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

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The model presented here is a very timely and relevant contribution to the growing group of global glacier models since it is, to my knowledge, the only global model that has been developed in the framework of a land surface model. It thus follows a concept that is very different from most other glacier models, adding important diversity and opening a potential path to coupled modeling of glaciers (i.e., allowing interactions between glaciers and ocean/atmosphere, e.g. through the changed freshwater balance), as well as a more integrated perspective on hydrologic impacts of glacier mass loss.

In principle, I therefore think the authors present a very valuable contribution. However,

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there are numerous results (particularly concerning calibration and validation) that require much more in-depth analysis and discussion than presented in the manuscript. This concerns particularly (i) the substantial (and very consistently negative) bias; (ii) modelled negative winter and positive summer mass balances that are currently uncommented; (iii) the lack of a representation of the terminus altitude-mass balance feedback which might also contribute to stronger mass loss projections, and (iv) the Nash-Sutcliffe coefficients that are included in the tables, are mostly negative in the validation, but not at all explained or discussed in the text.

If these issues – outlined below in more detail – are appropriately addressed, I would be able to recommend the manuscript for publication.

Specific comments:

Abstract and Introduction: I understand the desire to express the considered CMIP5 projections in terms of temperature above pre-industrial, which has become a quite common measure with the formulation of the goals in the Paris Agreement. However, most readers will be familiar with the RCP scenarios. I was wondering until page 10 whether a "mixed" scenario was used, e.g. selecting CMIP runs based on warming. A quick statement in the abstract and the introduction that RCP8.5 is used will make things a lot clearer.

P2 L5: In some regions this is true, but since glacier water release is not causally related to demand for water, this sentence should be reformulated.

P2 L7: Most of the authors are native speakers, which makes me doubt myself – but I learned that when used as a compound adjective, sea level should be hyphenated (as in "sea-level rise")?

P3 L14ff: It would be nice if the authors could comment on how strongly this limitation affects the usability of JULES. I.e., is it realistic that the new glacier scheme would be used in a default setup of JULES, or would the limitations inflicted on the other surface

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classes be too strong?

P4 L5f: If I understand correctly, this implies that the negative feedback between terminus elevation and mass balance is missing, and that the only way for a melting glacier to reach equilibrium with climate is by melting completely (similar to Slangen & van de Wal, 2011, https://doi.org/10.5194/tc-5-673-2011). This is a major limitation that should be discussed in greater depth. E.g., Marzeion et al. (2014, https://doi.org/10.5194/tc-8-59-2014) find that depending on scenario and accumulated mass loss, this may contribute a few tens of mm SLE (their Figs. 9 to 11). The differences in Tab. S3 could be plausibly explained by this alone. I also think that the discussion of the lack of a parameterization of ice dynamics (end of Sec. 4.4) is flawed with respect to this feedback.

P4 L19ff: Units of the constants are partly missing or wrong.

P4 L28: Goff-Gratch (typo), and Landolt-Bordstein 1987 is incomplete in the references, and probably should be Landolt-Börnstein.

P5 L25: It would be better to say that the energy balance calculated in JULES includes the sensible heat flux, since the snow melt is not a direct (or separate) consequence of the sensible heat flux alone.

Eq. 13: I think this equation is a bit problematic if justified by katabatic winds, since the katabatic winds should not be expected to be proportionate to the large-scale wind field.

P6 L25: “at the beginning” (typo)

P9 L 11: In Marzeion et al. (2012), it is 3 %/100 m.

Tab. 2: Since the Nash-Sutcliffe efficiency coefficient is included, it should be discussed in the text. It would be particularly good to address the reasons for the numerous negative values – are they caused by bias, too great/small variance, etc.?

Tab. 3: Please add global mean values.

P10 L1f: The tropical glaciers are really small; there are probably numerous more likely explanations for a warm bias than glaciers lacking in the model.

Figs. 3 and 4, and discussion around them (also P10 L14f): I'm not convinced the Pyrenean glacier is to blame for the low correlation. How much does the correlation change if you exclude it? I'm also wondering why the point cloud for Central Europe in Fig. 3 looks different from the one in Fig. 4?

Tab. 4: Please add global mean values.

Tab. 4: Again, it is necessary to discuss the negative NS-values. There is only one positive value in the table, which indicates that only for summer mass balance in Scandinavia, the model has better predictive skill than taking the mean of the observations. Also, given that all the biases are negative (with the exception of one that is close to zero), the implications for the projections need to be addressed. E.g., if the bias was compensated for in the projections: how would that change the results? Could the differences to previously published projections be explained by the global mean bias?

Fig. 5: the RMSE mentioned in the caption is missing in the panels.

Fig. 6: It is surprising that the model produces many substantially negative winter mass balances (and not quite as many positive summer mass balances). This behavior should be looked into and discussed in the text.

Caption of Fig. 6: "number of glaciers" – isn't that the number of grid boxes?

P10 L23: "sensitivities" (typo)

P11 L12: "Arctic" (typo)

Fig S7: It is hard to see anything here; perhaps just leave out the East African and Indonesian glaciers (which is sad for sentimental reasons, but I think they are mostly irrelevant for sea level and water availability).

P11 L16: Figs. S1 to S7.

Sec. 4.3: It is good to see that the model results appear to be robust; on the other hand, this may indicate the negative bias may be hard to overcome. In the calibration, minimizing the RMSE was used for identifying the best parameter set(s). Another way of looking into "parametric" uncertainty (in a wider sense) would be to minimize the bias, or to maximize the correlation or the NS coefficient. These experiments might give valuable insights into the causes of the sometimes problematic model performance, which needs to be better explained.

P13 L2: "equivalent" (typo)

P14 L27: Reduction in glacier mass, not necessarily in mass balance.

P15 L18: "periphery" (typo)

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