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Geoscientific Model Development Discussions

Interactive comment on "The Cache la Poudre river basin snow water equivalent modeling with NewAge-JGrass" *by* G. Formetta et al.

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This paper describes a snow accumulation and melt module for the model NewAge-JGrass. The model description is rather condensed with some points that are not entirely clear (see detail comments). Furthermore, the literature review does not seem complete and the testing of the model does not tell the reader how useful it is for actual hydrological simulations at the catchment scale. The language should be revised; there are easy to remove spelling and grammar mistakes.

Detailed comments:

- From the abstract alone it is not clear that the model is an improved temperature-index approach, it gives the impression of a physical model

- The introduction does not discuss temperature-index methods, their shortcomings and why a new method is proposed, and in as far it is comparable to the standard method of Regine Hock using potential radiation. Such a literature review seems important. I would also mention in the abstract that you use a modified degree-day formulation (the term does not appear in the text?)

- The introduction of a smooth threshold for accumulation is very useful. Are the parameters of eq. 4 calibrated? And if yes: why is there still a bias in the snow simulations?

- Eq. 5: does the equation apply for negative temperatures?

- Are there other papers that suggest to use a different melt formulation for night and day or is this new her? I think you should refer to the paper by Tobin et al., 2012 Adv. in Water Resources that suggests a temperature-index approach with a quasi-sinusoidal cycle of the melt factor (a similar idea to obtain different relation between melt and air temperature during the night)

- What makes the formulation different from the classical Hock-method? What is the advantage of this formulation? As far as I understand, both use potential radiation, which represents a certain drawback (see also a discussion in the above paper by Tobin et al.). Does the underlying complete hydrological model not account for real weather conditions? I do not entirely understand the description on p. 4455, especially what the energetic index is.

- Testing against observed point data: the model seems to do a good job on a daily time step to reproduce the observed point data for the calibrated stations. But since the model has separate formulation for day and night, it should be tested against hourly data.

- Furthermore, it should be discussed in as far the many parameters might lead to overparameterization. This point is important since the test against stations for which it has not been calibrated shows poor results.

- The Nash criterion is not very useful to test models with a very strong annual cycle (see Schaefli and Gupta, Hydrol. Proc. 2006). The test against stations for which the model has not been calibrated gives poor results, this should be illustrated with a time series. I get the feeling that the text does not sufficiently underline how poor the results are (I would think that Nash values below 0.6 or even 0.5 mean that the series are completely off); given the strong annual cycle much higher Nash values are to be expected even for a not well performing model (that's why the Nash criterion is not very useful here). What goes wrong here? Overparameterization? Other problem?

- What does the bias criterion actually tell you about the quality of the snow model? Since all incoming snow melts at some point during the next melt season, a bias can only be due to bias in the input fields. Could you comment on this?

- I would certainly explicitly state the hydrological performance criteria, since this is an interdisciplinary journal.

- Why did you choose to calibrate on the Kling-Gupta criterion rather than on a criterion that makes proper assumptions about the model error distribution (see e.g. the work of Kavektski et al., Water REsour. Res.) or simply least-square (assuming normal error)?. This should probably motivated (again: interdisciplinary journal, with readers not familiar with hydrological model calibration practice).

- The test with spatial SWE maps is not a test of the model performance; it simply shows that the model can produce maps. It should somehow be tested (against a more physical model, data or at least within a complete catchment-scale discharge simulation). In the present form, the paper stops very abrubtly with almost no comments on the SWE maps.

- Tables: please add the units to the parameters

- It would be nice to have some further information on the availability of the model (even if I guess this will become clear once it is duly linked to previous papers).

Interactive comment on Geosci. Model Dev. Discuss., 6, 4447, 2013.