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

Anonymous Referee #1

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This study uses an ice-flow model, glaciological observations, and climate-model output to project the evolution of Mocho-Coshuenco ice cap in the 21st century. The stated goals are to calibrate the model such that it reasonably captures the current state, and use it to project future ice loss under different forcing scenarios. In their projections, the authors find a range of outcomes depending on forcing scenario, namely relatively little ice loss under the RCP 2.6 scenario, and near total loss of the ice cap by 2100 under RCP 8.5.

As the authors note, there are relatively few glaciological modeling studies targeting this area, and even fewer have used models that explicitly incorporate ice dynamics, so a case study on this glacier complex has the potential to add useful understanding of glacier change in the region. This setting is a good target for applying and evaluating

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a 2D ice-flow model (SICOPOLIS), as there exist ice thickness, mass balance, and velocity data to aid model calibration. I think the choice of model is appropriate for the setting and think the overall framework of using these observations and model together is promising and worthy of investigation.

However, in my view, some fundamental considerations for calibrating and initializing the model are missing and/or flawed, giving me concerns about whether it is indeed calibrated well enough to yield reliable projections into the 21st century. I detail these below, but briefly, they are (1) whether the mass-balance parameterization is consistent with observations; and (2) the assumption of a steady-state to initialize the model in the early 21st century, when these glaciers have already been responding to industrial-era warming. Both of these factors could affect the dynamical response of the simulated ice cap.

Given the stated goals of the paper, I think these factors need to be evaluated before this study can be published. I am not sure whether this will call for re-running the projections with a re-calibrated model, but as it is described now, I am not yet confident in the projections beyond the qualitative conclusions (e.g., massive loss of ice under RCP 8.5 and a significant contrast between RCP scenarios).

I expand on these issues below, and also provide minor and technical comments. I hope the authors will consider these factors, as I do think they have the tools set up for a nice modeling study and a positive contribution – but I think the methodology does need to be adjusted.

Major comments

1) Calibration procedure and surface-mass-balance (SMB) scheme

My first concern is that the parameter calibration isn't adequately evaluated against available mass balance observations. I would argue that matching the overall volume and/or extent is not necessarily a sufficient constraint to conclude the model cap-

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tures the most important features of the present-day glacier (as is concluded at several points). Given that there are mass balance observations available (already used to calibrate the sensitivity to future warming), there is yet more information that could be used to evaluate the calibrated model parameters and the initial glacier state that they yield.

In particular, the calibrated value for maximum snowfall (S0 = 1.07 m/yr) strikes me as surprisingly low given the high precipitation and mass turnover rates discussed earlier in the paper. The mass balance gradient (line 167) is fixed at a (quite high) value of M0 = 0.023 yrˆ-1 (2.3 m w.e./yr per 100m vertical?). How was this chosen?

One thing that strikes me from these values of S0 and M0 is that mass balance must plateau very quickly (< 50 vertical meters) above the ELA, implying a large area where mass balance is uniform at the maximum value. Is this backed up by the available mass balance data? From my own look at Schaefer et al., (2017), it appears that both accumulation and ablation are substantial all the way to the summit in the seasonal balances (e.g., their Fig. 7). If there is in fact non-zero ablation over the entire glacier surface, the entire surface should be susceptible to surface-mass-balance anomalies caused by further melt-season warming. However, if I understand the existing SMB scheme correctly, the model assumes warming doesn't actually cause a SMB anomaly in grid cells above the maximum snowfall cutoff. And, as noted above, this seems like a large initial area, due to the values of S0 and M0. Should warming really only affect the lower reaches of the glacier?

Obviously, no simulation can be expected to capture every detail of the mass balance, and I think a simple elevation/aspect dependent scheme can be a reasonable approach. But as described it seems like there are some embedded assumptions that may not be consistent with observations. The pattern of mass balance and the pattern of anomalies driven by warming should be include in the evaluation of optimal model parameters, given that stake data are available. If I have misinterpreted the SMB scheme, please clarify.

A figure showing the initial spatial pattern of SMB could really aid the reader in interpreting how the maximum snowfall and aspect-dependent ELA affect SMB on the actual topography (e.g., beyond the schematic in Fig. 3).

2) Assumption of steady-state for spinup

My second major comment is on the steady-state spinup for the initial condition. Assuming a steady state is questionable, given more than a century of global warming over the industrial era, to which glaciers respond with a dynamical lag. This lag means that glaciers, in general, are out of equilibrium with current climate (see, e.g., Lüthi et al., 2010; Christian et al., 2018; Marzeion et al., 2018; many others). Forcing a steady state can throw off parameters in the initial calibration (e.g., the ELA), and could throw off the initial transient response when forcing is applied.

The observations of negative mass balance (noted on line 58; from Rivera et al., 2005 and Shaefer et al., 2017) are themselves an indication of disequilibrium, and another reason to include SMB in the model calibration (granted, 5 years is not many observations to define a mean balance). The Rivera et al. study also shows substantial retreat since at least 1976, which would also suggest that the early 2000s extent is not likely to be a steady state.

At the very least, I think it is necessary to estimate how far from steady state the ice cap is at the time the simulation starts, in case the projections need to be qualified in light of this assumption, or the model recalibrated. Some first-order estimates could be made based on the ice cap's estimated response time (e.g., H/b_terminus, see Johannesson et al., 1989). If the response time is long and disequilibrium substantial, it would be necessary to start the simulation earlier to properly capture the transient response for future projections. This is especially true for evaluating the difference between RCP 2.6 and 8.5 trajectories, as the "committed" response to past warming may be a substantial part of the true 2.6 trajectory (with little additional warming), but this would not be captured if the model starts in a steady state in the 2000s.

Minor comments (line by line)

- Line 23: change 10000 mm/yr to 10 m/yr ? (That is a lot of zeros for the readers' eye!)

- 27: "As these are best represented in ice-flow models, they are. . ." Clarify wording: "these" and "they" presumably refer to different things here, but sentence is ambiguous

- 59-61: how are you defining mass turnover here? Can you elaborate on how these temperature measurements indicate this?

- 87: "we replaced solving the energy balance by.." somewhat awkward wording, consider rephrasing

- 100: "SMB should be lower" . . . is it? Based on observations or simulations? It would be helpful to discuss what processes likely lead to this pattern, to help the reader understand how much the model may be simplifying reality.

- 100–104 and Fig. 3b: I'd suggest making the angle in the schematic correspond to the angle used in the actual simulations. I initially got confused as the wording in the text (referring to Mocho) doesn't correspond to the orientation in the schematic.

- 107: if the ELA is defined by the angle with respect to the summit, and the mass balance gradient is constant, doesn't this lead to very sharp spatial variations in mass balance as points near the summit? I suppose the maximum snowfall could limit this, but is this a realistic pattern? Again, a spatial map of SMB could be useful for the reader.

- 119: Clarify: model mean, time mean or both?

- 136: Is precipitation taken into account for this regression? That will affect ELA variability too. . . It is one thing to only consider temperature for the future projections (but see later comment), but I would think the effect of each year's accumulation should be taken into account to calibrate this relationship, especially with only 4 years available.

- 136: Also, is there a particular reason the temp-ELA relationship and future projections are based on annual-mean rather than melt-season temperatures? Do the climate models predict melt-season temps warm at the same rate as annual mean?

- 167: Again, where are the values for M0 and phi taken from?

- 172: “does not reproduce well” . . .Consider rewording for clarity.

- 177: 200 m/yr is very fast! Can you comment on why the model might give velocities an order of magnitude higher here than most values in Table 1? Is it the geometry, or SMB pattern that allow this?

- 181: I’m just curious if you know why there are seasonal but not annual velocities? I’m surprised annuals weren’t derived from, e.g., mass balance stake locations. Were seasonal velocities only measured in one year?

- 183: As I understand from the SMB parameterization, precipitation is implicitly assumed to not change. Is this roughly consistent with the model projections and/or observed trends for the area? I’d expect that temperature is the main forcing, but recommend at least stating that this assumption is made.

- 187: Retreat is strongest in the north for RCP 2.6. . . is this partly because of the imposed higher ELA and cap on mass balance?

- 215–17: I find this statement on internal variability confusing. Do you just mean that the spread due to different climate models and scenarios hasn’t had time to diverge? Consider rewording for clarity.

- 220: When considering a different ELA-temperature relationship, doesn’t this imply other parameters are also different (e.g. the vertical SMB gradient?). Does the initial state reflect differences in these parameters, if any?

- 238: “high observed velocities” . . . do you mean high modeled velocities (referring to an area without extant ice)

- 248: I think you mean thinner here, right?

- 246âĂŤ48: Here you have explained the low velocities in terms of anomalously thin ice, but why is the ice too thin? The combination of too-thin and too-slow together indicate that overall fluxes are underestimated in these areas. . . which ultimately seems like a mass balance issue. Could this be related to the rather low cap on mass balance (see major comment above)?

- 253âĂŤ54: I find it a bit circular to invoke a "stable position at the moment" to explain a model result, when the stable position is imposed by your choice of a steady-state initial condition. This is one area where the steady-state assumption (see major comment) can affect projections.

- 266: Suggest word choice other than "unstable". . . there's no instability in a dynamical sense here, just a larger forcing.

- 316: "would have disappeared" » projected to disappear?

- 319: "maintained glacier area constant" » "maintained a constant glacier area" ?

- 330: "lost majority of their ice mass" . . . relative to preindustrial?

- 331: "The" » this

- 331-332: general comment here that different proportions of volume lost over a given timeframe can be due simply to different glacier geometries/hypsometries. I think that should be borne in mind for all of the comparisons in this section. . .

- 357: missing period

- 370: then » than

References (beyond those already cited in manuscript)

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