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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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I start my review by quoting the short abstract of Van der Veen (1999):

"Numerical models of the cryosphere cannot be verified and their truthfulness in providing an accurate description of actual physical processes cannot be proven conclusively. At best, models can be confirmed by comparing predictions with independent data that were not used to calibrate model parameters. The more such confirmations are achieved, the greater the confidence that can be placed in the model as a representation of Nature. Most prognostic cryospheric models have not be adequately calibrated and confirmed, and skepticism towards their predictions is therefore warranted."

We do not have to completely agree with the above quote to see that the present discussion paper by MacDougall et al. may be a very important contribution to this vexed question of evaluating cryospheric model performance.

The melt model is "calibrated" for one situation, e.g. one melt season on one glacier, by calculating the parameter value(s) needed to make the model predictions "fit" the measured data for the situation in questions. For example, the degree-day factor in a simple degree-day model could be calculated as a regression coefficient if one has a series of simultaneous data for melt and temperature within the period in question. If the regression coefficient is accompanied by a relatively high correlation coefficient between observed melt-temperature data, we might be tempted to say that we have "verified" the model for the dataset that we have. However, we still cannot simply assume that our value of degree-day factor is valid for another situation, e.g. for another melt season on another glacier, and we should follow MacDougall et al. by applying the degree-day factor found for one situation to another situation to "confirm" the model in the terms of Van der Veen (1999). Such confirmation will obviously be limited by the available data to only a few melt seasons on a few glaciers but should help to build general confidence in our model. MacDougall et al. deserve full credit for reinvigorating this important problem but I do have some problems with the present paper, which I outline below.

Although interesting and important, this discussion paper claims far too much in its abstract. Applying the transfer principle to only two melt seasons on two nearby glaciers is rather minimalistic and insufficient to make claims about one model being more transferable than another model. On the second page of the paper, the assessment is correctly described as "an optimistic one". The paper's abstract and conclusions should include this important reservation.

I understand the transfer concept of MacDougall et al. for a simple melt-climate model like the degree-day model. For example, Braithwaite (1995) calculated degree-day factors for large data samples at two locations in Greenland. His samples were so

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big, e.g. daily melt and temperature data for 415 days at Nordbogletscher and 512 days at Qamanârssûp sermia, that sampling errors were relatively small. However, following the approach of MacDougall et al., Braithwaite (1995) should have compared models between smaller samples, e.g. for six or seven melt seasons at each of the two glaciers. I will probably attempt this as soon as I have finished this review. As MacDougall et al. correctly note, Hock (2003) gives a summary of degree-day factors for glaciers in widely ranging conditions and it may be possible to test the transferability concept for some of these data.

The above paragraph refers to the degree-day model, i.e. a model with one parameter and one input variable. I find the transferability concept more difficult to understand when applied to the other three models considered by MacDougall et al., i.e. models with more than one parameter and more than one input variable. I doubt if many people have tried to transfer a complete energy balance model from one situation to another. For example, the model of Arnold et al (1996) seems to involve transfer of longwave radiation and surface roughness parameters from Braithwaite and Olesen (1990) but these are combined with a much-improved approach to shortwave radiation.

The conclusion that the full energy balance model is the most transferable of the four models almost sounds inevitable as it must be easier to transfer a model with many parameters and many input variables. However, the reason why some workers do not use energy balance models is that there is little evidence that they perform better than a simple temperature-index for the modelling of day-to-day variations in melt at some location. Therefore, in the terms of Van der Veen (1999), I suggest that we need to discuss the "calibration" of our models before we discuss their "comparison". Perhaps we need a single statistic to combine the two.

The paper is not always easy to understand as it refers the reader to other papers, included in the reference list, for "further details". A recent search on the ISI Web of Science (12 January) failed to detect either MacDougall (2010) or MacDougall and Flowers (2010). No doubt, these papers are on their way and will appear sometime but,

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if they revise this discussion paper, MacDougall et al. should add further information to make the paper more self-contained.

Although MacDougall et al. apply the "enhance temperature-index model (ETIM) as it is described in the literature, I think equation (2) is physically incorrect as the radiation melt factor is effectively multiplied by the air temperature. The simplified energy-balance model (SEBM) in equation (3) is physically more correct in splitting the energy balance into separate components for shortwave radiation and temperature. As a last point, MacDougall et al. quote the wide range of estimates of 21st century sea-level rise as a motivation for re-examining glacier melt models. However, the large differences between the estimates they quote are probably more down to different estimates of glacier volumes and areas in different regions of the world.

MacDougall et al. certainly raise model transferability as an important issue and I value MacDougall et al. as an item of discussion. However, I wonder if they should revise this paper at all as I suspect that they will do further work that may be more reliable, for example for more seasons on more glaciers. Perhaps they should not embarrass their future selves with a premature conclusion!

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