Response to Referee Paolo Scussolini

General comments

The manuscript deals with the interesting and conceptual question of the evolution of sea levels in the coming millennia, and employs a Earth system model of intermediate complexity that is adequately equipped to address the question.The piece is fairly well-written, and the structure of the sections is suited to present and contextualize this set of results. The set of experiments with the extension of the RCP scenarios and the two additional scenarios crafted by the authors is well planned. Also, the implementation of the impulse response function seems well executed. The discussion of the results in the light of evidence and modeling of past, present and future sea levels is comprehensive and updated to the latest literature. The explanations for the changes observed from each component of the model are convincingly argued. Still, a large number of mistakes and imprecise statements exist; see below a long list of suggested corrections. Although these points of attention are many, I still consider that revisions necessary for the manuscript to reach publishable form are minor, and no further experiment nor analysis is necessary.

Author's response: Thank you very much for the detailed comments and the positive review.

Specific comments

The motivation for studying very long term climate evolution and equilibrium and sea levels is attempted, but it is not very convincing in my view. I think the authors could make a better case for the focus of this study: what gain does this knowledge represent for science or society? To play the devil's advocate, why not waiting a few years until we have a firmer grasp on sub-scale mechanisms of ice sheet loss, before attempting such long-term projections? The urgency of information on outcomes for the next millennia is not self-evident.

Author's response: The argument to wait a few years to get better insight in the sub-scale mechanisms of ice sheet loss is not entirely convincing. Models are continuously being improved but will never be a perfect match of reality. The reviewer possibly alludes to processes such as grounding-line mechanics or basal sliding. However on a millennial timescale, the mass loss of the Greenland and Antarctic ice sheets is mainly dominated by surface mass balance processes and feedbacks that play between the different components of the Earth's system, and less so by the details of the ice dynamics. This study also tries to investigate the effect and magnitude of these feedbacks. From a political point of view, there is a need to investigate the consequences on the very long term to inform decisions to be taken now. In our opinion, this is a valid motivation.

Also, the authors should acknowledge somewhere the somewhat speculative nature of the exercise.For example, it seems plausible that existing carbon capture technology will reach scalability (e.g., Wilberforce et al., 2019;https://doi.org/10.1016/j.scitotenv.2018.11.424), if not in the coming years or

decades,plausibly in the coming centuries. These solutions would make it possible to actively reduce CO2 levels, thus questioning the relevance of strong statements in the abstract, introduction and conclusions like 'long lifetime of atmospheric CO2', 'will continue torise on a multi-millennial timescale even when anthropogenic CO2 emissions cease completely' and 'irreversible'. A qualifying statement about the uncertainty associated with these long-term outcomes could be added in (some) of those places.

Author's response: We thank the reviewer for this suggestion. We took more care about the statements on irreversibility. Clearly the reviewer is right that irreversibility is a strong statement and that it depends much on future technological developments. We made clear that for our RCP-based scenarios, sea-level rise is irreversible in our simulations, but that we ignore a scenario where carbon dioxide is very efficiently extracted from the atmosphere to achieve pre-industrial CO2 levels. On the other hand, we do not exclude the use of carbon capture and storage technologies, since our lowest forcing scenario MMCP2.6 includes negative emissions during the 21st century.

I would reorganize sections, to account for the fact that section 6 is actually a part of the discussion. Maybe the discussion can be organized in two (or more) separate parts, one of which would deal with the contextualization of the results vis a vis the geological record.

Author's response: The reviewer is right that section 6 could be part of the discussion. However, we believe that section 6 is a logical bridge between the results section and the discussion of our results with respect to other studies on future ice sheet melting and sea-level rise and prefer to keep the structure as it is.

In the methods (L 104): the 'application' of temperature and precipitation 'anomalies' seems very important and requires further explanation: what is done with these two variables precisely? Does this step include statistical downscalingand/or bias-correction?

Author's response: The climate model output is bias corrected by applying the temperature and precipitation anomalies with respect to a reference period (1970-2000 AD). No statistical downscaling is explicitly included.

I second the inclusion of the additional scenarios MMCP-break and MMCP-feedback, but the motivation behind these choices should be made explicit.What type of situation and uncertainty do they aim to represent and address?

Author's response: We considered scenario MMCP-feedback to give a measure of the uncertainty that arises when global surface temperatures will increase significantly. In such a warm world, it is possible that other feedbacks arise that we wanted to account for. The reason to include MMCP-break is to have an intermediate scenario between MMCP6.0 and MMCP8.5. There is a large spread in global emissions and radiative forcing for these two scenarios in comparison to the other RCP scenarios. This information is added in the manuscript.

In various places in the paper, you bring attention to the process of ‘haline contraction’. I am not an expert in this aspect, but I suggest more clarity should be made here. Since the phenomenon under analysis here is sea level rise and not fall, and since the ocean is made less saline by addition of freshwater and thus water is made to expand, ‘contraction’ seems a misleading term. Commonly, this component of steric sea level change is considered very much second order. I cannot find a quantification in this paper, but if it is indeed the case that salt changes are very minor compared to the other processes included, maybe negating special mention to them is warranted and improves clarity.

Author’s response: This is a very good suggestion raised by the reviewer. After the model description, where the difference between thermal expansion and haline contraction is explained, we further refer to this combined term as the steric sea-level change component, which is the general term for thermosteric sea level change (thermal expansion) and halosteric sea level change (haline contraction).

Technical corrections

Abstract

L 27: is it not ‘greenhouse forcing’? Climate should be a product of the climate model.

Indeed, this is adapted.

L 28-29: I am not sure it is clear what the methane feedback does in the model, and why it is only switched on for the highest emission scenario. If this is too complex to beexplained in an abstract, maybe only mention in two words or leave out.

The methane feedback is now explained in a few words in the abstract.

L 34-35: this sentence makes little sense with no further explanation: what can proxies tell (directly) about the future?

This sentence is removed from the abstract.

Introduction

L 47: the statement on the ice sheet response time will be read as if associated with empirical evidence, whereas it is based – if I am not mistaken - on a highly conceptual paper one-dimensional model in a very dated study. Please qualify, and if applicable add empirical evidence or more recent science.

The sentence is rephrased and a more recent reference is added.

L 48-49: sea level change goes in both directions, so words like ‘expansion’ and ‘contraction’ seem in need to be complemented here.

We believe that these concepts are explained in the next few lines.

L 51: arguably Greenland and Antarctica are also ‘ice caps’? I suggest changing term for low-latitude mountain ice masses.

The large ice masses on Greenland and Antarctica are referred to as ice sheets. There is a clear distinction between ice sheets and ice caps in terms of ice extent and ice thickness.

L 54-55: the authors will have very good insight in this process thanks also to their modeling work, but this formulation of the long-term adjustment of thermosteric sea level may be misleading: once surface temperatures are stable it would seem that the heat exchange between the ocean and the comparatively thin mass of atmosphere and land surface will only continue for a short time (probably not ‘millennia’) before becoming negligible. Please consider this also in the light of your results.

This is a good remark from the reviewer and we changed this towards ‘multiple centuries’, in line with our results.

L 56: ‘steric sea level change’?

Done.

L 63-64: what part of the ocean-atmosphere coupling did those studies take into account, and which did they neglect?

We thank the reviewer for this good suggestion and we made clear how the ice sheets were interacting with the climate system for these different studies.

Why ‘full’ ocean-atmosphere coupling is most important due to a process between the ice sheets and the oceans?

We clarified this by changing the sentence towards ‘the full coupling between the ice sheets, the atmosphere and the ocean”. The freshwater fluxes also have a cooling influence on the atmosphere.

L 69: other EMICs exist beyond LOVECLIM that strike a balance between complexity and computational pragmatism. Please consider rephrasing to make more clear what are the specific merits of LOVECLIM here.

The merit of LOVECLIM is the inclusion of a fully coupled ice sheet component. It is further explained in the model description that this is a rare component for EMICs.

Also, ‘fully integrated coupling’ is the same as ‘full coupling’?

Indeed, this is changed to ‘full coupling’ to avoid confusion.

L 74: the reference for the ECP is necessary here. Also, ‘to span the likely range in climate uncertainty’ is not clear. Emission scenarios are meant to address emission uncertainties, rather than ‘climate uncertainty’. Last, I don’t think ‘in a warming climate’ reflects what you have implemented in these experiments.

The reference is added and the term ‘climate uncertainty’ is changed to ‘emission uncertainties’.

Model description

L 80: the last millennium is frequently simulated also with GCMs, see Jungclaus et al. 2017 (10.5194/gmd-10-4005-2017). Transient simulations, such as across full deglaciations, or full glacial-interglacial cycles, are rather a specialty for EMICs.

We added the information that Earth System Models with a higher spatial resolution are expected to be able to simulate the last millennium (Jungclaus et al., 2017), in addition to the EMIC’s that used to be the only tool.

L 91: consider adding specification of what you mean here by computational time, or it is rather meaningless: on how many cores/processors, are simulation executed in parallel fashion,...?

We do not have a good record of the spent core hours and therefore decided not to include specific numbers on the computational time.

L 92: SLE abbreviation does not seem useful.

We would like to use the abbreviation since it is used on line 94 and line 433.

L 106: PDD? Also, there is no discussion of the specifics for the Greenland part of the ice sheet model.

PDD is replaced by Positive Degree Day and the model is explained in more detail. The Greenland ice sheet model code is similar to previous model versions and we added references for the interested reader.

L 127: maybe change to ‘get ice sheets in equilibrium at 1500 AD with climate (forcing?)’?

It is the other way around: ‘to get the climate in equilibrium with the ice sheets at 1500 AD’.

L 128: ‘without seeing the changes of the climate model’ is not clear. Further, you refer to the ice sheet model as something external to the ‘climate model’, which in turn you have not defined. This is confusing as previously you implied that also the ice sheet component/model is part of the EMIC.

This sentence is rephrased towards: ‘the ice sheet models in this experiment evolve very slowly for a fixed climate’. In these precursor experiments, there is not a full coupling between all climatic components, but all model components will be in equilibrium at 1500 AD after which the fully coupled model run starts.

L 130: It is not clear why a different run is necessary to assess the drift with this ‘quasi-equilibrium’ run. Note that ‘quasi-equilibrium’ is not defined.

Our goal was to have a climate in equilibrium at 1500 AD. From 1500 AD, the model was forced in fully coupled mode with PMIP3 data. We performed the quasi-equilibrium run (the run where the climatic variables converge to a nearly constant value) to assess whether this statement was indeed true.

L 138: what do you mean by 'initial' here: are the differences in initial conditions applied at the end of the semi-equilibrium spin up, and to which component? Is this explained in the next sentence? If so, please check the terminology, i.e. are 'ensemble of five members' and 'five iterations of the reference state' the same thing reworded?

The initial conditions are slightly different for the boundary conditions of the atmospheric component. These initial conditions are applied at the end of the quasi-equilibrium run. The 'ensemble of five members' and five iterations of the reference state' are not the same and these sentences are rephrased for clarity.

Scenario description

L 145: Are Multi-millennial concentration pathways introduced here for the first time? Please clarify in the manuscript.

Yes, we explicitly mentioned now that they are introduced here.

L 158: Please explain that MMCP-feedback is based on RCP 8.5.

Done.

L 162: Maybe the methane release should be specified (also) as a rate here.

Done.

Also,instead of 'by adding constantly CO2 after 2250 AD', please explain that in the simulation it is assumed that all released CH4 instantly converts to CO2. Please also consider whether it is necessary to argue that this instant conversion is a warranted simplification, since the reader will know that a molecule of methane exerts much more greenhouse effect than a molecule of carbon dioxide, and this process may not be negligible even if it takes place on a time frame much shorter than the overall simulation length.

The explanation that all CH4 instantly converts to CO2 was present on L166. We added the remark that this simplification neglects the short-term warming effect of methane.

L 167: referring to the figure here would seem appropriate.

Done.

L 175 on: please revise this whole paragraph, as I am not sure that I can follow properly your explanation here.

Some parts of this paragraph are excluded to improve clarity and to avoid confusion. The main message is that methane and nitrous oxide have high natural emissions in a warmer world and therefore they do not seem likely to be reduced during the next 10,000 years.

L 183: 'included in the climate forcing' here is confusing. It implies that the model at the centre of attention here is only the ice sheet part, whereas you are running coupled climate experiments, for which the orbital forcing is external forcing.

This is rephrased.

L 186: this is misleading. Solar forcing for the future is not 'unknown': its orbital part is very well known, whereas what is unknown is the evolution of solar cycles.

This is rephrased.

L 188 on: please reword to 'The following sections show...'. Also, the ensuing list is not clear, it reads as if the climate responds to the sea level change. Please reword.

Done.

L 190 and other instances: you use the term 'haline contraction', but is it the case that the ocean becomes more saline and contracts in your simulations? If not, then the term is misleading.

This term is changed now and is called 'the steric contribution' to sea level changes.

L 219: change to 'i.e., the difference between accumulation...'

Done.

L 220: 'for all the forcing scenarios'

Done.

L 222: instead of the vague 'in a high warming scenario', please refer specifically to the scenario has you have named it.

Done.

In sections 4.3 and 4.4, and figure 2 and 3, there is confusion. Is SMB the same as mass balance in the figure? In the text SMB is the difference between accumulation and ablation, but the figure reports accumulation, calving and runoff, and apparently not the SMB, nor the amount of ice at any given moment, which would also seem a useful metric.

We thank the reviewer for this good remark. The SMB is indeed the difference between accumulation and ablation. The surface runoff is equal to ice melt and liquid precipitation minus retention. We chose to use this term because it also gives an idea

about the magnitude of freshwater coming from the surface. The other freshwater comes from ice discharge and basal melting below ice shelves (only for Antarctica). The total volume of ice that is lost is not practical for our purposes since not all ice that is lost contributes to sea-level, which is still our principal variable to investigate.

As a suggestion, the tiles of sections 4.3 and 4.4 could mention also ocean currents, since these results are also prominent there.

This is a good suggestion and we changed the titles towards: 'The Greenland ice sheet and the AMOC' and 'The Antarctic ice sheet and AABW'.

L 238: punctuation is missing.

Punctuation is added.

L 259: since glacial isostatic processes are included, consider mentioning this when mentioning the model description. Are these processes carried out by the is the land-surface module, which if I am not mistaken is part or ECBilt?

No, the glacial isostatic adjustment model is a component of the ice sheet models for Greenland and Antarctica. This is now clearly mentioned in the model description.

L 282: although an asymptotic behavior seems to emerge for all scenarios, it would be interesting to mention (and later discuss?) the late convergence between SLR of the 2.6 and 4.5 scenarios, which seems unexpected to me.

It is attempted to explain this by the slow melting of the Greenland ice sheet for scenario MMCP2.6. The overall ice sheet contribution is in the end very similar, part of the West Antarctic ice sheet retreats and the entire Greenland ice sheet.

L 291: after 10000 years of simulation, or after year 10000 of the simulation?

This has now been clarified by the statement: 'at 12,000 AD'.

L 298: what do you mean by 'inferred'? is it reconstructed/measured by use of/in proxies?

We mean 'reconstructed', and changed the terminology accordingly.

In the next sentence, I suggest adding mention of which two periods those combinations of sea level and CO2 concentrations refer to.

Done.

Next sentence still: I suggest mentioning here Figure 6.

Done.

On figure 6: it is bizarre that it does not show the -120m sea level for 180 ppm mentioned in the text, I guess because the figure only uses Foster and Rohling 2013

and that work did not include such low stand. Nevertheless,because that extreme is irrelevant to the range of values here, it seems acceptable.

Further in fig. 6: what is the vertical line, pre-industrial concentration?

While mentioned in the text, there is no red line for the linear fit in figure 6.

The vertical line is pre-industrial CO2 concentration. We added the red line as the best fit for the data.

L 315: eliminate 'both of'.

Done.

In this context, a test to assess the likelihood that the data from this study belong to the distribution of data from the geological records would seem informative.

We consider a likelihood assessment here out of the scope of our comparison. Moreover, the comparison between the geological data and the data from this study clearly shows a similar trend.

L 318: thermal expansion seems quite flat for all scenarios, 10000 years into the simulations, so this can't plausibly be a contribution to further sea level rise that's meaningful for the scale of fig. 6.

The reviewer is right here and we excluded the last argument.

Discussion

L 321: this does not seem a suitable reference for future sea level rise. Many good references for this have already been cited in the introduction. Further, it's puzzling to see the discussion open with a contributor to SLR that is not the most relevant at present and by far not in the time-scales of this study.

We gave a better reference regarding sea-level rise during the 21st century due to glacier melting. The logic that is followed is to start with the components having a large contribution to sea-level at present (glaciers and ice caps and the steric contribution) and to discuss the components with a larger uncertainty (the ice sheets) afterwards.

L 322: are RCPs more appropriate than MMCPs here?

We wanted to use the introduced MMCP naming here to make the distinction between the numbers from our study (MMCP) and those from other studies that simulate changes up to the end of the 21st century (RCP).

L 330: are these numbers on the steric contribution from this study? Please clarify.

Yes, this is clarified.

L 333: what do you mean by 'updated', is this a different generation of climate models?

The term ‘updated’ refers to the use of coupled models, while the former numbers don’t include the influence of ocean circulation changes on the steric contribution. On the other hand, the study of Hieronymus (2019) also doesn’t include the coupling with ice sheets.

L 335: verb tense is wrong.

This is corrected.

L 336-337: it should be stated more clearly that the models included in the reference cited do not have coupling between ocean and ice-sheets (if that is the case).

Done.

L 339: ‘local annual mean temperature’ and ‘mean SAT’ seem to mean the same thing here, but different terminology is confusing.

We changed ‘local annual mean temperature’ to ‘mean SAT’.

L 389: it is not clear whether the impacts on AABW and sea ice formation are from this study or from the references listed.

The impact on AABW and sea-ice formation are from the references listed.

Conclusion

L 406: Related to one of the main points above, it seems inappropriate to state that SLR is irreversible. That is, from your scenarios and results it appears irreversible absent active anthropogenic carbon sequestration, i.e., under the debatable assumption of no anthropogenic alteration of the carbon cycle beyond the atmospheric emission of CO2 and methane.

We brought more nuance to the formulation.

L 408: to ‘simulate’ ‘in the real world’ seems an oxymoron.

We removed ‘in the real world’.

L 431: change to ‘or the Antarctic ice sheet.’

Done.

Fig. 1, unlike other figures, has the two additional scenarios in dashed lines instead of solid lines. Whereas the reason is given in the caption, this lack of consistency across figures and panels is not advisable.

This is adapted and changed to solid lines.

Fig. 5a lacks the legend for the scenarios, which is all the more confusing because colors in 5b are used to another purpose.

A legend is added for Figure 5a.

Also, the caption may be confusing: is this GMSL due to all relevant processes, or is it necessary to list them all here making the reader think that maybe some other process is left out?

We changed the caption to ‘GMSL rise during the next 10,000 years’, to avoid confusion.

Finally, I am not sure that panel 5b is the most efficient way to show the timing difference between (cumulative) emissions and sea level change. A plot of those two quantities against time would have several benefits compared to 5b: it would show the timing aspect more clearly, it would show the scenarios, and would be much easier to read. Please take this as a suggestion.

We thank the reviewer for the suggestion. A plot of GMSL rise against time, showing the different forcing scenarios is already given in Figure 5a and we believe Figure 5b is a valuable addition by showing the sea-level rise in terms of cumulate CO2 emissions for different time snapshots.