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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.

C. Corbari et al.

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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.

1- 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

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snow routine.

The paper addresses the snow dynamic and its role in the distributed model, so that much of the effect is on this part then on the other parts of the model.

2- 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.

According to the referee comment, the total number of satellites images are given in the new text edited.

3- 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.

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.)

Summary / general evaluation: 4-5 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

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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 the English could be considered.

New references are added in the new manuscript

Specific remarks 6- 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.

According to the referee comment, we try to be clearer in the new text version specifying that the pixels aspect is considered : a) for the correction of snow cover map from satellite images; b) in the evapotraspiration fluxes for the model run.

7- The introduction is very brief, and do not contain sufficient references to place the current work in the context of existing knowledge.

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

8- 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 data in hydrological models are few. Lines 12-20 on page 3981 mainly repeats the abstract and could be deleted

We agree with the reviewer on the importance of effect of parameter range of variation and their mutual effect on the model output according to equifinality approach.

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Nevertheless this paper focuses on a description of physical of snow melting and accumulation dynamic with a simplified model where the driving parameters are only the two values of the air temperature that determine the dynamic of snow. These value are obtained from satellite image analysis and verified on ground data at four stations. New references were added to earlier work on satellite snow

9- 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.

The shadow analysis is performed on a dem sampled at the same scale of AVHRR image, so that shadows are computed from the sun position on the DEM. The procedure is a justification of the false snow free pixels on the images. From modelling point of view all pixels above a given elevation are covered by snow.

10- 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.

We agree in principle with the referee comment even if the space resolution of snow images 1.1Km*1.1Km is too coarse respect to the heterogeneity of alpine morphology in the studied basin. In any case we consider the problem of the shadowed areas which has an important role in a complex topography.

11- 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 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.

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We thought to reduce the session three but than the attention give 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.

12- 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.

The presented FEST-WB model is a model that simulate all the main processes of the hydrological balance trying to reduce the number of parameters. So we don’t focus our attention on an extremely detailed snow accumulation and melt routines.

13- 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?

The explanation that pixels are covered by snow comes from the knowledge of the characteristics of the studied basin.

14- 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.

The space resolution that is used in this work, determined from the operative satellite data, is to coarse to verify the lack of snow accumulation at ridges or in steep slopes.

15- Lines 11-12 state that all pixels are corrected, but isn’t this confined to pixels identified as shadowed by solar exposure analysis?

We stated that we identify as covered by snow all pixels above the mean altitude of the snow covered pixel determined from the raw satellite images.

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16- 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.

Section 2 reports comparison of modelled snow cover maps and observed from satellite. We enlarge the comparison reporting five combinations of temperature thresholds. In addition this section was also enlarged adding a comparison between the raw satellite images and the simulated ones. In this latter case the temperature thresholds, used in the snow model, that reproduce the raw satellite images are not reliable also respect the literature review.

17- Also, I would welcome a comparison of simulated versus observed elevation dependency, and the consistency of an 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 this connection would be desirable.

The effect of the elevation line between snow and no-snow is reflected in the number of snow covered pixels with different temperature thresholds reported in the paper.

18- 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.

New references were added.

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19- 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.

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.

20- 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.

In the last section of the revised paper, we added a comparison of the hydrographs simulated with different combination of temperatures.

Typos and technical errors 13- Page 3982 line 11: Should read fig 1, not fig 2.

Ok

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

Ok

13- Figure 10: These are not histograms.

Bar charts

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