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The Cryosphere
Discussions

Interactive comment on "Degree-day modelling of the surface mass balance of Urumqi Glacier No. 1, Tian Shan, China" *by* E. Huintjes et al.

Anonymous Referee #2

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Review of Huintjes et al.: Degree-day modelling of the surface mass balance of Urumqi Glacier No. 1, Tian Shan, China

Submitted to TCD

General: The manuscript (MS) presents and discusses an attempt to model the mass balance of Urumqi Glacier No. 1 (UG1) using an enhanced temperature-index method for for a 6-day period in 2007 where field data is available. The results are compared to those of a simpler degree-day model and the simulation period is extended over a 16 year period based on reanalysis data.

Criticism: The main problems with the MS are the formulation of the model and its use.

1) The algorithm used here to enhance the temperature-index method is new to me and

it seems to be documented elsewhere. Although I have some doubts on its usefulness (see below), it is definitely not advisory to develop new algorithms having so little data available for testing! Instead, to simulate mass balance in a data-sparse region, a well documented method should be used or if the focus is on introducing a new method, calibration and validation require an appropriate data set.

2) I do not understand why the potential solar radiation in Eq1 is scaled to the spatial mean rbar. In doing so, it only describes the spatial pattern for the given day. This pattern is controlled by topography and solar geometry and probably does not vary a lot over the year (this view is supported by Fig8 displaying more or less the same spatial pattern of mass balance.) Therefore, the term r/rbar does not introduce much temporal variation into the melt rate. however, if global radiation makes a large contribution to melt rate, it is to be expected that this contribution varies with time. Instead of adding a radiative component to the melt rate equation like Pellicciotti et al.(2005) or altering the DDF according to radiation (Hock, 1999), the authors add a (almost constant) spatial pattern to the DDF-term. It would have been much more straight-forward to add an unscaled radiation term to the DDF-term having the form a+b*r. Then the values of a and b could be derived by minimizing the misfit between model and observations. Nevertheless the importance of radiation is likely to vary over the seasons and therefore, the calibration data should cover a large part of a year (preferably several), not just a few days.

3) Since the contribution of the radiation term a+b*r/rbar is locally (almost) constant, the term could be used as a scale, multiplied to the DDF*(T-T0) term. Instead, it is added to the DDF-term, accounting for roughly half of the calculated melt rate (this estimate is derived from comparison of the DDFs in Table 1 for the simple and the enhanced models). While this figure may be appropriate for the 6 day period in July 2007, it is questionable whether the radiation contribution is the same during the rest of the year.

4) Data treatment and presentation: The entire calibration is done using 16 data points covering a period of 6 days. The MS states several times that daily measurements (102

measurements) were taken, but it was found that the daily data were not significant versus the noise level (pt 214-1 in the interactive reply to M. Pelto; but this should be stated in the MS as well to explain the motivation for aggregating data). Finally the daily readings were aggregated to represent the 6day period. However, it is unclear how the data were treated. From the MS I understood that it was done by averaging ("...were merged into a single mean value"), but this would not make sense if the individual readings are not significant. Instead the total change between Day1 and Day6 should be considered (probably done here, but not clearly described). Also, if daily readings of 16 stakes were performed over a 6day period, that would give 16*6=96 readings (vs 102 as stated in the MS). Where do the additional 6 readings come from? Was there an additional stake that has not been considered in the analysis?

Recommendation: I agree with the authors that mass balance modeling may represent a valuable tool to spatially and temporally extend information in data sparse regions. Nevertheless, I found the MS has considerable deficiencies concerning the choice of algorithms, calibration and validation of the model, along with shortcomings in presenting the material. Each of the requirements to rectify the individual shortcomings is manageable, but the amount of work to be done is more than "major revisions". My recommendation is to reject the MS in its present form but to encourage resubmission at a more mature level.

Specific comments: The few detailed comments below, partly of technical nature, should be taken into account when preparing a revised MS.

Introduction: the motivation for the work is not made clear.

p209 L,4 when referencing Ohmura 2001 instead of Oerlemans 2001. Ohmura stated that longwave atmospheric radiation is the dominating source for melting.

excessive use of acronyms, TGS is defined but never used later, the same with SRTM, and WGMS is defined twice (p209 L10 and p211 L5)

as mentioned above: it is obsolete to describe details of stake readings which are never used later one (P211 L9-14).

Eq1 (see also comments above): T_T is used as a condition but not incorporated in the equation: should be M=DDF*(T-T_T)+a+b*(r/rbar). and the value used for T_T must be reported as well (I didn't find it in the text nor the parameter table).

it is never stated explicitly but from the text it appears that the DDmodel time step is 1d.

section "Parameter calibration": first part refers to calibrating the radiation model, this needs to be stated to avoid confusion with the melt-model. also it seems that the radiation model time step was 1h. it seems overkill to calibrate the radiation model at such a high temporal resolution, including assumptions of albedo (not clear why this is actually needed for potential INCOMING radiation?) when finally radiation is scaled to somehow represent a spatial pattern.

P214 L9, "were adjusted within reasonable limits". it would be good to know what the authors think is reasonable.

P214 L22 daily circles of radiation –> daily CYCLES

P214 L26 topography effects..radiation –> AFFECTS

P215 information needed about how downscaling was done

P215 L24 onwards: after initial confusion, I figured out that another algorithm has been tested as well and the outcomes are presented here. expand the methods section of the MS and describe what is done to avoid confusing the reader. also here: it seems odd that the model performance in terms of correlation was better during the validation than the calibration period.

Fig 5: it is wrong to state in the caption that the number of samples is 102 when only 16 points are shown. also, it is not clear whether the mass balance numbers represent a rate (mm dˆ-1) or the total over the 6day period (mm). The caption states that the values represent "mean values over six days", that would be a rate. in that case, however, the numbers seem to be large.

Fig 7: if Fig7a and b are two panels of one figure, they should print in one single frame and have only one common caption. here, they are presented as two independent figures and in this case they should have different numbers.

Fig 8: I had to figure out that the contour-lines refer to the WGMS data and the colored surface represents model results. please note this essential information in the figure or at least in the caption.

Interactive comment on The Cryosphere Discuss., 4, 207, 2010.