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Hydrology and Earth System Sciences Discussions

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 #1

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This manuscript compares different products for estimating SWE at the scale of the upper Colorado basin. This topic fits well into the scope of HESS.

There are many things to say about this comparison of differently derived SWE. Without going into the details of this study it becomes obvious quite soon that

a) the precipitation fields used in the UEB-model are significantly underestimating measured precipitation at the SNOTEL stations. Therefore, it is not surprising that the simulated SWEs are accordingly underestimating SWEs observed at the SNOTEL stations.

b) The air temperature used as input to the UEB model is significantly underestimating

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SNOTEL-observed air temperature. Therefore, it is not surprising that snow ablation is much slower in the model than observed at SNOTEL stations.

c) MI-derived SWE-fields are only a weak representation of true (observed) SWE variability across time and space.

In addition, we know from many earlier studies the difficulty of comparing SWE at different scales. So, all-in-all the presented results don't provide much new insight. It's a properly done and honest exercise trying to compare the different products. But what's the added value of this work?

The analysis has three main weaknesses that affect the interpretation of the results:

1. The validation time period is too short. Knowing the considerable variability of winters with regard to SWE, a validation over 2 years is simply too short to make a proper judgment how well different methodologies are able to estimate SWE.

2. The performance indicators of the different methods are (probably; although not explicitly stated) calculated for very different time intervals: monthly for MI and daily for the UEB model. This makes a huge difference! For MI the number of samples must be in the order of 10, for the UEB model in the order of 300. To me, it's not ok to compare these performance indicators in the same table without clearly stating this.

3. Finally, let's talk about the issue of scales: First of all, it's quite obvious that comparing plot-scale (SNOTEL) data with large-scale estimates doesn't make sense, if we think about all the topographic variability in one single pixel of 0.05 x 0.05 degrees. A comparison of UEB with SNODAS – which is an upscaled product – is much more appropriate. Fig. 10 nicely illustrates this point: plot-scale SWE is in the first year 50% larger than aerial integrated SNODAS-SWE. In the following year, it's just the opposite.

In conclusion, this analysis leaves me back with an uneasy feeling that I have not received a useful answer whether or not these products are applicable for regional estimation of SWE.

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Interactive Comment

A few minor issues: - The authors talk (in the title, but also later) about "snow water status“, but they only show results and analysis of SWE. To me, the term "snow water status“ includes much more than SWE; for example the stability or the wetness of the snow cover. So I suggest to not using this term here in this paper where only SWE is addressed.

- page 3633, line 25: "be“ is missing before "able“

- page 3634, line 4: I think that "snow density“ should be "maximum snow density“; I assume that UEB models the temporal change in snow density during the winter season starting with a low snow density around 100 to 200 kg m-3.

- page 3641, line 9: Looking at Fig 8 (e, not d as written in the manuscript) I disagree with the statement “The SWE modeled with UEB driven with the MPE data was in good agreement with SNODAS and SNOTEL SWE.”

- page 3644, lines 23-25: I don’t really agree with the rather positive concluding statement that "both of the UEB-simulated and MI-estimated SWEs .... were found to be useful in mapping the SWE.“ I would be a little more critical in that respect.

- page 3645, lines 1 and 2: The outlook to the evaluation of snow albedo parameterization implies that snow albedo was a main reason for the mis-match between model and observations. The manuscript, however, provides no evidence for that. The large underestimation of model inputs precipitation and air temperature (compared to observations at SNOTEL stations) seems to be much more important. I would try to tackle this problem first.

- I think that the only one figure for the discussion of air temperature is necessary; not three (Figs. 3-5).

- Figs 9 b, d and f are strange to me, strongly influenced by the interpolation method. I would leave these figures out.

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