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Interactive comment on "Characterising patterns of heavy precipitation events in the eastern Mediterranean using a weather radar and convection-permitting WRF simulations" *by* Moshe Armon et al.

Anonymous Referee #1

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General comments and manuscript summary: In the submitted manuscript, the authors use 24 years of historical radar data to identify historical heavy precipitation events (HPEs) in Israel, based on various threshold criteria. These 41 HPEs are then re-simulated using the WRF model at convection-permitting resolution (1 km grid spacing). Following this, the manuscript is primarily focused on evaluating how realistically the WRF model simulates the precipitation of the 41 HPEs, compared with what the radar shows. In addition to that, the radar data are used to identify common characteristics of HPEs in the study region.

The manuscript is primarily a model evaluation study of high-resolution WRF for eastern Mediterranean HPEs, with some accompanying radar-based climatological analysis. From the scientific/technical perspective, everything seems OK. My comments which follow in the next sections are thus of a technical and minor nature, and the main question I need to answer here as a reviewer is if the paper presents sufficiently "novel concepts, ideas, tools, or data" to justify publication in HESS?

This manuscript is certainly not the first to evaluate if "the model description of rainfall during HPEs" in a convection-permitting model (CPM) is "credible", despite the claims of the authors (L62). There is even a study investigating just that with WRF in the eastern Mediterranean (Zittis et al., 2017), which surprisingly wasn't cited. For other studies asking similar questions in other regions see, for example, Berthou et al. (2018), Brisson et al. (2018), Chan et al. (2014), Chen et al. (2001), Hally et al. (2014), Kendon et al. (2012), Lean et al. (2008); many more CPM evaluation studies can be found – both event-based and climatological. This manuscript represents another contribution to this important topic. I think the publication of the manuscript can be justified on the following grounds: (1) the authors' event-based approach incorporates an unusually high number of events, which is different to the most common approaches of either continuous multi-year simulations (e.g. Ban et al., 2014) or just a handful of events (e.g. Coppola et al., 2018); (2) the authors incorporate a nice range of temporal and spatial diagnostics which are (to my knowledge) not prevalent in the extant CPM-evaluation literature, presumably because of the rarity of such long radar archives (24 years) with high spatiotemporal resolution as used by the authors; (3) CPM evaluation studies for this region of the world are not well represented in the literature.

Specific comments:

1. Structure of results. I wonder would the authors consider that it might make more sense to present some of the results from the characterization of rainfall patterns section (S4.2) at the start of the results section, i.e. before model biases are presented? For example, Section 4.2.1 is based on observations rather than model evaluation. It would seem more logical to me to first present the characteristics of the observed HPEs to readers and then examine if these characteristics are reproduced by the model. Indeed, in your abstract (L13-15) you present the manuscript contents in this order. However, this is for the authors to decide!

2. Title. It is not really apparent from the title of the manuscript that this is primarily a model evaluation study. I expect your results will be of most interest to readers concerned with the quality of CPM simulations, however I fear that due to the title the manuscript might be overlooked by readers searching for such information and not reach the full audience it deserves. If it was my manuscript, I'd go for a title along the lines of "Heavy precipitation in the eastern Mediterranean and its representation in a convection-permitting model". This is, of course, for the authors to decide!

3. Poorly simulated events. Of the 41 HPEs, you identify two which are simulated particularly poorly and observe that these were characterised by short storm durations (L256-257) and were highly localized (L500-501). You also suggest that the poor simulation may be due to a poorly represented moisture field in the ERA-Interim lateral boundary conditions (L466-467). Have you checked this (if possible)? It would be interesting to know if there was any trace of these precipitation events in (i) the ERA-Interim precipitation fields, or (ii) the coarser resolution WRF domains. If the boundary and initial conditions are inadequate, then there is of course no chance for WRF to well reproduce the event. But this doesn't mean that WRF itself is deficient

or is incapable of simulating such events! Maybe WRF could simulate the event using data assimilation techniques beyond the scope of this experiment, or with better boundary conditions.

4. Expectations of CPMs. My final substantive point is about what we should expect from convection-permitting models, i.e. should we expect them to match radar on a pixel-by-pixel basis? And if they can't do this, does it represent a poor simulation? This is discussed in the introduction of Roberts (2008), where it is argued that the main added value of higher-resolution precipitation forecasts should be seen in area averages – e.g. over a catchment – rather than at specific point locations. I think it's also important to remember that the observed event is also just one possible realisation of the event and WRF will never have perfect initial conditions. You correctly (L469-473) advocate the utility of ensemble simulations for HPEs in the discussion, i.e. as a means of characterizing uncertainty.

Similar information to the aforementioned could potentially additionally be presented in the introduction or during the results, as the authors see fit.

5. Data availability. I think that Section 8 about data availability is inadequate. If someone wants to reproduce your results, a bit more than the two non-specific web domains (L517-518) is needed. Is there a specific web page or ftp server where the radar and rain gauge data can be downloaded? If so, please provide the links. If not, then provide more information about how the data can be found. Additionally, what about the WRF model simulations? Will (have) you upload(ed) them to an open-access server? If so, provide the download link. Or are they available by contacting the corresponding author? Finally, I suggest uploading the WRF namelist.input as an asset when you are resubmitting the manuscript.

6. Proof reading. There are a large number of minor grammar errors throughout the text, which are too numerous to list. I therefore suggest a thorough proof reading prior to resubmission.

Minor and technical comments:

- Section 3.2. Could you please also state (i) the number of vertical levels and height of the model top, (ii) if shallow convection is parametrized in the inner nest, (iii) the interpolation method used, i.e. bilinear, nearest-neighbour, conservative, etc. (i) and (ii) could also be added to table 1, if appropriate.

- Figure 1. It looks like the domain boundaries have been drawn by simply finding the domain corners and drawing straight lines between them. The lower/upper boundaries of Lambert conformal domains shouldn't have constant latitudes. I think you need to extract the outermost rows/columns from WRF's XLONG and XLAT arrays and use these to plot your domain boundaries.

- Figure 2. I wonder would it make more sense to compute the %-bias? i.e. instead of bias = WRF/Radar, use bias = 100.*(WRF – Radar)/Radar. With the current formulation the dry biases are lower bounded whereas the wet biases are not upper bounded. With %-bias this would not be the case. I suppose it's not really that big of a deal. The authors can decide for themselves.

- Figure 2. Please add "a, b, c, d" labels to the panel plots, to match the text.

- L123: Note that it should be possible in WRF to just save precipitation at 10-minute intervals and other variables at a lower frequency, to reduce storage space.

- L128: I think the reference to "Sect 3.2" is wrong.

- L170: The abbreviation "TP" isn't defined anywhere.

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Discussion paper

- L396-398: It may prove difficult to identify which days to downscale from the GCMs, especially for convective events. There are some papers recently suggesting methods for identifying the best days to downscale (Chan et al., 2018; Meredith et al., 2018; Gómez-Navarro et al., 2019).

References:

Ban, N., Schmidli, J., Schär, C. (2014). Evaluation of the convection-resolving regional climate modeling approach in decade-long simulations. Journal of Geophysical Research: Atmospheres, 119, 7889–7907.

Berthou, S., Kendon, E. J., Chan, S. C., Ban, N., Leutwyler, D., Schär, C., Fosser, G. (2018). Pan-european climate at convection-permitting scale: A model intercomparison study. Climate Dynamics, 1–25.

Brisson, E., Brendel, C., Herzog, S., Ahrens, B. (2018). Lagrangian evaluation of convective shower characteristics in a convection-permitting model. Meteorologische Zeitschrift, 59-66.

Chan, S., Kendon, E., Fowler, H., Blenkinsop, S, Roberts, N. (2014). Projected increases in summer and winter UK sub-daily precipitation extremes from high-resolution regional climate models. Environmental Research Letters, 9(8), 084019.

Chan, S. C., Kendon, E. J., Roberts, N., Blenkinsop, S., Fowler, H. J. (2018). Large-scale predictors for extreme hourly precipitation events in convection-permitting climate simulations. Journal of Climate, 31(6), 2115-2131.

Chen, F., Warner, T. T., Manning, K. (2001). Sensitivity of orographic moist convection to landscape variability: a study of the Buffalo Creek, Colorado, flash flood case of 1996. Journal of the Atmospheric Sciences, 58(21), 3204-3223.

Coppola, E., Sobolowski, S., Pichelli, E., Raffaele, F., Ahrens, B., Anders, I., ... Caldas-

Alvarez, A. (2018). A first-of-its-kind multi-model convection permitting ensemble for investigating convective phenomena over Europe and the Mediterranean. Climate Dynamics, 1-32.

Gómez-Navarro, J. J., Raible, C. C., García-Valero, J. A., Messmer, M., Montávez, J. P., Martius, O. (2019). Event selection for dynamical downscaling: a neural network approach for physically-constrained precipitation events. Climate Dynamics, 1-17.

Meredith, E. P., Rust, H. W., Ulbrich, U. (2018). A classification algorithm for selective dynamical downscaling of precipitation extremes. Hydrology and Earth System Sciences, 22(8), 4183-4200.

Kendon, E. J., Roberts, N. M., Senior, C. A., Roberts, M. J. (2012). Realism of rainfall in a very high-resolution regional climate model. Journal of Climate, 25(17), 5791–5806.

Lean, H. W., Clark, P. A., Dixon, M., Roberts, N. M., Fitch, A., Forbes, R., Halliwell, C. (2008). Characteristics of high-resolution versions of the Met Office Unified Model for forecasting convection over the United Kingdom. Monthly Weather Review, 136(9), 3408-3424.

Roberts, N. (2008). Assessing the spatial and temporal variation in the skill of precipitation forecasts from an NWP model. Meteorological Applications: A journal of forecasting, practical applications, training techniques and modelling, 15(1), 163-169.

Zittis, G., Bruggeman, A., Camera, C., Hadjinicolaou, P., Lelieveld, J. (2017). The added value of convection permitting simulations of extreme precipitation events over the eastern Mediterranean. Atmospheric research, 191, 20-33.

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