**Anonymous Referee #1**

*This paper is clearly written and the figures are good. It describes the results of following the ISMIP6 Greenland experimental protocol with a particular dynamical ice-sheet model. Although this is information of use to assessing uncertainties in projections, the scientific gain is not clear. It would be useful if the authors could emphasise scientific lessons we learn from studying this model in particular, beyond its inclusion in the ISMIP6 comparisons, for example? Looking at the conclusions alone, I think a reader who is familiar with the literature of the last several years would find nothing new or surprising, for instance. However, in the paper there are a few new things which ISMIP6 is helping to clarify, and there are moreover useful things which have or could be done with this model, because it is computationally cheap, to test sensitivities.*

It seems to us that such papers that show an individual group contribution to a large intercomparison exercise present three main added values:
- It is a way to document a specific model response for a set of forcings. For example, here, GRISLI shows a sensitivity to climate forcing close to the mean ISMIP6 participating models. This is a potential important information to analyse any further GRISLI results in a broader context.
- The uncertainty that arises from climate evolution (atmospheric and oceanic forcing) can be better quantify in such paper. Although it could also be quantify in the community paper, it is nonetheless only partially address in Goelzer et al. (2020) because of too large material to cover.
- Finally, the ISMIP6 participating models use a wide range of initialisation procedure and they show various biases and model drift. Such issues cannot be discussed in the community paper while it is extensively shown here.

We have added a few information in the introduction section:
"The aim of this paper is to discuss the role of the forcing uncertainties for future projections of the Greenland ice sheet contribution to global sea level rise when using our model. This individual model response can be put in perspective with respect to the multi-model spread discussed in Goelzer et al. (2020). This paper discusses additional experiments not included in the community paper (CMIP6 forcing and separate effects of the oceanic with respect to atmospheric forcing). Compared to Goelzer et al. (2020), we provide here a more detailed description of the initial state and its associated biases and model drift. A companion paper (Quiquet and Dumas, submitted) describes the results for the Antarctic ice sheet."

*A few of my comments relate to the importance of the SMB forcing, which the paper demonstrates. It would be useful to quantify (graphically or in numbers) how much of the spread among GCMs and scenario is due simply to the time-integral of the SMB forcing (as applied to the ice-sheet model), and not affected by the ice-sheet model itself. While it is certainly necessary to use a dynamical ice-sheet model to study large changes in ice-sheets, it would be useful if the authors could present evidence for the need to use one for the 21st century (when not coupled to the atmosphere or ocean), especially as doing so introduces complications of drift and spinup, as described by the paper.*

The spread among GCMs is now shown with a plot of the time evolution of the yearly SMB spatially integrated over the ice sheet.

If we are correct, with the time-integral of the SMB, the reviewer wants to see the SMB contribution to the Greenland melt with respect to the dynamical contribution. However, the time-integral of the SMB as applied to the ice sheet model already accounts indirectly for the dynamical changes because of: i- the SMB correction for the surface elevation change and; ii- the ice mask change. As a result, the time-integral of the SMB will not reflect the impact of SMB only but also, in part, the dynamics. An alternative would be to compute the time-integral of the SMB over a constant ice sheet topography instead of using the one simulated by GRISLI. Such methods has been widely used in the past (e.g. Fettweis et al., 2013; Meyssignac et al., 2017) as it allows to compute an ice sheet contribution to sea level rise from an atmospheric model only. However this is a crude approximation since the sum will aggregate the strongly negative SMB values at the margin of the ice sheet where the ice will soon disappear and hence overestimate the ice sheet contribution to sea level rise. This overestimation has been quantified with GRISLI to be about 6% (Le clec'h et al., 2019) in 2150 (for 150 simulated years).

We think that the best way to separate the two effect is to compute the dynamical contribution to ice thickness change as explained in Sec. 3.2.3. Note that we also show in this response (Fig. R2) the integrated surface mass balance together with the dynamical contribution to ice thickness change and the ice thickness change, with the same colour scale.

*I have some concern about the prescription of the large melting near the edge (o4 line 12) and the retreat masks (p4 line 32). With both of these enforced, is the dynamical behaviour of the model distorted?*

The very negative SMB outside the present-day ice mask can be seen as a way to correct two type of biases:

- For some areas, the atmospheric forcing computed by MAR presents a positive annual value (ice accumulation) outside the observed present-day ice sheet mask. Uncorrected, this atmospheric forcing bias will result in an overestimation of the ice sheet extent and thickness.
- We use an inverse procedure to infer the basal drag coefficient that best represent the observed ice sheet thickness. By constraining the extent of the ice sheet with an artificial negative SMB, we infer a basal drag coefficient that best reproduce the dynamical behaviour of the ice sheet since the marginal slopes are closer to the observations.

This artificial negative SMB correction does not directly alter the dynamical behaviour of the model but it prevents any ice advance in the future. However, it is probably very marginal for the Greenland ice sheet in the future.

The glacier retreat parametrisation is slightly different. It could alter the dynamics since it is related to an imposed changed in ice thickness. However this is done on purpose, in order to account for a sub-grid process that is not accounted for otherwise. The effect of the glacier retreat can be quantified thanks to the AO experiments. To answer your concern, we have computed the dynamical contribution to ice thickness change (former Fig. 8) for the AO experiments compared to the full forcing experiment. In fact there is only very limited difference between the two (less than 10 metres difference).

*p1 line 10-11. I would not jump to such a strong general conclusion (also on p7).*

Reformulated to:
"Amongst the models tested in ISMIP6, the CMIP6 models produce larger ice sheet retreat than their CMIP5 counterparts."

*p1 line 17-18. I don't think that this statement (of a most likely contribution of 1 m from ice sheets by 2100) is a correct representation of the current state of scientific knowledge. In the first place, you can't state a likelihood independent of scenario, since there are no probabilities for scenarios. Bamber et al. write "For a +5degC temperature scenario, more consistent with unchecked emissions growth, the [median and 95-percentile] are 51 and 178 cm, respectively." I'm not sure what "most likely" means, but 1 m is twice their median. Also, Bamber et al. report an expert elicitation, whose reliability is debatable since it's opaque. For comparison, the AR5 assessment of* *the likely range of ice-sheet contributions by 2100 under RCP8.5 is 0.09 to 0.28 m from Greenland and -0.08 to 0.14 m from Antarctica.*

We agree with the reviewer. We have chosen to cite the Special Report on the Ocean and Cryosphere in a Changing Climate (SROCC, Oppenheimer et al., 2019) instead of Bamber et al. (2019) here. We have reformulated:
"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)."

*p2 line 1. Why "asymptotic"?*

"approximations" has been replaced by "expansions" since SIA and SSA are the series expansion truncated at the order 0 of the Stokes equation. As such, they are asymptotic expansions.

*p3 line 4. I suppose that strictly you could say an ice-sheet model satisfied momentum conservation, but as far as I know this model and others used for such purposes do not contain terms for acceleration or inertia. That is, momentum is always negligible, and they assume a balance of forces at all times.*

Rephrased to: "[…] that solve the mass conservation and force balance equations".

*p3 Eq 1. I think that BM is a single quantity, isn't it? Typeset like this in a formula it looks exactly like the product of two quantities B and M (like Ubar H is a product). It would be clearer to use a single symbol. Is it just the surface mass balance, or is basal mass balance included too?*

BM has been replaced by M. It is the total mass balance (including basal mass balance). It is now specified in the text.

*p3 line 11. "the total velocity is simply to superposition of the two main approximation". I would suggest "the total velocity is the sum of the velocities predicted in their respective areas by the two main approximations".*

Thanks for your suggestion. We prefer to avoid the use of "respective areas" though, since both velocities are computed for all glaciated grid point. We reformulated as:
"For the whole geographical domain, we assume that the total velocity is the sum of the velocities predicted by the two main approximations: [...]"

*p3 line 15. "for which there is infinite, respectively none, friction at the base." I think this should read "for which there is infinite friction at the base or none, respectively." "None" is a pronoun, not an adjective.*

Thank you, it has been corrected.

*p4 line 5. How accurate are the SMB and the surface topography in the control state?*

The surface mass balance used for the control simulation comes from MAR v3.9. This present-day reference climate is also the one used for the initialisation procedure. This has been clarified:
"This present-day reference climate forcing is used for the initialisation procedure and for the control experiment *ctrl*."
MAR v3.9 is one of the few regional climate models that have been extensively validated against observations. On top of the two reference papers cited, there is an extensive literature that shows the

model performance. We think that MAR offers an accurate representation of the present-day climate over Greenland even though, as any model, it might present some biases (for example a possible overestimation of the precipitation in South-East Greenland, discussed in the manuscript).

Since there is virtually no floating points in the model, the simulated surface topography in the model is the sum of the bedrock topography with the ice thickness. Isostasy being desactivated (now stated in the manuscript), the bedrock topography remains to the one in the observational dataset (Morlighem et al., 2017). Thus, the simulated topography accuracy in the control experiment can be measured by the error on the ice thickness, discussed in Sec. 3.1.

*p4 line 11-14. Does this term strongly interfere with, or even overwhelm, the simulated discharge across the grounding line?*

No, it is only a way to prescribe an ice extent that fits the ice sheet mask in the observations. It has consequences on the initial ice mask and topography and as such it defines the ice dynamics in the initial state (through surface slopes and basal drag coefficient). However, it does not interfere with potential changes in the ice dynamics.

*p4 line 24. State that these are vertical gradients. I would say that they are vertical gradients of surface quantities in the atmosphere model, rather than in the atmosphere.*

Right, we have followed your suggestion:
"[...] yearly values of vertical gradients in the atmospheric model for these two surface variables are also provided."

*p5 line 11. branched to -> branched from.*

Corrected.

*p4 line 21-22. What do you need the surface temperature for, if you're using SMB as forcing?*

Surface temperature is a boundary condition for the temperature diffusion equation. Since the model is thermo-mechanically coupled, temperature affects ice velocities (through viscosity). It will also play a role on the thermal conditions at the base of the ice sheet which also affect ice velocities (frozen grid-points have an infinite friction at the base).

*p5 line 22, p8 line 3, p11 line 11, Fig 5 caption. Although the reader may sympathise with the authors, it's better to avoid "pessimistic" and "optimistic", which are value-judgements.*

Replaced by high emission and low emission scenarios.

*p5 last para. I don't understand the reason for these two experiments. Do they start from the same initial state? Since they have the same forcing, they ought to evolve identically.*

The experiments *ctrl* and *ctrl_proj* have two different initial states since they start at two different dates: 1995 and 2015, respectively. The *ctrl* experiment can be used to quantify the drift in our model during the whole time period (including the historical and the projection). In turn, the advantage of the *ctrl_proj* experiment is to be directly comparable to the projection experiments as they cover the same time period and they use the same initial state (which was not the case with the *ctrl* experiment). To clarify this point, we added the following:

“The *ctrl* experiment can be used to quantify the simulated model drift over the whole time period (1995-2100). Instead, the *ctrl_proj* can be directly used to quantify the importance of climate forcing evolution since it uses the same initial state in 2015 as the different projection experiments.”

*p5 line 34. alike -> like.*

Corrected.

*p6 line 22. best -> better.*

Corrected.

*p6 line 24. "In doing so" means doing what? - absolute or logarithm? I would have assumed logarithm, but the next sentence suggests otherwise. What are the units of 0.55? What are the units of velocity before taking the logarithm? (Strictly you can only take the log of a dimensionless quantity, but the conversion factor between different velocity scales will be a constant offset in the log so doesn’t affect its RMSE, I suppose.)*

We meant logarithm of the velocity (expressed in metre per year but as you rightly point out an other choice will not affect the RMSE). We have rephrased to:
“When using the logarithm of the velocity GRISLI slightly improves compared to the other participating models since the RMSE is about 0.55 (eleventh worst value out of 21). This means that the error [...]”

*p6 line 30. Why is this "on the contrary"? If I read this correctly, all the errors are in the same direction (too slow in the model). Can you suggest the reason for this systematic bias? What implication does it have for projections?*

It should have been “On the contrary, the South East glaciers, Kangerdlugssuaq and Helheim, are too fast in the model.” (and not “too slow”). There is no systematic biases for the velocity: amongst the largest ice streams, the NEGIS, Petermann and Jakobshavn are too slow but the Kangerdlugssuaq and Helheim are too fast.

*p7 line 4-5. What implication will this bias in SMB have for projections?*

It is difficult to give a definite answer to this question. An overestimation of the precipitation might moderate the effect of the expected decrease in SMB in the future. However a too wet climate could also be the sign of a too intense penetration of warm (thus humid) air over this area which could also facilitate melting at high elevation. Such atmospheric processes are best quantified with dedicated atmospheric model experiments.

*p7 line 9 and 15. Are these large drifts in thickness and velocity related? What effect will they have on projections? It’s not obvious that you can simply subtract an unforced drift when it’s large compared with the forced response.*

The drift in thickness and velocities are related since the two variables are tightly coupled together in the model. However, we think that the velocity drift mostly derived from the ice thickness drift. For example, the ice thickness drift in South-East Greenland near the Helheim glacier is negative, which induces a decrease of the ice velocity (less ice to export).

In the paper, the plots of the time evolution of integrated variables show the control experiments (i.e. the drift) as well as the projections without the drift subtraction. We subtract the drift only for

2D maps to better highlight the impact of climate change. However, the drift shown in Fig. 1 (original manuscript) is small when compared to the ice thickness change induced by climate change.

*p7 line 18. start by -> start with.*

Corrected.

*p7 line 21-24. Presumably this spread comes mostly from the spread in SMB forcing from the GCMs. Could you also add the ice-sheet area- and time-integral of the SMB perturbation to the graphs?*

In addition to the response we made earlier on your main comment, we can add a few information here. We have preferred to not plot the time integral of the mean SMB over the ice sheet since it may be more difficult to interpret than the yearly evolution. The time integral of this variable is essentially positive with only negative values for some models towards the end of the century. This is because the area-integrated SMB becomes negative only after 2060 for some models (and remains positive for others). Since the simulated ice sheet shows only a small drift in the control experiment, the positive area-integrated at the beginning of the simulation is almost balanced by the melt at the base of the ice sheet and the calving flux. Thus, the time integral of the spatial mean SMB draws an incomplete picture of the evolution of ice volume and does not allow for a separation of the ice dynamics versus SMB contribution.
Nonetheless, to show the spread amongst GCMs, we have added the time evolution of the SMB integrated over the ice sheet mask and added a few sentences:
“The differences in ice volume evolution are tightly linked to the surface mass balance evolution for the different climate forcing. Amongst the CMIP5 climate models, IPSL-CM5-MR and MIROC5 simulate a mean surface mass balance negative as early as 2060 while it remains positive over the next century for CSIRO-Mk3.6 (Fig. 4).”

*p7 line 21-24. It seems that these projections imply quite a low sensitivity to climate change compared with the models on which the AR5 was based; their assessment of the Greenland contribution by 2100 under RCP8.5 is 90-280 mm, of which 40-220 mm is from SMB change.*

Although slightly smaller perhaps, GRISLI shows a climate sensitivity close to the mean of the ISMIP6 participating model. The community paper (Goelzer et al., 2020) reports a range of 70-135 mm (mean of 100 mm) using MIROC5 RCP8.5 while GRISLI shows a range of 75-95 mm (low to high oceanic sensitivity) under the same forcing. This is now specified in the text:
“The 2100 sea level contribution simulated by GRISLI is close to the mean model response amongst the ISMIP6 participating models.”
The numbers in the AR5 for RCP8.5 were larger (Table 13.5 reports 0.07 to 0.21 m from which 0.03 to 0.16 m due to SMB change). However, these estimates were derived only from a small number of studies/models, compared to the 21 ice sheet models in ISMIP6. They were also obtained sometimes with a simpler methodology : the Special Report on the Ocean and the Cryosphere in a Changing Climate (SROCC) reports a median value for process-based approaches of 11.9 cm under RCP8.5.

*p7 line 25. What sort of "tipping point" do you have in mind, that you might see in the volume evolution? Can you give references to relevant suggestions?*

We were imprecisely referring to a sharp change of slope. This has been reformulated:
“However, we can not distinguish any sharp inflexion in the volume evolution over the next century.”

*p7 line 26-27. I think we should be more cautious in drawing conclusions. There are only four CMIP6 models considered in this study, out of dozens in total, and two of the four are at the edge of the CMIP5 distribution in your projections. Only two show much greater sensitivity, and those results are within the AR5 range.*

Thanks for pointing this out. We have reformulated:
"CMIP6 models show generally a much larger Earth climate sensitivity than their equivalent in the former CMIP5 generation (Forster et al., 2020). In particular, the CMIP6 models used in ISMIP6 have an Earth climate sensitivity from 4.8 to 5.3, i.e. larger than the CMIP5 models used here, which show a range from 2.7 to 4.6 (Meehl et al., 2020)."

*p8 line 6-7. It's not the GHG itself which is the driver, but the warming it produces; that is also the reason why the rate of mass loss goes up with time, and the main reason for the spread among models.*

Reformulated:
"The future atmospheric and oceanic warming induced by the greenhouse gas mixing ratio is thus a major driver for the Greenland ice mass at the century time scale."

*p8 line 13-14. Since the point you wish to make is the similarity of the patterns, it would be better to show these maps divided by the integrated change in each case i.e. normalised to the same GMSLR contribution. That would reveal the patterns themselves, so they could be compared, which I agree should be the purpose of this figure.*

Such figure is shown below in this response (Fig. R1). It is true that the new figure shows nicely the similarity of the patterns for the different GCMs. However, we think that the absolute ice thickness change for a given climate forcing is more informative for the reader as it is a way to show how the volume change (integrated value) translates into ice thickness change. However, if the reviewer believes that we should add this figure in the supplementary material, we would be happy to do so.

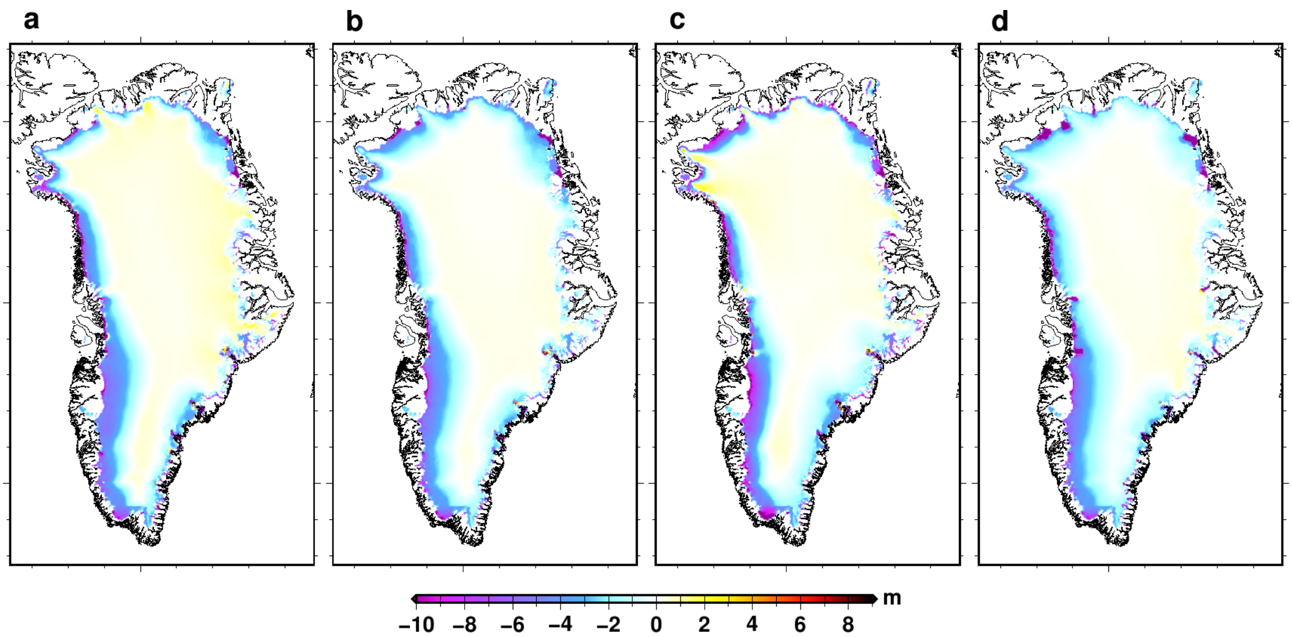


**Figure R1.** Simulated ice thickness change (2100 – 2015) normalised its spatial average (i.e. volume change) for: (a) CSIRO-Mk3.6 (RCP8.5); (b) MIROC5 (RCP8.5); (c) MIROC5 (RCP2.6) and; (d) UKESM-CM6 (SSP585) climate forcing. The medium oceanic sensitivity has been used for this figure, except for UKESM-CM6 (d) for which we use the high oceanic sensitivity.

*p9 line 6. It would be interesting to see the time-integral of the applied SMB perturbation here, to compare with the AO experiments (as I also suggested on p7 for Fig 3). Any difference is due to the dynamical response to the SMB forcing.*

The integrated SMB indirectly accounts for dynamical changes. First through the elevation feedback on SMB with the vertical lapserate. Second because the ice mask can change due to ice dynamics. will reflect indirectly the dynamical response, through the elevation change correction and ice mask change. We do not think that such a figure will allow to distinguish the dynamical response from the SMB forcing.

In Fig. R2 of this response, we show the integrated surface mass balance together with the dynamical contribution to ice thickness change and the ice thickness change, with the same colour scale.

*p9 lines 16-23. The text says "Fig. 8b shows the difference in ice flux convergence in 2100", and the fig caption says "change in the dynamic contribution to ice thickness change in 2100". I don't think either of those is a correct description, if I have understood correctly. You also say, "This can be considered as the dynamical contribution to ice thickness change," which I think is correct. The quantity shown is the difference (change in topography during the experiment) minus (time-integral during the experiment of the local mass balance change with respect to control) - is that right? It would be useful to compare this difference with the change in topography in the same experiment, using the same color scale, in order to see the relative importance of the dynamical change. If it's a small fraction, you might argue that there's no need to use a dynamical ice-sheet model for projections on this timescale. Where it's not small, you can comment. Part of the dynamical contribution near the coast is a response to the ocean forcing, I presume. Therefore it would also be useful to show the same comparison for the AO experiment. That is, would it be good enough to make the projection without a dynamical model, simply by time-integrating the local SMB perturbation?*

Yes you are right with the definition and thank you for pointing this terminology inconsistencies. It is now referred as “dynamical contribution to ice thickness change” throughout the manuscript.

We have added the ice thickness difference in Fig 9, to compare with the dynamical contribution to ice thickness change and added a few information in this manuscript:
“The integration in time of Eq. 1 over 2015-2100 suggests that the integrated ice flux convergence is the difference between the ice thickness change from 2015 to 2100 and the integrated mass balance (surface and basal mass balance and calving) over this period. The integrated ice flux convergence can be considered as the dynamical contribution to ice thickness change. It should be noted that the integrated mass balance here also includes the effect of ice mask change and surface elevation change. As such, it is not comparable to what would have been obtained with an atmospheric model only. Fig. 9b shows the difference of the dynamical contribution in 2100 for a selected climate forcing with respect to the control ctrl_proj experiment. The pattern mostly follows the one of velocity change (Fig. 9a). There is an important positive dynamical contribution to ice thickness change (ice flux convergence) at the margins that tends to partially compensate the decrease in surface mass balance. Conversely, upstream regions show a slightly negative dynamical contribution (ice flux divergence). This pattern is similar amongst the different climate forcings. To compare the relative importance of the dynamical contribution with respect to surface mass balance to explain the ice thickness change we show the ice thickness change in 2100 with the same colour scale in Fig. 9c. The dynamical contribution shows generally much smaller value suggesting that surface mass balance explains the largest changes in ice thickness. However, locally, for example in the South-East and central West regions the dynamical contribution can be the largest driver of ice thickness change.”

Fig. R2 is the same as Fig. 9 in the paper, the only difference is that it shows the integrated surface mass balance as well. The dynamical contribution is directly constructed from the difference of the ice thickness change and the integrated total mass balance (from which surface mass balance is the main driver). In the paper, we keep the version of the figure with the dynamical contribution to ice thickness change together with the ice thickness change, but we omit the integrated surface mass balance since we do not think it brings an additional value.

There is virtually no change in the dynamical contribution to ice thickness change when comparing the standard experiment to the AO experiment. The glacier retreat parametrisation can be seen as a calving process. It implies a slightly greater ice thickness change but its effect is affected to the integrated mass balance change (which include surface and basal mass balance in addition to calving). The difference in thickness and surface slope change between the AO and standard experiment does not seem to be sufficiently large to affect the ice dynamics.

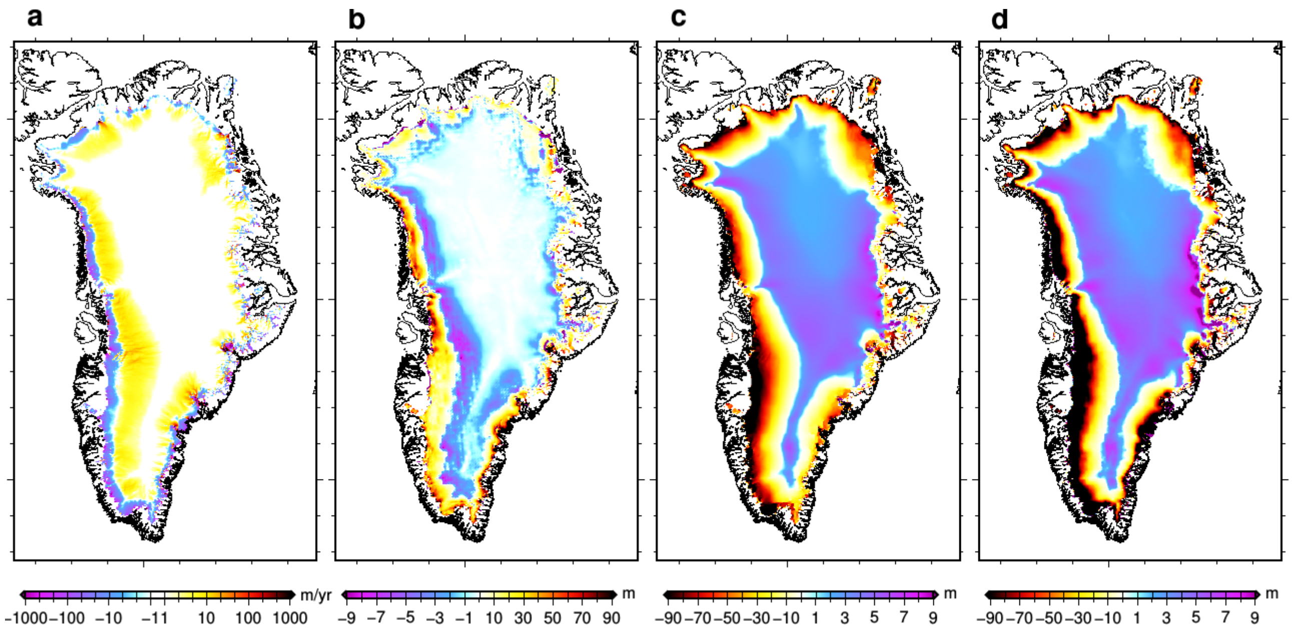


**Figure R2.** Simulated surface velocity change during the projection run (2096-2100 with respect to 2015-2019) using MIROC5 forcing under RCP8.5 with a medium oceanic sensitivity. **b**: change in the dynamical contribution to ice thickness change in 2100 (see text for definition) for this same experiment. **c**: simulated ice thickness change (2100-2015). **d**: time integral of the surface mass balance (2015-2100). For all panels, we corrected the changes by the ones simulated in the control experiment *ctrl_proj* over the same period. Note that the colour scale is not symmetrical for **(b)**, **(c)** and **(d)**.

*p9 line 30. As a guide to the possible magnitude of this underestimate, you could state what the presently observed ice-sheet imbalance would give if it continued as a constant rate to 2100 and compare with your projected changes in response to forcing.*

This is a very interesting point indeed, and maybe one of the major point of this paper but also the community paper. Our ice sheet models do not reproduce the recent accelerations and as such probably bias our projections towards low estimates. We have added the following:
“This means that, by constructions, our simulations underestimate the Greenland ice sheet contribution to future sea level rise. A simple linear extrapolation of the 2006-2016 rate (0.77 mm $yr^{-1}$, Oppenheimer et al., 2019) up to 2100 would result in a 6.5 cmSLE from the Greenland ice sheet. This number is large compared to the GRISLI results discussed in this paper, and more generally it is large compared to the spread amongst ISMIP6 models (3.5 to 14 cmSLE, Goelzer et al., 2020). This suggests that model initialisation is one of the largest source of uncertainty for

model projections. Instead of using a methodology that produces ice sheet at equilibrium, some promising alternatives exist, [...]”

*p10 line 4-16. This is useful, but it’s not really discussion, I’d say. It’s another sensitivity test, and it would go well in sect 3.2.3 about change in ice dynamics.*

We have moved this part in the Sec. 3.2.3.

*p10 line 20. Why is it necessarily an overestimate?*

Because the diffusion of the cold temperature within the ice sheet is not accounted for. This is now clarified:
“Our internal temperature field is the result of a long thermo-mechanical equilibrium under perpetual present-day forcing and as such, it is necessarily overestimated since the diffusion in the ice sheet of the cold temperature of the glacial period is not accounted for.”

*p10 line 27-29. Yes, it would! Since your model is particularly computationally inexpensive, please could you do it and tell us the answer? :-)*

Since we think that it makes little sense to perform long multi-millenial integrations with a constant prescribed basal drag coefficient, we are currently working on the calibration of the model parameters for an interactive computation of the basal drag coefficient as in Quiquet et al. 2018. However, although our model is relatively cheap it nonetheless currently requires 11 days on our local computers to perform 10 kyr with the 5-km grid resolution used in the paper. Hopefully in the future we will be able to show the behaviour of our model for two completely independent initialisation procedure.

*p10 line 31-32. Could you quantify the elevation-SMB feedback here, or earlier, and compare it with Edwards et al. (Cryosphere, 2014)? You could directly quantify it by running a sensitivity test in which the lapse-rate adjustment is excluded, I suppose.*

We have performed a sensitivity experiment using MIROC5 RCP8.5 and the medium oceanic sensitivity in which we did not account for the lapse-rate correction. We found a reduction by 5.1% of the Greenland contribution to sea level rise in this experiment with respect to its counterpart in which the correction is applied. This number is close to the 4.3 reported by Edwards et al. (2014). We have added the following:
“The forcing methodology used for ISMIP6-Greenland does account for the vertical elevation feedback on temperature and surface mass balance. In order to quantify the impact of this correction on the simulated evolution of the ice sheet, we run a sensitivity experiment in which this correction is not accounted for. Using MIROC5 under RCP8.5 scenario with a medium oceanic sensitivity, we simulate a Greenland contribution to future sea level rise 5.1% smaller in this sensitivity experiment compared to the same experiment in which the vertical correction is applied. This number is slightly higher than the effect reported by Edwards et al. (2014) and Le clec’h et al. (2019a) (4.3 and 4.2% respectively) but smaller to Vizcaino et al. (2015) (8-11%) and Calov et al. (2018) (about 13%). Differences in resolution and/or physical processes implemented in the atmospheric model could explain this diversity.”

*p11 line 8. is systematically loosing -> systematically loses*

Corrected.

*Fig 1 caption. Does "respective to" mean "with respect to"? For clarify please state the years of the end of the historical and end of ctrl_proj.*

Done.

**References**

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