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The Cryosphere Discussions

Interactive comment on "Glacio-hydrological melt and runoff modelling: a limits of acceptability framework for model selection" *by* Jonathan D. Mackay et al.

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The study presents an innovative approach to evaluate the performance of glacio-hydrological models. Basing on data from Virkisá river catchment in Iceland, the study demonstrates a framework to constrain the acceptability of results from different models against observational data. Different setups combining relatively simple glacier melt and discharge models are tested for their capability of reproducing measurement data results of melt, snow cover, and river runoff. Such a comprehensive framework for model evaluation is certainly an important contribution. I got the impression that the setup presented manuscript was designed very thoughtfully. The manuscript is also

very well written, with well-done figures illustrating many key aspects.

Nevertheless, I see three major weaknesses of the manuscript in its current form which need to be addressed before publication: (i) The weak precipitation data set, (ii) patchy result data, and (ii) that no scripts or technical details are presented.

(i) The AWS data used to force the models does not contain information on snowfall, and even rainfall with consecutive three above-freezing days was used. The manuscript provides few information of how much data is actually lost though this procedure (s. also ii), but I guess it is quite a lot in Iceland. These measures will certainly have substantial effects on glaciological-hydrological outputs and may account to some extent for the models inability to calculate winter melt. However, the authors use the data very thoughtful and have made quite some effort to ensure validity. Ideally, the framework should additionally be tested in a setting with high-quality AWS/snowfall data to constrain specific effects originating from input data characteristics. Arguably, this is beyond the scope of this manuscript; Nevertheless, I find it very important to discuss in more detail to what extent the strengths and weaknesses identified for individual model setups might also be affected by shortcomings in input/observation data.

(ii) Result data tends to be patchy, making it hard for the reader to get the broader picture. For instance, Fig. 13 only shows modeling results for May, Fig. 14 only the year 2013, Fig. 16 only selected months, etc. I totally understand why these selections were made. However, I strongly recommend to add full input and output datasets as supplements to show that the examples are no cherry picking, and to allow readers to see the greater picture.

(iii) Intuitively, I'd say the term 'framework' suggests that the aim is broad application, and I think the conclusions chapter encourages application and further testing of the LOA framework. However, the underlying structures and algorithms remain vague. If the aim is to let other scientists follow this approach, I suggest providing more detail on how to set up a LOA framework. As an open science and open source enthusiast, I

personally would like an open git repository where interested persons could access the code and maybe help developing it further; This would certainly boost the resonance to this good work.

In combination, (ii) and (iii) render it impossible to reproduce your analysis in the current state of the manuscript.

Ultimately, I find there is too much interpretation in the results chapter and suggest a clearer distinction between results and discussion.

Technical comments

Inconsistent order of citations P4L19 - Introduce, omit, or at least quote the abbreviation SEHR-ECHO P5L10 - Provide unit for slope (radians, I guess). Personally, I find slope angles in degree more convenient. Fig. 9: Why are boxes dark gray where the score is 0? Fig. 13: What is '1e3 m^3'?

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