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Hydrology and Earth System Sciences
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Discussions

Interactive comment on "The evaluation of the potential of global data products for snow hydrological modelling in ungauged high alpine catchments" *by* Michael Weber et al.

Anonymous Referee #3

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Comments on the manuscript: "The evaluation of the potential of global data products for snow hydrological modeling in ungauged high alpine catchments" by Weber et al.

This manuscript examines the sensitivity of the simulated snow characteristics in a small alpine basin related to meteorological input data and DEMs using a hydrological model with a sophisticated snow module. Studies like this are of a great importance as high elevation snowpack critically affects alpine ecosystems and water resources in a number of regions in the world. The experiment is comprehensively designed and well executed. The paper is informative and interesting.

The main concerns on this manuscript include:

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Discussion paper

(1) The analysis of the experimental results and the organization as well as the presentation of the analysis output need improvements,

(2) Insufficient evaluation of the reference simulation across entire HRUs is also problematic. Because of the large elevation range within the test basin (RCZ), the snow field within the basin is expected to vary widely. Without the evaluation of the elevation-dependent model performance with the reference data, the accuracy of the reference run cannot be well established.

(3) The lack of the analysis and evaluation on monthly time scales (i.e., annual-cycle resolving analysis). This is important because the forcing and snow fields undergo strong seasonal cycle, hence, seasonal cycle-resolving analysis & evaluation can be useful in assessing key sources of the simulation errors. This will also help to link the seasonal cycle of the runoff to monthly snow ablation.

(4) The terminology "topography parameterization" is confusing. The true meaning of "topography parameterization" in the manuscript is "characteristics of topographic parameters" such as slope and azimuth that vary according to the DEM resolutions. "Parameterization" typically means representing unresolved properties using resolved values or representing a property using a related variable(s).

(5) The writing is OK, but I found occasional awkward/unfamiliar sentences. As a speaker of American English as the second language, I don't like to suggest any specific changes in grammar and writing. I strongly recommend the authors to consult native English speakers to go through the entire writing.

Specific comments and suggestions are presented in the below.

(1) The authors present results from elaborate model runs and analyses, but the key messages are not clearly presented and are sometimes confusing.

(2) Please improve Abstract so that the key findings in the experiment are presented more concisely clearly. Separating the sensitivity to DEMs and associated orographic

parameters from the sensitivity to the forcing data may help organizing with more clarity.

(3) The statement in Abstract, L23:24, contradicts the statement in Conclusion L625-634.

(4) L149:151: How the mean snow field (e.g., SWE, SCA) over the entire RCZ is calculated? This is among the key evaluation variables.

(5) L156:157: The met data at LWD are not used as the forcing. Why the snow precipitation at LWD is corrected for undercatch?

(6) Spell out HRU at its first appearance.

(7) L227: Check the spatial resolution of the CFSv2 data. The finest resolution I could find is T382 that correspond to approximately 0.313degree.

(8) L265: Section 3 → Section 4

(9) L288-291: I don't understand what "contrary development" indicates. This sentence is ambiguous. Please provide more explanations.

(10) L293-295: This sentence is ambiguous. Please rewrite.

(11) L288:289: This sentence cannot explain the peak in March.

(12) May change the title of Section 4.3. I suggest "Landsurface parameterization on basis of DEMs" → "Sensitivity of the land-surface parameters to DEMs (or DEM resolutions)"

(13) Also suggest a new title for Section 4.4: "Influence of the DEMs and associated land-surface parameters on meteorological conditions"

(14) L339: 100 m → 200 m (195 m). (200m elevation difference also corresponds to a lapse rate of 5K/km, slightly more stable than the standard atmosphere which is understandable over a cold surface like snow/ice.)

(15) Section 5: The evaluation based only on NSE and R2 is insufficient. Need more

metrics, at least the ‘mean bias’ and RMSE. Also provide additional evaluations for each month (i.e., annual-cycle resolving model evaluations)

(16) Section 5.1: If there are problems in evaluating the snow simulations at DWD and DLW as stated in Section 5.2, how can you justify evaluating the daily snowdepth against the observations at these sites?

(17) L395: “snow towers pile up” → Is this due to the forcing errors or model errors or combined focing-model errors?

(18) L395: ‘downscaled temperature’ → ‘temperature downscaling’

(19) L406:421: This paragraph repeats the statements in the previous paragraph. May be removed and present a summary of this paragraph in Conclusion.

(20) Section 5.2

A. Statements in this section is too qualitative without solid supports from acceptable level of evaluations.

B. If missing data and site characteristics at LWD and DWD, respectively, prevent using these data for evaluation, how can we trust the reference data are accurate enough for evaluating model data?

C. If DMSWE cannot be validated, how can MSWE can be validated? MSWE is supposed to occur on DMSWE.

(21) L457:470: How can the data of largest warm bias, precipitation underestimation, and insolation overestimation perform best in simulating the amount and timing of runoff? This may indicate major flaw in the model physics and/or forcing combinations. Need further discussions. This also indicates the need for a more rigorous evaluations of the reference data and other model data.

(22) L475: Monthly Qsi needs to be evaluated as it is directly involved in snowmelt.

(23) L506:507: This may be an overstatement for the CHRPS data. CHRPS yields good results for snow cover and runoff, but not in NSE and R2 of the daily snow depth.

(24) 511-530: Can the inter-model difference also be related to their snow models? Do all of these models use the same snow model?

(25) Please discuss the poor NSEs and R2s with the transferred and CHRP data.

(26) L550-551: This statement ignores the substantial differences in runoff between GTOPO30 and ALOS/SRTM.

(27) L571: "the choice of the DEM has far less impact" Incorrect. DEMs have large impacts on the simulated streamflow annual cycle

(28) L600:604: Misleading. The 'borrowed' data performed poorly in terms of NSE and R2.

(29) L510-512: There is a mystery. How can the forcing data sets of such a wide variation can produce such similar simulations? This needs answers from the authors.

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