

Anonymous Referee #2

The manuscript presents a study focusing on HPEs using weather radar data and convection-permitting numerical simulations. Overall, it is an interesting study that merits publication. In particular, the consideration of a long radar data time series is important, deviating from the common practice of considering a few HPEs. Further, the methodology followed for evaluating model performance is thorough, providing useful insights. I recommend publication subject to minor revisions summarised as follows.

We highly appreciate the reviewer's comments regarding our manuscript. We will address all of the comments raised, as detailed below, in the revised version of the manuscript.

Comments

1. Title: I believe that the title of the manuscript is a bit misleading. To my view, the authors focus more on evaluating the WRF model at convection-permitting scales, than providing a study for the characterisation of HPEs in the study region. Hence, I would suggest changing the title of the manuscript, to better reflect the real subject of the presented study.

This idea was also raised by reviewer #1. We understand that we did not focus enough attention to our presentation of HPEs' characterisation, which is unique due to the high resolution rainfall data and the relatively large number of events. We intend to better emphasise this part of the paper in the revised manuscript. Moreover, to better present our paper, we plan to substitute the current title with the following one: "Radar-based characterisation of heavy precipitation in the eastern Mediterranean and its representation in a convection-permitting model".

2. Sect. 4.1.2: Two poorly simulated events were identified and some reasoning is provided in the Discussion, mainly focused on the quality of the large-scale driving reanalysis. Therefore, it would be interesting to know if the authors did check the driving ERA-Interim data for these two events, and if so, what can be concluded? Were it really an issue of bad boundary conditions? In addition, what were the results obtained from the coarser resolution domains? Were they equally poor? Such an elaboration would strengthen the authors' claim about the poor model performance.

This is a good point, also raised by reviewer #1. To address it, we plan to include some analyses of the coarser domains in the WRF simulations. It is hard to tell for sure if the boundary conditions are bad, because we do not have better data than the reanalysis, and comparison of other reanalyses or data assimilation techniques are beyond the scope of this manuscript. However, in contrast to most of the model simulations, in which rainfall was quite well simulated, in these two HPEs the innermost domain exhibited almost no rain. At the coarsest model domain, there is no rainfall simulated over the region, but rather only hundreds of km from the observed rain (compare the figure below with Fig 5). Below is a preliminary analysis of rainfall for one of the two events (event #5 in table S1).

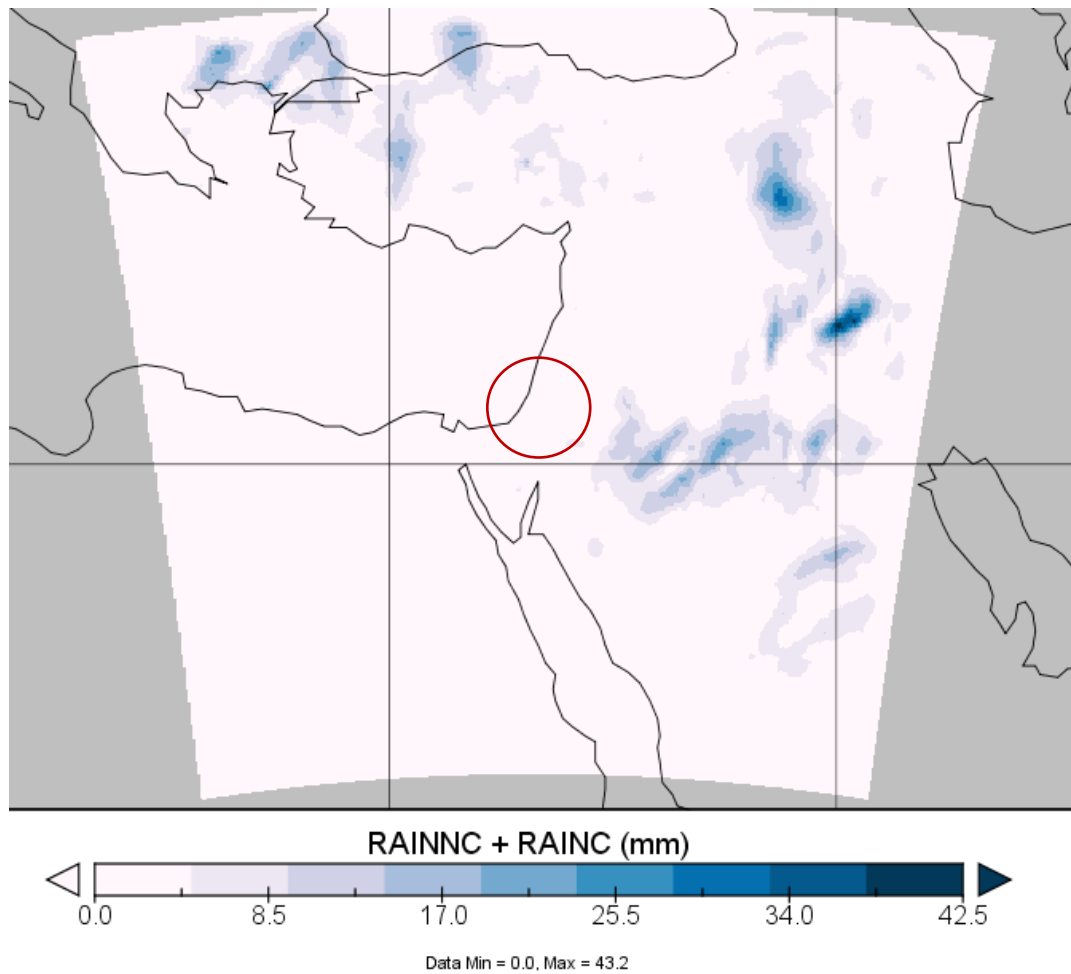


Figure: Rainfall in the coarsest WRF domain during HPE #5 (Table S1) and the approximate range of the Shacham radar (Figure 1).

3. Section 4.2.1 could be moved up, before the presentation of the model evaluation, as it discusses results based on observations.

We understand the reviewer’s point and we thought quite a lot on the right order – first HPEs characteristics from radar and then model skill to reproduce those characteristics (as suggested by the reviewer) or first model skill and then HPEs characteristics as manifested in observations (radar) and model. Our tendency towards the latter approach is due to our understanding that radar observations are not perfect and have their own limitations. Therefore, we prefer to present HPEs characteristics from the two sources and to emphasise both agreements and disagreements between them. This comparison follows model skill assessment. We do however agree that some of the HPEs characterisation can be moved to the first part of the result section, specifically those that are not relying on pattern analysis, i.e., seasonality and relation between HPEs at different durations (presently shown in Fig. 8 and 9). Therefore, we will make some changes in the structure of the results section: starting with general properties of HPEs, then model skill, following by space-time HPEs

characteristics detected from observations and model. Accordingly, we will make small modifications in the abstract section.

4. Fig.3: Instead of presenting the WRF/RADAR ratio, the authors should consider presenting either the bias (WRF-RADAR) or transform the ratio to %. This would facilitate the interpretation of evaluation results.

Agreed. We will change our bias definition into normalised difference (i.e. (WRF-radar)/radar).

5. L123: It would be useful to provide information on the interpolation method? Was it bilinear, bicubic?

The interpolation method we used is simply nearest-neighbour, and we will write it in the revised version of the manuscript.

6. Quality of the figures needs improvement for readability.

Thank you for this comment. We will review our figures and make them clearer if needed for the revised manuscript. Moreover, we plan to upload the final figures in a vectorised format (wherever possible), so that their quality in any case would be improved.

7. The manuscript text needs a thorough proof-reading for correcting numerous grammar and spelling errors.

Accepted. We will proof read the manuscript thoroughly.