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The Cryosphere Discussions

Interactive comment on "Modelling regional glacier length changes over the last millennium using the Open Global Glacier Model" *by* David Parkes and Hugues Goosse

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Parkes and Goosse present millennial-scale simulations of glaciers to investigate the ability of climate and glacier models to represent long-term glacier behavior including advancing and stable conditions, to evaluate model performance compared to observations, and to study the relative importance of temperature and precipitation anomalies for explaining glacier variability on such time scales.

The study is clearly of interest for the readership of The Cryosphere and definitely has the potential to progress understanding of glacier-climate relations on long time scales, as well as the utility of models to investigate these relations. However, the

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Discussion paper

full potential of the study is not realized, and with an extension of the analysis (in particular, adding more quantitative measures of model performance) much more could be gained. The objectives are stated, but the abstract and introduction need to be refocused (and potentially adjusted significantly, depending on the authors' choice if and how to implement the ideas I sketch below). Finally, a bit more background is needed on the use of the glacier model.

All in all, I think the paper needs major revisions (detailed below) before it can potentially be accepted for publication in The Cryosphere.

General/major comments:

- I find the focus of the paper a bit unclear. From the abstract, I get two objectives, which are obviously linked, but not spelled out very clearly: (i) test whether calibration of the model during the retreat phase is good enough and leads to adequate results also in times of advance or stability, and (ii) identify whether precipitation or temperature anomalies are more responsible for glacier length changes at multi-centennial time scales. I think it might help to use (ii) as the main objective, which would require (i) as an intermediate step. This also relates to L38-39: "it is important that these models are examined over time periods where more stable glacier geometries were expected". It could be argued that if a model is not foreseen to be applied in conditions where glaciers are stable or advance, the model does not need to be able to show such behavior (e.g., to my knowledge, the representation of ice geometry change of the model of Huss and Hock, 2015, does not provide for advancing glaciers). It would be good to give some explicit reasons why it is important. In the introduction, the attribution of glacier length change to either precipitation or temperature anomalies seems like an afterthought. I think it would help the paper a lot to restructure with one clear objective (which I think could be this attribution – but the authors may disagree).

- It probably would be helpful if a bit more is said on the setup of climate forcing in OGGM: the authors are referring to "level 3" preprocessing of OGGM, but don't give any details on how OGGM treats climate model output before application to the mass balance model (this concerns bias correction/anomaly coupling, estimation of solid precipitation, any corrections etc). As it stands now, readers might by surprised by the apparent ability of CMIP5-type models to represent mountain climate conditions accurately enough, which is only half the truth. (see also L166, “as provided”, which is not true)

- Regional averages are presented and discussed, and this comes at the relatively high cost of having to find a way how to calculate regional length changes (see discussion around L120-125). It is my impression that the calculation of regional averages is a purely graphical requirement, needed in order to avoid having to inspect 339 individual glacier time series. This points to a more significant problem, which is that the assessment of the model results depends too much on this graphic representation. I would recommend the authors to expand the analysis of results to a more quantitative evaluation, such that the assessment of regional differences depends less on visual inspection of graphs that necessarily are associated with shortcomings (such as the spikes resulting from changes in the observational ensemble). At least, it might be worth to extract data from the modeled glaciers at the same time as observations exist (adding a third version to Fig. 1 and 2), so that the modeled regional average would have the same spikes as observations if it was perfect. But I think this would not be the optimal solution. The analysis based on linear trends aims in the right direction, but doesn’t really relate to the observations, so it is not helping with this issue.

- The discussion of Fig. 4-6 would form a nice basis to infer something about the adequacy of the climate models if the comparison of the model results to observations was more quantitative. E.g., it would be possible to quantify how much the glacier model performance is reduced (presumably) if either temperature or precipitation information is withheld, giving further insight into their relevance (and the climate model’s ability to represent precipitation and temperature evolution – after all, it is possible that a model’s performance increases when one of the two variables is held constant). I also think that the discussion of variances misses the opportunity to say more about physical, climate related causes of regional differences. This is also true for the discussion of relative variances > 1, which basically imply that there is dependency between temperature and precipitation. This is discussed on a technical level (it might be added how the way the solid fraction of precipitation is calculated in OGGM adds to this dependency), but there are also climatological reasons that should be considered here. This should include a discussion of the relevant literature on precipitation vs. temperature influence of glaciers (which is currently almost completely missing). Also, a discussion of Marzeion et al. (2014, DOI: 10.5194/tc-8-59-2014), which includes similar experiments, may be helpful.

Specific/minor comments/suggestions

- L1-2: “observational record for glacier changes falls within the post-industrial period, associated with global glacier retreat”: I would suggest to delete “post-”, and to replace “associated” by “coinciding”

- L3 (and throughout manuscript): delete “post-“ from post-industrial (unless this is a standard term – but to me it sounds like “after the industrial age”, which is not what you mean)

- L13-16: suggest break in two (or more) sentences

- L23: rephrase to avoid nested parentheses

- L26: avoid nested parentheses

- L29: avoid citation of webpage (OGGM e.V.) if there is a proper publication (Maussion et al., 2019)

- L32: OGGM also include precipitation in the calibration

- L35: please spell out some of the additional challenges

- L65: suggest to rephrase to “variability over time output of OGGM driven by”

- L78: fix double citation

- L80-81: avoid nested parentheses

- L101: fix citation

- L115-117: “smaller in relative magnitude” yes, but for many applications, the relative magnitude of changes is not that important, but the absolute mass change. So this is maybe not such a big problem.

- L119-120: it is unclear to me here why a mean regional glacier length estimate is needed? Up to here it seems all comparisons to observations are done on a per-glacier-basis (which seems like a better idea to me).

- L 151: from my own experience I appreciate the difficulty of doing the matching, but 38 “not found” glaciers strikes me as a high number. Typically, glaciers with length observations tend to be more “famous” than the average glacier. Might it be worth checking in other data bases (e.g., GLIMS) based on glacier name?

- L 153: please provide a table linking RGI-ID to Leclerq’s database as a supplement (as a service for similar, future studies)

- Fig. 1 & 2: I’m surprised that also the model results are very spiky in some regions. Why is that so? It would also be helpful to use the same vertical axis range in all subplots (even if it cuts off parts of some graphs), since the normalization make the regional comparison easier (and the different axis limits make it harder).

- L182-186/Fig 1&2: I don’t really understand what makes regions 14, 15 and 18 stand out from the other regions? Again, I think a more quantitative comparison of the model ensemble to the observation ensemble would be very helpful.

- L189: not sure what is meant by “stratified”, so I also don’t understand the following argument

- L199 and following: would it make sense to do this analysis first for each glacier in the region, and then build the regional mean? This could result in an indication how robust the estimation of the “inflection” year is, and also give some more information on intra-regional variability of the glaciers’ behavior.

- L213-125: I find this argument a bit weak, given that the “inflection” year is probably very sensitive to short-term interannual variability in the climate time series

- Fig. 5: was the non-zero melting threshold temperature of OGGM taken into account when calculating the temperature time series shown here (also, in L226, it says “degree-days” – is it really days, or months)?

- L262-263: it is not only OGGM, but (probably at least as much) the GCMs that are responsible for the level of agreement.

- L270: add citation to Goosse et al. (2018) here

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