The Cryosphere Discuss., 7, C18–C21, 2013
www.the-cryosphere-discuss.net/7/C18/2013/

The Cryosphere Discussions

Interactive comment on "High-resolution interactive modelling of the mountain glacier–atmosphere interface: an application over the Karakoram" *by* E. Collier et al.

Anonymous Referee #2

Received and published: 31 January 2013

General Comments

The paper describes a coupled high-resolution mesoscale atmospheric and glacier mass balance modelling system and its application to the Karakoram – although the analysis is focussed on a single glacier, Baltoro. The method builds on existing work, namely, the widely-used Weather Research and Forecasting (WRF) mesoscale atmospheric model and a physically-based glacier mass balance model developed by the authors over recent years, which has principally been applied to glaciers on Kilimanjaro and in Tibet. The key advance and novelty of the approach is that the two models are coupled interactively. Not only does meteorology force the glacier model in the usual

way, but the glacier feeds back into WRF through surface heat and moisture fluxes, and surface and subsurface temperatures, snow cover, surface roughness, albedo and surface specific humidity which are updated at each time step. This marks an improvement over previous approaches where the surface conditions are prescribed in a land surface model which may be unrepresentative of glaciers. Indeed, the paper is a significant step forward in wider attempts to model glacier-climate interactions at a basin or regional scale in a physically realistic manner. The paper is well written and presented (aside from a few minor points below) with little superfluous material, makes an important contribution and will be of wide interest. My only concern regards the validation data which are recorded on supraglacial debris, a surface type with distinctive processes and conditions not specified in the model, for which I make some more detailed comments on below. However, I consider these, and my other comments, to be of a minor nature which should detract from what is an excellent paper.

Specific Comments

1. Validation stake data.

Make it clear in the abstract and introduction that detailed evaluation of model performance using in situ data is conducted only for Baltoro glacier. Currently, one gets the impression from the paper that validation is performed at a regional scale, so the information in Section 2.4 and Tables and 4 is a bit unexpected.

The fact that ablation data are all recorded on debris covered ice, while the mass balance model ignores energy-transfer processes specific to debris covered ice, needs more consideration in the interpretation of these results. Given the 2 cm critical debris thickness (p.123, l15) the model should overestimate melt at all stakes, except L2 and L3 where the model should closely match the measured rate. This is in fact what your results show, so say this!

P116, l12-15. There are too few data and too much scatter in Fig. 4d to make this interpretation. In any case, the logic is confused. If the model is working well, it should

give a consistent melt overestimation, not a decreasing one over time, because it is ignoring the insulating effect of debris on these stakes. On the other hand, if you are saying the reduction in melt overestimation after 6 days is due to a snowfall event, say exactly when this event happened and which stakes are (most) affected. Finally, the axes on this graph should be reversed because you are saying melt overestimation is a function of length of observation period, not the other way around.

P115, l14-15, more specifically, the model underestimates the amplitude of the daily temperature cycle, but this would be expected given the surface type (debris) is different from that of the model (ice).

2. Wider significance of the results.

p.122, l8-9 and elsewhere. The difference in mass balance between the offline and interactive models is quite modest for this study. However, are there situations where interactive coupling might have larger impact on simulated glacier mass balance? Conversely, are their instances where interactive coupling can be ignored?

Also for consideration is that many glaciers have debris-covers, particularly in the Karakoram, with variable thicknesses, thermal properties and critical thicknesses. Is this the next challenge for the interactive modelling approach?

Technical Corrections

P104, l22, and throughout the paper, data should be referred to in the plural, e.g. 'these data', not 'this data'.

P105, l28, 'Traditional approaches. . .'

P112, 117, should that be '. . .from longitudinal transects along. . .'?

P116, l2-3, was this all solid precipitation? Please clarify.

P122, l4, '. . .reduces modelled ablation. . .'.

P123,l9-10, '. . .represents an important uncertainty. . .'.

P124, l18, replace 'paleo' with 'past'.

Figure 2, (b) and (c) are the wrong way around in the figure caption. Are these data for domain D3 only? Please state which domain they apply to.

Figure 4 (d), the axes need to be reversed, as by convention the dependent variable is plotted on the y (vertical) axis.

Interactive comment on The Cryosphere Discuss., 7, 103, 2013.