Hydrol. Earth Syst. Sci. Discuss., 10, C6919–C6921, 2013
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Hydrology and Earth System Sciences
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 #1

Received and published: 26 December 2013

General comments

This study proposes and evaluates a new approach for simulation of snow processes in the Noah LSM model. The proposed approach implements the UEB simulation scheme, which is based on the estimation of snow surface temperature and accounts for the internal energy of the snowpack. The proposed methods is evaluated at 22 SNOTEL stations by simulating snow water equivalent (SWE) and one station by comparing observed and simulated surface temperature. The results indicate that modified approach substantially reduces SWE estimation, which is attributed to improved description of snow melt processes.

Overall, the topic is interesting and important for regional snow modeling, even if the

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UEB snow model approach is already presented and tested in numerous other studies. The manuscript is concisely written, however the storyline and presented examples (figures) are not fully convincing the message that it is the UEB snow process representation which improves the Noah model simulations. The first part of results mixes the bias of model inputs into the model output interpretations, which makes difficult to make a clear story. I'm wondering whether it would be possible to show a clear case study (e.g. a point simulation), where the model inputs are right (i.e. measured at the same place as the SWE measurement) and where the other effects (i.e. sub-grid variability) are eliminated. In the current form, it is not clear, whether the model errors are caused just by the simplified Noah process representation/simulations or to what extent are model outputs affected by biased model inputs or local effects and subgrid variability (e.g. drifted snow or the altitude difference between SNOTEL stations and mean grid elevation). The second part (comparison at Utah station) also does not clearly demonstrate the deficiency of Noah scheme (or the value of surface temperature estimation by the UEB) and it is also not very geographically consistent with the first part of the results.

Thus, I would suggest to make some revisions, which will more clearly demonstrate the deficiency of the old and/or the value of the new approach, particularly what is the value of the improved process representation (not just the final SWE simulation).

Specific comments

1) The reference to Fig. 1 is missing.

2) The UEB model: Some more detailed information would be useful. E.g. How many model parameters has the old and new snow models, and how were these estimated? What values (of model parameters) were chosen? What is the numerical stability of the force-restore method? Did you use the drift model parameter in UEB model?

3) Results (p.13376, l.16): Fig.2 or 3?

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4) I would suggest to discuss the results in a separate section. The context and implications of findings (with respect to other studies) are not clear.

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