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Interactive comment on "Semi-equilibrated global sea-level change projections for the next 10 000 years" *by* Jonas Van Breedam et al.

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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.

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.

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 to rise 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.

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. 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 downscaling and/or bias-correction? 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?

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.

Technical corrections

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

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 be explained in an abstract, maybe only mention in two words or leave out.

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

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 one-dimensional model in a very dated study. Please qualify, and if applicable add empirical evidence or more recent science.

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

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

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.

L 56: ‘steric sea level change’?

L 63-64: what part of the ocean-atmosphere coupling did those studies take into account, and which did they neglect? Why ‘full’ ocean-atmosphere coupling is most important due to a process between the ice sheets and the oceans?

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

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.

Model description

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

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,. . .?

L 92: SLE abbreviation does not seem useful.

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

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

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.

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.

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?

Scenario description

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

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

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

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

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

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.

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.

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.

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.

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

L 220: 'for all the forging scenarios'

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

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Discussion paper

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

L 238: punctuation is missing.

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?

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.

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

L 298: what do you mean by 'inferred'? is it reconstructed/measured by use of/in proxies? In the next sentence, I suggest adding mention of which two periods those combinations of sea level and CO2 concentrations refer to. Next sentence still: I suggest mentioning here Figure 6. On figure 6: it is bizarre that it does not show the -120 m 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: hat is the vertical line, pre-industrial concentration? While mentioned in the text, there is no red line for the linear fit in figure 6.

L 315: eliminate 'both of'. 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.

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.

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.

L 322: are RCPs more appropriate than MMCPs here?

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

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

L 335: verb tense is wrong.

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).

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

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

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

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

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.

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

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