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The Cryosphere Discussions

Interactive comment on "Assessment of glacier melt-model transferability: comparison of temperature-index and energy-balance models" *by* A. H. MacDougall et al.

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Review of "Assessment of glacier melt-model transferability: comparison of temperature-index and energy-balance models" by A.H. MacDougall, B.A. Wheler, and G.E. Flowers.

General Comments

This paper presents the results of a set of model intercomparison experiments for 4 glacier melt models, applied to two small glaciers in the St Elias Mountains of Canada, for 2 years, 2008 and 2009. The models tested consist of a simple degree day model, an enhanced degree day model (including the spatial distribution of potential solar radi-

ation) and two energy balance models with different complexities. Rather than a more traditional intercomparison experiment, in which different model experiments are conducted with the same data and with optimal parameter sets for the model and dataset under investigation in order to assess the differences between predictions made by different types of model, this paper focuses on the differences between the results of each model type when run with locally-optimised parameters versus runs with 'non-local' parameter sets in order to assess the transferability of models (more specifically in fact, the optimised parameter sets) in both space and time. It therefore assesses the degradation in model performance when parameter sets derived from a different glacier or different year are used to drive each class of model, in comparison with the optimised runs for each glacier for each year.

The key conclusion of the paper is that the most complex energy balance model is the most transferable, and suffers the smallest degradation in model effectiveness when used with non-local parameter values. The other models, however, show no consistent relationship between complexity and transferability. Overall, the experiments presented in the paper provide a useful addition to the literature on modelling glacier mass balance and melt by quantifying the differences produced by a set of models when used with differing, non-local parameter values. Within itself, the paper fulfils the criteria for publication well; it is well written; the methods used are clearly presented and appropriate for the nature of the study, the results are clearly presented (with the possible exception of Figures 3 and 5), and support the conclusions reached.

My main concern with the paper is this very simplicity, however. The model runs conducted with parameter values derived from the 'other' glacier and/or the 'other' year in the study have an intuitive 'logic' as to why those parameter values were selected, but in fact form just 3 experiments from an effectively infinite set of model experiments which could be performed with randomly chosen parameter values. In a similar way, the optimisation strategy (based on minimising the RMS error between model predictions and stake measurements), whilst entirely valid and one which is commonly used

within studies of glacier melt, is only one of a possible set of method for optimising model results. Given the stake measurements, the model could have been optimised with the simple R2 value, the Nash-Sutcliffe measure of model efficiency, or the mean absolute error, for example. Alternatively, the stake measurements could have been used to calculate summer mass balance gradients for the given glaciers in the study years and the models optimised to fit this gradient. All these would have produced a different set of model results, with different though equally valid sets of parameter values, and could have produced different estimates of model transferability. This limitation could perhaps be acknowledged a little more within the paper; the simplicity and limited nature of the transferability experiments conducted in the study is already partially acknowledged within the paper (e.g. at the end of the introduction), but I think that acknowledging the limitations of single optimisation measures, whilst accepting that they are often necessary in practice, would be also useful.

In spite of this discussion, I am not suggesting that the authors be requested to revise the paper by conducting a 'full' sensitivity analysis or evaluate the range of model parameters produced by different optimisation strategies; however, I do think there is another set of model experiments which would add to the paper, and which would further the aim of assessing model transferability. These experiments should consist of driving the four models with optimised parameters derived from the complete mass balance dataset (i.e. both glaciers and both years), and then assessing the model skill for each individual glacier and each individual year. In some ways, this 'reverses' the experiments in the paper as it stands, which assess the performance of 'local' parameters (i.e. derived from a single glacier, in a single year) in 'regional' model experiments (i.e. applied for a different glacier, in a different year). The runs I suggest would instead allow the assessment of the performance of 'regional' parameters on 'local' model experiments. Whilst there may be compromises in model performance when compared with the locally-optimised runs (the 'control' experiments in the paper) for the individual glaciers for individual years, it would be instructive to see which class of model can best simulate local mass balance values with regionally-derived optimisations for both

glaciers in both years. The results reported in the paper suggest that the DEBM should perform best given it is the 'most' transerable, but I think it would add to the paper if this could be tested with this extra parameter set, given the relatively small set of model runs tested in the paper. This would add to the aim of the paper to evaluate which type of model would be best suited to investigate regional trends in mass balance, and/or which longer term hindcasting or forecasting of mass balance.

Specific comments

I have very few specific comments or corrections to make, as the paper is commendably free of grammatical errors or uncertainties. However, although in general I think figures are easier to interpret than tables, in this case I wonder if Figures 3 and 5 could be usefully replaced (or at least supplemented) with a table of the actual RMS error (or As for Figure 5) values. The differences between some model runs (e.g. South Glacier 2008 CTIM vs ETIM, North Glacier 2008 DEBM control, temporal and spatio-temporal runs) are very slight, and not easy to discern from the Figures; given the relatively small set of numbers involved, I think simple tables of the results would be more effective. My other specific comment concerns the title of the paper; overall, I feel that the paper really sets out to compare model parameter transferability rather than the transferability of the models themselves, as the paper does not comment much on the actual model results themselves. This is fine given the aims of the paper, but I think it should be reflected in the title.

Overall

To conclude, I would be happy to see this paper published with the additions and corrections suggested. I do not think that these require the paper to be re-reviewed, but if the editors felt this was necessary I would be happy to review the paper again.

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