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Discussions

Interactive comment on “21st century fate of the Mocho-Choshuenco ice cap in southern Chile” *by* Matthias Scheiter et al.

Anonymous Referee #2

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Scheiter and co-authors present a model for the mass balance and ice flow of the the Mocho-Choshuenco ice cap, and apply it to produce projections of mass change during the 21st century. Projections of glacier mass change are generally relevant, as glaciers contribute strongly to sea-level rise, and to seasonal water availability in some regions of the world. In the paper, it remains a bit unclear what is the motivation to study this specific glacier, given that a number of models (not all of them of much lower complexity) have projected glacier mass change globally. Either the specific interest of this particular glacier, or the specific advantages of the methods used here over those used in the other projections of the glacier need to be clearly stated and evaluated by the authors to demonstrate the interest of the study to a wider audience.

Additionally, there are two major concerns that call the results into question (both of

them detailed below): (i) the assumption of steady-state used by the authors for the spin-up of the glacier model, which is in stark contrast to the observed mass change rates; (ii) the complete lack of evaluation of the model’s mass balance results. Because of the combination of these two issues, I would be very surprised if the results are not flawed by a very substantial positive mass balance bias, such that the mass loss projections are substantially too low.

Addressing these issues would be possible (see below for a few specific suggestions), but it would require not only new experiments, but also a completely new rationale of model setup and evaluation. This exceeds the scale of work that I would consider a “major” revision, such that I would recommend to address this in a new submission, if the authors decide to attempt it.

General/major comments:

- Assumption of steady-state for spin-up: the paper starts by introducing the glaciers of the southern Andes having among the highest mass losses of all glacier regions worldwide, and specifies a SMB of almost -1 m w.e./year in observations for the Mocho-Choshuenco ice cap (L58). However, the method assumes a zero mass balance of ice cap under present-day (2006-2020) conditions, by requiring that the ice cap’s geometry is closely reproduced by the model as a steady state at a temperature anomaly of zero. This is a strong internal inconsistency which is currently not at all addressed in the paper. Closely related is the lack of discussion of the parameter values obtained by matching the steady-state thickness as closely as possible to observations: equation 9 indicates B_ELA = 1777 m from the observation as opposed to 2035 m from the observation, which (again according to eq. 9) corresponds to a temperature offset of almost 3 K ((2035 m-1777 m)/88m/K). The turnover of 5 m w.e./year (L59) is in apparent contradiction to a maximum annual snow fall S_0 of about 1 m as best parameter values (L166). The initialization of an ice flow model is a complex task, but has been addressed before (e.g., Eis et al. 2019, DOI: 10.5194/tc-13-3317-2019; Zekollari et al. 2019, DOI: 10.5194/tc-13-1125-2019). These studies may be helpful for coming up with an adequate initialization approach.

- Lack of validation of modeled SMB: the authors chose the somewhat unusual way to calibrate parameters of the mass balance equation through matching observed and modeled ice thickness, which I would assume are closer related to parameters of the ice flow model (which are also included in the observation). I don't understand the rationale of this approach, given that mass balance observations are available, and could easily be used for optimizing the mass balance parameters. At the same time, an evaluation of the model's performance concerning SMB is completely lacking. Since a steady state condition is used for the recent past as spin-up, and observations of the SMB during a very similar period are 0.9 m w.e./yr, I suspect that the model has a positive bias of around +0.9 m w.e./yr (not exactly, because the observations only cover a fraction of the glacier's surface). If these presumptions are correct, this would imply that also the projections have a strong positive SMB bias, such that they would strongly underestimate the future rate of mass loss. It is good that the authors evaluate their results against ice thickness and velocity observations, but with the application in projections of mass change in mind, the evaluation of the SMB results is even more important. Without a convincing evaluation the projections cannot be trusted.

- Discussion of comparable studies: At two occasions in the manuscript (L29ff L312ff, the authors state that there are few studies that have projected the future evolution of glaciers in den Andes). Among the studies they cite is Hock et al. (2019), which alone summarizes six studies; a more recent intercomparison is Marzeion et al. (2020, DOI: 10.1029/2019EF001470), which includes seven different models. These are additional to the ones discussed in the paper, but very different in that they don't focus on one (or a few) individual glacier(s), but include all glaciers worldwide. I think it is possible to turn this study into a publishable paper even though by now, there are many models around that have been applied to this specific glacier. But it will be necessary to go into the individual model publications (not just the intercomparison paper, as done now) and see how they are approaching the problem, and discuss the merits of the approach

used here approach in this context: what are the advantages of their mass balance parameterization over those used in the models summarized in Hock et al. (2019) and Marzeion et al. (2020)? What are the advantages of using SICOPOLIS instead of the (mostly simpler) approaches in the global models?

I am convinced that once the first two major issues are addressed by the authors, the results will change substantially. I have therefore abstained from providing detailed/minor comments to the sections that present or discuss these results. These should be addressed at a later stage, if the authors decide to revise and resubmit the paper.

Specific/minor comments/suggestions

- L14: I don't see this generalization backed up by the study results.

- L24: I assume there is a strong seasonality in this number; it would be helpful to be a bit more specific.

- L54: repetitive, can be shortened.

- L61: it can be explained by the climatology, as documented in the data – not the data itself.

- L62: based on the setting of the station and the glacier (orography, wind direction, etc.) would you expect precipitation at the glacier to be higher or lower than in Puerto Fuy? A qualitative assessment would be helpful for readers unfamiliar with the area.

- L76: is an uncertainty assessment available either the total volume?

- Fig. 3b and discussion around it is a bit confusing. Assuming that A_ELA and B_ELA are both positive, and that y is latitude, the maximum ELA would be in the north-east sector. However, the text says the SMB should be lower (equivalent to a higher ELA) in the north-western sector. Since you prescribe phi_0 anyway (L167), why not make Fig. 3b using the actual values used?

- L136: by the ELA, M_0 and S_0.

- Eq. 5 and following: I'm unfamiliar with this notation. Please explicitly define N. Shouldn't it be Gˆhat in the equation? Also, I think it would be correct to speak of a standard error, not standard deviation (Fig. 5, and L154).

- L167: how were the values for phi_0 and M_0 determined?

- Table 1: a statistical evaluation would be helpful: what is the correlation, the RMSE, the bias of the model?

- Fig. 6/Sect. 3.1: instead of using the interpolated ice thicknesses for evaluation, the profiles should be used. Only this will allow a quantitative and robust assessment of the model results (e.g., what is the correlation between observations and model results? What is the RMSE? Is there a bias?).

- L224-225: the description of the lines should be in the caption of Fig. 9, not in the text.

- Sect. 4.4: see general comment above, but additionally: the selection of studies you compare to that project glacier evolution for individual glaciers seems a bit random. I would suggest to focus only on these that include glaciers in the Southern Andes.

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