Hydrology and Earth System Sciences

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Discussions

Interactive comment on "Estimating degree day factors from MODIS for snowmelt runoff modeling" *by* Z. H. He et al.

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The authors present a smart method for deducing degree-day factors from in situ snow depth and satellite snow cover area data. This topic is relevant for HESS: hydrological model parameters, and as a consequence snow melt/accumulation model parameters, are always difficult to estimate, especially when data are scarce.

The paper is very well written, both regarding the English and the scientific aspects, apart from minor flaws. Methods are well presented and are adapted to the study, and conclusions are well supported by the results.

I however have a major methodological concern. The authors do perform a split-sample test to the results of the hydrological model, which allows identifying the transferability

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in time of the model parameters and an independent evaluation. Unfortunately, this test is not performed for the DDFs estimation from MODIS. It should in my opinion be done. Snow conditions are evolving from a year to another, which has an impact on the DDFs values. It is difficult to assume the reason why this test has not been performed: maybe the authors judged that the 10 snow data availability is not enough for such a test (but apparently it was enough for splitting the discharge data). However, I would appreciate that the authors present the results of the transferability in time of the DDFs estimated values as a preliminary step of the presented results.

Minor comments:

"degree-day" is sometimes written "degree day" in the manuscript. Please make a choice. The same thing is for "ground-based". I prefer using the hyphens.

In the abstract the study area / basins should be briefly introduced.

p. 3, l. 11-12: is "degree-day temperature" the correct name here? I would say it is a difference in temperature.

p. 4, l. 12: "point-measured" is more correct

p. 4, l. 11-14: please rewrite this sentence to make clearer that the first cited study allowed the second one to do theirs. The used "and" does not reflect this dependence. The expression "the ratio of ... and ..." is present in several places. It is better to use "the ratio of ... to ..." or "the ratio between ... and ...".

p. 7, l. 9-12: I think that the ratio defined here is incorrect. The dimension of this ratio is equal to the inverse of the dimension of the degree-day factor.

p. 7, l. 14: please remove the second occurrence of the word "model".

Section 2.1: since this section is a methodological one, there is no need to specify that the SCA data come from MODIS, and that the snow depth data are interpolated from pixel values. Knowing that spatially-distributed SCA and snow depths are used is

enough here, the origin of data will be described later in the paper, in section 3.

p. 9, l. 25-26: how are rainfall and snowfall distributed for this window? Is it a linear interpolation? Please specify.

p. 10: What is "I"? The day index? Please specify.

p. 10, l. 17: "... the number of subcatchmentS that ARE covered...".

Equations 6 to 10 should be inserted in section 2.2 instead of 2.3.

Section 3.2: please specify the version of the MODIS data as well as its origin. Does it come from the NSIDC? If yes, please respect the articles you have to make reference to. Please also add the time extent of availability of MODIS data.

Section 3.3: a description of the differences of climate and snow conditions between the two periods could help to better understand later in the paper the results over these two periods.

p.12, l. 5: we don't know at this point on which period the DDFs values have been estimated. Please specify. As I said earlier, the article would benefit from testing and validating the method over two subperiods.

Section 4.2: I am quite surprised about the better results on the validation period than on the calibration period that we often observe in the results. Please comment.

p. 17, l. 11: why did you use RMSE here, instead of the other metrics (NSE...) used for evaluating discharges earlier?

Figures 10 and 11: on these figures, SWE from the two modelling choices and SCA from MODIS are presented. However, p. 19, l. 17-19, the authors say: "Correspondingly, the simulated snow covered areas using calibrated DDFs are higher than those observed from MODIS (see Figs. 10 and 11 on 10 June 2003 and 27 May 2008)". I don't know what allows the authors to state that. On these figures, different things are presented and cannot directly be compared. There is no simulated snow covered

areas. I assume that the authors speak about the green and purple surfaces to differentiate covered and non-covered areas. I am a bit skeptical about this choice since a SWE of 18 mm was defined earlier. I would urge the authors to be cautious in this sentence and the end of this paragraph with what they say, and maybe also to modify the figures following my comments.

p. 20, l. 26 to p. 21, l. 2: please pay attention to the fact that on snowmelt driven basins (or any basin with high discharge seasonality) high NSE values are easier to be reached.

Globally the figures are good, but I am afraid that some of them would not appear clearly in the final version of the paper. The legend fonts are too small for Figure 2. Figures 4 to 7 are difficult to read, please try to find a way to ease the distinction between the different curves.

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