

Interactive comment on “Precipitation and snow cover in the Himalaya: from reanalysis to regional climate simulations” by M. Ménégoz et al.

Anonymous Referee #2

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Overview:

The research article by Ménégoz et al. deals with a very interesting and pressing research topic. The uncertainty in snowfall and snow cover (especially SWE) data over data poor mountainous regions is of crucial importance to local but especially larger scale snow and climate studies. The authors compare several products including a higher resolution RCM model MAR that includes a specific cloud microphysics algorithm which allows for a better distinction between liquid and solid precipitation and a (for RCM standards) quite elaborate snow module. The authors conclude that this procedure produces a more realistic snow cover distribution over their study domain compared to the ERA INTERIM reanalysis data set. As mentioned by the authors, this conclusion is not unexpected due to the much coarser resolution of that data set which

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presents a problem especially in highly mountainous regions such as the Himalaya. In general, the authors point out that they found very large discrepancies between the estimations of precipitation provided by reanalysis, rain gauges networks, satellite observations, and our RCM simulation. Unfortunately, other than fairly subjective visual comparisons and some theoretical discussion as to how much snow “should and could be expected where” and how the different methods match these theoretical ideas the authors cannot really provide any definitive conclusion as to how accurate the chosen method really is. Nevertheless the presented method seems to return quite reasonable results and is certainly a valid contribution to the overall research question as to how best represent the snow cover in highly mountainous data poor regions of the world. Thus the manuscript is generally acceptable for publication after some minor changes.

As mentioned by the first reviewer the manuscript could benefit greatly from a rigorous review of the language. Reviewer 1 has done a commendable job in pointing out the most serious concerns in general terms and at specific points of the paper. I wholeheartedly agree with his recommendations in this regard. Please see some other comments below.

Specific comments:

p. 7652 line 5: “. . .Than those of the reanalysis. . .” You should probably mention here which reanalysis you refer to

p.7652 line 9: “However, our simulation brings an interesting estimation of liquid and solid precipitation in high altitude areas, where satellite and rain gauge networks are few reliable.”

The sentence should be reworded. “interesting” does not have any conclusive meaning here. Are your results (at least in your opinion) better, more realistic, or simply different than those of other data sets? Also the last part of the sentence should probably read something like: where satellite and rain gauge networks are not very reliable, or where few reliable satellite and rain gauge networks are available. . .

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p. 7653 line 4: "...smaller in the Western and in the Eastern..." "and" should be replaced by "than"

p. 7653 line 20 "area" should be areas

p. 7654 line 3: "Therefore, we can expect from them to simulate also better and better snowfall." Please reword.

p. 7655 line 2 ...such "an" approach...

p. 7659 line 23 You note here and on the next page when discussing Figures 3 and 4 that the MAR results match the APHRODITE results better than the ERA INTERIM results. This conclusion, I guess, is based on "subfigures" 3b vs.3d and 4b vs. 4d. I have to admit that I'm not sure I agree with this assessment. While there is much more spatial detail in f.e. 3b there also seems to be a lot more areas with deep red or deep blue colors in Figure 3b compared to 3d. Is there any way to make this analysis more objective, for example by using some statistical comparison for either the entire domain or parts of it? This would considerably enhance your conclusions.

p. 7662 line 13 You introduce and discuss the results of the CMC snow product in some detail before you tell the reader how this product was obtained. The explanation of the product should precede the discussion of its results.

p. 7662 Figure 5 and 6 seem to me to be the key figures of the study. At several points of the paper the authors point out: "we have to be very careful when estimating which of these products is the closest to reality, considering their large uncertainties." Here you have satellite based datasets which clearly represent "true" and reliable data against which you can compare your simulation results. However, in Figure 5 you compare April SWE output from your model and other (model) data sets to snow cover fractions obtained from MODIS. Is there any way to get snow cover fractions from the MAR model or the other data sets so that a direct comparison can be made to the satellite product? Figure 6 shows the snow cover duration for a satellite data set

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compared to MAR. Can you show the same for the ERA and/or the CMC data set, like you did in Figure 5, so that the improved quality of the MAR simulation becomes readily apparent?

Comments on Figures:

Color bars need to be labeled with units.

Axis labels are very small and hard to read in Figures 3, 4, and 8.

Figure 6: The bottom two (MAR) figures should be labeled c and d not a and b.

Figure 8: I realize that the histogram for subregion 4 has a different y-Axis scale to accommodate the very small SWE values. However, I would suggest to maybe use the same scaling as for the other 3 even though not much would show up. However, I think that this would actually better bring home the point of how dry subsector 4 is compared to other 3.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 7651, 2013.

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