Anonymous Referee #2

In this manuscript, the authors report on their ISMIP6 Greenland projections with the model GRISLI. The paper is easy and straightforward to follow. Its scientific value beyond the community publication (Goelzer et al., 2020, in press) lies in a more detailed description of the set-up of GRISLI, a more detailed analysis of the results and the fact that the entire suite of ISMIP6 experiments (Tier 1-3) are dealt with.

Overall, I found the results interesting and the presentation adequate. I'd only like to raise some issues that should be dealt with as follows:

Thank you for your positive evaluation, we reply to your individual comments below.

The English writing clearly has some room for improvements. I am not going to point out all the issues, but just some examples from the first page: P. 1, l. 3/4: "an increase_d_ mass loss". P. 1, l. 5: "the largest single source contribution _after_ the thermosteric contribution". P. 1, l. 19/20: Assessment of projections? Either "need for assessment of future SLR by projections" or "need for projections of future SLR". P. 1, l. 22: Strange formulation: "from changing boundary conditions such as climate change". Before resubmission, the entire manuscript should be very carefully proof-read by a (near-) native speaker or a professional language editing service.

Thank you for your corrections, we have followed all your suggestions. We are indeed non-native English speakers but we put a lot of effort to write in English since it is the international language for Science. Even after careful reading, we are aware that our manuscript will contain grammatical errors or poorly formulated sentences but we think that it is generally understandable. If not, we will be more than happy to follow your corrections. Also, it might be relevant to note that The Cryosphere journal includes a language editing service for all accepted manuscripts.

Throughout MS (e.g., p. 1, l. 10, l. 14): "mmSLE" -> "mm SLE"

Corrected.

Throughout MS (e.g., p. 2, l. 4): "in term of" -> "in terms of"

Corrected.

P. 1, l. 17/18, "most-likely amplitude exceeding 1 metre in 2100": This is not what has been found in the ISMIP6 ensemble projections (Goelzer et al., 2020, TC, in press; Seroussi et al., 2020, TC, in press). Even with the most sensitive model results, it is less than half a metre combined. At some point in the paper, this should be mentioned.

We apologise, this was an exaggerated statement since the range of Bamber et al. (2019) is 51 to 178 cm for unmitigated emissions. We have chosen to cite the Special Report on the Ocean and Cryosphere in a Changing Climate (SROCC, Oppenheimer et al., 2019) instead of the expert judgement of Bamber et al. (2019) here:
"Amongst the different contributions, the Greenland and Antarctic ice sheets have a potential to raise substantially the global mean sea level, with a weakly constrained trajectory (Oppenheimer et al., 2019)."

The numbers for the contribution of the Greenland ice sheet to 2100 sea level rise in Goelzer et al. (2020) and in our manuscript are within the range of the SROCC.

P. 3, l. 7, 17: Add commas after the displayed equations.

Done.

P. 3, l. 11-13: The description of the SIA and SSA is over-simplified. Starting from full Stokes, in both cases, some horizontal and some vertical derivatives of the components of the stress tensor are neglected. In very compact form, this is shown in the tutorial at http://doi.org/10.5281/zenodo.3739009, p. 22 (for SIA) and p. 24 (for SSA).

Reformulated:
"For the whole geographical domain, we assume that the total velocity is the sum of the velocities predicted by the two main approximations: the shallow ice approximation (SIA) in which the deformation is entirely driven by the vertical shear and the shallow shelf approximation (SSA) in which the vertical shear is neglected and the horizontal stresses are predominant."

P. 3, l. 14/15: Is floating ice included in the simulations? If so, what is assumed for the sub-ice-shelf melt rate?

Only few glaciers in Greenland present a floating tongue and when they do it is located in very narrow valleys. The 5-km grid used in our model is not precise enough to represent these floating tongues and the physical processes related. This is why we have imposed a very large basal melting rate in our simulations (200 m yr^{-1}) to avoid floating points. This is now stated in the manuscript:
"Since 5 km is too coarse to represent Greenland floating ice tongues, sub-shelf melting rate has been set to a large value (200 m yr^{-1}) to discard simulated floating points."
We agree that is a simplification that can bias the future projections. In fact, this is not a problem specific to GRISLI since most ISMIP6 participating models do not have the resolution needed to represent such floating ice tongues. The ISMIP6 glacier retreat parametrisation has been developed (Slater et al., 2019) to account for such a process in models that would not otherwise.

P. 3, l. 16: "till _layer_"?

Corrected.

P. 3, l. 24ff: 30 kyr is likely not long enough to reach thermal equilibrium for an ice body as large as the Greenland ice sheet. This should be commented on. Further, does the inferred sliding depend on the basal thermal state, or is basal sliding applied everywhere?

We agree. This is why we performed more than ten cycles (thermal equilibrium + multiple 200-yr simulations). The basal drag coefficient and the basal temperature are coupled and in doing more cycles, we expect the two variables to be consistent with each other. This is now more clearly stated in the manuscript:
"After a few 200-yr experiments, we repeat the thermal equilibrium computation restarting from the previous equilibrium state with the newly inferred basal drag coefficient. In doing so, the basal drag coefficient and the temperature at the base are consistent with each other."

P. 6, l. 18ff: I cannot see it so well in Fig. 2, but it seems to me that the simulations do not include/reproduce the floating ice tongues (at least off the NEGIS). If so, this may also be partly responsible for the velocity misfits because buttressing effects are missing.

You are correct, we do not simulate the floating ice tongues which can exert buttressing. However, the simulated velocity in the NEGIS is underestimated so it cannot be linked to the missing buttressing (which would reduce the velocity even more). The velocity misfit is most probably linked to the basal drag coefficient.

P. 9, l. 7: It would be interesting to quantify this. How large (e.g., in per cent) is the difference between full forcing and (AO+OO)?

The AO+OO explains 93.6%, 91.6% and 92% of the full forcing for MIROC5, NorESM1 and CSIRO-Mk3.6 respectively. This is now stated in the manuscript:
“Also, the sum of the ice loss of AO and OO experiments approximate closely the ice loss simulated when using the full forcing (92 to 94% of the full forcing). ”

P. 10, l. 15: This is the first time in the paper that the enhancement factor is mentioned. It should be defined and specified earlier in the paper (section 2.1).

Added in the description of the model:
“Like most ice sheet model, GRISLI uses a flow enhancement factor that favours longitudinal deformation in the SIA (Quiquet et al., 2018). However, here we use a flow enhancement factor set to 1 (no enhancement). Similarly, the flow enhancement factor for the SSA is also set to 1.”

P. 11, l. 18/19: This should be made a proper reference and cited here as Quiquet and Dumas (2020). And, BTW: _Z_enodo.

Done.