The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2022-20-RC1, 2022

Comment on tc-2022-20

Anonymous Referee #1

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-RC1, 2022

Authors present numerical simulations of mass loss from Humboldt Glacier. Historical runs and observations (2007-2017) are used to constrain parameters in the description of basal rheology and the calving law. Optimal model parameters are used to produce projections of mass loss for a range of future forcing scenarios (2017-2100). Results highlight the importance of the basal sliding exponent, and best estimates of mass loss exceed previous projections by about a factor of 2.

The objectives and methodology of this study are clear; the results novel and well-presented; the conclusions well-supported, and overall this work is a good fit to The Cryosphere. I recommend publications with scope for some minor revisions, as outlined below.

The experimental design is mostly straightforward and easy to follow, though I wonder about the need to distinguish between the perturbed parameter ensemble, and the additional sensitivity experiments. After all, these are all sensitivity experiments, and personally I think the distinction overcomplicates the structure of the paper. An overview of the sensitivity to all physical parameters in a single Table would be nice. Some experiments that do not test the significance of physical parameters, such as the mesh resolution and potentially, bed topo, could be included in an Appendix to reduce the amount of information in the main text.

The validation approach is interesting, though I miss a more in-depth description/motivation of the validation criteria. Fig3b suggests that the optimal choice of q is critically dependent on the velocity itself, so why choose 1/5-1/7, which only provides a best match for u>600m/yr? In this regard, you might find the discussion in section 3.3 of [De Rydt et al. 2021] of interest, where authors show that the optimal sliding exponent for Pine Island Glacier is spatially heterogeneous. On a related note, I wonder if you can show the difference between observations and model in Fig3a, rather than the absolute model speed.

I think some further details about the melting and calving paramterization would be instructive for readers less familiar with the different (model) approaches. For example, in line 151: can you be more explicit about what you mean by 'if there is no floating ice', line 146-150 and 160-170: how does this discussion relate to quantities displayed in figure 2 (e.g. I'm unsure how 'mean ocean thermal forcing' is translated into a depth-dependent parameterization of melt), section 2.5: how is the calving front tracked in the model, and what happens to ice that has calved – is a minimum ice thickness applied?

Line 274 you refer to Table 1 here, but this table does not contain any information on SLR. Also, is there a reason why \sigma_max for q=1/7 in Table 1 is different between MIROC5 and the other forcing scenarios?

Line 427 I assume you are using annually averaged velocities, rather than seasonal products, for the 2007 initialization and 2017 validation? Given the large amplitude of seasonal speed-up/slow-down along this section of Greenland's margin, how important is the choice of velocity product? In lines 590-595 you allude to possible important implications, but can you provide quantitative insights in how alternative intialization approaches (e.g. by using summer-only values of surface speed) might alter/bias your results?

FigS1. Can you provide a legend for the different colours please?

Ref.

De Rydt, J., Reese, R., Paolo, F. S., and Gudmundsson, G. H.: Drivers of Pine Island Glacier speed-up between 1996 and 2016, The Cryosphere, 15, 113–132, https://doi.org/10.5194/tc-15-113-2021, 2021