Natural Hazards and Earth System Sciences Discussions

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2
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Comment on nhess-2020-397

Anonymous Referee #2

Referee comment on "Evaluation of Mei-yu heavy-rainfall quantitative precipitation forecasts in Taiwan by a cloud-resolving model for three seasons of 2012–2014" by Chung-Chieh Wang et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2020-397-RC2, 2021

Review to "Evaluation of Mei-yu Heavy-Rainfall Quantitative Precipitation Forecasts in Taiwan by A Cloud-Resolving Model for Three Seasons of 2012–2014" from Chung-Chieh Wang et al.

This paper evaluates the performance of a convection-permitting model (the Cloud-Resolving Storm Simulator; CReSS) in simulating heavy precipitation over Taiwan during three mei-yu seasons in 2012, 2013 and 2014. The simulations are validated against rain gauges, radar data and NCEP analyses. A well-chosen classification criteria for the events is suggested, dividing the data into segments depending on whether at least 10% of the gauges showed heavy, moderate, some or no rain. For the validation, the authors employ several verification metrics based on contingency tables, namely Threat Score (TS), Probability of Detection (POD), False Alarm Ratio (FAR) and Bias Score (BS).

Although the methods and findings of the manuscript are not totally ground-breaking or unknown to the scientific community, the study presents a compelling analysis of Quantitative Precipitation Forecasts (QPFs) validation. Provided the evaluated model works at a convection permitting resolution and the investigation region is prone to suffering heavy precipitation events with high impact, the manuscript can be relevant to scientists working on the topic as well as stake-holders dealing with the impacts of such events. Therefore I believe there is interest in its publishing although it requires major revisions.

General Comments

- The conclusions in Section 5 (also in the Abstract) should be further elaborated. There is no need in these sections to write again the numbers of TS, FAR, etc. (e.g. lines 554 to 555 or 563 to 564). By doing so the main conclusions of the paper are hidden e.g.

that QPFs are clearly improved by the use of convection permitting in heavy precipitation situations. Please, rewrite the conclusions and the Abstract to clearly point out the findings of the paper.

- It is not demonstrated that the CReSS model does a better job in orographic precipitation (phase locked) situations compared to transient systems as its stated in the conclusions (Lines 573 to 574). I agree that presumably, predictability is larger in those situations, due to 1) the fact that stationary systems have larger intrinsic predictability and 2) the better representation of the model orography. However the paper does not demonstrate this aspect. The concept "predictability" is lightly used and no quantification is provided (see for instance Hochman et al., 2021 where intrinsic predictability is quantified using dimension and persistence metrics for the systems). Instead only one case is shown (09-10 Jun) and two more cases are mentioned (L480) but no results are provided. From the case shown (09-10) the larger predictability of orographic precipitation is assumed by the fact that the location of the precipitating front (convective line) is not well represented but the TS scores are high (TS=0.4). This is not sufficient proof. It is advised that, given the type of information and analysis provided, the paper focuses on the "accuracy" of the simulation avoiding the analysis on "predictability".

Specific Comments

Title and Abstract (L18): It is highly possible that the reader is not familiar with what the Mei-Yu season is and when it occurs. Please include this information.

L20-L21, L26-27, L71-73, L79-80: A perfect forecast would show a TS of 1. Why are TS values close to 0.1 a good result then? To support this statement either provide the information about the skill for that score or provide the TS values of the "past results and 5-km models". Since at this point of the paper the TS has not yet been defined please also include the information that a perfect forecast has a TS=1.

L88. The "dependency property" regarding the link between large events and improvement of the QPFs has not yet been explicitly explained. If I understood correctly these are defined in the papers W15, W16. A brief explanation of what this is, is required here.

L89: The subobjective “further evaluate the model QPFs for larger and extreme events” should be part of the purpose 1) .

L100-101 and L113: What do you mean by “CReSS needs no nesting”. Dynamical downscales always need initial and boundary conditions from other, coarser, model”.

L104-109: More information is needed about the parametrizations used. You need to explicitly mention the shallow convection parameterization scheme and the turbulence scheme. Also referring to Table 1 and W15 and W16.

Table 1: How is the turbulence closure treated? Some models use a TKE 1D parametrization, others use a 3D, etc. What is the case in your simulations?

L228: Why is TS > 0.15 the threshold for predictive skill? Please explain, also if this information comes from previous literature provide the corresponding references.

L297-303: The statement “In model forecasts, when errors grow and the evolution deviates, the chance to become less rainy (not as favourable) is higher than more rainy, more so in forecasts made earlier at longer ranges.” needs demonstration, either from literature or results.

Figure5: Could the authors elaborate on why are the scores lower for the events during June 2013 (either Day 1 or 2)? This aspect should also be included in the manuscript.

Figure 7: In some panels, it seems as though the scores (TS or BS) are better on the third day of the forecast (day 3, blue line) than for the previous 2 days. Could you please explain this behaviour?

Figure 12: The understanding of the Figure, its caption and explanation provided in the text is incomprehensible. Please rewrite. Why is the number of appearances of the different weather types, proof of the better model performance? Besides, large precipitation totals are usually linked to large-scale systems rather than localized convection, this is already known to the scientific community.

Summary and Concluding Remarks: Please recap the aim of the paper, main steps carried out and briefly the relevance of the study before enumerating the conclusions.

L556: Please, again, include what do you refer to by “compared to previous results”.

L564: When you mention “larger groups” are you referring to events with large coverage? Please reword.

L573: The statement “and such QPFs with high hit rates are clearly very useful for hazard mitigation.” is not documented by your investigation. This was not shown in the paper. Delete.

Writing Comments

L17: What does in real-time mean? Please consider deleting.

L33: Should read: “Quantitative Precipitation Forecasting (QPF)…”

L37: Should read: “… mainly during two periods …”

L63-64: Delete “While the scores at the CWB will be compared with our results later,” and “obviously much”.

L68-69: Model resolutions of 2.2 km are already being used in operational centres, for example de German Weather Service not only in research.

L78: Change “more rain” by “the larger the rain”.

L84: Wang (2015) has already been defined as W15. Please correct.

L85-86: Delete “For mei-yu rainfall in Taiwan, we are certainly keen to find out how this CRM performs, especially for the extreme events.”

L90-91: Delete “To answer these questions above are our objectives”.

L113-114: Delete “which are also run four times a day, each out to 72 h (now 78 h).”

L116: Change: “highly dictated” by “forced”.

Table 1: Provide the grid spacing of the topography in km as well.

L135-136: Include this information about the relevance of the study in the abstract.

L189.: Include the information that BS=1 implies no biases and that BS>(<)1 implies overestimation (underestimation) of the events.

L218-236: Why is the explanation of Fig. 4 before the results concerning Figure 3?

L276: Rephrase “…under-prediction for low…”

L295: The word “serious is not appropriate in this context”

L447: Reword: “… June are compared…”

L496: Rephrase: “… sizes, as shown in Fig. 10…” and “… as an example…” by illustrated.

L580: The sentence “it is also recommended that such events should be examined with caution and proper classification. ” does not bring any information and its colloquial.

Reference

Hochman, A., Scher, S., Quinting, J., Pinto, J. G., and Messori, G.: A new view of heat wave dynamics and predictability over the eastern Mediterranean, Earth Syst. Dynam., 12, 133–149, https://doi.org/10.5194/esd-12-133-2021, 2021.