

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

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We would like to thank the Editor to follow the peer-review process of our manuscript. We also would like to thank the referee #2 for its useful and thorough reviews. Our replies to its comments follow below. In addition with this response, we submitted a revised version for our manuscript, including most of the suggestions of the two referees (see the supplement material).

Specific comments: 1. p. 7652: As recommended, we specified in the abstract of the new manuscript the name of the reanalysis that we used: “. . . than those of the reanalysis of the European Centre of Medium-range Weather Forecast (ECMWF) used as lateral boundaries”. 2. p. 7652: Sentence reworded: “However, our simulation

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provides a first estimation of liquid and solid precipitation in high altitude areas, where satellite and rain gauge networks are not very reliable.” 3. p. 7653 line 4: the sentence does not have to be changed, to keep the opposition between precipitation rates in central Himalaya (previous sentence) and the precipitation rates in the Western and the Eastern parts of the Himalaya (this sentence). 4. Area -> areas. 5. Sentence reworded: “Therefore, we can also expect them to simulate better snowfall.” 6. “Such an approach”. 7. Our model is evaluated according to two different criteria: Firstly, the spatial variability of precipitation: p. 7659 line 23: “The spatial heterogeneity of precipitation observed in the APHRODITE network (Figure 3g) is better reproduced in the MAR simulation (Figure 3a) than in the ERA-INTERIM (Figure 3c), in particular over the Himalaya, where the precipitation can vary by a factor of 10 within a distance of only some km. As mentioned in the text, such assumption is based on the regard of Figures 3a, 3c and 3g. Secondly, we analyse the differences between model and observations: When mentioning the strong bias of our model, more pronounced than the reanalysis, the referee can find the following sentence pointing its remark (p. 7660, l.10): “Nevertheless, we see that the supposed dry bias of the ERA-INTERIM on the southern slope (Figure 3d) is further amplified in the MAR simulation (Figure 3b).” Note that for more clarity, references to the different figures were reorganised in the new version of the manuscript. 8. As required by referee #1, we added in the new manuscript a new section (Section 3), containing a table describing all the observational data used in our study. As suggested by referee #2, we mentioned this table in the new manuscript, before analysing these observational data (p. 7662, l. 20): “As mentioned previously, MODIS, CMC and ERA-INTERIM products are described more in detail in Table 1.” 9. p. 7662 Figure 5 and 6: Analysis of snow cover fraction and snow water equivalent. In its current version, the surface scheme of MAR takes into account grid cells with or without snow, and has not been designed to simulate the snow cover fraction inside the grid cells, which explains that we can not provide it for more rigorous comparisons with MODIS product. The analysis based on Figure 5 shows that both ERA-INTERIM and CMC products cannot be used to study

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the snow cover in the Himalaya (in comparison with MODIS and MAR). Hence, we excluded them from the analysis of the snow cover duration for the years 2000-2001 and 2001-2002, in order to lighten Figure 6. Comments on Figures: We modified the Figures according to the suggestions of referee #1 and #2. However, we did not modify the histogram for sub region 4 as recommended by referee #2, since the very small values of SWE are not readable under this format.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/10/C4602/2013/hessd-10-C4602-2013-supplement.zip>

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