Johannes Sutter

Quiquet and Dumas present the results of the GRISLI-LSCE contribution to ISMIP6, which is an extensive model intercomparison showing the 21st century evolution of the Antartic Ice Sheet (AIS) under a variety of climate scenarios (Seroussi et al. 2020). I think it is a worthwile excercise to present individual model contributions of ISMIP6 in depth, as due to page limitations and readability the main ISMIP6-paper can only illustrate the general findings of the model intercomparison in broad strokes. Therefore, in my opinion this manuscript is well suited for the scope of The Cryosphere.

An in-depth analysis as the authors attempt here should identify the key features as well as strenghts and weaknesses of the individual model contribution so the reader can appreciate the respective models skills and peculiarities when it comes to projecting the future evolution of the AIS.

The authors present interesting details regarding their model projections and how they differ from or agree with the ISMIP6 ensemble. The paper generally reads well and the figures are of good quality. To make this a valuable addition to the "TC-ISMIP6-canon" I would suggest a number of modifications and extensions mainly with regard to the Results-section as well as some stilistic overhaul to improve the general readability.

Thank you for your thorough reading and your positive appreciation. In the following, we provide a point by point response to your individual comment.

I will first list general comments pointing out where certain sections need more substance to elevate this manuscript above a mere documentation of GRISLI ISMIP6-results, followed by specific point by point edits/comments to the text.

1. (section 3.1 Present-day simulated ice sheet)
The authors discuss the modelled present day state of the AIS in detail covering mostly thickness and velocity changes in the different regions of the ice sheet. This gives a nice first impression as to how well GRISLI is capable to reflect the current available observations. If I understand correctly the underlying assumption of the initialisation procedure was to create an ice sheet in equilibrium with the late-20th century mean climate state as opposed to one with ongoing mass loss. If this is correct it could be stressed more, and the consequences of the initialisation for the projection runs (potentially to stable) should be discussed. Furthermore, it would be really interesting to hear the authors opinion on inverting for ice thickness versus inverting for surface velocity. How are the ice sheet's future regional dynamics primed in the projections as a result of the inversion approach? What is the advantage/disadvantage of thickness inversion (e.g. realistic inital geometry/unrealistic flow patterns) in comparison to velocity inversion (e.g. realistic initial ice dynamics/unrealistic surface elevation)? Also, the authors focus a lot on ice shelf thickness and area changes which is important for buttressing and thus marine ice sheet stability. However, the ice thickness close to the grounding line is probably also an important indicator whether the initial ice sheet configuration is resistant to grounding line retreat or facilitates the latter. In general, it would be nice to have a more explicit discussion as to how the initial state of the ice sheet impacts the projections.

The fact that it produces an ice sheet in quasi-equilibrium with the present-day climate forcing is an important feature of our initialisation procedure and we agree that it should have been discussed more. In the method section, we have now added:
"It should be noted that such initialisation procedure produce an ice sheet in quasi-equilibrium with the late-20th century mean climate state. By construction it does not simulate the accelerated mass loss observed in the last decades (Rignot et al., 2019)."

And in the discussion:
"This methodology is thus not suited to reproduce the recent acceleration in mass loss, particularly important in West Antarctica (Rignot et al., 2019). For example, a simple cumulative value of the observed 2012-2017 loss rate (219 Gt yr^{-1}, The IMBIE team, 2018) from 2015 to 2100 will result in an Antarctic ice sheet contribution to sea level rise of 52 cm SLE. This number is much greater than the simulated contribution by GRISLI and more generally, it is much greater than any ISMIP6-Antarctica participating model simulated contribution. This highlights the importance of initial conditions for century scale projections."

About inverting the ice thickness or the ice velocity. Ice thickness (or surface elevation) inversion procedures should, in principle, provide an ice velocity close to the observations (as it should correspond to the balance velocity). Locally, it presents nonetheless some important differences with the observations, differences that can ultimately bias the projections. Compared to the inversion of ice thickness, the inversion of ice velocity can provide a mean to reproduce the recent trend in the observations. To date, only adjoint-based approaches have been followed to inverse the ice velocities. Instead of inverting ice velocities, in future developments of our model we plan to modify the target of the inversion procedure by adding the recent observed ice thickness changes to the observed ice thickness.
We added this in the discussion:
"Assimilation of surface velocities in transient ice sheet simulations are promising methodologies to overcome the limitations inherent to methods that assume steady state (Gillet-Chaulet, 2020). However, they require a complex modelling framework not currently implemented in our ice sheet model. In future developments of our model, we plan to modify the target of the inversion procedure by adding the recent observed ice thickness changes to the observed ice thickness. This would provide a more realistic initial state for the projections."

With respect to the initial state. It is not straightforward to assess the sensitivity of the results to the initial state. We only have one initial state after the initialisation procedure. An alternative could have been to perform multiple inversion procedure for different values of the enhancement factor for example as in Le clec'h et al. (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). This question of the sensitivity of the results to the initial state was part of the objective of the sensitivity experiments in which we apply uniform perturbation to the basal drag coefficient and/or to the enhancement factor. This part has been completely rewritten and extended.

2. (section 3.2.4)

The modelled grounding line response in the Ross and Filchner Ronne sectors seems to be very large for higher sensitivity runs if forced by e.g. NorESM1-M as discussed by the authors and shown in Figure 5 d. I suggest to expand the discussion of this response a little shedding light on the mechanisms and whether this response differs from the ISMIP6 ensemble substantially. Is this solely due to the strong forcing of NorESM1-M in these sectors or is there a model dependence if comparing the different ISMIP6 ensemble members?

The sector of the Ross ice shelf, together with the sector of the Totten ice shelf, are the two regions that present the largest ice thickness decrease when looking at the average response of the ISMIP6 participating models (Seroussi et al., 2020, Fig. 6a). The Filchner-Ronne ice shelf sector also shows a large ice thickness decrease although this decrease is more localised close to the grounding line. Our results are thus close to the average model response within the ISMIP6 ensemble for these regions. The Pine-Island sector is the one that present the largest standard deviation of ice thickness change amongst participating models (Seroussi et al., 2020, Fig. 6b), with some models that retreat substantially and other (like GRISLI) that not simulate any substantial changes. Here again, the response of GRISLI lies within the range of ISMIP6 models.

The strength of the oceanic forcing explains why this sector retreat in our simulations. For example, we have a much more limited retreat for IPSL-CM5A-MR since this model has a much smaller thermal forcing in the Ross basin.

We have added this discussion in Sec. 3.2.1 as it is the first time we show the pattern of retreat:
"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)."

3. (section 3.2.5) I think it is an important finding that ice shelf collapse does not seem to have a considerable effect until the year 2100 at least for GRISLI. Section 3.2.5 should be expanded by a discussion as to why this is the case. Did the authors carry out longer projections under ice shelf collapse (e.g. until the year 2300/3000)? Is MISI initialised in certain regions for longer simulation times? Or is the model setup so stable as to not allow MISI (doesn't seem to be the case if looking at Ross Sea grounding line retreat under NorESM RCP8.5 forcing). Is this result similar to the ISMIP6 model ensemble (i.e. do all models show negligible grounding line response to ice shelf collapse untill 2100 CE?), or how does GRISLI differ here? As of now this section is very short and does not really allow for an assessment how sensitive this GRISLI setup is with regard to removal of buttressing force.

The community paper only discussed the impact of ice shelf collapse under CCSM4 (medium oceanic sensitivity). For this forcing, the retreat mask by 2100 has removed ice shelves in the Peninsula and in the Pine Island sector, but affect only very marginally the other ice shelves. First, these sectors are poorly sensitive in GRISLI. Second, CCSM4 is one of the forcing that produces an ice volume evolution mostly driven by the increased precipitation (very limited oceanic forcing with respect to atmospheric forcing, Fig. 5). The ice shelf collapse induces a smaller ice extent (150000 km^2 reduction) compared to the standard experiment and, as a result, a smaller integrated surface mass balance. This is why for this forcing the ice shelf collapse is associated to a decrease of the Antarctic contribution with respect to the standard experiment, except in the last 15 years of the century. As a result the impact of the ice shelf collapse is limited with this forcing (2.6 mm SLE) when compared to the average ISMIP6 models (28 mm SLE). However, the standard deviation amongst the ISMIP6 models is also very large, suggesting that some models also show a low sensitivity to this process.

The retreat mask are computed from the outputs of climate models and do not go beyond 2100. Thus, we cannot perform longer experiments. However, we participated with GRISLI to the ABUMIP project in which we quantified the impact of ice buttressing on the simulated ice sheet. To do so, from an equilibrated initial state (different from the one used here), we removed the ice shelves and performed 500 yr simulations. We show in Sun et al. (2020) that GRISLI is able to simulate an important grounding line retreat of the West Antarctic ice sheet (including the Pine Island sector) when the buttressing force is removed.

We have expanded the discussion on the ice shelf collapse scenarios:
"A greater sensitivity to this process has been reported in Seroussi et al. (2020) (multi-model average of 28 cm SLE in 2100 under the CCSM4 forcing) although associated a wide spread of

response amongst participating models. For most climate models, the retreat masks by 2100 have removed the ice shelves in the Peninsula and in the Pine Island sectors, but affect only very marginally the other ice shelves. In the standard experiments, these sectors show a low sensitivity to the oceanic forcing. In fact, even under the strongest oceanic forcings, GRISLI shows there a limited grounding line retreat. This suggests that the buttressing force is not the reason why the model does not retreat in these sectors. Instead, it is most likely the topographic biases in the initial state that make the model weakly sensitive to the oceanic conditions. Using a different initial state, we have shown in a recent intercomparison exercise (ABUMIP, Sun et al., 2020) that we were able to simulate large grounding line retreat when the buttressing induced by the ice shelves is removed."

4. (Discussion)
Here, the authors discuss a parameter sensitivity study not shown in the results section. Is this on purpose? I would suggest to include a section in the Results and present the main findings of these experiments there. As for Figure 12 I suggest to include a graphical aid for the reader which delineates what the authors think is a realistic parameter range (e.g. good fit to present day observables). I assume the fringes of the parameter range would generate an ice sheet configuration which are not in agreement with the general present day features of the AIS.

This part has been moved to the results section. It has been considerably extended.

We have also performed an additional set of experiments for which we apply the perturbations to a control experiment. These perturbed control experiments helped us to define the range of acceptable values for the perturbations. To define this range we have selected the perturbed control experiments that show a mass change lower than 0.15% with respect to the standard control experiment (no perturbation). We chose 0.15% of mass change as it represents one tenth of the simulated ice loss in 2100 when using NorESM1-M under RCP8.5 with a medium oceanic sensitivity. The ranges now appear on the figure.

Point by point edits/comments:

general points:
-review your use of "important" (e.g. important acceleration, p1, l18) throughout the manuscript. Important for what? This is very implicit. I know what you mean but the word "important" should be replaced by an explanation of why the change is relevant throughout the manuscript.

Done.

-check throughout manuscript "consists in" and change to "consists of" where applicable.

Done.

-check your use of "pessimistic" and "optimistic" scenario and replace with e.g. "unmitigated" and "strong mitigation scenario" or alternatively just with the official CMIP abbrev.

Changed to "low/high emission scenario"

-check use of "All together" and replace by e.g. "Overall"

Done.

-check use of word "systematic" throughout the manuscript.

Done.

-you use the form "on the one hand ... on the other hand" exhaustively, especially in the second half of the manuscript. This is not technically wrong, but it would improve the reading experience if you use other forms to express contrasting things from time to time.

Done.

-for sake of readability I suggest to modify occurrences of ice volume changes and write in exponential form (e.g. 3e5 km3 instead of 300000 km3) and provide the sea level equivalent volume change in brackets right after.

We have changed the units, the volume change is now expressed as a mass in Gt. We have adopted an exponential form. However, we prefer to keep separating the discussion of total mass change and sea level equivalent as the two numbers show two different evolution.

Abstract:

p1,l2 this sentence could be changed to: The Antarctic ice sheet's contribution to global sea level rise over the 21st century is of primary societal importance and remains largely uncertain as of yet.

Done

p1,l2-3: ISMIP6 itself suggests a range from negative to positive sea level contribution, while you write "from a few milimetres to more than one metre". This seems inconsistent to me.

We were referring to the list of papers cited in P2L6-8 of the original manuscript. However, since the ISMIP6 community paper is now published we should include it. Changed to:
"In particular, in the recent literature, the contribution of the Antarctic ice sheet by 2100 can be negative (sea level fall) by a few centimetres to positive (sea level rise) with some estimates above one metre."

p1, l5-6 I suggest to omit: "While in a companion paper we present ..." and shorten the sentence to "Here, we present the GRISLI-LSCE contribution ...".

Done.

p1,l8 omit "of sea level equivalent".

We prefer not. It does not make sense to give a volume change in mm if there is no indication of area, does it?

p1, l9: suggest to rephrase to "... of the ice shelves resulting in grounding line retreat while increased precipitation partially mitigates or even overcompensates the dynamic ice sheet contribution to global sea level rise."

We have followed your suggestion.

p1, l12: change "retreats" to "retreat" and check use of retreats throughout the manuscript.

Done.

p1 l12-13: change to "... in ice sheet models for projections of the Antarctic ice sheet's evolution."

Done.

p1, l17 include reference of potential total sea level equivalen ice volume (e.g. ∼58.3 m BEDMAP2 or ∼57.9 m BEDMACHINE).

Done:
"Given its size, the Antarctic ice sheet represents the largest single potential contributor in the future, as it represents 58 m of sea level rise if melted completey (Fretwell et al., 2013; Morlighem et al., 2020)"

p1, l18 rephrase this sentence and include reference, suggestion: "While the ice sheet was probably in a quasi mass-equilibrium in the eighties (citation?), it has since then lost ice at an accelerated pace, reaching a yearly sea level contribution of up to 0.7 mm yr-1 during the last decade (...)"

Suggestion followed, reference Rignot et al. (2019).

p1, l21: replace "inexorably" with "irreversible".

Done.

p1, l22: change to "While the increase in mass loss is mostly associated with ocean warming, the increased precipitation ..."

Done.

p2, l3: "the projected sea level contribution"

Done.

p2, l4. please rephrase model formulation, unclear what you mean here. "Overall, the uncertainties related to XY"

Done.

p2, l9: "... contribution to ISMIP6-Antarctica in detail, while its …"

Done.

p2, l34: what about shorter timescales such as the one you are looking at here? Has GRISLI taken part in e.g. MISMIP? please elaborate.

No, we have not performed the MISMIP experiments. We have slightly expanded the presentation of GRISLI:
"For century timescales, with the same model version that the one used here, we participated to initMIP-Antarctica (Seroussi et al., 2019), ABUMIP (Sun et al., 2020) and LarMIP (Levermann et al., 2020). Slightly earlier version of the model has been used to simulate the evolution of the Greenland ice sheet until 2100 and 2150 (Peano et al., 2017; Le clec'h et al., 2019a)."

p3, l10: "... and the total velocity results from the addition of the …"

Done.

p3, l20: "initialisation procedure consists of ... which aims at determining the geographical …"

Done.

p3, l23: "... under a constant present day …"

Corrected.

p3, l31-32: please rephrase this sentence, unclear and poor style.

This sentence no longer exists. We now provide more information about the initialisation procedure.

p4, l9 "is derived from" ?

Changed.

p4,10. "The geothermal heat flux is taken from …"

Corrected.

p4, l19 "is derived from"

Changed.

p4, l30: "GCMs".

Corrected.

p5, l12: "... as the initial …"

Corrected.

p5, l15: unclear what you mean by " ... even though the time evolution has no incidence on the forcings."

Rephrased:
The *ctrl* experiment starts in January 1995 and ends in Decembre 2100, even though it uses a constant present-day climate forcing (RACMO2.3p2 averaged over 1979-2016).

p5,l15: if I understand correctly you are using annual forcing, so I guess you can omit the specification of the month.

Yes, you understand correctly. However, we prefer to keep the specification of the month, for consistency with the community paper and also because we believe it makes it easier to know exactly the length of the different simulations. For example it is not necessarily obvious to know if a 1995-2100 simulation includes the year 2100 or stops after the computations for the year 1999.

p5, l21: unclear: are they branched of from the historical experiment at 2014?

Rephrased:
“The different ice sheet projection experiments start in January 2015 and they are all branched from the end of the historical experiment *hist* (Decembre 2014). They end in Decembre 2100 (86 simulated years).”

p5, l27: using different sub-shelf melt rate sensitivities ..

Corrected.

p5, l28 : ...of the sub-shelf melt model calibrated …"

Corrected.

p6, l1: rephrase " In order to allow for the interpretation of the model response to the forcings, a control experiment, ctrl_proj , has been performed in addition to the ctrl experiment. As in the ctrl experiment …"

Done.

suggest to omit the month as it is not relevant for the simulations

We prefer to keep the month for consistency with the community paper and also it might provide a more precise idea on the beginning and end of the simulations.

p6,l 19: check your use of "important" and rephrase with an explicit description of what the relevancy is.

New version:
“There are large ice thickness underestimations, locally reaching more than 200 metres, in the Getz ice shelf region in the Amundsen sea and upstream the grounding line of the Filchner-Ronne ice shelf.”

p6,l21: the extend

Rephrased:
“The ice front of the Ross and Filchner-Ronne ice shelves is located about 80 km away from the observations.”

p6,l26: rephrase "location and magnitude" and check use of "important"

Reformulated:
“The model generally reproduces the pattern and the magnitude of the observed surface velocities, depicted in Fig. 3b, even if substantial errors remain (Fig. 3c).”

p6,l28: rephrase to "Surface velocities of the major tibutaries of the Ross ice shelf (Mercer- and Williams Glacier) and the Filchner-Ronne ice shelves (Foundation Glacier) are largely overestimated (include range here, e.g. up to factor 2 or what-ever it is)"

New version:
“Surface velocities of the major tributaries of the Ross ice shelf (Mercer and Williams glaciers) and Filchner-Ronne ice shelf (Foundation glacier) are largely overestimated (locally up to a factor 4 with errors larger than 1000 m yr^{-1}).”

p6, l31. explicitly explain why ross ice shelf is largely over- and Ronne ice shelf under estimated.

These biases mostly come from the limitations of our initialisation procedure in which some glaciers can show important velocity errors. We added:
"The velocity errors for the grounded part of the ice sheet mostly explain the velocity errors for the floating ice shelves. 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."

p7,l2: which other regions could be affected by the 16 km resolution, i.e. where do you think the grid size plays a dominating role on projections?

Added:
"More generally, spatial resolution could explain most of velocity errors in the coastal regions where topography together with spatially variable surface mass balance and sub-shelf melt exert a strong control on simulated velocities."

p7,9-10: suggest to provide names of ice sheet models which use velocity inversion in ISMIP6 so the reader can compare in Seroussi et al. 2020.

The initialisation procedure used by the different model is listed in Tab. 3 of Seroussi et al. (2020). Velocity inversion is used in DA and DA+ models. We now provide a few examples:
"[…] ice sheet models that use the velocities in their initialisation procedure (e.g. JPL1_ISSM, UTAS_ElmerIce)."

p7,l21 "mass balance uncertainties" - please specify regions with large mass balance uncertainties so the reader can grasp where these are relevant.

There are large mass balance uncertainties all over Antarctica primarily due to the sparse distribution of observational data (e.g. GLACIOCLIM-SAMBA). P7L21 of the original manuscript discusses the biases in the Amery region and it is not intended to discuss Antarctic-wide biases.
We have added some elements for the biases in the Amery region:
"This inconsistency can be due to surface mass balance overestimation in the forcing in this area. This overestimation could be corroborated by the fact that another regional climate model than the one used here simulates a surface mass balance 30% smaller than RACMO2.7 in the Amery region (Agosta et al., 2019)."

p7,l33 "... acceleration of ice volume loss over the course of the century."

Corrected.

p8,l1 as suggested in the general points please write volume changes in exponential form and provide sea level equivalent changes right after in brackets.

We have kept separated the discussion for the total mass change from the ice mass contributed to sea level rise. We changed the notation though.

p8,l6 "(i.e. above floatation)"

Thanks for noticing. Corrected.

p8,l9 This is not necessarily the whole story as mass gains in grounded ice above sea level could overcompensate mass change in marine ice sheet regions. Please elaborate.

Locally, this is indeed possible. However, when looking at the spatially integrated numbers, such overcompensation is not reached in the experiments discussed here.

p8,l9 : rephrace to "ice shelve volume is shrinking over the course of the century"

Done.

p8,l10 : please elaborate "ambivalent"

Reformulated:
"This means that the ice shelves are reducing in volume for all forcings while the grounded ice volume can increase or reduce depending on the forcing used."

p8,l11 replace "perpetual" with "constant"

Replaced.

p8,l18 what is the reason for the decresased surface mass balance in HadGEM2-ES? If I plot precip alone over the AIS I get an ~30% increase. From Seroussi et al. 2020 I gather that surface mass balance anomalies are computed from "changes in precipitation, evaporation, sublimation, and runoff".

The HadGEM2-ES surface mass balance anomalies in the future are positive for elevated areas (>2000m) but negative for the coastal areas. The anomalies have been indeed been computed from precipitation minus evaporation minus runoff. We do not have locally the different variables to check but it is possible that the model simulate increased runoff in the future.

p8,l18 I assume you mean: basal melting underneath ice shelves is increasing?

Yes, we have followed your suggestion.

p8,30: "... wide spread thickening of the grounded ice sheet"

Corrected.
p8,31: rephrase to "When using NorESM1-M this thickening is present to a lesser extent and compensated by the thinning that results from the grounding line retreat in some areas (Ross or Totten ice shelves for example)."

Thanks, done.

p9,l7: "Similar to CMIP5 climate models, the CMIP6 …"

Changed to: "Similarly to CMIP5 climate models, the CMIP6 [...]"

p9,l10 is this also the case for the ISMIP6 ensemble or a specific feature of your model?

The CMIP6 model results have not been studied yet within the ISMIP6 ensemble.

p9,16 "largest" instead of "greater"??

Yes, corrected.

p9,l19: "maintain" what?

We meant: “[…] are able to survive in the course of the century.”

p9,l24-25: please rephrase these sentences, I know what you mean, but the formulation as it stands is unclear.

Reformulated:
“However, compared to the high emission scenario, the simulated total ice mass evolution using the low emission scenario is closer to the mass evolution of the control experiment. This means that, in this case, the simulated ice sheet changes in the future are dampened with respect to an higher emission scenario.”

p9,l29: again, "maintain" what??

We meant: “[...]” are able to survive until the end of the century [...]”

p10,l2: "The computation of the sub-shelf melt rate ..."

Done.

p10,l2 unclear what you mean by "largely derived". I guess the basal melt rate is tuned to observational data.

Clarified:
“[…] is a parametrisation tuned to reproduce a combination of observational datasets (Jourdain et al., 2019).”

p10,l13: "the NorESM1-M forcing under RCP8.5 ..."

Corrected.

p10,l26-28: please completely rephrase this sentence.

Rephrased:
“These models show a limited sub-shelf melt (Fig. 5b) and one of the smallest ice mass loss in the future (Fig. 7a). Thus, they produce a large ice shelf extent with respect to the other climate models. CNRM-ESM2 and CNRM-CM6-1 also simulate a pronounced atmospheric warming in the future. This warming is indirectly visible in Fig. 5a since the precipitation increase is primarily driven by the increased temperature. The atmospheric warming together with the large ice shelf extent explain why the CNRM-ESM2 and CNRM-CM6-1 models show the largest mass loss resulting from ice shelf collapse.”

p10,l30: suggest to omit "Respective" in section header

Done.

p11,l12-14. Please rephrase.

Rephrased:
"Conversely, the total ice mass change (Fig. 11a) mostly reflects the mass loss from the ice shelves which respond primarily to the oceanic forcing. The ice shelf mass loss in the OO experiments can be large with an important acceleration in the last 20 years of the century. This late response might be a reason why the volume above floatation is not drastically different from the control experiment in the OO experiments."

p11,l19: suggest to rephrase to : "Modelled grounded ice surface velocity changes are limited with the notable ..."

Suggestion followed.

p11,l26: "Another way ... this century …"

Corrected.

p11,l27: replace "different natures" with "different causes"

Done.

p12,l1: replace "somehow" with a quantification.

The quantification is given at the beginning of the sentence ("a few centimetres"). We have replaced "somehow" by "slightly":
"In East Antarctica there is a widespread very small (a few centimetres) negative dynamical contribution to ice thickness change (ice thinning) that slightly moderates the ice thickening due to increased precipitation."

p12, l4: unclear what you mean by "in line". Close to ensemble mean?

Yes, rephrased:
"Although close to the ensemble mean of the ice sheet models participating in [...]"

p12,l8 :"(e.g. Bamber et al. ...)"

Corrected.

p12,l28: "Such an approach ..." please quantify "much more computationally expensive"

Added:
"Such approach is much more computationally expensive since it requires multiple regional climate model simulations. For example, the MAR regional climate model (Agosta et al., 2019) requires about 15 days to compute 100 years (C. Agosta, personal communication). That is why this approach has been discarded so far for the Antarctic ice sheet where [...]"

p13,l1: "While the atmospheric forcing ...

Done.

p13,l13: "providing the means to investigate"

Corrected.

p13,l20: "partly mitigating or over-compensating the effect of loss of buttressing due to ice shelf melt."

Done.

p13,l23: "do not drastically change the simulated ice sheet volume ..."

Corrected.

p13,l24: "...emission scenarios..."

Corrected.

p13,l24: replace "present" with "exhibits"

Done.

General point for the volume figure captions:

you often use the sentence " Simulated ice volume change for the historical experiment hist (1995-2015), the control experiments ctrl (solid grey lines) and ctrl_proj (dashed grey lines) and for the projections using climate models run"
which is a bit bulky and only after that the description of what the panels show follows.
For sake of readability I suggest to modify the respective captions so it reads: "Simulated ice volume change and sea level contribution for projections XYZ ..." and in the end include a sentence stating that the plots begin with the historical run and that ctrl and ctrl_proj are depicted in gray (dashed and solid).

Thanks for the suggestion. We have followed your advice.

Figure 9: How come that for some experiments the AIS sea level contribution is negative for ice shelf collapse in comparison to standard approach? This should be discussed in the results! It seems only those runs which show AIS growth in standard approach show a relative AIS mass loss in the shelf collapse scenarios.

It is true that it is counter-intuitive. For some climate forcings, the ice shelf collapse scenario produces a local thickening in the vicinity of some grounded line. This is mostly related to local non-linearities. We have added the following:
"The impact of the ice shelf collapse scenario on the sea level contribution ranges from -8 to +17 mm SLE. This range is much smaller than the range of the simulated sea level contribution for the different climate models (-50 to 70 mm SLE). Surprisingly, for some models, the ice shelf collapse scenario contributes negatively to the sea level contribution (e.g. UKESM1-0-LL). This is most probably due to local non-linearities of grounding line dynamics. However this effect is limited to small changes in the grounded volume."

CCSM4 shows a negative contribution to future sea level rise with an evolution very similar to CNRM-CM6-1, CNRM-ESM2 and CESM2. However, while the shelf collapse scenario increases the contribution to sea level rise in 2100 for CNRM-CM6-1, CNRM-ESM2 and CESM2, it has a negligible impact on CCSM4. It is therefore not obvious to draw a general conclusion.

Figure 12: it would help if you indicate the parameter range which produces a "realistic" present day ice sheet with respect to observations for present day forcing, so the reader can identify which parameters are still "OK" to use. Also please remove double brackets e.g. ((a) and (b)) -> (a and b).

We have added a vertical grey band for the acceptable range (volume change in perturbed control with respect to the standard control lower than 0.15%). Double brackets removed.

References

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