

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/nhess-2022-82-RC2>, 2022
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Comment on nhess-2022-82

Anonymous Referee #2

Referee comment on "Investigation of an extreme rainfall event during 8–12 December 2018 over central Vietnam – Part 1: Analysis and cloud-resolving simulation" by Chung-Chieh Wang and Duc Van Nguyen, Nat. Hazards Earth Syst. Sci. Discuss.,
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Review of "Investigation of An Extreme Rainfall Event during 8-12 December 2018 over Central Vietnam. Part I: Analysis and Cloud-Resolving Simulation" by Wang and Nguyen.

In this manuscript, the authors first analysed the synoptic environment of a record-breaking rainfall, the D18 event, then the local thermodynamic conditions prior to the D18 event, and finally used the Cloud Resolving Storm Simulator (CReSS) to study the role of topography in the D18 event over central Vietnam. I think the authors have done a lot of analyses to achieve certain objectives, yet, some of the results and discussions are poorly presented. I suggest return to author for **major revision**.

Major comments:

- I think the manuscript needs an extensive and thorough reorganisation to improve the presentation of the authors' idea.
- The motivation of the sensitivity study on the role of local terrain on the D18 event is unclear as the role of local terrain in heavy rainfall in central Vietnam seems to be well understood (Lines 76-79; 83-85).
- The motivation of using CReSS is not well presented in the introduction. Yet, the motivation of using CReSS can be found in later sections (Lines 171-172; 480-482).
- In the conclusion, the authors stated that "according to previous studies, the heavy and extreme rainfall events are usually due to the multi-interaction between the northeasterly wind and preexisting tropical disturbance over the SCS and local topography or tropical cyclone or impacts by ENSO or MJO. However, these factors have not appeared during the D18 event". I found this conclusion quite problematic:
 - Although it should be obvious that the D18 event was not related to preexisting tropical disturbance/cyclones (see Figures in Supplement), the authors should have pointed this out in the analysis.
 - The potential impact of ENSO and/or MJO on the D18 event was not analysed in this

study, thus I am not sure how the authors drew such a conclusion.

- I think the authors could have compared the cause of extreme rainfall events, which are not related to tropical distributions/cyclones, and the cause of the D18 event. This can truly pin down the key factors that led to the D18 event.
- Some analyses appear to be irrelevant to the overall objectives of this study. For example, the use of TRMM and related analysis could be excluded from this study.
- Some sections appear to be repetitive, for example, Section 3.2 and part of Section 3.3 give very similar information.

Minor comments:

Lines 12-13: Remove "and its simulation ... is evaluated."

Line 15: what "easterly wind" is the author referring to? What region of "high sea surface temperature" is the author referring to?

Lines 17-18: This statement is too general, perhaps add some details of the results.

Line 34: Remove "at high resolution".

Lines 38-40: I think the author forgot to mention a key finding here as the spatial distribution of the rainfall is also different in the NTRN experiment.

Line 44: I suppose the words "disasters" and "hazards" have the same meaning in this manuscript. It might be a good idea to stick with one of them for consistency.

Lines 54-55: Change "... according to climate change and sea-level rise scenarios for Vietnam..." to "... according to a publication by the Ministry of Natural Resources and Environment of Vietnam (Tran et al. 2016) ..."

Figure 2: This figure could be merged with Figure 1.

Lines 72-75: This paragraph should be merged with the next paragraph.

Lines 72-85: This should appear before the paragraph starting in line 58. the description of the local topography (lines 62-64) should be included in this paragraph.

Lines 86-102: Currently this paragraph is disjointed from the previous paragraphs. The authors can add a sentence to connect this paragraph with the previous paragraphs.

Lines 104-106: The authors would need to offer more evidence to support these statements. One idea is to plot the time series of annual maximum 72-h accumulated rainfall from 1980-2018 (depending on the available data) and highlight the maximum 72-h accumulated rainfall of the D18 event.

Line 108: At this point, I am not sure why local terrain is an interesting factor to investigate. As the authors have pointed out, many factors including the local topography can cause heavy rainfall in central Vietnam (Lines 76-79). Perhaps there is a very good reason behind it, but it is not well communicated at this point.

Line 109: Remove "or high-resolution".

Line 119: Change "This dataset is ..." to "The NCEP GDAS/FNL Global Gridded Analyses and Forecasts is ...".

Lines 128-129: Why ERA5 is used for this purpose instead of NCEP GDAS/FNL? Conversely, why ERA5 is not used as IC/BCs for the CRM simulation?

Lines 141-146: Why TRMM is used? What is the added value of using this dataset in this study?

Line 172: The meaning of "large computers" is not clear.

Table 1: Not sure about the meaning of "Real" for the data source of topography input.

Line 193: Remove "=".

Line 197: Change "correct negative" to "true negative" or "correct rejection".

Line 199: Remove "(where CN is not used)".

Table 2: The definition of the "worst score" for BS is not very clear. Based on the formulation of BS, $BS = 0$ if $FA = 0$ and $H = 0$, i.e. the model always predictor negative. In a way, the worst-case scenario could be $H = 0$ and $FA = \text{All Negative}$, i.e. the model is predicting the opposite result 100% of the time, i.e. $BS = FA/M$ but this is not equal to N .

Lines 211-212: "As introduced earlier, ... central Vietnam." This sentence seems to be redundant.

Line 215: The term "windward side" might be a bit confusing as the wind field is only shown in Figure 4. Perhaps change it to eastern (or north-eastern) side?

Line 216: It might be useful to indicate the Quang Nam province in Figure 1.

Line 221: "In this subsection, ... are analyzed." This sentence seems to be redundant.

Lines 221-283: I think this part can be streamlined in a more concise manner.

Line 290: Perhaps rename the subtitle as "The local thermodynamic conditions prior the D18 event"

Line 291: Remove the sentence "In this part, the ... analyzed."

Line 325: "atmospheric"?

Figures 9, 10, 11: It might be better to merge all the graphs together and to remove the (a) panels as they more or less carry the same information as the (b) panels. The resultant 3x3 panel plot would be a better presentation to the evolution of the environment of each day. Readers would find it easier to understand the changes.

Figure 12: The date convention is not consistent with the earlier Figures, e.g. Figure 1.

Line 389: Is the difference in prevailing surface winds between CTRL and OBS linked to the use of two different data sets (NCEP GDAS/FNL for CTRL and ERA5 for OBS)?

Lines 423-442, Figure 14: I am not sure about the added value of this part. It seems to be repeating the previous section.

Line 470: I think the authors downplay the impact of displacement errors in their simulations. These errors would have significant impact from the disaster risk reduction perspective. In a way, being able to correctly forecast the location of extreme rainfall occurrence is as important, if not more important, as being able to forecast the amount of extreme rainfall.