Editor review

Thank you again for taking the time to serve as Editor on our manuscript.

I would like to see the revision of the manuscript based on the comments by the referees, with the response point by point. Especially I expect to see more discussion, which is more than describing the single model, after seeing the ISMIP6 now published. Please use this opportunity to expand the discussion on the response of the grounding line to the oceanic forcing, for example.

We apologise since we did not provide information about grounding line implementation in GRISLI in previous versions of the manuscript. We have added the following:

"Since the model is generally used at a coarse resolution (greater than 5 km), we use an analytical formulation of the flux at the grounding line following either Schoof (2007) or Tsai et al. (2015). The sub-grid position of the grounding line is estimated with a linear extrapolation of the floatation criteria. From this sub-grid position of the grounding line, the ice flux from Schoof (2007) or Tsai et al. (2015) is extrapolated to the neighbouring velocity grid points. More details on this implementation is provided in Quiquet et al. (2018). Using a 40 km grid resolution the model was able to reproduce glacial-interglacial grounding line migration in agreement with geological data (Quiquet et al., 2018). A 16 km version was also used to assess the importance of buttressing for grounding line stability in the ABUMIP intercomparison exercise (Sun et al., 2020), where GRISLI shows an important grounding line retreat although amongst the lowest within the other participating models. Here, we use the analytical flux of Schoof (2007) at the grounding line."

We have also added a discussion on the grounding line sensitivity in GRISLI compared to other models. The first paragraph of the discussion section now reads:

"Amongst the different experiments, the largest contribution by 2100 is 150 mm SLE (NorESM1-M PIGL with a high oceanic sensitivity) while most experiments produce a contribution no greater than 80 mm SLE. Thus, it appears that the contribution of the Antarctic ice sheet to global sea level rise simulated by GRISLI is relatively limited. Since ISMIP6-Antarctica was a large intercomparison exercise that involved 13 research groups and 21 model versions, it is useful to compare these numbers with the ISMIP6-Antarctica ensemble. For this ensemble, using a medium oceanic sensitivity, HadGEM2-ES produces the largest mass loss with an ensemble mean of 96 mm SLE and CCSM4 produces the largest mass gain with an ensemble mean of -37 mm SLE. Although GRISLI does not stand up as an outlier within the ISMIP6 ensemble, it shows a more limited sea level contribution with 58 mm SLE for HadGEM2-ES and -45 mm SLE for CCSM4. This could suggest a moderate sensitivity of the grounding line migration in response to the oceanic forcing when compared to the other ice sheet models. However, it is important to note that some outliers are largely influencing the ISMIP6-Antarctica ensemble mean towards higher contributions. In particular, some ice sheet models that do not use the standard ISMIP6 approach to compute sub-shelf melting (*open* experiments) produce much higher ice sheet mass loss. Notably, for NorESM1-M (RCP8.5 medium oceanic sensitivity), ULB_FETISH32_open, ULB_FETISH16_open, VUB_PISM_open and NCAR_CISM_open simulate a 2100 mass loss ranging from 72 mm SLE to 163 mm SLE where all the other models show an ensemble mean close to 0 mm SLE. In addition, when models use both the *standard* and the *open* approach to compute the sub-shelf melting, the *open* approach tends to produce much higher mass loss (NCAR_CISM, UCIJPL_ISSM, ULB_FETISH32, ULB_FETISH16). Thus, it seems that the consideration of how the different groups have implemented this process is crucial to understand the multi-model spread. When we consider only the models that use the *standard* approach, GRISLI shows a mass loss much closer to the ensemble mean. However, it is not excluded that GRISLI shows a relatively low oceanic sensitivity. It is for example unable to simulate any substantial grounding line retreat in the Pine

Island glacier area for the different climate scenarios tested here, even though this could be linked to initialisation model biases that induce an ice thickening in this area in the control experiment. Also, in the ABUMIP intercomparison exercise (Sun et al., 2020), GRISLI shows one of the lowest grounding line retreat due to the loss of buttressing (3rd lowest grounded ice loss in 500 years with respect to the control, out of 15 participating models). Sun et al. (2020) suggested that plastic friction laws produce greater grounding line sensitivity than linear friction law as the one used here. This was also suggested by Brondex et al. (2019). A foreseen improvement of our ice sheet model will be the implementation of various friction laws to better assess the sensitivity of grounding line dynamics to this process."

One question from my side on the model description is how you treat the calving of this model.

We have added some information in the model description section:

"Calving is based on a simple threshold criterion: the ice thickness at the front reaching a minimal value is automatically calved if the upstream flux is not sufficient to maintain a thickness above this critical threshold. The minimal ice thickness is set to 200 m in the experiments presented here."

Johannes Sutter review

I thank the authors for their revision of their manuscript. The authors addressed the majority of my previous comments but some parts of the manuscript still lack a more rigid discussion of aspects such as grounding line sensitivity and the differentiation between precipitation and surface mass balance. There also remain some stylistic and spelling issues but they are very minor.

Please refer to the attached commented-pdf for my remaining comments/remarks.

Thank you for these valuable comments. We provide a point by point response to each one of them in the following. A track-change version of our manuscript is uploaded with this response.

I recommend to:

1. sharpen the discussion of the grounding line sensitivity in the model used here, and provide an assessment whether it under- or overestimates the response to 21st century oceanic forcing.

We added a discussion on the sensitivity of the grounding line in GRISLI. This new paragraph can be found in this document as a response to the first Editor comment.

2. distinguish between surface mass balance and precipitation throughout the manuscript or even better only use surface mass balance.

Thank you for pointing this issue out. We now mostly mention the surface mass balance and we only use "precipitation" for very specific discussions.

P1 L19 please specify what kind of (if any) grounding line parameterisation is used here? The MISMIP intercomparison excercise has shown that coarse resolution models (i.e. several km or > 10 km) do not exhibit reversible grounding lines. Feldmann et al. have shown e.g. for PISM, that a subgrid interpolation of the grounding line position can alleviate that allowing for reversibe grounding lines even at coarse resolution. I do not know whether this is discussed in Quiquet et al. but it would be certainly important to quickly not here what is done with respect to grounding line parameterisations.

We realise that we did not provide any information on the grounding line formulation in GRISLI in our manuscript. We have added more information in the model description section with this respect:

"Since the model is generally used at a coarse resolution (greater than 5 km), we use an analytical formulation of the flux at the grounding line following either Schoof (2007) or Tsai et al. (2015). The sub-grid position of the grounding line is estimated with a linear extrapolation of the floatation criteria. From this sub-grid position of the grounding line, the ice flux from Schoof (2007) or Tsai et al. (2015) is extrapolated to the neighbouring velocity grid points. More details on this implementation is provided in Quiquet et al. (2018). Using a 40 km grid resolution the model was able to reproduce glacial-interglacial grounding line migration in agreement with geological data (Quiquet et al., 2018). A 16 km version was also used to assess the importance of buttressing for grounding line stability in the ABUMIP intercomparison exercise (Sun et al., 2020), where GRISLI shows an important grounding line retreat although amongst the lowest within the other participating models. Here, we use the analytical flux of Schoof (2007) at the grounding line."

P9 L14-15 either there is a typo in the mass loss specs or the adjectives are swapped. I assume there is a typo as HadGEM2-ES is one of the models showing the most warming.

There was a typo (a zero missing), HadGEM2-ES produces effectively the largest mass loss ($300x10^3$ Gt in 2100). It has been corrected.

P9 L22 actually, it could be worth including a plot of the change in grounded ice area as this gives an indication of integrated grounding line changes

Thank you for your suggestion. You have included such a plot and added an additional point in the result section:

“An other way to show this is to investigate the grounding line migration in the course of the century. In Fig. 5 we show the grounded ice extent evolution which is an integrated indicator of grounding line migration. For all the projection experiments, the grounded ice extent is always smaller than in the control experiment, and this extent decreases in the course of the century. Thus, even for models that produce an important grounded ice volume increase in the future (e.g. CCSM4), the grounded ice extent is decreasing. This can be only explained by an increase in surface mass balance over the grounded area.”

P9 L22-24 this is a very important result. To make your figures consistent with the figures in the main ISMIP6 publication, I strongly suggest to change figure 3 and all figures showing the sea level equivalent mass change so they are showing the change relative to the ctrl_simulation. I know that you plot the ctrl and historical simulation as well but it is easier for the reader if she/he does not have to subtract it in her/his head.

We understand your point. However, we would prefer to keep the plots that show the absolute changes and not the anomalies relative to the *ctrl_proj* simulation, for several reasons:
- the absolute changes reflect what is really happening in the model while the anomalies have no real physical meaning. It is true that it is sometimes convenient to show the anomalies in order to remove the potential drift of the model but in doing so we assume that the drift is preserved between the control and the projections (the effect of the projections being simply added to the drift). Since it is an assumption that is not necessarily verified, we prefer to show the absolute changes for both the control and the projections. We believe that it is straightforward for the reader to substract the control if needed.
- the ISMIP6-Greenland paper shows absolute values (Goelzer et al, 2020). For consistency between our papers on Greenland and Antarctica, it makes sense to stick to absolute changes.
- the paper has been reviewed by 2 other reviewers (+2 for the Greenland paper) with no comment on this matter. Changing all the figures at this stage would ideally require the agreement of the other reviewers.
For these reasons, unless you have strong argument against it, we prefer to stick to our version of the plots showing the absolute values.

P9 L26-27 From Figure 5 I read that only in HadGEM2-ES does surface melt, runoff and evap overcome the increase in precip? Worth stating that explicitly.

We now make a better distinction between surface mass balance and precipitation:

“In fact, most GCMs simulate an increase in precipitation in Antarctica related to the projected warming. This increase in precipitation can be partly compensated by an increase in runoff and evaporation. However, overall, most GCMs produce an increase integrated surface mass balance in the future. The difference in terms of surface mass balance change amongst the GCMs explains the large spread in [...]”.

P10 L1-2 is it really a lack in precipitation increase or rather a considerable increase in coastal surface melt and runoff? Or did you mean to write "lack of surface mass balance" here?

HadGEM2-ES produces effectively increased precipitation but not an increase in surface mass balance. We replace “precipitation” here by “surface mass balance”.

P12 L28-29 but should also be associated with increased surface melt if temperatures are hight enough? How does surface mass balance change look like?

Agree, this was an oversimplification from our part. We now refer here to Nowicki et al. (2020) instead, since the changes in surface temperature and surface mass balance are presented in their paper.

P13 L6 Seroussi et al also show that ice area loss due to ice shelf collapse is 6 times larger than in the simulations without collapse (section 4.8 in their paper). This probably means that GRISLI models rather stable grounding lines compared to other models which took part in ISMIP6. This should be discussed more explicitly in the paper as it is an important piece of information for the interpretation of the results shown here.

In GRISLI, the ice shelf area when using CCSM4 under RCP8.5 is reduced by 86 000 km^2 between 2015 and 2100. When using the ice shelf collapse scenario, the shelf area is reduced by 240 000 km^2. The floating ice area loss due to ice shelf collapse is 2.8 larger than in the simulations without collapse. This number is smaller than in Seroussi et al. (2020), who reported a 6 times decrease in shelf area due to collapse. However, Seroussi et al. (2020) also reported a mean ice shelf area reduction of 11 000 km^2 without the ice shelf collapse when using CCSM4 under RCP8.5 (compared to 86 000 km^2 in GRISLI) and 66 000 km^2 with shelf collapse (compared to 240 000 km^2). This means that GRISLI shows a large ice shelf shrinking when compared to the ensemble spread. This higher ice shelf extent sensitivity to the oceanic forcing in GRISLI compared to the other models could explain why the ice shelf collapse scenario has a relatively lower importance in GRISLI.

We modified the manuscript in the results section. First:

“Overall, the ice shelf collapse scenario systematically induces a decrease in the ice shelf extent. For example, when using CCSM4 under the RCP8.5 the ice shelf extent decreases by 86 000 km^2 from 2015 to 2100, but it decreases by 240 000 km^2 with the ice shelf collapse scenario (extent loss 2.8 times larger).

A greater sensitivity to this process has been reported in Seroussi et al. (2020), although associated with a wide spread of responses amongst participating models. In terms of ice shelf extent loss, Seroussi et al. (2020) reported a loss 6 times larger with the ice shelf collapse scenario (66 000 km^2 compared to 11 000 km^2). However, the numbers in Seroussi et al. (2020) are much smaller than the one in GRISLI (240 000 and 86 000 km^2 with and without the shelf collapse scenario, respectively) suggesting a high sensitivity of the ice shelf extent in GRISLI to the oceanic perturbation. This might explain why the ice shelf collapse has a relatively lower impact on the ice shelf extent. However, Seroussi et al. (2020) also reported a larger impact of the ice shelf collapse scenario on the volume change contributing to sea level rise (multi-model average of 28 mm SLE in 2100 under the CCSM4 forcing). This can indicate a low sensitivity of the grounding line retreat in GRISLI compared to the other participating models. However, it can also be linked to the local model biases. In fact, for most climate models, the retreat masks by 2100 [...]”

Second, as mentioned earlier in this document, a dedicated discussion to the sensitivity of the grounding line in GRISLI with respect to the other models participating in ISMIP6 has been added in the discussion section.

Also, there was a typo in the manuscript, the average impact of the shelf collapse in Seroussi et al. (2020) is 28 mm SLE (not cm SLE).

P13 L11-13 However relatively little grounding line retreat compared to the other models which took part in ABUMIP. Actually GRISLI has the lowest volume equivalent SL change of the whole ensemble (together with PISM-PIK).

We agree with the reviewer with the fact that GRISLI shows a relatively modest grounding line retreat when compared to other ABUMIP participating models. However, it has not the weakest ice sheet response to the loss of buttressing.

The reviewer has probably in mind Fig. 5 of Sun et al. (2020) which shows absolute volume change in ABUK (no shelf) and ABUM (artificially very high shelf melting). This figure does not account for the fact that some models show a large volume drift in their control experiments. This drift is an other manifestation of the importance of the initialisation procedure which produces an ice sheet in equilibrium with the forcings (as in GRISLI) or not. For example, PISM1 produces a volume change in ABUK of about 3 mSLE in 500 years but it also shows a large drift in the control simulation (ABUC) that leads to about 1.5 m SLE (Fig. 1). When corrected for the drift in the control, buttressing is thus accounting for about 1.5 m SLE for this model while it accounts for about 2 m SLE in GRISLI. The role of buttressing is even weaker for ISSM (which only participated to ABUM) since it produces a 1 m SLE in ABUM with a drift in the control of about 0.5 m SLE. PISM-PIK in turn has a drift towards larger ice volume and as such shows a more important role of buttressing than GRISLI (accounting for about 3 m SLE compared to about 2 m SLE in GRISLI).

To conclude, GRISLI shows indeed a relatively modest grounding line retreat induced by buttressing loss when compared to other ABUMIP participating models (3rd lowest out of 15). We added this precision in the manuscript:
“ […] we were able to simulate large grounding line retreat when the buttressing induced by the ice shelves is removed, although amongst the lowest within the other participating models (3rd lowest ice volume change with respect to the control experiment in 500 years, out of 15 participating models)”

We also have added a discussion about the GRISLI grounding line sensitivity (see our previous comment).

P14 L24-25 why the year 2045? please explain.

This choice is somehow arbitrary. Our idea was to test the effect of a potential change in ice mechanical parameters in the course of the century for the projections in 2100. Thus it should start not too early after the start of the simulations (for which the mechanical parameters have been tuned) nor too late (otherwise the perturbation will have no effect).

We added the following:
“These perturbations are imposed abruptly at the end of the year 2045, in order to mimic a potential change of these parameters in the course of the century. The timing of these perturbations is somewhat arbitrary: not too close from the start of the projections but also not too late so that they affect the ice sheet evolution to 2100.”

P14 L27 & L34 what is meant by "standard value" here?

Unperturbed experiments. This is now specified:
“Fig. 13 shows the mass change in 2100 for the perturbed experiments with respect to their unperturbed counterpart (shown in Sec. 3.2.1).”
And later:
“For the basal drag coefficient, the acceptable perturbations lead to an additional sea level contribution ranging from about -30 to +30 mm SLE, with respect to the unperturbed NorESM1-M under RCP8.5 experiment that produces a 20 mm SLE in 2100.”
And finally:
“The perturbations induce a change in total mass of -12×10^3 to $+12 \times 10^3$Gt for the basal drag coefficient and of -30×10^3 to $+25 \times 10^3$Gt for the enhancement factor, with respect to the mass loss in 2100 of -165×10^3Gt obtained with the unperturbed NorESM1-M under RCP8.5 experiment”.

P15 L4 “They”

Replaced by “The perturbations”.

P15 L30 This also means that the following two sentences don't really make sense.

We are sorry but we do not understand your point here. What we meant is that a simple linear extrapolation of the observed recent mass loss rate gives 52 mm SLE for the Antarctic ice sheet in 2100. We do not expect a linear trend for this contribution since it exists multiple feedbacks that could lead to increased future loss rates. Since GRISLI and the majority of ISMIP6 models produce a 2100 contribution to global sea level rise lower than 52 mm SLE, there is an apparent disagreement the observed recent mass loss rate and the ISMIP6 projected contributions in 2100. We think that a large part of this disagreement comes from the initialisation procedures used by the ice sheet models since most of these procedures produce an ice sheet at equilibrium for the present-day.

P16 L9 I think at this point you should talk about surface mass balance not about precip, as SMB is ultimately the decisive variable for ice sheet mass changes.

Right, corrected.

P29 Fig. 7 caption I suggest to remove this line from this and the other figures as it is misleading. Mass changes in the AIS contribute differently to SL changes depending where the are occuring. The specification 1 mm SLE = 372 Gt does not correspond to what is shown in the figure and therefore will confuse the reader.

Agree. We put this only so that the reader can have an easy way to convert the two units but it is true that we do not used this factor to draw our plots. We removed it in the revised manuscript.