The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-35-RC2, 2018

Interactive comment on "Global glacier volume projections under high-end climate change scenarios" *by* Sarah Shannon et al.

Anonymous Referee #2

Received and published: 8 June 2018

1 General comments

The proposed manuscript presents new estimates of future sea-level rise obtained with a new model of global glacier evolution. The model is quite original because it is integrated in a land surface model. Also, the mass-balance is computed from the surface energy balance, which could lead to different and more contrasted results than classical "temperature-index" mass-balance models. For these reasons, it is a useful addition to the literature.

That said, I think that the paper needs serious revisions before being considered for publication. I have some major concerns listed below. My recommendation would be to:

- use additional datasets for model validation
- put less focus on the validation with point mass-balance data
- spend more time on the energy balance instead

1.1 Model set-up

I found it difficult to understand some aspects of the model set-up, and I believe the text could be meliorated by reorganizing some of its sections. In particular:

- the decription of the snowpack/ice meld model is scattered around Section 2/ Intro, and page 7.
- the description of how and where glaciers lose mass is unclear to me (and to the other reviewer as well)

The manuscript would also benefit from a clear discussion and listing of the particularites (strengths/weaknesses) of this model in comparison to previous modelling attempts. This could be a way to highlight the strengths of your model (see "Energy balance" below).

1.2 Calibration/validation

This was already mentioned by reviewer 1: some model results are poorly explained or not discussed at all.

Validating and calibrating against in-situ elevation dependant MB data is really hard. Point mass-balance observations reflect a number of local and glacier specific factors, and there is a risk that these local factors affect parameter sets chosen for entire RGI regions (this is particularly true for regions with few observations). I am not asking to change you procedure as this will likely represent too much work, but I'm allowing myself to provide a couple of suggestions:

- I would personnally recommend to use one global set of parameters instead of the regional ones, unless there is a compelling reason not to do so (a good candidate would be a physical explanation of the regional parameter sets). The resulting parameter sets would be more robust and would allow statistical scrutinizing using cross-validation (or more advanced) methods. This is even more relevant for physically based approaches like yours.
- Having some kind of independant validation would consideralby strengthen the readers' confidence in your results. Albeit not without their own problems, regional geodetic mass-balance estimates could be useful to at least get a quantitative estimate of the model performance at the regional scale.

1.3 Energy balance

The real strength of the model is its use of an energy balance model instead of a temperature index model as the majority of the other global models. Whether or not this increase in complexity is actually leading to better results remains (and will remain) a controversial topic, but this study should make use of this novel approach. In particular, I would find it very interesting to see plots of the energy balance components as a function of altitude, and how these energy balance components change in the future. This is interesting because energy balance models are likely to be less sensitive to temperature change and incorporate other processes instead.

I would also welcome new analyses of not only the total volume change, but the volume changes per elevation band.

1.4 Code availability

Please add a statement about where and how people can access your code and that of JULES.

2 Specific comments

P3 L15 why is this limitation about the partial coverage necessary? In view of the objective of developping a fully coupled model, it would be good to overcome this limitation one day.

P3 L23 0.5° and 46 elevation bands: What motivated the choice of these resolutions? Can this be changed at whish?

P4 L5 Snowpack: do I get this right that there is no distinction between ice and snow in the snowpack model? What are typical values for ice density in the model? How much time does it need to transform snow to ice?

P4 L10 I must admit that I dislike the current approach to temperature downscaling, which in my opinion is an unhealthy mix of thermodynamics and tuning. I'm not asking to change it, but the fact that the lapse-rate is tuned in non-saturated conditions (the rate changes quite a lot according to table 2) but not in saturated conditions is likely to create odd non-linearities in the model's response to certain forcings.

P5 L1 *"we only tune the dry adiabatic lapse-rate"*. Here and throughout the rest of the manuscript: do not use the term "dry adiabatic lapse-rate". The dry adiabatic lapse-rate **is** the dry adiabatic lapse-rate and is 9.8K per 1000m. What you are tuning though is the near-surface temperature lapse-rate, which might vary according to surface conditions and moisture content.

P5 L22 wet-bulb temperature is a much better indicator for solid precipitation than regular dry-bulb temperature. This could mitigate parts of the dramatic changes in snowfall projected by your scenarios.

P5 L27 here and at some other places in the manuscript, the missing katabatic flow is given a high prominence in the list of missing processes to be addressed. This might be the case (also not the most prominent on my list), but the proposed solution (scaling the synoptic wind field) does not sound really physical to me. The katabatic flow is notoriously decoupled from the synoptic conditions and is likely to be strongests when the synoptic flow is weak. What the scaling of the modelled wind achieves, though, is an increase of the turbulent fluxes: I would be very interested in seing more discussion about why this is necessary (see major point 1.3 above).

P7 L1-2 If I get this right, the glacier tiles are able to lose mass per elevation band, right? The information is scattered in the manuscript (P7 L10, P7 L24 . . .) and should be clarified much earlier to avoid confusion (see also the comment from reviewer 1 who seems to have understood something different than me).

P7 L10 I don't understand this part. Can you be more specific about how glaciers grow/shrink and lose/gain mass in the model?

P7 L25 What happens at the end of the initialisation? Setting 500 m of ice everywhere is maybe ok for a spin-up, but what happens next, or at the start of the 2011-2100 simulation?

P9 -L27 I don't understand the statement "with the notable exception of the low latitude and Central European regions where melting is over estimated". According to Table 3 and the BIAS measure, melt is over-estimated in 9 regions with Svalbard, Southern Andes and New Zealand striking out with more than 1 m negative bias.

Central Europe even has a positive bias. A quick look through the table indicates a general negative bias.

P10 L3 and Figure 4 : the explanation about the Maladeta is irrelevant. Figure 4 shows that there are other pink dots around the Maladeta starts, and there is no need for a case by case explanation here.

Section 3.2 This does not represent an independant validation because the same data was used for calibration also. What is striking in Table 4 is that all regions now have a significant negative bias both in winter and summer. How can this be explained?

P10 L21 I would like to see more explainations about the “downscaling” procedure. If only SST and Sea-ice are used, this sounds a lot more like a full atmosphere GCM simulation to me than a “downscaling” of a GCM product. In particular, what happens to the land-surface components in HadGEM3? What is actually left from the original GCM signal after “downscaling”?

P11 L2 remove “which is suitable for capturing precipitation variability over complex topography”

Section 4.3 Parametric uncertainty analysis is one aspect of parameter uncertainty. A further uncertainty would be revealed by doing data-denial experiments (cross-validation) and assessing the sensitivity of your calibration procedure to those.

Section 4.4 Comparison with other studies I would welcome future studies based on this model to use the same forcing data and same conventions as other global glacier models where possible in order to facilitate model intercomparisons.

P13 L25 My understanding is that your study is using the global volume estimates provided by Matthias Huss.

Supplementary material I suggest to invert the color scale: red seems more intuitive for mass loss.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-35, 2018.