The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-309

Anonymous Referee #1

Referee comment on "Net effect of ice-sheet–atmosphere interactions reduces simulated transient Miocene Antarctic ice-sheet variability" by Lennert B. Stap et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-309-RC1, 2021

Stap et al. present numerical experiments of the Antarctic evolution during the mid-Miocene. They use a climate matrix interpolation scheme from GCM outputs to force their ice sheet simulations. They present equilibrium and idealised transient simulations and discuss a few feedbacks.

The methodology is sound and the numerical experiments well chosen. However there are several points that could/should be improved:

- Paper objective. The title is about the competing influence of the ocean, the atmosphere and the solid Earth and indeed the three aspects are discussed in the paper. However, it is not clear at the end of the introduction which are the main questions related to these processes and the added value of the present study. The literature (at least for the numerical part) is correctly acknowledged (not necessarily in the introduction) but it is not clear yet how the paper builds up from this. For example, the authors present three major improvements with respect to earlier works: 1- updated bedrock topography; 2- transient experiment and; 3- ice sheet – climate interactions accounted for. However, these are not always discarded in the recent literature (e.g. Halberstadt et al., 2021; Colleoni et al., 2018; Paxman et al., 2020). There is a lot of material in the paper but what can we learn from it can be more precisely described.

- Description of the methods. I would suggest to add a section on the matrix interpolation method after sec. 2.2 but before 2.3. Something simple that explains the main principle, keeping all the details in the appendix. I would like to read the rationale behind eq. B6 (insolation weighing). Also, climate model description section should be rewritten: it is not clear what existing simulations did you use ("nowabi" is not mentionned here) and the style can be largely improved (3 line parenthesis with embedded parenthesis is not easy to read).

- Results, shelves (particularly p. 10-11 and p.12 l. 386-390). You use a highly parametrised sub-shelf melt model that does not depend on the climatic simulations. If I understand correctly it is the result of an ad-hoc combination of linear functions of CO2. I understand the need for something simple and do not think that this is a critical flaw in the paper, however it prevents you for providing insights on the different thresholds for ice shelves formation and stability.

- Results presentation. You present multiple times the CO2/volume and hysteresis curves (6 out of 9 figures). I think you should explain better what can you learn from such a figure when you first show it. The style of the figure can also be improved and you will find a few suggestion in the "specific comments" section. It would also be very nice to show what the simulated ice sheets actually look like at different moment of the hysteresis (for a given volume I assume that the ice sheets of the ascending and descending branches do not look the same).

- Methods. You use a bias correction based on present-day bias with respect to ERA40. I have two questions related to this: 1- why ERA40, which is an old dataset now, and not a regional climate model (such as RACMO or MAR) as the reanalysis generally does not perform well for southern high latitudes? 2- Does it really make sense to use the same bias correction also for the climate simulations in which there is no ice in Antarctica? The effect of topography could eventually change drastically the wave pattern and associated biases.

- I am not entirely convinced that you should use the terminology "volume-precipitation feedback". There is a negative feedback associated with precipitation but I would say that it is more related to large-scale temperature. As a first approximation, colder air contains less humidity even though the process is more complex. Your have a volume-precipitation feedback because it is parametrised this way. I guess you could have written Eq. B10 using the ice extent instead of volume (you assume that precipitation change is more related to ice volume change, you did not demonstrate it). Alternatively, you could have use a local correction as for insolation to account for precipitation changes (using surface height for example).

- Try to be more specific instead of speaking of "warm/cold". You have three factors that can influence this warm/cold definition: CO2 concentration, orbital parameters and topography/ice mask in the climate model. This can be confusing sometimes in the text. Check and be consistent throughout the manuscript.

- Introduction. A justification for the cyclicity at 40k, 100k and 400k would be nice.

- Equations. Please write the x,y dependency when appropriate.

- Since albedo is an important driver for your ice sheet evolution, please provide more details on its implementation in IMAU-ICE.

Specific comments

- l. 9. Related to one of my previous comment, I would suggest rephrase this in the abstract since the ice volume-precipitation feedback sounds unconventional.

- l. 22. But with drastically different timescales.

- l. 81. Specific treatment of the grounding line?

- l. 103. Why?

- l. 109. These are probably computed on the fine resolution grid. How do you do the downscaling of the climate fields from the T31 to the 40km grid?

- l. 137-138. Why do you keep the "bi" in the manuscript if it is not relevant?

- l. 137-138. Even in the "nome" simulations? Vegetation is not allowed to grow when ice is absent? Clarify this in the text. I would have assumed that vegetation is an important feedbak for ice sheet inception.

- l. 152. If this figure is cited first, it should be numbered Fig. 1.

- l. 156-160. I found the description of the transient simulations unclear.

- l. 168. The present-day Antarctic ice sheet is the result of a deglaciation so it is not that bad if the volume obtained after an equilibrium is not in agreement with the present-day volume.

- l. 172-173. I might not understand you here: Reese et al. (2018) discuss the impact of the buttressing on the grounded part of the ice sheet. Also the ABUMIP project has shown a strong impact of buttressing for the present-day ice sheet (most ice sheet models in ABUMIP looses almost all the WAIS in the abuk experiments).

- l. 187- 191. It would be nice to have maps here. As it stands it is very descriptive.

- l. 191-192. Unclear.

- l. 200-201. "...we now interpolate..." how? What equation is used? You replace the "nomebi" model outputs by the "nowabi" model outputs?

- l. 201. You presented the "nomebi" as warm Antarctic summer (l. 135). "nowabi" was never presented before.

- l. 204-205. Does "nowabi" warmer than "nomebi"? Why do you obtain larger ice volume then? It seems that it is simply an artefact due to a different epsilonCO2 that gives more importance to the cold state.

- l. 207-208. What is your point here? And why this is observed?

- l. 208-209. Remove this sentence?

- l. 230-231. If I understand correctly, you are saying that ablation change is more efficient than precip change to explain ice volume change. I guess that this is not particularly surprising and have been shown earlier in different context. But I might be overlooking something?

- l. 230-231. You have chosen a c3 parameter that maximises the hysteresis. You might have ended up with slightly different conclusions with a smaller c3 parameter?

- l. 237-238. Consider adding a map of bedrock topography difference in Fig. 5. Or alternatively a map of the various bedrock topographies would be nice for Sec. 3.3.

- l. 293. Halberstadt et al. (2021) use a more sophisticated surface mass balance scheme. The might have a better representation of orographic precipitation for example?

- l. 318. I am not sure you can really quantify this since you use a very simple ocean-ice interaction representation in your modelling setup.

- l. 408-409. Why? Did you asses the sensitivity of your results to sea level change / value?

- l.424-428. Iabs is computed from the albedo given by the ice sheet model internal routine. But then how Eq. B3 works? Is the albedo kept identical in "mod" and for "cold"/"warm", only changing the value of QTOA? Please provide a bit more details here.

- l. 428. cold/warm refers to the CO2 level? This is confusing since you have three factors for warm/cold: CO2 concentration, the orbital parameters and the topography/ice mask in the climate model.

- l. 431. Eq. B4: do you have any justification for this 1:7 ratio? Any number would work the same?

- l. 436. Please provide a literal justification of Eq. B6. You could drop the "ref" on the left hand side of the equation since you computed this only once right?

- l. 437. Insert a new line here from "The weighing..." since this applies to wice and wco2 (not epsilonCO2) if I am not wrong?

- Fig. 1. Highlight the points corresponding to the equilibrium simulations (stars, big dots, anything).

- Fig. 1. Try to avoid using the same colour for different things (red in a and b). Add a better description of the different coloured lines in each sub-panel. For example: we have to guess that the black dotted and dashed in b) is the same as in a) when the red continuous represents something else in a). In d) it seems that you have two ascending branches for the simulation "regular CO2" (solid black line). "Regular" transient simulation sounds weird.

- Fig. 3. In d) what are the dashed red and blue lines?

- Fig. 4. In b) the orange is transient for FEEDB while the dashed red/blue is equilibrium counterpart? This is really confusing in my opinion.

- Fig. 5 and Fig. S2 / S4. The colour gradient for the difference plot is hard to read, it mostly gives an indication on the signs. You could try with a discontinuous colour scheme or with isocontours?