The Cryosphere Discussions

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Comment on tc-2022-20

Anonymous Referee #2

Referee comment on "The contribution of Humboldt Glacier, North Greenland, to sea-level rise through 2100 constrained by recent observations of speedup and retreat" by Trevor R. Hillebrand et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-20-RC2, 2022

In this work the authors investigate the contribution to sea-level rise of the Humboldt Glacier (North Greenland) for the next century. The model initial conditions are optimised through a three step procedure: first, the basal friction coefficient is optimised from surface velocity inversion at 2007; second, the basal friction exponent is tuned through imposed calving rates to match the observed ones for 2007-2017 and velocities for 2017-2018; third, the calving retreat parameterisation is tuned to match calving front positions and velocities for 2017-2018. The resulting initialisation is then used to launch an ensemble of model simulations for the period 2007-2100 and estimate sea-level rise due to future glacier retreat.

Overall I find this a very interesting work. It is well framed, the experimental design is novel and clever, and the results are comparable to previous estimates, although higher. I think this work suits very well the scope of The Cryosphere. Yet, I am not 100% convinced about the initialisation procedure that led to such results. Since your estimated SLR contributions are considerably higher than previous estimates and you attribute this “primarily to calibration of the basal friction law to match observed surface velocity changes”, I am wondering to what extent the validation procedure you apply in the optimisation+tuning experiments does affect the choice of the basal friction exponent, and so your final SLR estimates. I think that the strength of your results must be proven with some further verification of the tuning procedure for the historical runs. Moreover, I think section 2.3, as it is now, is missing some important clarifications. Therefore I suggest major revisions before publication.

Most of my comments concern the tuning of basal friction parameters in the initialisation procedure. I outline them here:

- Could you explain better how the effective pressure N is calculated in your basal friction law (line 95)? From what you write I understand it is rho_i g H - rho_w g z_bed, right? How is N treated during the basal friction coefficient optimisation? Is it kept fixed to initial values for the whole procedure assuming that ice thickness doesn't change? See also next point.
- The relationship used to tune the basal friction exponent (line 129, μ = μ_opt |u_opt|^(1/3-q)) should be explained more in detail. To my understanding, you derived it by solving the equation N*μ*|u_opt|^(1/3-1) = N*μ_opt*|u_opt|^(q-1), having assumed same basal friction and velocity from the inversion procedure. However, this relationship is defined under some important assumptions that should be explained. You assume that the effective pressure is the same between the optimisation and the tuning procedure, but I would expect the ice thickness has varied between 2007 and 2017 due to margin retreat, and so did N. This argument is also valid for surface velocities. How did you account for velocity changes that come out due to glacier retreat in your tuning procedure? I would expect that the choice of the best basal friction exponent ultimately depends on these assumptions. Since your results strongly depend on the value of q (Fig. 5), to what extent do you think these assumptions affect your sea-level contribution for year 2100? What happens if the relationship you wrote is not supported, i.e. the N and velocities are not constant and, still assuming that the basal friction is the same for optimisation and tuning, you have this relationship instead: μ = N(2007) / N(2017) * μ_opt * |u_opt|^(1/3-q) * u_obs(2007)/u_mod(2017) ? Also, have you tried to do the inversion with $1/7<q<1/5$ to corroborate your tuning procedure?
- In the basal friction exponent tuning experiment you compared modelled to observed velocities only for year 2017-2018. Why didn't you test your velocities for the whole historical period (2007-2017) and choose the q that best matches the velocities on a 10yr mean? Also, would considering seasonal velocities instead of annual mean lead to a different q? Would in these cases the choice of $1/7<q<1/5$ still be confirmed and so your SLR estimates?
- How did you impose the calving front retreat rates for years 2007-2018 (line 131)? To my understanding the calving tuning procedure described in section 2.5 is done after the basal friction optimisation. How did you calculate the calving rate then? Also, is the submarine melt taken into account for such tuning tests?

Regarding the structure of the manuscript, I don't really understand why you separate the perturbation from the sensitivity tests. In fact, their design is comparable (you fix some parameters and perturbed some others) and they all contribute to build the uncertainty range of sea-level rise due to glacier retreat. To lighten the structure of the paper, I would suggest to include all sensitivity tests into the perturbation experiments and introduce a summary table describing the whole experimental design (which parameters are varied and which are fixed for each run). I suggest also to mark out those tests do not take part in the final estimates of sea-level contribution (e.g. tests for q=1, calving rate limit > 5km/yr). Finally, I would suggest to leave the mesh convergence test to the supplementary material, since it is more a precondition for your tests rather than a functional part of the study, and the bedrock sensitivity test too, since it does not involve any change in physical variables.

Specific Comments

- Figure 1: is this the Humboldt Glacier or the regional model domain? To me that is the catchment containing the Humboldt glacier. Also, I suggest to make the black rectangle in a) with a bigger line and with a different colour. I would add the modelled effective pressure and instead of panel b) and d) I would only show the velocity difference (modelled velocity - observed velocity).
- Line 128: do you mean q instead of m? Also, "to find the appropriate range of values of q in the basal friction relationship, we recalculate the friction parameter ð□□□…" is misleading. You should add you did that to match the velocities upon retreat.
- Line 132: Do you mean 2017 instead of 2018? Generally, I found quite confusing the definition of the period used for hindcast, which sometimes ends in 2017, sometimes in 2018. Please check that in the whole manuscript.
- Line 156: Please change to "Connectivity Temperature Depth (CTD) and Airborne eXpendable Connectivity Temperature Depth (AXCTD)".
- Line 207: where does this SMB forcing come from? From which model? And why did you choose this period, and not a climatology close to year 2000 since you initialise the model at 2007? To what extent might the choice of a more recent climatology for the control run affect your results and reduce your estimated sea level contributions?
- Line 274: Table 1 does not show the results, rather summarises the experimental design. I think that table is missing.
- Line 281: where does the upper bound of SLR for the 2017 Calving front experiment (6.7 mm) come from? Is HadGEM 2 predicting ~6.5 or 6.7mm?
- Line 286: with "variability due to … climate forcing" you include also the variability in submarine melting, right? Could you be more precise since the choice of the oceanic thermal forcing influences your results?
- Line 304: looking at Fig. 2c it seems that CNRM-CM6 has a higher ocean thermal forcing than HadGEM2. So why does only the latter lose all the ice shelves within 2100?
- Line 321: could you introduce the undercutting already in the submarine melting parameterisation section since you have a precise parameterisation for it?
- Line 366: why not repeating the experiment also for MIROC for consistency with the other tests?
- Figure 9: could you plot also the change in volume above flotation and compute the associated sea-level contribution?
- I am missing a Figure summarising the sea-level contribution from all sensitivity/perturbation experiments compared to previous estimates. For example, you could plot the latter as superimposed to the uncertainty range in SLR raised from your runs. I think it would help the reader to have your results recapped in one plot.
- Figure S3, S4: don't think you really need to show the bathymetry here. In case you want to keep it, please change the colour palette to a scale of greys. Also, specify that grounding line colours follow legend of Fig.4. Finally, please change the colour of small areas with speed>3km/yr to red or green.