Hydrology and Earth System Sciences

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Discussions

Interactive comment on "Evaluating the Utah Energy Balance's (UEB) snow model in the Noah Land-Surface Model" *by* R. Sultana et al.

Anonymous Referee #2

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The authors present an improvement of the SWE predictions by the Noah land-surface model (LSM), operated by implementing the Utah Energy Balance (UEB) snow model. This because the present formulation of the Noah LSM is known to underestimate SWE. To validate this operation, they compared model results with measurements of SWE coming from a number of SNOTEL sites in California, and one site in Utah. This last one has been introduced since it reports observed data of surface-temperature. Such a validation returned better predictions of SWE than the ones obtained by the original model, although underestimations are still present, but discussed.

According to my opinion, the presented issue is remarkable, even if quite technical. In fact, the UEB model has been already widely discussed and applied (see the references that the authors provide in the text). Nonetheless, the general topic is very

interesting. As a consequence, I would suggest reconsidering the manuscript for final publication in HESS after some revisions.

In the following I will suggest some points for the revision of the manuscript:

1. The introduction should undergo some modifications. While the first part (line 20, page 13364 to line 10, page 13365) is well written and coherent, the second one (line 10, page 13365 to line 10, page 13366) seems to be quite dispersive. I would suggest renewing it, reorganizing the discussion about the causes of the underestimation of SWE data, and trying to sum it up a bit. The state of art is clear and satisfactory;

2. As for Section 2, I would suggest to give a brief, but exhaustive, general introduction to the two models compared in terms of the state variables used, the hypothesis, the parameters, the general laws used in the models, and the input variables required, since the current description results in being insufficient to completely understand the context of this contribution without knowing a great amount of information from other publications;

3. In section 2.2.2., I would appreciate some specifications about the reason why 20 m/h is felt to "reasonably describe the snowmelt rate and timing at the study sites", since they are at different elevations and geographical locations. Moreover, I would spend some words commenting the pros and cons of a matrix-flow approach, if compared with preferential-flow approaches;

4. Section 3.2: firstly, please clarify what you mean stating that NSE is "potentially a reliable statistic". Secondly, I am not sure that a negative NSE denotes a predictor as "not good". Since, as you say, it is a mere comparison with the errors one would have when using a long-term mean in place of the model, its general quality in modeling the data depends on many other considerations. The same can be said about the proposed threshold value of 0.7 (in this case, I would appreciate at least some references for this choice);

5. Section 4 should be separated in subsections, since in this version it is too long;

6. Figure 3 (probably erroneously indicated as Fig. 2 at line 16, page 13376?): please consider to add to the X axis label the indication of each water year, since at this stage the only way we have to individuate the different years is to count the seasons on the same Figure;

7. The precipitation correction presented at lines 16-20, page 13377, is just a first attempt to correct the possible biases, since it is not able to remedy to under-catch, evaporation or leaks, which could affect SNOTEL original rain-gauge data. I agree with you that it could be sufficient in such a general context, but please be clearer on this point. In fact, I think that elaborating a more refined routine could help in obtaining better SWE simulations since, in my opinion, much of the underestimation (at least, the residual one after the application of the UEB model) could be ascribable to uncertainties in input data quantification;

Minor comments:

- Page 13364, line 2 and 21, please define what NCEP-NCAR is;

- Page 13367, lines 18-20: I think at least one word is missing here, since the statement is inconclusive;

- Page 13368, line 9: "the one" . . .? I think a word is missing here;

- Page 13374, line 15: please specify which is the instrumental accuracy, since in my opinion reasonability depends on the application you are dealing with;

- Page 13380, line 3: please quantify the "reasonable agreement", e.g. using NSE values;

- In the text, you firstly cite Figure 2, and then Figure 1. This is quite unusual, please consider switching them.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 13363, 2013.

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