Anonymous Referee #2

1 General comments

This paper is based on the Ice Sheet Model Intercomparison project (ISMIP6) on the Antarctic ice sheet. The results of individual ice-sheet model GRISLI are discussed. Apart from the standard experiments described in Seroussi et al., 2020, forcings derived from some CMIP6 model simulations are implemented in this study. Furthermore, experiments with atmospheric forcing only and oceanic forcing only are taken to study their roles separately. Finally, the authors did sensitivity tests on the basal friction coefficient and enhancement factor to address the influence of initial conditions.

Thank you for your careful reading. In the following we provide a point by point response to your comments.

Generally, I believe studies based on individual models could be a good complement or further study beyond the intercomparison paper (Seroussi et al., 2020). For example, by implementing different schemes in the single model, uncertainties could be better understood. Though, it's not clear to me what the strong points of this paper are. I have a few concerns about this paper:

• The main results and the induced conclusions are in line with the model intercomparison paper and don't add more information. Therefore I'm not sure why is it important to publish the single model result? There should be more discussion about the regions where the GRISLI model shows different behavior compared to the mean ISMIP6 model results. (See also specific comments).

We acknowledge that the conclusions of our paper are not drastically different from the one in Seroussi et al. (2020). This is in part due to the fact that GRISLI shows a model response close to the mean of the ensemble of ISMIP6 participating models. However, beyond the general conclusion, we think that papers that show an individual group contribution to a large intercomparison exercise have three main advantages:
- Documentation. The model response to the forcings is clearly reported in a single model paper while this information can be buried in the community paper. The documentation of a specific model response is important to analyse any further studies that use this model.
- Climate forcing uncertainty quantification. The community paper is best suited for a quantification of the sensitivity to the choice of the ice sheet model while the sensitivity to the climate forcing is better shown for individual model.
- Model bias description. Very limited information on individual model biases is given in the community paper. Such issues are more extensively discussed in a single model paper.

We have added these ideas in the introduction section:
"The analysis of a single model response to the different forcing scenarios presents some important added value with respect to the community paper of Seroussi et al. (2020). First, single model paper allows for a documentation of a specific model response to the forcings while this information can be buried in the community paper given the large material to cover. Second, the community paper is best suited for a quantification of the sensitivity of the projections to the choice of the ice sheet model. The sensitivity to the climate forcing is better shown for individual ice sheet model. Third, single model paper can provide a more complete information of model biases."

• Apart from the standard experiments introduced in Seroussi et al., 2020, the authors added sensitivity experiments on basal drag coefficient and enhancement factor by simply changing the value proportionally. The experiments are only shortly described in the discussion without any

contribution to the conclusions. The authors didn't work deeper in this direction of studying the uncertainties from initial conditions.

Seroussi et al. (2020) only describe the results for Tier 1 and Tier 2. These experiments are limited to CMIP5 climate forcing and only cover a subset of the sensitivities to RCP/SSP scenarios, sub-shelf melt calibration and shelf collapse scenarios. Excluding the "open" experiments (which are mutually exclusive with the "standard" experiments), Seroussi et al. (2020) discuss 12 different experiments. Here we discuss 60 experiments from which new features not discussed in Seroussi et al. (2020), such as results for the CMIP6 forcing and atmospheric and oceanic only experiments.

We added this in the introduction:
"Thanks to a relatively low computational cost, we performed the full list of experiments of ISMIP6 described in Nowicki et al. (2020), where Seroussi et al. (2020) only cover a subset of these experiments."

In addition, we also performed 38x2 additional experiments with a perturbed basal drag coefficient and 17x2 additional experiments varying the flow enhancement factor. We have completely rewritten the description of the results of these perturbed experiments. This part has been also largely extended and we think it brings valuable information on the choice of the initial ice sheet state. However, in order to fully explore the sensitivity of the model results to the initial state we would have needed different initial state. For example, we could have run multiple initialisation procedures for different values of the flow enhancement factor as in Le clec'h (2019). However, the whole initialisation procedure is relatively long to perform and we have done it only for one value of the enhancement factor (=1 which allows for a good performance of the initialisation procedure).

2 Specific comments

Hyphenation should be used between adjective-noun pairs, such as "ice-sheet model", please check through the manuscript.

Hopefully corrected, unsure for some cases. There is a great variety of spellings in the published literature, even among native speakers. Eventually, the Copernicus language editing service will be able to correct the mistakes that we might have overlooked.

P1L10: 'sub-shelf basal melt' is a repeated expression. → 'sub-ice-shelf melting/melt rates'.

Corrected.

P1L22: 'increased in mass loss' → 'acceleration of mass loss'

Corrected

P2L3: 'ice sheet dynamics' → 'ice-sheet dynamics', again, please check through

Done.

P2L2:'....remains largely uncertain' need references.

We though that the 9 references in the following sentence should suffice. We have nonetheless added a reference to the special report of the IPCC (Oppenheimer et al., 2019).

P2L2: delete 'Thus, altogether' ?

Changed for “Overall”.

P2L5: a wide spread in the prediction/assessment of the magnitude

Corrected.

P2L9: cite Seroussi et al., 2020

Done.

*P3L10: I wonder if the total velocity is a weighting function of SIA and SSA as Bueler and Brown, 2009 described or simply added the two velocities? In the later case, the reference should be Winkelmann et al., 2011 (*https://doi.org/10.5194/tc-5-715-2011*).*

It is a simple addition indeed. We have added the reference to Winkelmann et al. (2011).

P3L24: ’and impose’

Done.

P3L28: ’basal drag coefficient reduced for ice thickness overestimation’, so is the next sentence ’basal drag coefficient remains...’

Corrected.

P3L28: ’e.g. basal drag reduced for ice thickness overestimation’: how does the coefficient reduce corresponding to the thickness change? The authors should describe the formula clearly, or supply the related references. Similarly, in the sentence of L30, ’The ice thickness mismatch...is used to modify the basal drag coefficient for the next iteration.’ How does the ice thickness mismatch modulate the basal drag coefficient?

We now give more information on the manuscript, including the two main equations.

P3L33: ’Le clec’h et al. (2019)’ → ’(Le clec’h et al., 2019)’

Corrected.

section 2.2 Model and initialisation: Sensitivity experiments are taken for basal drag coefficient and the enhancement factor, however, the enhancement factor is not introduced in this section. I think it’s necessary to describe the parameter, how it influence the stress field and what value do you use in the standard simulations.

We have added the following:
“As in most large-scale ice sheet models, GRISLI uses a flow enhancement factor to artificially account for ice anisotropy (Quiquet et al., 2018). In the model, we specify the value of this enhancement factor for the SIA velocity and we use a fixed ratio to determine its smaller SSA counterpart. For the experiments presented here (except in Sec. 3.2.7), we use a flow enhancement factor of 1 (no SIA enhancement) and a ratio close to 1 for the SSA (1.2:1).”

P4L8: ’an observational dataset’ → ’a combination of observational datasets’

Done.

P4L25: ’of’ → ’at’

Corrected.

P4L25–: I suggest to give the non-local quadratic parameterisation formula instead of only refer to the paper. The manuscript heavily discussed the influence of ocean forcing, such as ’sub-shelf melt rates sensitivity to temperature’ and the uncertainties related to the ’low’,’high’ and ’medium’ methods. However, It’s not explained what’s the parameter, and what do ’low’,’medium’ and ’high’ mean.

Ok, we have substantially rewritten the description of the sub-ice-shelf melt parametrisation, providing the equation and more details on the calibration.

P4L28, In the standard experiments, the gamma (sensitivity parameter) has been calibrated to reproduce the total amount of observed sub-ice-shelf melt rate around Antarctica (Rignot et al., 2013).

Thanks, we have clarified this.

P4L33, also because there are dense observational data available in Pine Island glacier.

Added.

P5L3: Maybe also label the standard calibration as MeanAnt to be consistent with Jourdain et al., 2019.

Done.

P5L4: The first sentence need a reference.

Added, Scambos et al. (2009).

P5L7: I didn’t find ’SC’ used thereafter. Is the sentence needed?

Right, removed.

P5L13: ’climate forcings (surface temperature...)’ is surface temperature implemented as a forcing?

Yes it is. The model is thermo-mechanically coupled and surface temperature is a boundary condition for the temperature diffusion.

P5L15: Which forcing is used for the ctrl experiment?

RACMO2.3p2 averaged over 1979-2016. Precision added in the manuscript.

P6L8: delete ’namely GRISLI’ ?

Done.

P6L11: 'These errors are the results of ...' I guess the errors are also from the iterative procedure of initialisation?

Not directly since we restart from the observations for the ice thickness: the errors are simply due to the drift during the 65-yr relaxation. Of course the chosen map for basal drag coefficients will drive this drift.

P6L15: What do you mean by 'most of the time' ?

Simplified:
"The differences over the East Antarctic plateau are smaller than a few metres but increases towards the ice margins or in the vicinity of major ice streams (e.g. Amery ice shelf tributaries)"

P6L19 Figure 1: It's not easy for me to tell the yellow color from white. It seems that in the Amundsen sea embayment, there are ∼50 m underestimation of ice thickness in the Getz ice shelf region but ∼50 m overestimateion in Pine Island glacier and Thwaites glacier?

We have changed the colour palette, hopefully it is now clearer. Yes, we have ~50 m overestimation in Pine Island and Thwaites glacier regions. The error in the Getz ice shelf region is slighly larger, reaching 200 m locally.

P6L20 'the Filchner-Ronne ice shelf grounding line'→ grounding line of the Filchner-Ronne ice shelf

Done.

P6L30: 'The velocity errors for the grounded part...' Why?

For a large upstream flux, mass conservation will favour a large downstream flux as well.

P6L31: 'Thus,...' need a more detailed explanation.

We give slightly more information:
"Thus, the velocity in the Ross ice shelf is largely overestimated since its tributaries show generally a large ice velocity overestimation. The western part of the Ronne ice shelf shows an opposite behaviour with feeding glaciers showing a velocity underestimation."

P6L7: It's declared in the section 2 that the initialisation method is same with Le clec'h 2019, where the basal drag coefficient is also modulated by velocity. But here you does not have any constraints on the velocities?

It is exactly the same methodology as in Le clec'h et al. (2019). There is no constraints on velocities in Le clec'h et al. (2019).

P7L20: 'This inconsistency can be due to...' Why? Could you give more specified explanation?

The control experiment should ideally have no drift in ice thickness as it is based on the assumption that the ice sheet is at equilibrium. In the Amery region we have an ice thicknening in the control, suggesting that the ice velocity should be higher. However, the ice velocity is already too high when compared to the observations. This inconsistency can be the results of a too high mass balance in the climate forcing.
We have rephrased this idea:

“The ice thickening during the control experiment could suggest an underestimation of the ice velocity, i.e. underestimation of the ice export, which seems in contradiction to the overestimation of the simulated ice velocity with respect to the observations. This inconsistency can be due to surface mass balance overestimation in the forcing in this area.”

P7L24: ’1000 km3 ’ Could you use consistent unit when mentioning the mass change? km3 , Gt or sea level equivalent? Right now all of the three units are implemented, making it hard to compare.

Sea level equivalent and total ice mass (or volume) can not be used interchangeably, since only a fraction of the total mass (or volume) contributes to sea level rise. However, we have switch for total mass (in Gt) change instead of total volume (in km^3). In doing so, the mass balance and the total mass are given in a comparable unit.

P7L26: ’...and Filchner-Ronne ice shelves’. Upstream Pine Island, Getz and Totten ice shelves are also quite high? It’s not easy to tell from Figure 2d.

The colour palette in these maps have been changed. We have added:
“Although more localised, the changes in Pine Island, Getz and Totten areas can be larger than one hundred metres per year.”

P7L32: Using ’MeanAnt’ same as Jourdain et al., 2019 instead of ’sub- shelf...dataset’ will make it much easier to follow.

We have added the label *MeanAnt*.

P8L27 & Figure 5: ’For both forcings,...’ For NorESM1-M the ice-shelf thinning of Totten ice shelf is more pronounced?

For both forcing this ice shelf has disappeared by 2100.

P8L31: delete the second ’also’.

Done.

P8L33 & Figure 5: This is a very interesting figure which could compare to the Figure 6 of Seroussi et al., 2020. There the mean model result shows an important thinning as well as acceleration in Pine Island, Thwaites and Totten glacier, while the model result for these regions are all quite stable here. However, the explanation here ’ This is likely due to the fact that our control experiment tends to produce an ice thickening in this region (Fig. 5b) which tends to stabilise this region, resulting in a smaller sensitivity’ is insufficient. Why do you have a thickening trend in the control experiment and why it results in a smaller sensitivity to climate forcings? I noticed from the equations that GRISLI implement linear basal friction law. Brondex et al., 2019 claimed that the Pine Island glacier is sensitive to the sliding laws and an exponent of 8 is suggested for the region. As descriptions of models are listed in Seroussi et al., 2020, I hope the authors can have a more specific discussion.

We have added the following elements:
“Our model does not simulate substantial changes in the Pine Island glacier area. In this region, there is a thickening of the ice sheet during the control experiment (Fig. 2b) with underestimated surface velocities (Fig. 3c). These biases can be due to the inferred basal drag coefficient during the initialisation procedure that leads to an underestimation of the velocities. The linear friction law implemented in our model can also result in an underestimation of the velocity (Brondex et al.,

2019). Finally, the biases can also be the result of the complex topographic setting that might not be well captured at 16 km. The underestimated ice sheet velocity at the grounding line in this area, together with the thickening bias, result in a small sensitivity to oceanic warming. However, for other intercomparison exercices we have shown that our model is able to produce a grounding line retreat in this area (Sun et al., 2020).

For the variety of climate forcing used, the Ross and Totten sectors are the ones that most frequently present grounding line retreat and inland thinning. The Filchner-Ronne sector presents also an ice shelf thickness decrease although associated with a limited grounding line retreat. This is consistent with the average response of the ISMIP6 participating models (Fig 6 in Seroussi et al., 2020). The lack of sensitivity of the Pine Island sector is also a feature common to other participating models since the standard deviation of ice thickness change in this area is very high (>~200 m)."

P9L6: From Figue 6 and Figure 3,4, we can see UkESM1 has more total mass loss compare to NorESM1, and their surface and basal mass balance have similar trend, why NorESM1 has ∼20 mm sea level contribution and UkESM has negative contribution? Is it because of the spatial distribution of forcing?

Until 2080, UKESM1 shows a larger surface mass balance than NorESM1 (about 200 Gt/yr difference early in the century) and a smaller basal mass balance than NorESM1 (reaching about 1000 Gt/yr difference circa 2050). With this, it is somehow expected that UKESM1 shows the largest total mass loss (ice shelf melting) but the smallest sea level contribution to sea level rise when compared to NorESM1. The spatial distribution of the forcing can explain partly the difference (NorESM1 has only a larger SMB than UKESM1 at the margins) but it is most probably of the second order in this case.

P9L13: The first sentence can be removed.

Done.

P9L16: 'scenarios'

Corrected.

P9L16: 'The model that...' the colors for the three models are really similar.

We have changed the colours used for the different models.

P9L30: Again, the comparison with the ensemble model results could be interesting.

No map showing the impact of the scenario is shown in Seroussi et al. (2020). However, the response in term of ice sheet contribution to sea level rise is discussed for two climate forcing. We have added the following in the manuscript:
"In Seroussi et al. (2020), two climate forcings (NorESM1-M and IPSL-CM5A-MR) were evaluated for both the RCP2.6 and the RCP8.5. The simulated contribution to sea level rise in the ISMIP6 ensemble is very similar to the GRISLI response: no change in grounded ice mass for NorESM1-M but an increase in grounded ice mass for IPSL-CM5A-MR under RCP8.5 with respect to RCP2.6. CNRM-CM6-1 shows a response similar to the one of the IPSL-CM5A-MR since the grounded ice mass is increasing under the SSP585 with respect to the SSP126."

P11L17: 'NorESM1-M climate forcing' → 'NorESM1-M climate forcing under RCP8.5'

Done.

P11L18: How does the decrease of surface velocity of ice shelves associated with ice thinning?

It is more the thinning that induces a velocity reduction (the SSA velocity is positively correlated with the ice thickness). Locally, the ice thinning in the vicinity of the grounding line can induce a smaller ice flux feeding the ice shelf.

P11L31: From Figure 11b, the dynamic contribution in West Antarctica has strong spatial variabilities, e.g. thinning of Siple coast and thickening in Amundsen sea region.

Yes it does although the positive contribution in the Amundsen sea region are generally small (less than 10 metres). We have added the following:
"In West Antarctica, the dynamical contribution has a strong spatial variability. It can reach up to more than 50 metres decrease in ice thickness and [...]"

P12L8: '...suggested in other studies' Could you give the numbers from these references?

We now refer to the IPCC special report here:
"A relatively moderate Antarctic ice sheet contribution to future sea level rise by 2100 has also been suggested in other studies since the IPCC special report on the ocean and cryosphere in a changing climate (Oppenheimer et al., 2019) reported a range from 30 to 280 mm SLE (RCP8.5)."

P12L9: 'One reason for this disagreement...This methodology is thus not suited...' Why this type of initialisation cause the disagreement? And is this the only reason causing disagreements?

By construction, a methodology that produces an ice sheet in equilibrium under present-day climate cannot, at the same time, reproduce the recent observed acceleration of mass loss. Other source of uncertainties are listed in the discussion and, notably, the sub-ice-shelf melt model since the largest simulated mass loss among ISMIP6 participating model is systematically obtained with ice sheet models that use their own sub-ice-shelf melt model (open experiments) instead of the standard ISMIP6 approach.

P12L19: Enhancement factor appears here for the first time. It should be defined in the methodology. And the author should explain why this parameter is interesting for a sensitivity test.

The enhancement factors are describe in the methodology section now.

Figure 12: Explain in the caption or in the text what's the meaning of positive and negative percentages.

We have added the following:
"The perturbation starts from +100% (i.e. a doubling of the base value) to -90% (i.e. a reduction to 10% of the base value)."

P13L8: '...when using the same forcing' I don't think the parameterisations in the open experiments are using the same forcing. At least for PICO, PICOP and Plume, ocean temperature and salinity are used instead of thermal forcing.

The ISMIP6 thermal forcing is also computed from the ocean temperature and salinity. But it is true that in the standard experiments, the ice sheet models do not use directly the temperature and salinity as forcing. We have changed the sentence to:

“when using forcings elaborated from the same climate model realisations.”

P13 section Conclusion: There is not much new information comparing to the Seroussi et al., 2020 paper.

GRISLI is not an outsider within the ISMIP6 ensemble and as a result the numbers given in the conclusion are not out of the ISMIP6 range. We have added few elements regarding the additional sensitivity tests we performed:
“Finally, with additional simple sensitivity tests we have shown that the simulated ice sheet contribution to sea level rise by 2100 could be largely affected by changes in ice-sheet mechanical properties such as basal dragging. Given the weak understanding on such processes, they could also represent a large source of uncertainty.”

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