Review of Mas e Braga et al. "Sensitivity of the Antarctic ice sheets to the peak warming of Marine Isotope Stage 11"

June 10, 2020

General comments:

The manuscript by Mas e Braga et al. presents simulations with the ice-sheet model SICOPOLIS of the Marine Isotope Stage 11c (MIS11c). This interglacial period is close to pre-industrial conditions in terms of orbital parameters and therefore a good analogue for present-day conditions. The authors perform a number of sensitivity experiments in which they investigate the influence of different parameters (e.g. sea-level, initial geometry etc.) on the evolution of the Antarctic ice sheet. They also attempt to identify key processes that may also be relevant for future ice-sheet changes.

I find the topic and the idea of the paper interesting and the results should be of interest to the cryospheric modelling and paleoclimate community. Overall, I think the manuscripts presents enough novelty and hence merits publication. However, the presentation of the results in its present form is not ready for publications and I recommend the authors take into account my comments listed below. I hope the authors find my comments helpful.

Specific comments:

Main concerns:

1. In its present form, I find the manuscript hard to follow in many places (more in the technical corrections below) and really difficult to judge the results of the simulations because there is almost no description of the ice-sheet model and its boundary datasets and conditions. I suggest to expand section 2.1 to add this required information. To be more specific, what type of stress balance does SICOPOLIS use? What kind of basal friction law do you apply? I know you list the parameters in Table 1, but without the corresponding equation, they are rather useless. Does your basal friction coefficient

vary spatially and/or temporally? What are your boundary conditions for your enthalpy equation (e.g. do you specifiy a geothermal heat flux? Is it spatially constant?)? How do you treat calving in the model? There are a number of ways how to parameterise this. Since you talk about this in your results, it is essential to know how this is handled in your model.
Also you should mention with what geometry your initialise your model. I believe it is with present-day geometry, but with which dataset (Bedmap2, Bedmachine)? Do you take the bedrock and ocean floor topography from the same dataset?

2. Could you please motivate the ensembles or parameters changes that you are investigating a bit more? As it stands now, it seems like you picked a number of parameters, but there also could an argument be made for a bunch of other parameters to be varied.

3. I find most of the figures (e.g. 4, 6,7,9) not very informative. Looking at integrated quantities is OK, but having five Figures like that is too much. I suggest to combine them into a Figure with several panels. I also find it hard to judge in these volume plots whether differences are small or large (Is 2000 km3 a lot?). Maybe better to plot it in percent normalised to your starting volume? Also just because your ice volume is similar does not mean you cannot have regional differences in grounding-line position or ice thickness. For example on P16L279 you state "... show similar retreat rates..." but I cannot find a Figure where this is actually shown. So I suggest to add some Figures, where we can also look at some spatial differences (a few suggestion in the technical corrections below). For example, you could plot some grounding-line positions from different simulations in 2D on top of each other to see the differences in retreat or lack thereof. I also encourage the authors to discuss their results more in depth. For example, they state in L276ff that different initial ice sheet configurations converge to the same geometry for the same climate forcings. This alone is quite surprising to me and at least warrants a discussion why potential feedback mechanisms (e.g. stabilising grounding-line on topographic height) are not triggered in these simulations?

4. I think you should scratch your attempt to identify drivers for future change. You have it in your research questions, but other than in the conclusion section you never mention it again. And your statement in the conclusion statement is extremely vague (and we know this already) and to be honest not backed up by your simulation results.

5. The abstract in its current form is much too long and too descriptive. Please shorten and make more concise.

6. This is more an optional point and maybe a matter of taste, but I think you could also add a model limitations section. There are a few places where you can shorten the main text (see below), so that this would not much increase the length of the manuscript.

I always find it helpful in modelling papers to have a section in which limitations and potential future avenues for improvements are discussed. I must admit that as the paper stands now with very little information about the ice-sheet model, it is hard to examine what the benefit of your model setup is?

Technical corrections:

Abstract:
L8 I do not think that the Greenland information is necessary in the abstract. Also the latter half of the sentence makes no sense to me "..., both configurations of the Antarctic ice sheets..."? What configurations?

L12 Does LR04 need to be introduced as an acronym? I did not know straight away what it is.

L17 Here and throughout, I find the term "ice-sheet contraction" unusual. I know what you mean, but I think more commonly it is referred to as "ice-sheet retreat". Please consider changing it.

L29-34 This sentence is way too long and confusing. Please split up and make clearer.

L43 What do you mean by "reduced stability"? And why would that trigger stronger glacial-interglacial cycles?

L49-55 I think this paragraph can be thrown out, as it is irrelevant to the Antarctic simulations in the paper. It suffices to say, I believe, that the ice-sheet history in Antarctica is more uncertain than for Greenland.

L56-58 The first half of the sentence is confusing. The way it is written, it makes it sound as if Raymo and Mitrovica estimated it to be 6-13 m above present-day? But why is there a reference to Dutton et al. then? Here and throughout, could you please try to keep sentences shorter. It makes it easier to follow for the reader.

L61-64 Again a very long sentence which I do not understand. Please break up the sentence and clarify.

L65-80 Here, I would like to see what your study adds to studies like the one from Sutter et al. 2019. What is the advantage of your study/model setup ?

L81 I do not agree that you are presenting model reconstructions. What you present are sensitivity experiments. But as far as I can tell, you are not trying to match any geological constraints which is what I understand as model reconstruction.

L85 As said above, I do not think you really address the last question about future ice-sheet changes. Therefore, I recommend removing it from the manuscript altogether.

L106 In addition to the changes suggested above. How do you initialise for the different ice-sheet configurations? Do you use the same temperature spin-up and let it evolve afterwards? Or do you let it evolve to a different geometry and do the temperature spin-up then with a fixed geometry?

L106 From where do you get your surface temperature distribution? An ice core only provides you with temperature changes with respect to a certain baseline. Please add this to this section.

L107-109 This means you just move this shock outside of your time period of interest? This is in general OK, but raises the following questions: What forcing do you apply for the 5 ka in which the ice geometry is allowed to freely evolve? And how far away do you get from your initial geometry? And I am also missing a plot where you show that your ice sheet is close to steady state. I would appreciate if you could add a plot for this.

L129, equation (2): From this equation I gather that you apply the same temperature differences to the ocean as you do to the atmosphere? And you also do not apply a time lag to the ocean warming/cooling? Is that really realistic giving the long response time of the ocean compared to the atmosphere? At the very least, this choice should be discussed somewhere in the text.

L137 To me all headers in this section should rather read "Model sensitivity to XXX". Because this is ultimately what you do in this paper, rather than rigorously quantifying uncertainties.

L139-141 This sentence needs rewriting. I am not sure I understand what you are saying.

L154 should be "mean sea level"

L166 Here I believe you say that you also initialise with present-day conditions, but this needs to come much earlier and with more info as to what datasets you used for this.

L221 you state: "... ice sheet contraction is associated with strong basal melting close to the grounding lines ...". First of all this comes a bit out of the blue. Secondly, you show little evidence that this is actually the case. In Fig. 5 you show that basal melting is dominating, but if you have different SMB rates, the basal melt rate could be either 1.5m/yr or 6 m/yr. Please also avoid relative terms like "strong" without giving any numbers. Do you mean 5, 50, or 500 m/yr when you say "strong" melting. Related to

this, do you apply melting to partially grounded grid cells or only to fully floating? This makes a big difference how much your grounding line retreats for similar melt rates.

L222 should read "... Siple Coast, at the Ross Ice Shelf, and underneath ..."

L223-224 & L227 Since your basal melt rate is a quadratic function of your ocean temperature, stating that it is a combination of warming of the upper ocean layer and high melt rates is saying the same thing. Please reformulate.

L228 Two things here. First, since you have a separate results and discussion section, I was expecting only a description of the results. However, here and in other places (e.g. L245, L256-259) in your results section you are interpreting and discussing your results already. So either you have a combined results and discussion section or you move this material to your discussion section. Secondly, I cannot confirm your statement that ice loss is dominated by surface ablation on Amery in Fig.5. First of all, the panels are too small, so I am not sure if Amery is hatched or not? I do not really understand the purpose of Fig. 5, but to me Amery looks pretty red which means to me that there is a lot of ablation in this area. So why would it not retreat there and why is ablation so high in this region compared to basal melting?

L237 "..., the resulting ice sheet histories are quite similar." This is true for the integrated ice volume, but again I find this quite superficial and it could be different when we look at 2D fields.

L242 If it is problematic why did you include it?

L256-259 This is discussion for me (see comment above).

L268-L274 This paragraph should rather be part of your experimental design section. By now there are so many simulations that you performed that I think it is really necessary to add a table where you list all the simulations with important forcing parameters in a table. It is really hard to keep track of the simulations.

L276 To me that is really surprising. From my experience, the initial geometry is quite important with regard to what your results look like at the end of the simulation. You glance over this, but this needs a discussion. Why do you think this is the case?

L279 "... also show similar rates of retreat ...". Again this is nowhere shown. I mean in Fig. 10 it looks like they actually have exactly the same grounding-line position. Is that true?

L282 Could you please add these locations to the respective Figure for better orientation.

L289-301 This is a weak introduction to the discussion and repeats most of the material that you covered in the introduction. Consider removing it.

L321 "... it seems that ice-shelf calving plays a role just as big". This again comes totally out of the blue and at the moment there is no way to check this statement as it is simply not described how calving is handled in the model. I also do not quite follow the explanation for this. Could the authors please elaborate on this?

L393 delete objective

L406-407 Delete last sentence (see comment above).

Comment hyphenation: I noticed that throughout the manuscript your use of hyphenation is inconsistent. You write ice-shelf calving, but then grounding line advance without hyphen. I am not sure what the TC policy is, but please make sure that you are at least consistent throughout the manuscript.

Figures:

Fig. 1: Please add a scale bar. Glacial index plots and their labels could be bigger.

Fig. 2: Why do you show the time series until present-day? I think a zoom in into the period of interest would be better.

Fig. 3: It is really hard to see any differences in the upper panel (a-d) with the current colour scale. Also the grounding-line position should be made more prominent (thicker line or different color). In general there is too much white space and subplot labels (a-g) are too small. Please make each subplot bigger for better readability. Please also add a scale bar.

Fig. 4: In the lower plot it looks like your model run for LR04 is not really in steady state or is your initial perturbation that large compared to your spin-up forcing? As mentioned above, I do not find the current y-axis units very intuitive for the lower panel. Labels (a,b) are too small.

Fig. 5: I do not really understand the point of this Figure as I do not get any information about the magnitudes of basal melting or the SMB. This Figure also needs a scale bar.

Fig. 6: See Fig. 4

Fig. 7: Labels (a,b) are too small.

Fig. 8: Labels (a,b) are too small.

Fig. 9: Labels (a,b) are too small.

Fig. 10: It is really hard to see any differences in the upper panel (a-d) with the current colour scale. Also the grounding-line position should be made more prominent (thicker line or different color). In general there is too much white space and subplot labels (a-g) are too small. Please make each subplot bigger for better readability. Please also add a scale bar. Consider using a different colour range for plots e-g as they are mostly white now and show little information. It would also good to add place information that you mention in the text. Ninnis and Totten glaciers.

Sincerely, Clemens Schannwell