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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Spatio-temporal assessment of WRF, TRMM and in situ precipitation data in a tropical mountain environment (Cordillera Blanca, Peru)" *by* L. Mourre et al.

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1 General comments

This study is a comprehensive assessment of precipitation products over the Cordillera Blanca, a region where relatively few atmospheric modelling studies have been conducted to date. A considerable amount of data have been gathered and analysed, and the study is generally well designed. The scientific quality of the manuscript makes it

suitable for publication in HESS. The study suffers from the short period of time considered but this is acknowledged by the authors.

I have a few suggestions for improvements that are rather "minor" in nature, but that could still require substantial changes in the manuscript.

Critical evaluation of the kriging products

The algorithm relies on the calibration of a statistical interpolation model, with the addition of the topography as a further predictor. As acknowledged by the authors, this does not work very well since the precipitation maxima are found at the mountain tops. The authors however do not discuss another (to my opinion, more important) shortcoming of this method: the direction of the air flow. It is well known that the the leeward and windward sides of orographic barriers have opposed precipitation patterns. The shortcomings of KED are best shown in Fig. 05: the north-eastern part of the domain (towards the Amazon Basin) should be much wetter, as shown by TRMM and WRF. Here the effect of topography is overestimated by the KED model.

This might not be too problematic within the Rio Santa basin thanks to the reasonable number of stations on each side of the basin. Outside of the basin the KED results should be interpreted with great caution. I would argue that the omission of the arir flow directions is the major reason why the daily-evolving variogram performs best for the cross-validation (Table 5): it contains indirect information about the air flow through the current precipitation patterns that day (K-DE works also better than KED-M for the daily values).

Furthermore, when comparing the KED products to the other products at the station locations (Figs. 3, 4, 7) the authors should not use the full-model products but the cross-validation ones. For example, the frequency diagrams at Corongo should not rely on the observations at Corongo for the calibration. Maybe the authors did this

already it is not clear from the text. This might well mitigate the good results of KED in these analyses.

Description and purpose of the WRF simulation

The authors rely on one single WRF simulation, but WRF is sensitive to the parametrizations and forcing data used. From the literature and my own experience I don't expect that the conclusions of the present study will be affected substantially (over-estimation of precipitation, for example, is a known feature of the model), but this limitation should be acknowledged in the discussion. I also have a few concerns about some aspects of the simulation design. I do not formally ask for new simulations, but I suggest the authors to consider the possibility of adding a small number of sensitivity studies (see specific comments below). This would help to strengthen their conclusions by also adding some recommendations for scientists wishing to conduct modelling studies in the region.

Shorten the text to focus on the essentials

The manuscript could attract more readers with a clearer and more concise writing. There are some repetitions throughout the manuscript, the introduction material is sometimes only indirectly related to the study.

2 Specific comments

Abtract It is a matter of taste but I suggest to avoid using paragraphs and to shorten it. I suggest to remove certain details ("*largely due to operational constraints*", "*glacial area*", "*Thompson microphysical scheme*", ...) and to avoid repetitions

("*– ground based, satellite derived, RCM outputs –*" repeated afterwards anywhere, "*here*", etc.).

P6639 you write: "*The driving question of this study is to identify and compare the precipitation data sets that can be used for properly characterizing the water balance over catchments of the region, from the sub-daily and daily temporal scales driving flooding to the decadal and multi-decadal scales*". You should be more careful here because you actually don't (and can't) address all these scales (data availability, computational cost of WRF, etc.). I would welcome a more concrete formulation of the study's objectives.

P6639 "*the precipitation produced by climate models*" → I suggest to use a more precise formulation ("climate models" cover a wide range of models). I would also refrain to use the term "RCM" for your WRF simulation: a one year long simulation data forced by analysis data cannot be termed a "climate simulation". Some studies prefer "Mesoscale Atmospheric Model" (MAM), or simply "Numerical Weather Prediction model" (which is, after all, what WRF was designed to be).

P6645, WRF simulation More information is needed here:

- rationale for the choice of the first domain boundaries. Domains 2 and 3 are close to the eastern boundary of domain 1, which is usually not a good thing for the consistency of the boundary conditions (the influence of the large-scale driving data is much larger at the boundaries).
- for future studies, consider using reanalysis products instead of FNL analysis data to avoid time-consistency problems, as discussed by Maussion et al. (2014).
- did you use a spin-up for your simulation? Did you consider using some kind of nudging during the simulation time? I suspect that the good temporal performance of WRF (Fig. 7) can be attributed to the small simulation domains and to the fact that the nested domains are all close to the boundaries.

- from Table 3 it seems that you have used a cumulus parametrization for all three domains. If this is the case you should justify it. There are arguments against using cumulus parametrizations for spatial scales well below the resolutions they have been designed for (so called "gray-scales", see e.g. Arakawa (2004) or the introduction by Grell and Freitas (2014) for more references).

P6652, L12 "*WRF overestimates rainfall, probably due to errors in the NCEP-FNL forcing*": the discussion about this "overestimation" is not new. From all possible reasons, the "wrong boundary conditions" would not be my first choice. In the absence of more detailed information (further forcing data experiments? other WRF parametrizations experiments? Underestimation of rain gauges?), I suggest to remove this sentence.

Conclusions about TRMM: I wonder if these are not an over-interpretation. Most of these conclusions are general and not directly related to the material presented in the paper.

3 Technical corrections

Fig. 1 Consider using mm yr^{-1} as unit instead, this is more easily interpreted.

P6641, L12 "*In order to improve (. . .)*" sounds like a repetition and does not belong to this section.

P6642 "*one hydrological year (August 2012 to July 2013)*". Unless there are good reasons to call this "hydrological year" I would suggest to simply call it "year" (most HESS readers would expect an hydrological year to span Oct. to Sep.).

P6643 "*summertime*" → austral summer. Are temperatures really higher during austral summer in the region?

P6642, "In-situ data" The authors gathered an impressive number of stations for the region. I think that some readers will be interested to know what is the "availability" of these data. If they are available, state where they can be downloaded. If they are not publicly available, say it too (this will spare some searching time for the curious reader).

Table 2 it is confusing to call the nests "simulation 1, 2, 3" since it is one single simulation in reality.

P6646, L10 "*daily scale which is the corner scale for the comparison carried out in this paper*": you also provide an analysis of diurnal cycles.

P6650, L18 "*WRF precipitation areal averaging effect is the only one that is not similar at all stations inside the Rio Santa watershed, and this complex problem, beyond the scope of this study and probably related to the internal thermodynamic of the model, will not be addressed here.*": I don't understand this sentence.

P6652, L5 there are approx 13 grid points of 27km resolution in the catchment. Random sampling errors are very likely: how did you compute the area-averaged precipitation for the catchment? Did you use sub-pixel masks, or did you consider the center coordinates of the grid points?

Fig 2, 5, 7 consider using another colortable (see http://www.climate-lab-book.ac.uk/2014/end-of-the-rainbow/ for example)

Fig 6 : maybe use colors?

P6654, L1 "*But we have to keep in mind that it corresponds to precipitation averaged for 3 km grid cells that could include lower area in this zone of strong altitudinal*

gradients.":in the "real world", yes, but not in the "WRF world". There is no subgrid topography in WRF and it is a common misinterpretation: the grid cell height in the one "true" height in WRF and the solid precipitation estimations of WRF are based on this altitude only.

P6654, L16 "*as ice on the ground scatter energy in a similar way as precipitation drops in the atmosphere (Maussion et al., 2011)*": Let's try to avoid reference chains. Maussion et al. (2011) (wrongly) attributed this sentence to Yin et al. (2008) who in fact referred to their earlier study (Yin et al. 2004).

Fig. 8 : specify which time is used (LT?). Station observations: the hours of day where precipitation is not observed at all seem unlikely (e.g. from 4H to 9H at Shancayan). Any explanation?

P6657 "*at this 9 km resolution, non-hydrostatic effects are significant and since convection is partially solved in the model more realistic precipitation quantities are produced*": I don't understand this sentence.

References

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