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Interactive Comment

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

Anonymous Referee #3

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Review of hessd-2007-0161:

Snow satellite images for calibration of snow dynamic in a continuous distributed hydrological model

By C. Corbari, J. Martinelli, G. Ravazzani and M. Manchini.

Perception of the paper

The paper presents a distributed model for continuous precipitation-runoff simulation, and provides three sections of results from model calibration/testing: Local calibration of the model against point snow depth measurements, basin scale calibration against remotely sensed snow cover images, and finally comparing the model with and without the snow subroutine, demonstrating the necessity of this routine. In addition, an elevation based method to correct shadow-induced misclassification of satellite images is suggested and motivated by a brief analysis.

It is not absolutely clear how much of the model development is a novel contribution from this particular paper, two references are cited which this reviewer has not read (one in Italian). From the context it is assumed that the new part consists of adding the snow routine.

The satellite images are binary classified (snow, no-snow), the local comparison is performed at a single site. Although not explicitly stated, it seems that a quite large number of images are used; from fig. 7 one may suggest about 25. From this (and supported by the title), I consider the most important part of the paper to be the use of satellite images in calibration.

The calibration experiments address two parameters; the min and max thresholds in a linear temperature dependency of precipitation type. These parameters are clearly important for timing and length of the snow season. The calibration of other parameters (snow routine and remaining model) is not mentioned.

Summary / general evaluation:

The paper presents a substantial effort in bringing a multi-year time series of satellite images into the calibration of a distributed model. However, the authors need to identify the main contributions of the paper, and ensure that the literature review, the presented background theory, and the analysis results are focused towards the main conclusions. The manuscript needs some extensions in both its review/theory part and its result/analysis part, in order to provide a contribution which is both substantial and properly put in context of existing knowledge. Having said so, papers reporting different approaches to calibrating distributed models by satellite data are highly welcomed, and the value and workload of processing a time series of images is clearly acknowledged. The paper is clearly structured and easily read, though some improvement of

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the English could be considered.

Specific remarks

In the first paragraph of the abstract (lines 2-4), the importance of complex topography and hillslope exposition is highlighted as a main motivation. These problems are not addressed in the main results or discussions in the paper, only in relation to the satellite classification correction routine.

The introduction is very brief, and do not contain sufficient references to place the current work in the context of existing knowledge. Judging the calibration with satellite snow data as being the most important part of the manuscript, I in particular miss reference to the substantial body of recent literature on model calibration, in which equifinality, parameter dependency and identifiability, multi-objective calibration etc is discussed. Also, the references to earlier work on satellite snow data in hydrological models are few. Lines 12-20 on page 3981 mainly repeats the abstract and could be deleted

Section 2.1: I am missing a treatment of how the AVHRR images were classified, and a comment on sub-grid snow cover variability in relation to the binary classification used both in simulations and data. In particular this relates to the abstract’s identification of rough terrain and hillslope aspect as serious challenges, to which I agree.

If the authors consider the elevation based correction routine to be a substantial contribution of this paper, its assumptions and potential uncertainty should be discussed more thoroughly, in particular with respect to snow covered heterogeneity in high alpine terrain.

Section 3: The theory section gives a through presentation of the FEST-WB model, with particular focus on the rather detailed radiation equations used for evaporation estimation. Since evaporation is not subject to analysis, and radiation is disregarded in

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the simple degree-day snow melt model, the level of detail in section 3 should be reconsidered, and the background theory be more focused towards the presented analysis.

The presented degree-day snow melt routine does not contain sufficiently novel concepts to stand as a theoretical contribution from this paper. Challenging this widely applied concept with multi-response data, however, is valuable; in particular for a distributed model. A theoretical discussion on where its limitations might be revealed by snow coverage validation, could lead up to some strategic tests and comparisons.

Section 5.1. page 3989 line 1: “Area covered by snow were classified as no covered pixels”. Where does the ground truth come from here? In the following paragraph (lines 1 through 15); discuss if other processes than shadowing may occur, for instance lack of snow accumulation at ridges or in steep slopes, and hence correctly no-snow even at high altitudes. Lines 11-12 state that all pixels are corrected, but isn’t this confined to pixels identified as shadowed by solar exposure analysis?

Section 5.2: Considering the amount of work obviously spent in preparing, classifying and error filtering of the images prior to their use in calibration, the analysis presented from this section appears a bit short. This is in my opinion the best part of the manuscript, and I think it should be extended in order to fulfill the substantial contribution one should expect from a scientific paper. For instance, I suggest that the calibration is reported to a greater detail than comparing two or three points in the parameter space. In particular the two-dimensional calibration easily allows a highly informative contour plot of performance, to illustrate dependency and sensitivity.

Also, I would welcome a comparison of simulated versus observed elevation dependency, and the consistency of a elevation line between snow and no-snow. For instance this could be a snow cover versus elevation xy plot (averaging over elevation bands), or (if the snow/no-snow divide is very sharp), time series of the snow/no-snow altitude front. In particular because the authors use elevation as a key to compensate for satellite image mis-classification in shadowed areas, an assessment of the uncertainty in

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this connection would be desirable.

Page 3989, the result in lines 22-25: Consider relating the thresholds found to similar values found by other authors, since this result appears to be one of the main findings in the paper.

Section 6: The last section of the paper demonstrates by hydrograph comparison that a snow routine is necessary in this alpine catchment with a stable seasonal snow cover. In my opinion, this result should be obvious and unnecessary to present in a scientific journal. A largely more valuable experiment involving simulated hydrographs would be to compare the same parameter sets that were tested against AVHRR observations, and discuss if the two sources of calibration information places conflicting requirements to the parameter set.

Typos and technical errors

Page 3982 line 11: Should read fig 1, not fig 2.

Page 3989 line 4: Should read fig 1, not fig 7.

Figure 10: These are not histograms.

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