Hydrol. Earth Syst. Sci. Discuss., 4, S2083–S2089, 2008
www.hydrol-earth-syst-sci-discuss.net/4/S2083/2008/

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.

C. Corbari

chiara.corbari@mail.polimi.it

Received and published: 25 January 2008

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). 1- 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 paper was deeply modified, after further analysis of available data, to better highlights its aims and novelty.

The structure and content of sections needs to be revised taking into consideration following general and specific comments: 2 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.

The introduction is enlarged in the new version and new references are also added.

3 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.

According to the referee comment, the total number of satellites images is given in the new text. Table 1 was added to give details on used images. We selected only images without clouds to easily study the entire basin. For the four years of study we found many images without clouds, sufficient for an accurate calibration of the model.

4 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 comparison: is snow, is not snow, performed for individual stations and pixel basis as

HESSD

4, S2083–S2089, 2008

Interactive Comment

well.

We reported in more detail how normalization is done. According to the reviewer we used binary comparison (snow – not snow) for the individual station, while for pixels we already use this type of comparison. The performance of the snow model was assessed by means of contingency tables.

5 - 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.

We justified the elevation adjustment of snow cover classification with: 1) temperature thresholds derived from the comparison between simulated maps and satellite images before the elevation correction are: -5°C for Tinf and -2°C for Tsup. These values of temperature thresholds exceed the literature values (Tarboton et al., 1994).

2) Validation at the basin scale comparing simulated and observed hydrographs at the three main basin outlets. Discharge simulated with temperature parameters calibrated with raw images overestimate peak and volume flow.

6 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).

As one of the aim of the paper is to present a distributed model for alpine basins we think that it is important for the completeness of the model results.

7 - 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!!!).

In the revised paper we highlights that the snow depth observations are used for the

HESSD

4, S2083–S2089, 2008

validation of the calibration procedure based on satellite images.

8 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.

Calibration was based on the satellite images, while the snow depth observations are used for the validation of the procedure.

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

Specific comments 10 p. 3981 The main aims of the paper have to be defined more clearly in the introduction section.

The introduction is enlarged in the new version.

11 p. 3982 Are the stations with air temperature observations identical with the rain-gauges? How many and at which elevation is air temperature measured?

These new details are reported in the new version

12 p. 3982 (l.11) The rain gauges are plotted in Fig.1 not in Fig.2.

Ok

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

We think that for the purpose of the paper information is sufficient

14 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 ob-

HESSD

4, S2083–S2089, 2008

scuration? How many snow gauges are available? What is the temporal resolution of the measurements? Daily?

Ok, these new details, requested from the reviewer, are reported in the new version, but we don’t use images with cloud obscuration.

15 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.

We thought to reduce the session three but than the attention given to radiation and its geometry is due to: a) the analysis of the shadow areas, that is done off line from the modelling; b) the computation of evapotraspiration for those pixels without snow cover, that play an important role in the continuous in time simulation.

Due to the focus on snow dynamic and its modelling, the calibration phase of the other parameters of the model is not reported in the text for reason of space. We mention in the text only the calibration of the snow routine. For the calibration of other parameters look Rabuffetti et al.. (D. Rabuffetti, G. Ravazzani, C. Corbari and M. Mancini.: Verification of operational Quantitative Discharge Forecast (QDF) for a regional warning system. The AMPHORE case studies in the upper Po river, Nat. Hazard Earth Sys., accepted, 2007.)

16 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?

The method used is the "classical" lapse rate (gradient) method. The international Civil Aviation organization defines an international standard static atmosphere with a temperature lapse rate of -0.0065 °C / 1000 m.

17 p. 3989 Please give more details about the calibration strategy applied. What grid

HESSD

4, S2083–S2089, 2008

resolution was selected for the comparison, 500 or 1100m? How many satellite images are used for the comparison?

According to the referee comment, the total number of satellites images is given in the new text edited. For the comparison of satellite images and simulated maps we use satellite grid resolution of 1100 m as stated in the paper.

9) Figures: 18 - Fig. 3 is not relevant to the paper objectives. I would suggest to remove it.

As we stated before, we think that the description of the radiation balance is important on complex topography.

19 - Fig.4: correct the X axis label

Ok

20 - Fig5: the legend is not clear

Ok

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

Ok

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

Ok

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

We have decided to cancel this plot, in fact the new graph reports the comparison between observed volume and simulated ones with different temperature thresholds for snow accumulation.

HESSD

4, S2083–S2089, 2008

10) English proof is recommended.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 3979, 2007.