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Interactive comment on "Robust uncertainty assessment of the spatio-temporal transferability of glacier mass and energy balance models" *by* Tobias Zolles et al.

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1. Review

The paper proposed a two-step procedure to assess the uncertainties in a widely used distributed glacier mass and energy balance models. First the global sensitivity analysis identifies the sensitivity of the parameters. The reduction of parameters through sensitivity test and the optimization of model by allowing a wider range in parameter variation yields promising results. Then the best setting parameters are determined by a Monte Carlo multi-objective optimization. The transferability is investigated by applying the optimal pareto solutions to other summer seasons and another glacier.

The results show that the average parameter uncertainty over the whole glacier is 30% and reaches 50% at point scale, which underlines the significance of using the spatio-temporal cross-validation for model parameterizations. Although transferability tests of the model shows that the performance is worse when applied to the other studied glacier, the magnitude of uncertainty is the same order as the temporal transfer. In conclusion, the authors suggest that the optimized model can be applied to other glacier under similar climatic conditions. The introduction is a very detailed review of the previous studies studying the transferability of different models. I find it really enjoyable to read. And the following sections are well connected with the aim of the article. The method is well explained and is developed from the referenced articles in the last paragraph of introduction. I think the experiment is well designed and written.

The applications of glacier mass and energy balance models in regional upscaling and projections are limited by the high variability of climatic parameters. The authors did a thorough investigation of the sensitivity and uncertainty of models. The proposed two-step process has proved its efficiency in reducing parameter space and identifying the model uncertainties. The identified non-sensitive parameters could be fixed to constant literature values. Then the optimization is performed based on the remaining sensitive parameters. The method is well designed, and the results support the assumption that the single performance measure is not appropriate since parameters are varying in both time and space. The results suggest that the net short-wave radiation and turbulent fluxes are the most uncertain energy balance components, reasserting that the snow and ice albedo representation are the most critical factors. This conclusion explains the difficulty in optimizing the model performance simultaneously in both the accumulation and the ablation zone. Altogether, I regard the paper as a valuable contribution Still, I have a few comments which I hope the authors can address so that I could learn more about their research.

2. General comments

The performance of the model is not that encouraging when applying the 17 optimal solutions based on the Pareto set for HEF 13 to other summer seasons and another glacier. The conclusion suggests that the large spatial and temporal transfer uncertainties are acceptable when applying to other glaciers with similar climatic settings. How does the result compare to previous research (e.g. the referenced an enhanced temperature-index model by Carenzo et al. (2009) which shows pretty good agreement of transferability in space and time)? The uncertainties of transferability are quantified only through the Euclidean distance towards the utopian point, which is quite clear and straightforward. However, it would have been better if R2 values were also reported, which is helpful for facilitating comparison to earlier transferability studies.

The article has a clear structure with a very thorough description of the parametrization. Some descriptions however need some clarification as specified below in specific comments. The length of the abstract could be shortened by reducing some of the detailed descriptions of the methods.

3. Specific Comments

1) P13, L5:Figure 4 could be improved. It is written that a minor change of a model bias (25Âůkgm-2) could lead to an improvement in MAD by 200-300. However, this statement excludes many outliers which should not be ignored. A log-transform might be able to help to improve

2) P13, L6: “the MADs plane is more curved” in Fig 4(c), (similar statement for line 2 on the same page) seems to be just a vague description. It might help to add a reference line here to support this sentence.

3) P20, L32: A minor typo is spotted where “TFor” is assumed to be “For”.

4) Figure 5: the y axis should be “Euclidean” not “euclidian”.

5) Figure 6: Maybe it would be good to compare the optimized best setting with the “classical best guess solution” in fig. 6? It’s good to have a comparison between the optimal results and the classical best settings. Then the quantified uncertainties

can be compared with previous research? For instance, the transferability of the enhanced temperature-index model (Carenzo et al., 2009), which is reported to have a good transferability (R2 = 0.78 under the over cast conditions and R2 = 0.925 on average under normal conditions). Another study of a distributed energy balance model (MacDougall and Flowers, 2011) concluded that an error of ∼30% is expected without calibration during transferability test.

References

Carenzo, M., Pellicciotti, F., Rimkus, S., Burlando, P., 2009. Assessing the transferability and robustness of an enhanced temperature-index glacier-melt model. Journal of Glaciology 55, 258–274. https://doi.org/10.3189/002214309788608804

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