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Hydrology and Earth System Sciences Discussions

Interactive comment on "Snow satellite images for calibration of snow dynamic in acontinuous distributed hydrological model" *by* C. Corbari et al.

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General comments

The study of Corbari et al. presents the application and performance of a simple snow algorithm implemented into the continuous distributed hydrological model (FEST-WB).

The main objective is to propose the use of satellite snow cover images in the model calibration and validation.

The title of the manuscript outlines a general objective of the study. The assessment of the potential of satellite snow images in hydrological modelling is a relevant scientific topic which is definitely within the scope of HESS. The application of remote sensing data for calibration and validation of hydrologic models is appealing because satellites may provide an alternative source of information with good temporal and spatial resolution. This is particular of interest especially in regions with sparse observations (e.g. in mountains).

Unfortunately, the paper in present form does not sufficiently outline and highlight the novelty of concepts and data applied. Presented results and conclusions are brief and do not fully support the main objectives of the paper, and interpretations and discussion made. The structure and content of sections needs to be revised taking into consideration following general and specific comments:

1) A clear and detailed identification of the main objectives will improve the readability of the manuscript. In the introduction section a more detailed review of existing studies is a must. The studies which are not written in English should be addressed in more detail.

2) A more detailed summary of available satellite images would be very interesting. The authors should provide more information about the number of images available, about the cloud coverage distribution (both spatial and seasonal) and the ground observations as well. Why not to use all snow cover images available for the evaluation? If a distributed model is tested, then a restriction to different clouds threshold is not so important.

3) The methodology applied for the model evaluation needs to be extended. Especially the normalisation of snow depth is not clear and should be reported in more detail. I would suggest also to extend the snow model assessment using a simple binary

HESSD

4, S1782–S1786, 2007

Interactive Comment

comparison 8211; is snow, is not snow, performed for individual stations and pixel basis as well.

4) The elevation adjustment of snow cover classification should be justified with some quantitative assessment, e.g. using snow course measurements or data from climate stations. It is not clear why the proposed approach is more reliable in comparison to original snow cover classification.

5) The evaluation of model performance that compares the application of hydrologic model with and without the snow component is unimportant (in such alpine region, the hydrologic model should include the snow component in my opinion). Instead, I would suggest compare the case when the model is traditionally calibrated (e.g. using just local snow depth observations) and the case when satellite images are applied. This will shed more light on the potential of satellite images for calibration of hydrologic models (which is the main objective of the paper!!!).

6) The methodology proposed for model validation (both on local and basin scales) is not clear. Is it really the verification (performed e.g. as split-sample test suggested by Klemes, 1986) or just the evaluation of calibration efficiency? It is not clear if different periods are used for model calibration and verification? Potentially, it will be valuable to discuss the overall runoff model efficiency of the model (expressed e.g. by Nash-Sutcliffe, volume errors, etc.) obtained in the calibration and verification periods.

Since incorporation of the above-mentioned suggestions needs some additional work, I suggest to accept the paper with major revision.

Specific comments

1) p. 3981 The main aims of the paper have to be defined more clearly in the introduction section.

2) p. 3982 Are the stations with air temperature observations identical with the rain-

HESSD

4, S1782–S1786, 2007

Interactive Comment

gauges? How many and at which elevation is air temperature measured?

3) p. 3982 (l.11) The raingauges are plotted in Fig.1 not in Fig.2.

4) p.3982 (l.20-23) More details on how are the parameters maps derived would be interesting, especially for the traceability of results.

5) p.3982 and 3983 Snow data section: More detailed information is needed. How many images are available? What is the temporal and spatial frequency of cloud obscuration? How many snow gauges are available? What is the temporal resolution of the measurements? Daily?

6) I would suggest shorten the hydrologic model description, especially the part focusing on evaporation. This is not the main objective of the paper. Instead I would suggest present more about the calibration strategy, parameter uncertainty and sensitivity.

7) p.3987 What is the advantage of the proposed methodology used for air temperature interpolation over the "classical" lapse rate (gradient) method. Is the proposed lapse rate (-0.0065) representative for hourly air temperature measurements?How sensitive are snow model simulations and model efficiency to different lapse rates values?

8) p. 3989 Please give more details about the calibration strategy applied. What grid resolution was selected for the comparison, 500 or 1100m? How many satellite images are used for the comparison?

9) Figures:

Fig. 3 is not relevant to the paper objectives. I would suggest to remove it.

Fig.4: correct the X axis label

Fig5: the legend is not clear

Fig.6: The readability should be improved. I would suggest use the colours instead of hatching.

HESSD

4, S1782–S1786, 2007

Fig. 8. The histograms are not presented, missing legend (white colour is snow?)

Fig.11. It is obvious that the volume does not change. The snow model component does not alter the overall water balance.

10) English proof is recommended.

Klemes, V 1986. Operational testing of hydrological simulation models. Hydrological Sciences Journal 31: 138211;24.

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HESSD

4, S1782–S1786, 2007

Interactive Comment