

Interactive comment on “Large scale snow water status monitoring: comparison of different snow water products in the upper Colorado basins” by G. A. Artan et al.

Anonymous Referee #2

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Overview:

The research article aims on the monitoring of regional snowpack status in upper Colorado basins using a snowmelt and ablation model based on the Utah Energy Balance (UEB) model driven by remote sense data and near-real-time meteorological output fields from forecast models. In order to analyze the snow water equivalent (SWE) a comparison between simulated and best estimated SWE has been carried out for three years of study from 2006 to 2008. The results exhibit a strong spatial coincidence between simulated SWE and observed SNODAS and SNOTEL data for the entire snow accumulation period. High spatial correlations between simulation and data

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as well as among the different observations point this out except for SWE derived from the passive microwave imagery. The authors suggest an underestimation bias of air temperature which effects the weakened agreement between modeled SWE and the SWE estimated by SNODAS and SNOTEL during the snowmelt period. Further snow albedo parameterization on the snowmelt processes has been addressed to be major issue for future ongoing research. The study addresses to an interesting and highly up-to-date research topic since the realistic monitoring of snow water status in remote mountainous areas as Colorado basins with sparse dense observation network is currently of special focus in particular with respect to the feedbacks between snow cover and the local/regional orographically induced climate effects under global warming. In this context, modeling studies are a useful tool to better resolve the regional patterns in high elevated areas with regard to existing uncertainties and errors in the few available observation data. The manuscript represents a substantial contribution to scientific progress. However, some open questions, which had been already concerned by reviewer 1, are not answered appropriately during the revision process. Besides there are only some few clarifications and typesetting errors (see my comments below). If these comments are addressed appropriately and some more discussion is done, the paper is recommended for publication in “Hydrology and Earth System Sciences”.

Specific comments:

1. First of all, in order to better evaluate your corrections in the paper according to the reviews, I would prefer to see the revised manuscript coeval with the correspondent response uploaded as attached file. Thanks.
2. In general I would suggest emphasizing more on the added value of your model to convince the science community. In this regard you could highlight the motivation in abstract and introduction as well as improve the writing style of your result part using more sophisticated and precise sentences, which will make the paper more interesting for the general readership. Especially the result part is sometimes difficult to follow.

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3. Concerning your response to reviewer 1 (page C3292): “The underestimating bias of the GFS temperature is only important for the first week of the snow ablation seasons; consequently, the GFS temperature bias should not affect the total snow ablation or the monthly melt values that we have used for our comparison.” - Can you please discuss this more in detail? Did you check it?

4. I totally agree with reviewer 1 that a validation over some years is too short for statistical significant model validation. In this context, you answered that the short simulation period from 2006 to 2008 is based on limited time coverage of Microwave imagery. What about the other data sets and its temporal availability? I assume some data must be available before 2006? It would really make more sense to simulate the climatology of about 20 to 30 years. If the data for comparison are not available, please advise me or else give me some more explanations for using only these three years.

5. As you suggest in the abstract and throughout the paper that the underestimation bias of air temperature (and precipitation) is one (maybe the most) important factor contributing to the disagreement between modeled and observed SWE during the snowmelt period, I would also ask you (in agreement with reviewer 1) to discuss this problem more in detail. Unfortunately you didn't reply appropriately on this comment from reviewer 1. Hence, I highly recommend you to analyze the impact of temperature and precipitation bias on your model performance. If it is not possible within this paper, you should add the outcome for ongoing work in addition to your snow albedo parameterization problem, which can be another interesting task in improving the model.

6. Tables and Figures:

Tab.1: I would replace “Atmospheric pressure” using the standard term “Sea level pressure”. Please add the units of the different parameters.

Tab.2: Please add the units to geographical coordinates and elevation.

Tab.3: Please don't use different symbols for the resolution: either ° or degrees and

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replace km with ° or degrees.

Tab.4: Please add the units in the first row, if not standardized.

Tab.5: Same as Tab.4. Is it possible to combine Tab.4 and Tab.5?

For the figures I agree with the comments of reviewer 1.

Technical corrections (typesetting errors):

1. Throughout the paper you mix “data was” and “data were”. Since both is possible, please use a uniform type.

2. page 3630, lines 25-26: Please replace the second every year by another word to avoid the repetition of words.

3. page 3632, line 6: “The objective of this study was...”. Please use present tense throughout the paper except in the conclusive part.

4. page 3632, line 15: It has to be “a SWE product” instead of “an SWE product”.

5. page 3632, line 19: Please use a better description for “good results”. Further “of” is missing after “parts... the world”.

6. page 3634, lines 20-24: “The precipitation data used was from sources: ...” This is one example of sentence construction which has to be improved regarding the style to better understand. This has to be done in the entire manuscript and as I already mentioned especially in the result part (e.g., page 3639, lines 5-7).

7. page 3641, line 18: It has to be “lack” instead of “lacks”.

8. page 3642, line 17: “known” – a “n” is missing.

Please check also your reference list of spelling errors (especially the correct usage of small and capital letters in the titles).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 3629, 2013.

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