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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.

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1. General comments

The manuscript presents results of a numerical SMB model applied for Urumqi Glacier No. 1. Although this glacier is one of the best-investigated glaciers in Asia, most information on its mass balance is derived from field measurements. The authors therefore applied a spatially distributed numerical model for computing surface mass balance (SMB) on a daily basis for a time period covering the mass-balance years from 1987/88 to 2004/05. The model approach, which has been widely used in studies of other glaciers, is based on a temperature index model including potential shortwave radiation, aiming at improving SMB computations by incorporating small-scale variations of SMB not resolved by simple degree-day (DD) approaches.

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Interactive Discussion

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The main problem the authors are facing is caused by the lack of suitable data for calibrating and forcing the model, as well as for validating the results. Since this is a general problem, especially for Asian glaciers, the scientific significance of the manuscript would be excellent if the authors would be able presenting novel solutions for reducing this problem. However, the manuscript in its present form needs to be improved in this respect. Some suggestions in this direction will be made in the next section.

The authors are generally using scientifically sound methods (except the aspects addressed in the specific comments), and have considered the relevant scientific work in their study, which is also seen in the appropriateness of the references. The outline of the manuscript is well-structured. Some technical corrections as specified in the third section of this review need to be made, including minor improvements of the English language.

2. Specific comments

a) The goal to be achieved by the study should be made explicit! Is it to improve the time series of annual mean SMB over the selected time period (e.g. by gap filling), or is it to improve our knowledge on the spatial patterns of the annual SMB and the driving forces behind? If the first goal is the major focus, then the authors have to prove that their model approach delivers better results than those from observational data available through the WGMS. However, the manuscript implies that it is more the spatial patterns that are addressed by the study. Then, the authors have to demonstrate that the inclusion of potential short-wave radiation in the SMB model improves the results. This has not been shown, so far. In both cases the model validation strategy has to be improved as discussed below.

b) Calibration and forcing of the model needs to be improved! A general challenge of DD based SMB modelling is how to deal with processes that are not constant over time. Some studies treat DD factors as seasonally varying parameters, making model calibration even more complicated. In the SMB model used by the authors two further

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parameters for inclusion of potential short-wave radiation are existing. The authors have kept them constant over time. In addition, the sub-model used for modelling the spatio-temporal distribution of short-wave solar radiation (Kumar et al. 1997) requires albedo data as input. The authors empirically derived albedo values for only two surface types (glaciated and non-glaciated surfaces) using data from only one day (20 July 2007). Albedo values determined for this day have then kept constant over time. This is not adequate and needs improvement. One could see the problem in the north-eastern part of the glacier, where the effect of solar radiation on annual SMB is overestimated by the model. This part of the glacier shows lower elevations, thus higher air temperatures and less positive or more negative SMB values as indicated by both the observational data and the simple DD model (Figure 8). One of the reasons for this exaggeration of the influence of solar radiation on annual SMB could be that clear-sky conditions are characterised by lower-than-average diffuse radiation. Although the absolute values of short-wave radiation are not of relevance for the SMB model due to normalization of radiation by the spatial average, the spatial pattern is affected.

The authors assumed that precipitation does not increase with elevation. Since there are no AWS data that could be used for directly deriving the elevation-dependency of precipitation, this kind of treatment avoids speculation. However, as figure 8 demonstrates, the ELA is generally modelled at higher positions than observed, independent whether potential solar radiation is included or not. One of the reasons could be that precipitation increases with elevation. The authors may try to utilise observational data for inverse modelling of the vertical gradient of precipitation.

Figure 6, in combination with figure 7a shows that model calibration is not optimal, since the model systematically underestimates variability of mean annual SMB (model values are never positive when observed values are positive, and less negative when observations are strongly negative). The calibration period of six days is obviously too short. The authors may try to calibrate the model by an inverse approach as proposed for precipitation, taking annual values instead of instantaneous values into account.

c) Validation of the results needs to be improved! The authors do not utilise the observational data for quantitative analysis of the model performance, but discuss their results solely on a qualitative level. The discussion of errors should also be improved. Then, it would become obvious that the argument of the authors that the static glacier mask from 1999 leads to a systematic error is of second order compared to the above-mentioned calibration problem. There seems to be no statistical trend in difference between observed and modelled annual SMB as shown in figure 7b.

If the authors target on improving the spatial distributions of annual SMB then they need to use spatially distributed validation approaches. Again, quantitative analyses are required.

3. Technical corrections

a) Minor changes in the English language, especially concerning use of commas.

b) Use SI units, e.g. -0.71 K (100 m)-1 should be specified as -7.1 K km-1.

c) Avoid qualitative expressions like ‘satisfactory way’.

d) Use coefficient of determination (r2) instead of correlation coefficient (r).

e) Explain how RMSE values have been computed (some values seem to be strange).

f) Radiation factors a and b in table 1 do have SI units, i.e., mm d-1; see eq. 1.

g) Include error bars in figures 2 to 7.

h) Improve readability of figure caption 8, which is confusing.

i) Include some details on the simple DD SMB in the text.

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