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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.

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Title: Large scale snow water status monitoring: comparison of different snow water products in the upper Colorado basins

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We thank the reviewer of our above-referenced paper. The following letter gives our responses to the referee #1 comments.

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Point-by-Point Responses to the Reviewer's Comments

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. Response: We agree with the reviewer that the precipitation fields used to run the UEB model have a negative bias when compared with the precipitation recorded at SNOTEL stations. The precipitation measured at the SNOTEL stations comes from gauges with an opening of 100 cm2, and the precipitation fields we have used correspond to areal extents of 100 km2 and 625 km2; therefore, the three datasets will never be the same. One of our aims was to show that snowpack conditions could be monitored by using energy balance models run with precipitation data that have large uncertainty. b) The air temperature used as input to the UEB model is significantly underestimating SNOTEL-observed air temperature. Therefore, it is not surprising that snow ablation is much slower in the model than observed at SNOTEL stations. Response: 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. 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.

Response: The SWE products from the UEB simulations were validated using data from 3 seasons of snow accumulation/ablation datasets (2006, 2007, and 2008). The validation is only 2 years long for the SWE derived from the Microwave imagery. The Microwave data from National Snow and Ice Center is not available after spring 2007. 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. Response: The performance indicators were calculated consistently for all monthly data and averages of all seasons at SNOTEL locations. The comparison periods are stated throughout the manuscript. For example, on page 3643, lines 6 – 7, it states that "Figure 11a–d presents the linear relationships between the average monthly values of the SWE products," and on page 3640, lines 17 - 20, we wrote "Statistical indexes (correlation coefficients, percent biases, RMSE, RMSEs, and RMSEu) were calculated at each of the 39 validation sites between the SNODAS SWE and MI- and UEB-produced SWE."

We agree with the reviewer about the lack of clarity of the period of comparison as seen in the manuscript in its present form. In the revised manuscript we will make clearer how long is the averaging time lengths of the various SWE products that are compared. We hope the changes that we will make will adequately respond to the referee's concerns.

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. Response: We agree to a point that comparing data measured by a gauge with data from a grid is problematic. We make that clear on page 3638, line 14 – 16: "By comparing gridded data of varying spatial scales and point data there should not be an expectation of perfect agreement even if both data are correct." The data we are comparing are average values at regional and monthly time scales; we believe that uncertainty introduced by the scale is reduced at those time scales and spatial averaging. A few minor issues: - The authors talk (in the title, but also later) about "snow water

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Interactive Comment

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.

Response: In the title we will change “snow water status” to “snow water equivalent status” to read as recommended by the reviewer.

- page 3633, line 25: "be“ is missing before "able“ Response: will be changed. We will add be 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. Response: Snow density is correct; the UEB model does not change snow density with time. - 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.” Response: The SWE modeled with UEB driven with the MPE precipitation had a correlation of 0.95 with the SNODAS and a correlation of 0.73 with SNOTEL SWE. While there is room for improvement, our judgment is that SWE product provides an adequate indicator to monitor seasonal SWE conditions. - 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. Response: We agree with being critical on MI-estimated SWE, and we state on page 3644, line 27 and page 3645, line 1 that the UEB-simulated SWE were “data and were found to be unreliable sources for mapping SWE in the study area.” Although not perfect, we found UEB-simulated SWE to be useful in mapping the SWE, and the results presented in the paper indicate that. The sentence pointed out by the reviewer will be revised. - 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). Response: Figures 3 and 4 show the bias of the GFS's estimated air temperature when compared with air temperature recorded at SNOTEL sites, while Figure 5 was used to show the lack of elevation bias. - Figs 9 b, d and f are strange to me, strongly influenced by the interpolation method. I would leave these figures out. Response: We agree that Figures 9b, d, and f can be taken out without compromising the message.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/10/C3291/2013/hessd-10-C3291-2013-supplement.pdf

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