Answer to reviewers of the paper: The Cache la Poudre river basin snow water equivalent modeling with NewAge-JGrass

General Comments

The authors thank the editor and the reviewers for interesting comments and suggestions that highly improved the quality of the paper.
We revised the language of the whole paper and the spelling errors as expressly requested by both the reviewers. We changed the title and the abstract of the paper following Dr. Schaefli's suggestion. As suggested by Dr. Parajka we further clarified the novelty of the approach in the revised version of the paper.

From the reviewers comments we derived the idea to add two further snow melt models in addition to the one by Casorzi and Dalla Fontana presented in the previous version of the manuscript. We added a traditional degree-day approach (as requested both reviewers) and the Hock model (requested by Dr. Schaefli). These additions were done also to illustrate the versatility of the modeling system we implemented, which allowed this addition in the time frame of the revision. These two new models were added as additional OMS components, maintaining all the other system settings such as the input-output interpolations, the visualization tools, the automatic calibration, and the other ancillary components. In the case of the Hock model, which required the computation of the potential clear sky solar radiation, the new component presented in Formetta et al. (2013) was seamlessly linked to the modeling solution (MS). This in line with our goal not to present a model, but to introduce a system for easily implementing hydrological process components and enabling the interchange of components according to user needs and input data availability. Because of the above changes, we modified the format of the paper, which now includes the description of the three modeling components.

Finally we removed from the paper the application about the model parameter sensitivity. Thanks to reviewers' suggestions, we decided that the argument could be discussed in a new paper where analysis and simulations focus on the topic, and where comparison with physically based models and snow cover retrieval from satellite measurements could be addressed and discussed within the appropriate space.
We decided instead to leave in the paper the application on raster map production of snow water equivalent to illustrate some of the model's capabilities, i.e. the possibility to work not only in a point mode but also in a raster mode and to visualize model results immediately within the GIS uDig-JGrass, an important part of the modeling system.

In the following, we respond directly to each of the reviewer comments.

Referee: Dr. Bettina Schaefli

Comment n.1

From the abstract alone it is not clear that the model is an improved temperature-index approach, it gives the impression of a physical model

Answer n.1

We agree with the reviewer's suggestion and modified the abstract. The first sentence of the revised abstract is: "The paper presents a package of a modified temperature index based snow water equivalent model as part of the hydrological modeling system NewAge-JGrass."

Comment n.2

The introduction does not discuss temperature-index methods, their shortcomings and why a new method is proposed, and in as far it is comparable to the standard method of Regine Hock using potential radiation. Such a literature review seems important. I would also mention in the abstract that you use a modified degree-day formulation (the term does not appear in the text?)

Answer n.2

The authors agree with the reviewer's suggestion. We added the following sentences:

"In other studies (Cazorzi and Dalla Fontana (1996); Hock (1999)) the degree-day (or temperature index) snow modeling was improved by taking in account of a radiation term in addition to temperature. In Cazorzi and Dalla Fontana (1996) the radiation term is an energetic index computed for each pixel of the grid as shortwave solar radiation integrated over time, as explained in section 2.
In Hock (1999) the melt factor depends on the value of the clear sky solar radiation, following on studies by Kustas et al. (1994) and Brubaker et al. (1996). Hock's model depends on two separate terms: a constant value (melt coefficient) and a value function of the potential solar radiation (radiation co-efficient). A third temperature-based snow modeling approach was presented by Tobin et al. (2012) who proposed to use a varying degree-day factor throughout the day to improve simulation of snowmelt rates at sub-daily time steps as a component of a runoff model. In this paper we implement three of these temperature-based snow models: a degree-day (C1), Cazorzi and Dalla Fontana's model (C2) and Hock's model (C3) of snow water equivalent, that estimates SWE from spatially distributed radiation and temperature."

Comment n.3

The introduction of a smooth threshold for accumulation is very useful. Are the parameters of eq. 4 calibrated? And if yes: why is there still a bias in the snow simulations?

Answer n.3

No, the parameter of the smooth threshold was not calibrated. We used literature value as suggested in Kavetski et al. (2006): "Experimentation shows that smoothing the melting temperature over 1–2 °C works well; stronger smoothing is usually also innocuous".

Comment n.4

Eq. 5: does the equation apply for negative temperatures?

Answer n.4

We agree that the formula was not sufficiently clear: it is valid only for positive temperature. In the revised paper we changed the melting formulation including the additional two melt models and for all of them we better specified the melting formula as suggested by the reviewer:

$$M = \begin{cases} \alpha_{m1} \cdot (T - T_m) & \mathrm{T} > \mathrm{T_m} \\ 0 & \mathrm{T} \leq \mathrm{T_m} \end{cases} \qquad (5)$$

The equation for the melt process during the day is:

$$M = \begin{cases} \alpha_{m2} \cdot EI \cdot (T - T_m) \cdot V_S & \mathrm{T} > \mathrm{T_m} \\ 0 & \mathrm{T} \leq \mathrm{T_m} \end{cases} \qquad (6)$$

The equation for the melt process during the nigth is:

$$M = \begin{cases} \alpha_{m2} \cdot min(EI) \cdot (T - T_m) \cdot V_S & \mathrm{T} > \mathrm{T_m} \\ 0 & \mathrm{T} \leq \mathrm{T_m} \end{cases} \qquad (7)$$

$$M = \begin{cases} (\alpha_{m3} + \alpha_e \cdot R_s) \cdot V_S \cdot (T - T_m) & \mathrm{T} > \mathrm{T_m} \\ 0 & \mathrm{T} \leq \mathrm{T_m} \end{cases} \qquad (8)$$

Comment n.5

Are there other papers that suggest to use a different melt formulation for night and day or is this new her? I think you should refer to the paper by Tobin et al., 2012 Adv. in Water Resources that suggests a temperature-index approach with a quasi-sinusoidal cycle of the melt factor (a similar idea to obtain different relation between melt and air temperature during the night)

Answer n.5

We added a reference to Tobin et al. and we are going to implement this method in future. In this revision of the manuscript, we implemented instead a classical degree-day model and Hock melting formulation.

Comment n.6

What makes the formulation different from the classical Hock-method? What is the advantage of this formulation? As far as I understand, both use potential radiation, which represents a certain drawback (see also a discussion in the above paper by Tobin et al.). Does the underlying complete hydrological model not account for real weather conditions? I do not entirely understand the description on p. 4455, especially what the energetic index is.

Answer n.6

The classical Hock method, which is now a component of the NewAge snow melt model, uses the theoretical (under cloud-free conditions) solar radiation for each time-step. Our method, which is a modified version of Cazorzi and Dalla Fontana (1996), uses an energetic index computed for any site in the basin. This energetic index is a map containing the average theoretical solar radiation (under cloud-free conditions) received during a given period. In our paper we used five maps. The five maps were computed from 21 December to the end of February, March April, May and June respectively. This time averaging is useful when a single prediction has to be made several times (for instance when calibrating the parameters) because it avoids the calculation of radiation for each time step. Finally as shown in the equations, in the Hock method melting is function of an additive coefficient (melt plus radiation coefficient), and in Cazorzi and Dalla Fontana (1996) method melt is function of one multiplicative coefficient.

However, with respect to the original paper, we calculated radiation with the model component presented in Formetta et al. (2013) that incorporates the diffuse component of the solar radiation and the presence of shadow in mountain locations, so the calculation of the energetic index is more complex than in the original Cazorzi and Dalla Fontana (1996) approach.

Comment n.7

Testing against observed point data: the model seems to do a good job on a daily time step to reproduce the observed point data for the calibrated stations. But since the model has separate formulation for day and night, it should be tested against hourly data.

Answer n.7

We accept the reviewer's suggestion and added an hourly time step test for the three different melting formulations. This served also to show the capability of the model, which is able to switch melting component according the user's needs preserving all the remaining components of the modeling solution (input data format, calibration and verification component). Results were thoroughly presented and discussed in the revised paper.

Comment n.8

Furthermore, it should be discussed in as far the many parameters might lead to overparameterization. This point is important since the test against stations for which it has not been calibrated shows poor results.

Answer n.8

Over-parameterization is a problem for many hydrological models, and it becomes more and more important in lumped model where the physics of the simulated process is reduced and simplified. We accept the critique of the reviewer and will address this problem in future research, which will also emphasize testing the model at stations for which model has not been calibrated. We added a new sentence to the conclusions in the revised paper: "Future research will address problems related to modified temperature index snow water equivalent models such as transferability of parameter values to new locations and time periods, over-parameterization, comparison with physically based snow models, and verification of how well simulated snow cover spatial patterns reproduce spatial and temporal variability of the snowpack."

Comment n.9

The Nash criterion is not very useful to test models with a very strong annual cycle (see Schaefli and Gupta, Hydrol. Proc. 2006). The test against stations for which the model has not been calibrated gives poor results, this should be illustrated with a time series. I get the feeling that the text does not sufficiently underline how poor the results are (I would think that Nash values below 0.6 or even 0.5 mean that the series are completely off); given the strong annual cycle much higher Nash values are to be expected even for a not well performing model (that's why the Nash criterion is not very useful here). What goes wrong here? Overparameterization? Other problem?

Answer n.9

We accept the suggestion of the reviewer, but we used NSE as goodness of fit index because it allows us to use the classification presented in Stehr et al. (2008) and Van Liew et al. (2005). We understand that it is not very accurate for cycling signals but, use of this performance metric provides consistency with prior research. We decided that this problem warrants further investigation and are preparing a new manuscript where comparisons with fully physically based models are planned. However we decided to specify in the revised version of the paper that: "While recent studies demonstrate that NSE is not very appropriate to test models with a very strong annual cycle, Schaefli and Gupta (2007), this metric is used to maintain consistency with prior studies of Stehr et al. (2008) and Van Liew et al. (2005)"

Comment n.10

What does the bias criterion actually tell you about the quality of the snow model? Since all incoming snow melts at some point during the next melt season, a bias can only be due to bias in the input fields. Could you comment on this?

Answer n.10

As specified in Formetta et al. (2011), Marechal (2004) and Van Liew et al. (2005), the PBIAS measures the average tendency of the simulated flows to be larger or smaller than their observed values. The optimal PBIAS value is 0.0, positive values indicate an overestimation of the model and negative values represent an underestimation. We specified the meaning of the goodness of fit as suggested by the reviewers.

Comment n.11

I would certainly explicitly state the hydrological performance criteria, since this is an interdisciplinary journal.

Answer n.11

The authors accepted the reviewer's suggestion. In the revised version of the paper a description of the goodness of fit indices, their meaning and their typical value was added in Appendix.

Comment n.12

Why did you choose to calibrate on the Kling-Gupta criterion rather than on a criterion that makes proper assumptions about the model error distribution (see e.g. the work of Kavektski et al., Water Resource. Res.) or simply least-square (assuming normal error)?. This should probably motivated (again: interdisciplinary journal, with readers not familiar with hydrological model calibration practice).

Answer n.12

We agree with the reviewer's comment. In the reviewed version of the paper the choice of KGE is presented and motivated:

"The Kling-Gupta Efficiency (KGE), eq. A1, presented in Gupta et al. (2009) was selected as the calibration objective function. KGE, unlike other goodness of fit indices, such as Nash Sutcliffe Efficiency, is able to synthesize in one objective function three different components from measured (M) and simulated (S) data: i) correlation coefficient (r), ii) variability error, $a=\sigma S/\sigma M$ and iii) bias error, $b=\mu S/\mu M$.

μS and μM are the mean values of measured and simulated time series, and σS and σM are the standard deviations of measured and simulated time series."

$$KGE = 1 - \sqrt{(r-1)^2 + (a-1)^2 + (b-1)^2} \qquad (A1)$$

However, this is just one choice of the many that is made possible by our system. Using other goodness of fit objectives is possible, but, in our opinion, does not add very much to the main message of the paper.

Comment n.13

The test with spatial SWE maps is not a test of the model performance; it simply shows that the model can produce maps. It should somehow be tested (against a more physical model, data or at least within a complete catchment-scale discharge simulation). In the present form, the paper stops very abruptly with almost no comments on the SWE maps.

Answer n.13

The authors agree with the reviewer comment, but the idea was just to provide the concept that the model as, all the NewAge components (see Formetta et al., 2013) is able to work both in raster and in vector mode. Providing snow maps and immediately visualizing them in the integrated GIS uDig-JGrass is, in our opinion, an important model capability. Comparison with a physically based model (for instance GEOtop, Rigon et. al., 2006) and discharge is topic of a different work that we are going to submit. The objective of the current paper was to present the new snow melting package and verify it against data measured at stations.

Comment n.14

Tables: please add the units to the parameters

Answer n.14

The authors agree with the reviewer comment, and tables were modified.

Comment n.15

It would be nice to have some further information on the availability of the model (even if I guess this will become clear once it is duly linked to previous papers).

Answer n.15

The stable version of the model will be available under GPL version 3 license at: http://code.google.com/p/jgrasstools/. The research version used in this paper is available on a GITHUB repository.

Referee: Parajka

Comment 1
1) The novel contribution is not clear. Maybe I missed something, but I did not find a paragraph (in the introduction section) clearly stating the main objectives of the paper and what is going to be novel. In case, it is a novel approach for snowmelt modeling, it needs to be tested more thoroughly against some existing approaches (e.g. by comparing the approach with simple degree-day model, SRM model, etc.). In the methodology section, It is essential to clearly state what part of the model is to be tested and why?

Answer 1
The goal of the paper is not simply to present a model but to provide the user a set of components for modeling a hydrological process, and make possible the interchange of components according the user needs (input data availability, etc.) by programming them according to the OMS version 3 framework (David et al., 2013). In the revised version of the paper we added also other two models whose results are compared with the one developed first: traditional degree-day model, and the Hock melting formulation. The three components were tested and results were presented and analysed in Test n.2 of the revised paper.

We wrote:
"In this paper we implement three of these temperature-based snow models: a degree-day (C1), Cazorzi and Dalla Fontana's model (C2) and Hock's model (C3) of snow water equivalent, that estimates SWE from spatially distributed radiation and temperature. They are provided as Object Modeling System version 3 (OMS3, David et al. (2013)) components and integrated with the other components of the JGrass-NewAGE system. This system provides an optimal framework for comparing modelling solutions, as all the ancillary tools used remain unchanged when switching from one SWE model to the other. The model components can then be executed using OMS3 implicit parallelism to improve computational efficiency in multicore or multiprocessor machines."

Comment n.2

The authors should very carefully select the validation examples. It is not only the Nash-Sutcliffe efficiency, which demonstrates the model performance. For example, I have some problems to interpret the parameter values presented for different stations. Why is the adjustment for measurement errors for rain larger than for snow? In this case I would not consider the option for automatic calibration as an automatic advantage of the modeling system. In this context, I would suggest to add some discussion about the compensation effects of model parameters on model performance.

Answer n. 2

As specified in the general comments we removed application 2 and substitute it with the model inter-comparison at hourly time-step. This was motivated mainly by the fact that we understood that model parameters sensitivity analysis and their variation in space needs more accurate analysis, and it can be the goal of a different paper and not just a subsection of a paper with a different goal.

Comment n. 3

I would suggest to present some verification of the simulated spatial patterns (i.e. by using freely available MODIS snow cover data). Validation of spatial patterns will clearly demonstrate and justify the value of newly implemented procedure for smoothing the threshold temperature and radiation correction of degree-day factor or show some advantages related to different packages used for model inputs preparation or model calibration

Answer n. 3

We agree with the reviewer comments, but as specified also in the general comments and in the last section of the revised paper, parameter sensitivity, model comparison with a physically based snow melting model (e.g. GEOtop), and spatial patterns analysis of snow prediction will be addressed in a second paper. In the current paper our aim is to verify the model at the point scale, show the model capability to provide raster maps of snow water equivalent, compare three different simple melt formulations, and show that they work all in the same model system.

Comment n. 4

In order to reproduce the results, it would be interesting and useful to provide more technical information on how to download, setup and use the system (e.g. by providing some brief tutorial and data example).

Answer n. 4

The stable version of the model will be available under GPL version 3 license at: http://code.google.com/p/jgrasstools/. The research version used in this paper is available on a GITHUB repository.

Comment n. 5

Abstract: The presented snowmelt model is based on a conceptual degree-day approach, so I do not agree that it accounts on the main physical processes. Please consider to revise the text accordingly. The last sentence is also not clear, please revise.

Answer n. 5

The authors accept the reviewer's comment and modified the abstract. In the revised paper the sentence is: "The paper presents a package of a modified temperature index based snow water equivalent model as part of the hydrological modeling system NewAge-JGrass. Three temperature-based snow models are integrated in the NewAge-JGrass modeling system and use many of its components such as those for radiation balance (SWRB), kriging (KRIGING), automatic calibration algorithms (particle swarm optimization), and tests of goodness of fit (NewAge-V), to build suitable modelling solutions (MS)."

Comment n. 6

Introduction, p. 4450, l.2: "..in this dissertation...". please revise.

Answer n. 6

This sentence was corrected.
Old sentence: "In this dissertation we implement"
New sentence: "In this paper we implement"

Comment n. 7

3) p.4449: snow water depletion curve? Is it not snow cover depletion curve?

Answer 7

We accept the reviewer's comment, and the mistake was corrected.
Old sentence: "SRM is a linear model in which the independent variables are an average of the daily temperature and an estimate of the catchment area covered by snow which is called snow water depletion curve. These are tricky to determine, but possible to be detected by satellites."

New sentence: “SRM is a linear model in which the independent variables are an average of the daily temperature and an estimate of the catchment area covered by snow. The snow covered area can be determined from airborne or satellite remote sensing data.”

Comment n. 8

4) p.4457, l.16: SNOOTEL.

Answer n. 8

The authors accept the reviewer’s comment, and the mistake was corrected.

Comment n. 9

5) p. 4458: what is the GOF?

Answer n. 9

The authors accept the reviewer’s comment, and the mistake was corrected:
Old sentence: “Three classical GOF”
New sentence: “Three classical goodness of fit indices (GOFs)”

Comment n. 10

6) The discussion section is missing. Please consider to discuss your finding with respect to existing approaches (literature).

Answer n. 10

In order to avoid two subsections for each of the three applications, one for results presentation and one for results discussion, the authors prefer to keep the structure of the submitted paper. Results are presented and discussed in one subsection for each application.

Comment n. 11

7) Table 1: Are the longitude values correct?

Answer n. 11
The longitude was wrong, and the mistake was corrected.

Comment n. 12

8) Fig.2: Decimal numbers in legend are not necessary.

Answer n. 12

The authors accept the reviewer's comment, and the mistakes were corrected.

References

Endrizzi et al., 2013 GEOtop 2.0: simulating the combined energy and water balance at and below the land surface accounting for soil freezing, snow cover and terrain effects, GMDD

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