

The Cryosphere Discuss., author comment AC1  
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## Reply on RC1

Lennert B. Stap et al.

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Author 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-AC1>, 2022

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## Reply to Anonymous Referee #1

Comments by the reviewer

### Reply by the authors

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:

**We thank the reviewer for a thorough examination of our work. We are pleased they agree with our set-up and appreciate our experiments. Below, we address their comments and indicate which actions we will take to revise the manuscript.**

- 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.

**The title of the manuscript will be adjusted to *'Net effect of ice-sheet-atmosphere interactions reduces simulated transient Miocene Antarctic ice sheet variability'* to reflect a stronger focus on the effects of ice-sheet-atmosphere interactions. In the last paragraph of the introduction, we will indicate more clearly how our work improves upon earlier research. Compared to Gasson et al. (2016), Colleoni et al. (2018), Paxman et al. (2020) and Halberstadt et al. (2021), we study transient variability in addition to steady-state results.**

**Compared to De Boer et al. (2010) and Stap et al. (2019, 2020), we include ice-sheet-atmosphere inter-actions and use an updated bedrock topography. Combined with a more verbose outline of the experiments performed and their purpose, this serves to clarify the intent of our study.**

- 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 mentioned here) and the style can be largely improved (3 line parenthesis with embedded parenthesis is not easy to read).

**As suggested by the reviewer, we will include a brief description of the matrix interpolation method in the methods section right after Sect. 2.2. In the revised manuscript, we discard the presentation of our experiment INSOL, because this does not contribute to the main story line and only confused the reviewers. Therefore, now in all our experiments GENESIS simulation 1fumebi (280 ppm, large ice sheet) provides the 'cold' forcing, and 3nomebi (840 ppm, no ice sheet) provides the 'warm' forcing. These GENESIS simulations are introduced in a clearer manner, with fewer parentheses, in the revised method section.**

**We will also explain the rationale behind Equation B6, which we use to determine the ratio between the effect of externally prescribed CO<sub>2</sub> changes and internally calculated ice-sheet coverage on the interpolated forcing temperature. To this end, we use GENESIS simulations 3fumebi (840 ppm CO<sub>2</sub>, large ice sheet) and 1nomebi (280 ppm CO<sub>2</sub>, no ice sheet), which isolate the separate effects of CO<sub>2</sub> and ice-sheet coverage respectively. The (spatially averaged) relative influence of CO<sub>2</sub> on temperature is then calculated as the isolated effect of CO<sub>2</sub> divided by the combined influence of CO<sub>2</sub> and ice-sheet coverage (Eq. B6).**

- 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 CO<sub>2</sub>. 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.

**We agree with the reviewer that the thresholds for ice shelf formation and stability that we find, are not to be considered robust quantifications. Therefore, we will refrain from presenting them as such in the revised manuscript. Instead, throughout the revised manuscript, we present the experiments using different basal melt rates (BMB LGM, BMB no\_shelves) as essential sensitivity tests. They serve to quantify where and under which conditions ice shelves form and perish in our simulations, and how they influence the variability of the grounded ice volume. According to this different assessment of these experiments, we will adjust the title of the manuscript, as well the abstract and conclusion section where these results will not be mentioned anymore.**

- Results presentation. You present multiple times the CO<sub>2</sub>/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).

**The hysteresis figures can seem a bit convoluted at first sight, with multiple steady-state and transient results. However, we think it is necessary that this information is contained in a single figure, to allow for a proper comparison between the different results. We will aim to improve the readability of these figures in the following ways:**

- **The equilibrium results are highlighted by diamonds.**
- **The evolution of CO<sub>2</sub> is indicated by pink and purple instead of red and blue lines in the a- and c-panels, so that it is clearly distinguishable from the equilibrium results in the b- and d-panels.**
- **Arrows are added to the transient results in all b- and d-panels, to indicate the progression direction.**
- **Legends are included to indicate which experiments are displayed.**
- **All the different symbols and lines are described in the figure captions.**

**Furthermore, we will guide the reader on how to read these hysteresis figures, upon their first occurrence when the REF experiments are described (new Fig. 1). Finally, we will include a supplementary figure with 'intermediate' states, i.e. the results of the steady-state 392-, 504-, 616-, and 728-ppm CO<sub>2</sub> REF simulations of both the ascending and descending branch.**

- 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.

**Similar to Stap et al. (2019), we use ERA40 reanalysis data for a bias correction. This is because that was the climate used to benchmark the different ice sheet models contributing to PLISMIP-ANT (De Boer et al., 2015). For the reasons mentioned by the reviewer, it may be more appropriate to use regional climate model results, or refrain from using a bias correction altogether. We will consider this for future research. However, for the current study, we submit that the ice volume differences are determined for the large part by the difference between the warm and cold forcing climates as well as the value of the ablation parameter (c3 in Eq. 6).**

**In the last paragraph of the revised discussion section, we will discuss the purpose of the bias correction, which is to obtain a more detailed reference climate, as well as the fact that it introduces a small but significant warm bias because we use a present-day rather than pre-industrial reference climate (also noted in Stap et al., 2019).**

- 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. You 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).

**Indeed, we called it 'ice-volume-precipitation feedback' because it is**

**parameterised that way in our model. In the revised manuscript we will explain that this represents our manner of capturing the ice-sheet desertification effect, i.e. the reduction of snow deposition on a growing ice sheet. We will refer to it as *a*, rather than *the*, negative feedback between ice volume and precipitation.**

- Try to be more specific instead of speaking of "warm/cold". You have three factors that can influence this warm/cold definition: CO<sub>2</sub> 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.

**We interpolate the forcing temperature and precipitation between two climates, a 'cold' and a 'warm' one. Because we no longer present the results of simulation INSOL in the revised manuscript (see above), 'cold' now always refers to a large ice sheet and low CO<sub>2</sub> level, provided by GENESIS simulation 1fumebi. 'Warm' refers to no ice, and a high CO<sub>2</sub> level, provided by GENESIS simulation 3nomebi. Both simulations use present-day orbital settings. This will be clarified in the method section of the revised manuscript.**

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

**We will indicate that these are quasi-orbital timescales.**

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

**The x,y-dependencies will be included in all equations.**

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

**In the method section of the revised manuscript, we include a description of the albedo scheme used in IMAU-ICE. A background albedo is first calculated: 0.1 for ocean-covered grid cells, 0.2 for soil, and 0.5 for ice. On top of that, the effect of snow, with an albedo of 0.85, is added. This effect depends on the transiently changing snow layer and ablation of the previous year.**

*Specific comments*

**The manuscript will be revised along the lines of these comments, as indicated below.**

- 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.

**In the revised abstract, we rephrase this as 'a negative feedback between ice volume and precipitation'. Please see our reply to the general comment on this issue above.**

- l. 22. But with drastically different timescales.

**This will be mentioned.**

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

**In the version of IMAU-ICE we use for this study, no additional grounding-line parameterisations are used.**

- l. 103. Why?

**The only calving routine implemented in IMAU-ICE is thickness calving, i.e. calving floating ice with a thickness below a certain threshold. We exclude this in our simulations, because it could hinder the growth of new ice shelves (e.g. Ritz et al., 2001).**

- 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?

**The GENESIS fields are remapped using bilinear interpolation based on longitude and latitude. Longitude and latitude coordinates are assigned to the ice-sheet model grid using an inverse oblique stereographic projection (Reerink et al., 2010).**

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

**The GENESIS simulations were introduced in Burls et al. (2021). To maintain congruity between articles, we use exactly the same nomenclature here.**

- 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 feedback for ice sheet inception.

**The use of BIOME4 in the forcing climate simulations affects the vegetation on non-glaciated land and hence the climate.**

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

**It will be Fig. 1 in the revised manuscript.**

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

**The description of the transient simulations will be rephrased in a clearer manner.**

- 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.

**Indeed, these PI-reference simulations are just meant as a minimal check for model performance.**

- 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 loses almost all the WAIS in the abuk experiments).

**We accidentally referred to the wrong paper, it should be:**

*Reese, R., Gudmundsson, G. H., Levermann, A., and Winkelmann, R.: The far reach of ice-shelf thinning in Antarctica, Nature Climate Change, 8, 53–57, <https://doi.org/10.1038/s41558-017-0020-x>, 2018b.*

**This article discusses which parts of the ice shelves are the most important for buttressing the grounded ice (see e.g. their Fig. 1). From their results, which generally show a smaller influence of the parts of ice shelves that are furthest**

**away from the grounded ice, we gather that overestimating the ice shelf area has a relatively small effect on grounded ice flow.**

- l. 187- 191. It would be nice to have maps here. As it stands it is very descriptive.
- l. 191-192. Unclear.

**Rather than including extra figures, we will point out that the reader can assess the growth and decay rates from the slope of the ice volume progression in both the a- and b-panels of Fig. 2 (which will be Fig. 1 in the revised manuscript).**

- 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 epsilonCO<sub>2</sub> 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?

**From the comments above, as well as from the comments of reviewer #2, it has become clear to us that the discussion of the results of experiment INSOL caused confusion. Since they were only meant a side note, and do not contribute to the main story line, we have chosen to refrain from discussing them in the revised manuscript. Therefore, this whole section will be removed.**

- 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?

**We quantify the effects of the *feedbacks* between temperature and ice volume, and between precipitation and ice volume. We do that by comparing runs using the matrix method, to runs where the temperature and precipitation forcing is interpolated based on the prescribed CO<sub>2</sub> level alone. In both cases, using the matrix and index method, precipitation and temperature can change during the simulations. We do not head-on investigate the separate effects of precipitation and temperature on ice volume change. That would require running the model with only temperature or precipitation changes from the climate simulations, as was done in Stap et al. (2019).**

- 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?

**Please note that we did not tune the c3 parameter to maximise hysteresis. Rather, our REF settings yield an ice sheet similar in extent to the results of Stap et al. (2019) and DeConto and Pollard (2003) at 280 ppm, which disappears almost completely at 840 ppm. This ensures that we can most effectively study large-scale variability of the East-Antarctic ice sheet. A different tuning would shift the CO<sub>2</sub> at which these ice sheet sizes are simulated, but does not (qualitatively) affect the shape of the CO<sub>2</sub>-ice volume relation.**

- 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.

**We will include a figure showing all the different bedrock topographies used.**

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

**We use an insolation temperature melt (ITM) scheme, accounting for the direct effect of insolation on surface melt, whereas they deploy a positive degree day (PDD) scheme. Additionally, they use a regional climate model for downscaling the forcing, which indeed allows for capturing orographic precipitation in more detail. We instead use a bias correction to obtain a more detailed reference climate.**

- 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.

**We would like to note that the CO<sub>2</sub> level at which the ice sheet first reaches the ocean does not depend on ocean temperatures in our set-up. Since up until that point, the ice is not in contact with the ocean, this CO<sub>2</sub> level is solely dependent on the atmosphere forcing and ice dynamics. In the revised manuscript, we will nonetheless make it clear that we are discussing the impact of ocean forcing on ice volume in *our* results, and that we do not draw general conclusions from this. The title of the revised manuscript, as well as the abstract and conclusion will also reflect that.**

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

**The sea level is kept constant at present-day level (0 m), because local Antarctic sea level changes are very uncertain during the Miocene as they are mainly due to Antarctic ice sheet changes. Local sea level changes can differ substantially from the global mean (e.g. Stocchi et al., 2013), and their calculation would require solving the sea-level equation which is not yet available in IMAU-ICE. We have performed three tests, applying a constant 7 m higher sea level (mimicking the absence of the Greenland ice sheet), as well as applying internally calculated global mean, and minus the global mean, sea level changes. These tests have shown negligible differences in comparison to our reference results. They will be briefly discussed, but now shown, in the revised manuscript.**

- l.424-428.  $w_{ice}$  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.

**In order to obtain  $w_{ice}(x,y)$ , first the absorbed insolation  $I_{abs}(x,y)$  is calculated from the incoming insolation and internally calculated surface albedo. This is done every 10-yr coupling time step for the interpolated modeled field. For the cold (1fumebi) and warm (3nomebi) GCM snapshot fields, this is done at the start of each simulation by running the SMB model (which is more extensively described in the revised manuscript), without ice dynamics, for 10 yrs to determine the albedo. Incoming insolation is kept constant at the present-day values, so that differences between the cold, warm and modeled  $I_{abs}(x,y)$  fields are solely determined by the albedo.**

- l. 428. cold/warm refers to the CO<sub>2</sub> level? This is confusing since you have three factors

for warm/cold: CO<sub>2</sub> concentration, the orbital parameters and the topography/ice mask in the climate model.

**As indicated above, 'cold' now always refers to a large ice sheet and low CO<sub>2</sub> level, provided by GENESIS simulation 1fumebi, and 'warm' refers to no ice and a high CO<sub>2</sub> level, provided by GENESIS simulation 3nomebi.**

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

**The viability of the (1:7) partitioning  $w_{ins,smoothed}(x,y)$  and  $w_{ins,avg}$  was demonstrated by Berends et al. (2018), who simulated ice-sheet evolution over the last glacial cycle, showing that model results agree well with available data in terms of ice-sheet extent, sea-level contribution, ice-sheet surface temperature and contribution to benthic  $\delta^{18}O$ . We deem a sensitivity analysis with respect to this setting to be beyond the scope of the current study.**

- I. 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?

**Please see our response to the general comment of the reviewer on this issue. Since we no longer discuss simulation INSOL in the revised manuscript, 'ref' can and will be dropped.**

- I. 437. Insert a new line here from "The weighing..." since this applies to  $w_{ice}$  and  $w_{CO2}$  (not  $\epsilon_{CO2}$ ) if I am not wrong?

**Weighing factors  $w_{CO2}$  and  $w_{ins}(x,y)$  are clipped at -0.25 and 1.25, to avoid spurious extreme temperatures and precipitation. This will be mentioned right after Eq. B3.**

- 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 CO<sub>2</sub>" (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. 1 and similar 'hysteresis' figures (i.a. Figs. 3, 4) will be altered, as described above in our reply to the general comment of the reviewer on this issue.**

- 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?

**We will use a colour scheme that more clearly shows the differences.**

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