:00 UTC on 3 December 2018, and the last member ran at 12:00 UTC on 10 December 2018…” à “The first member was initialised at 12:00 UTC on 3 December 2018, and the last member was initialised at 12:00 UTC on 10 December 2018. A new member was initialised every 6-hr within the period 1200 UTC 3 Dec 2018-1200 UTC 10 Dec 2018.”

Reply: Thank you for your suggestions. We will make change in the revision, as suggested.

Section 2.1.2, Lines 232-234, and Table 1: I am a bit confused. Did the authors use NCEP FNL (Lines 232-234, Table 1; also, from the first part of this two-part study [Wang and Nguyen 2022]) or NCEP GFS (as stated in Section 2.1.2) to drive CReSS?

Reply: Thank you for your comments. We corrected the information. It is NCEP GFS data as stated in Section 2.1.2.

Line 278: “If small spread …” à “For example, small spread indicates…”

Reply: Corrected.

Figure 3, Lines 312-313, Figure 5: I believe the OBS, which the authors are referring to, is not from GPM nor in-situ observations but from ERA5 as it has more similarity to Figure 1f than Figure 1e. The authors also mentioned stations but stations are not indicated in Figure 3. Perhaps, the authors meant to show another figure?

Reply: We apologize for the confusion. The data shown in Fig. 3 OBS is a combination of 24-h observed rainfall from the rain-gauge network and the surface wind analysis from ERA5, and the same is shown in Fig. 1f. We have clarified the information in the caption of both Figs. 1 and 3.

Figure 3: Perhaps I missed it, but it is not clear to me how good members (green) and bad members (red) are determined. For example, why does “00:00 UTC 9” is a good member whereas “18:00 UTC 8”, “06:00 UTC 9”, and “12:00 UTC 9” are not classified as good members but the spatial structures of these members are very similar to “00:00 UTC 9”.

Reply: We apologize for the confusion. In this study, good and bad members are determined by both visual comparison and the SSS skill score. Specifically, we compared the model results with observation, and good members were picked if

the quantitative rainfall and the spatial distribution of the rain band were closer to observation. For the skill score, we applied the similarity skill score to all members and picked good or bad members based on their score. Higher score means better members and lower score means worse member.

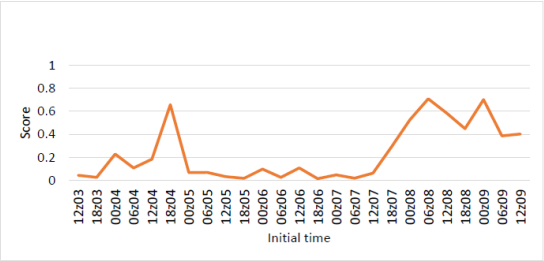

Figure 1. Similarity skill score of 25 time-lagged members for the 24-h accumulated rainfall of the Dec 10, 2018

Lines 318, 324: "… executed …" à "… initialised …"

Reply: Corrected

Line 341: Is it GFS or FNL?

Reply: Here, we are referring to the GFS forecast data, so it is the GFS instead of the FNL data, which is the analysis. Specifically, it is GFS version ds084.1. We hope that this is clear.

Lines 353-367; Figure 5: (1) Perhaps it might be clearer if the authors would rename the subgroups by using the period of forecast initialisations, e.g. "First 4 members" à "12 UTC 3- 06 UTC 4 Dec". (2) Similar grouping could be done by defining period using a "moving window period", e.g. "Subgroup 1: 12 UTC 3- 06 UTC 4 Dec", "Subgroup 2: 18 UTC 3- 12 UTC 4 Dec" etc.. The "moving window" approach could help the authors to better locate the optimal initialisation periods. (3) If the "moving window" approach is used, perhaps some kind of measure of the deviation of the 24h rainfall field between OBS and subgroups would be useful in quantifying the optimal initialisation periods.

Reply: Thank you for your comments and suggestions. We will add more information to our analysis to make it clearer.

Line 365: The rainfall of second 4 members is the highest "in" these… (A word is missing)

Reply: Thank you for your careful review. We have corrected it, as suggested.

Line 366: … due to “a single good forecast initialised at 1800 UTC on 4 Dec”.

Reply: Thank you for your careful review. We have corrected it, as suggested.

Lines 384-388: The definition of subgroups seems to be arbitrary. It is not clear to me why these subgroups are chosen for in-depth analysis. Furthermore, would this not be expected that the forecasts initialised closer to the target period would have a better forecast skill as certain features that are highly related to the extreme precipitation event are included in the initial conditions? In this sense, an interesting question would be: Why does the forecast initialised on 1800 UTC 4 Dec can (partially) capture the spatial extent of the extreme precipitation event of interest and such information was lost for the next 72 hours?

Reply: Our purpose in this paragraph is to examine how forecast quality evolved as the lead time shortened. Therefore, along with the evaluation on time-lagged results using batches of fixed number of successive runs (every 4 members) as presented in our analysis, this study also grouped the members using different ensemble sizes based on their behavior in order to better assess the temporal evolution of forecast uncertainty and event predictability as the lead time shortened

Figures 10, 11, 12: The authors should add some labels (u, v, qv) on the plot. Also missing labels on Figure 10a and Figure 10c

Reply: Thank you for your comments and suggestions. We will add this information to our figures to make it clearer.

Figure 10: Is it relating to surface wind, which typically is defined as 10-m wind, or 100-m wind (as stated in Lines 473-474).

Reply: Thank you for your comments. Here, we are referring to wind components at 100 meters. We have clarified the information in the caption of Figure 10.

Section 3.2: The authors should indicate which graph/panels the readers should be looking for. E.g. Lines 476-479: It should be referring to Figure 10a-c. etc.

Reply: Thank you for your comments and suggestions. Here, we are referring to wind components in Figures. 10a-f. We have added more information to our analysis to make it clearer.

Lines 502-509: I am confused about the Figures and the texts. What levels are actually showing and the authors are referring to. I guess the authors are comparing Figures 11 and 10? If this is the case, Line 504 should include something like "… 100 meters (Figure 10).

Reply: Thank you for your comments and suggestions. We also apologize for the confusion. Here, we are referring to wind components and water vapor mixing ratio at 5424 meters. Therefore, the Figure referred to is Figure 12. We also mentioned the relevant information in the first sentence of the paragraph. However, based on your experience, we have added more information to our analysis to make it clearer.