Reply on RC2
Lennert Bastiaan Stap et al.

Author comment on "Competing influences of the ocean, atmosphere and solid earth on transient Miocene Antarctic ice sheet variability" by Lennert Bastiaan Stap et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-309-AC2, 2022

Reply to Anonymous Referee #2

Comments by the reviewer
Reply by the authors

Stap et al. perform idealized transient and steady state ice sheet model experiments, driven by GCM climate simulations, to explore Miocene ice sheet variability and hysteresis. They build on the work of Stap et al., 2019, by investigating the albedo-temperature and precipitation-ice volume feedbacks on ice growth and decay.

The strength of this work is the explicit exploration of feedbacks that influence ice sheet behavior, and the use of transient ice-sheet simulations applied to a time period that has been primarily studied using equilibrium ice sheet modeling. The weaknesses of this study (acknowledged by the authors) are the highly parameterized ice/ocean interactions, and the omission of insolation variability within the climate forcing matrix. Despite these weaknesses, I believe that this work will be of interest to the community, given some substantial clarifications described below. I suggest that the authors refocus the emphasis in this manuscript on the temperature-albedo and precipitation-ice volume feedbacks primarily.

We thank the reviewer for a careful examination of our work, and we are glad they find our study of interest. The revised manuscript will be titled ‘Net effect of ice-sheet-atmosphere interactions reduces simulated transient Miocene Antarctic ice sheet variability’, reflecting a stronger focus on the ice-sheet-atmosphere feedbacks. The experiments that regard the influence of ocean forcing on our results will be presented as a sensitivity analysis rather than robust quantifications of the thresholds for ice shelf formation and stability. Below, we respond to the reviewer and indicate how we will change the manuscript along the lines of their comments.

(1) Lack of marine ice sheets: The ice sheets simulated here are almost exclusively terrestrial. Even under the lowest CO2 (280 ppm), the small WAIS seems to be grounded primarily above sea level (Fig. 1b); only with the Wilson topographies can this model setup produce marine-based ice. Therefore, the hysteresis curves presented here do not reflect marine ice dynamics. This is an understandable limitation of the study but given
that geologic records show marine ice advance out onto the continental shelf during the Miocene (most recently, Pérez et al., 2021), the authors should clarify that this work cannot fully represent ice sheet volume variability through the Miocene.

Based on my understanding of their model setup, the ability of the modeled ice sheet to expand into the marine realm is primarily dependent on $T_w$ which was linearly scaled based on CO2. Could marine ice sheet advance be simulated with different choices of $T_w$? In the modern validation run, the authors produce marine grounded ice (because they replicate a modern ice-sheet configuration; L170, not shown). Were the $T_w$ and basal melt rates used in this modern simulation generated using the CO2 scaling, as in the Miocene runs? In other words, is the lack of marine ice in the Miocene due to the heavily parameterized basal melt / ocean temperature scheme?

The authors briefly mention this small-WAIS (no significant marine ice sheet) issue in the first paragraph of page 11, suggesting that the lack of any large WAIS growth may be hampered by not enough buildup of ice over WAIS due to climate forcing ("in the cold case simulation 1fumebi there is only a small WAIS present in the forcing climate simulation"; L333). 1fumebi is characterized by a 'full ice sheet' (L136) so I assume this is a typo, but I don’t know what was meant instead. I would like the authors to elaborate on this and provide a satisfactory hypothesis (hypotheses) to explain why the 280ppm REF steady-state ice sheet under a cold orbit doesn’t seem to produce marine ice advance as suggested by the geologic record and previous modeling studies (Gasson et al., 2016, Halberstadt et al., 2021, who simulate large marine-based WAIS with similar climate forcing and a steady-state ice sheet model approach). This simulation was used to initiate many of the steady state and transient simulations performed in this study, so has cascading impacts on the results here.

Indeed, our study focusses on the variability of a generally small AIS, which is located primarily on the East side of the continent, while the WAIS size remains relatively limited. This may partly be due to the BMB parameterisation, but the fact that ice shelves do grow in our REF simulations - which were run with the same CO2 scaling functions for the BMB - and the simulations using Wilson topographies, suggests that the SMB is also decisive in this respect. It is therefore very important to note that our 'cold' GENESIS climate (1fumebi) was indeed generated with a relatively small WAIS; this was not a typo. We called it a 'full' ice sheet because this was how this simulation was described in Burlis et al. (2021), but we agree that this is not an entirely accurate description to use in the current study. In the revised manuscript, we therefore refer to it as having a large/substantial East Antarctic ice sheet. We will also include a figure showing the ice extent used in the GENESIS simulation. Furthermore, the limitation this poses on the maximum size of the AIS in our simulations will be emphasized more strongly. Finally, the implication of our results will be discussed more clearly and extensively, also in comparison the earlier model studies of Gasson et al. (2016) and Halberstadt et al. (2021), as well as data studies among which the recent publication of Marschalek et al. (2021).

(2) Ice shelf results: Given the heavily parameterized ocean melt scheme, I imagine that the CO2 thresholds of ice shelf formation are dependent on ocean temperature $T_w$ which is linearly scaled with CO2. A more meaningful way to discuss these results could be to simply report the ocean temperature $T_w$ at which ice shelves begin to form, instead of CO2. Given this dependence on the $T_w$ scaling, I think these results should be interpreted lightly.

In the revised manuscript, we will mention the values of $T_w$ pertaining to the formation and decay of ice shelves in our REF simulations. Furthermore, the results regarding ice shelves (BMB LGM, BMB no_shelves) will be presented as
essential sensitivity tests, aimed at quantifying the uncertain effect of ocean forcing on our results, rather than as robust findings of our study.

Also, I am a bit confused about the interpretation presented here regarding the impact of ice shelves, for example, the CO2 thresholds above and below which ice shelves are reported to be influential (L275-276, L319-321). Looking at Fig. 8 (no-ice-shelf-melt vs. LGM ice-shelf-melt experiments), it seems to me like the identified thresholds when ice shelves are affecting grounded ice (360 ppm and 728 ppm) are quite similar for both BMB no_shelves and BMB_LGM experiments (as well as in other 400kyr experiment that is shown; REF, Fig. 2b), so I don’t quite understand the attribution to ice shelves.

Simulations BMB LGM and BMB no_shelves ease and impede the formation of ice shelves respectively. Since all other settings are kept the same, the difference between these simulations yields a quantification of the maximum influence of ocean forcing on our results. Up until the threshold for formation of ice shelves, there is no difference because the grounded ice is not in contact with the ocean. After that, the results start to diverge, reflecting the influence of the ocean forcing. The purpose of the BMB simulations, and their implications, will be explained in more detail in the revised manuscript.

The observation that ice volume variability increases when ice shelves form is straightforward and intuitive, since the role of ice shelves in buttressing grounded ice is well known. I’m not sure I see an overly significant change in “hysteresis in the CO2-V relation” (L277), though. If hysteresis is defined as the difference in CO2 initiating the onset of major ice growth and decay, there is a small difference between the solid orange and dashed green lines in the 400kyr transient runs (Fig. 8b) but very little difference in the shape of the operating curves in the 40kyr runs (Fig 8d); if hysteresis is defined as the area encompassed by the ascending and descending curve, the green dotted line does span slightly more area than the solid orange in Fig 8d but not 8b.

We indeed define hysteresis as the difference between the ascending and descending curve. We agree that in the 40-kyr runs the difference in hysteresis between the results of BMB LGM and BMB no_shelves is relatively small. However, since the grounded ice volume is consistently larger in the decay phase when ice shelves are allowed to grow, we still think we should make note of it in the manuscript.

(3) Elaborate in the Discussion: In my mind, the strength of this work lies in exploring the transient evolution of Miocene ice sheets and specifically investigating the impact of the albedo-temperature and precipitation-ice volume feedbacks. Therefore, it would be nice to see more analysis of transient ice sheet behavior and the impact of the two feedbacks in the Discussion section. For example, the 400 kyr cycles produce much larger ice sheets than the 40kyr cycles, suggesting that prolonged low CO2 is necessary to produce a large ice sheet. How does that relate to the geologic record of ice sheet dynamics in the Miocene? I understand that Stap et al., 2019 focused on this, but I think some discussion of how these results fit into the context of the geologic record could be summarized, and better yet, elaborated upon in the Discussion section. Given that the main contribution of this work is related to the two competing feedbacks, is there more to discuss about how these feedbacks impact hysteresis on different timescales?

Reflected by the new title, the primary focus of the revised manuscript will be the effect of ice-sheet-atmosphere interactions on transient Miocene AIS variability.

We will extend the discussion of these results with the effects on different timescales. On all the timescales studied, transient AIS variability is suppressed.
by the ice-sheet-atmosphere feedbacks. This is partly due to the smaller equilibrium ice volumes at low CO\textsubscript{2} levels, and partly to decreased growth rates of the transient ice volume relative to the change of equilibrium ice volume. In short, the ice-sheet-atmosphere feedbacks cause a slower build-up of the AIS to smaller peak ice volumes. We will explain in the manuscript that this implies a smaller contribution of AIS changes to Miocene benthic δ\textsubscript{18}O fluctuations. However, because the actual strength of the impact will depend on the isotopic composition of Antarctic snow as well, a comprehensive quantification of these contributions is left to future efforts towards more realistic transient simulations.

All in all, we need very large CO\textsubscript{2} variations on relatively short orbital timescales (40 kyr) to obtain substantial East Antarctic ice sheet variability. This could point to a relatively stable EAIS, and a consequent reduced contribution to benthic δ\textsubscript{18}O fluctuations during the Miocene. Alternatively, the WAIS could play a more major role in establishing ice-sheet variability. This will be explained and put in perspective of earlier model and data studies, in the final paragraph of the revised discussion section.

Figures

Each figure caption should fully explain the elements of the figure or reference another figure caption where that information can be found. For example – the teal-colored areas in Figs 1, 9 are ice shelves, correct? For example, in all of the figs after Fig. 2, it would be helpful to state in the caption that the ascending branch is blue and descending branch is red. Arrows would be helpful for all figures not just Fig. 2. Also, for the equilibrium runs, I suggest adding the ice volume/CO\textsubscript{2} points for each discrete steady state simulation.

We will aim to improve the readability of the hysteresis figures in the following ways:

- The equilibrium results are highlighted by diamonds.
- The evolution of CO\textsubscript{2} 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.

The last item of this list also holds for the maps, where the teal - or cyan, to be precise - areas indeed indicate ice shelf extent.

Fig 9b: I wonder why there doesn’t seem to be grounded or floating ice in the Ross Sea Embayment. Surely there is ice sourced from EAIS that should be able to grow into the Ross Sea with the LGM basal melt scheme?

In the BMB LGM experiments, the basal melt rate does not drop below 0 m/yr in the Ross Embayment. Therefore, the lack of ice in this region must be due to insufficient influx from the grounded West- and East-AIS regions, in combination with the (negative) SMB.

Specific comments

These comments will be addressed in the revised manuscript, as indicated below.
When I see the phrase “influence of … solid earth on … ice sheet variability”, I think of glacial isostatic adjustment and solid Earth feedbacks. I suggest replacing ‘solid earth’ with ‘topography’ in the title and elsewhere in the manuscript.

The title will be changed to ‘Net effect of ice-sheet-atmosphere interactions reduces simulated transient Miocene Antarctic ice sheet variability’. Throughout the rest of the revised manuscript, references to ‘solid earth’ are replaced with ‘bedrock topography’.

It would be helpful here (or elsewhere; L51?) to provide more information about how to read and interpret the hysteresis plots that make up the majority of the figures. For example, what exactly is meant graphically by “increased/decreased hysteresis”? Is it the total area between the ascending/descending (blue and red) curves? Or perhaps the CO2 difference between growth and collapse of the ice sheet?

We will guide the reader on how to read the hysteresis figures, upon their first occurrence when the REF experiments are described (new Fig. 1). This includes giving our general definition of hysteresis as the difference between the ascending (blue) and descending (red) branches. Mind, though, that in principle a larger difference between the thresholds for ice sheet growth and collapse also implies an increased area between the ascending and descending branches.

Consider citing Pollard & DeConto 2005 (Hysteresis in Cenozoic Antarctic ice-sheet variations) as a seminal paper plotting hysteresis as CO2 vs ice volume.

This study will be cited in the results section, where the hysteresis curve is first discussed.

What are ‘exposed’ vs ‘deep’ shelf environments that the M_{expo} and M_{deep} melt rates are respectively applied to?

This is determined by the weighing factors z_{deep} (function of water depth) and z_{expo} (function of the widest subtended angle to the open ocean and the shortest linear distance to the open ocean). These factors will be introduced using numbered equations in the revised manuscript.

Ocean temperature T_w scales linearly from -1.7 to 2 degrees C based on CO2; how was this relationship established? What assumptions does this relationship rest upon? I would imagine that this choice greatly impacts the results presented here.

Similar to Gasson et al. (2016) and Halberstadt et al. (2021), we face the limitation that GENESIS has a slab-ocean component. Realistic water temperatures can therefore not be taken from the GCM results. Instead, we use an ad hoc global ocean temperature parametrisation, adapted from the ice-sheet model ANICE (De Boer et al., 2013), the predecessor of IMAU-ICE. The experiments BMB no_shelves (severe basal melt rates) and BMB LGM (very mild basal melt rates) are performed to quantify the maximum impact of the ocean forcing on our results. They are presented as sensitivity tests in the revised manuscript.

What ocean temperature and basal melt values were used in the modern steady state simulation, in order to match the modern ice sheet configuration? Were they based on the CO2 scaling of T_w and M? If so, that lends much more confidence to the Miocene results using that scaled approach.

Yes, they use the same CO2-scaling. The modern steady-state simulations are
forced with the 280-ppm CO₂ settings: $M_{\text{expo}} = 3 \, \text{m/yr}$, $M_{\text{deep}} = 5 \, \text{m/yr}$, and $T_w = -1.7 \, ^\circ\text{C}$. 

L197-198: I don’t understand this sentence – what are quantitative vs qualitative CO₂ levels?

We mean that the ablation factor only translates the ice volumes along the CO₂-axis but does not change the overall shape of the CO₂-ice volume relation (qualitatively). Hence, the result of any change in the ablation factor, can in principle be offset by uniformly changing the forcing CO₂-levels.

L215 / Table 1: The naming convention for these experiments led to some initial confusion on my part, because the experiment FEEDB is actually removing feedbacks rather than adding them, and the REF experiment is the one that incorporates the feedbacks – so the FEEDB experiments might be better named ‘NOFEEDB’ or something of that sort. Similarly, the wording in this paragraph would more intuitively (to me) highlight the results by presenting the impact of adding the feedbacks rather than removing them, e.g., as in L222 (“Stated the other way around, ice-sheet-atmosphere interactions decrease the amplitude of AIS variability”). I recommend this wording convention throughout the entire paragraph and manuscript when discussing the FEEDB (NOFEEDB) experiments.

We agree with the reviewer on both matters. In the revised manuscript, the experiments in which the ice-sheet-atmosphere interactions are excluded, are more aptly described as using an index method rather than glacial index method, and are named NOFEEDB, NOFEEDB-T, and NOFEEDB-P. Furthermore, we will use the wording convention suggested by the reviewer, i.e. presenting the impact of adding, rather than removing, feedbacks.

L291-295: The narrower CO₂ range between inception of EAIS and the marine-based WAIS in this work compared to other studies (e.g., Halberstadt et al., 2021 (and Gasson et al., 2016) seems more attributable to the different ocean melt scheme, rather than treatment of precipitation and ablation. With that said, the different precipitation and ablation schemes in previous studies probably explain the larger ice sheets they reconstruct at higher CO₂ compared to this work.

An extensive discussion of the similarities and differences between our results and those of Gasson et al. (2016) and Halberstadt et al. (2021) will be included in the final paragraph of the discussion section in the revised manuscript. A comparison to the recently published data study of Marschalek et al. (2021) will also be made there.

L321 “This transition from a land-based to a marine ice sheet at CO₂ levels around 400 ppm is in general agreement with other model results” - I don’t see evidence for a marine ice sheet in these simulations (BMB no_shelves and BMB LGM, Fig. 8, Fig. 9). In Fig 9, the 392 ppm ice sheet does not have a full WAIS, and ice volumes at 280 ppm CO₂ are similar to (Fig 8b) or less than (Fig 8d) ice volume at 392 ppm (i.e., no marine ice sheet).

We agree that we do not simulate a full modern-day-like WAIS. Nevertheless, small ice shelves are formed at 504 ppm, as indicated by the cyan-coloured areas in Fig. 9 (which will be Fig. 7 in the revised manuscript). At 392 ppm, small ice shelves fringe the entire continent. From this point onward, the ocean forcing starts to significantly affect the evolution of grounded ice volume both in the equilibrium and transient simulations. This is visible in Fig. 8 (which will be Fig. 6 in the revised manuscript) as a divergence between the results of BMB no_shelves and BMB LGM.
The increasing sensitivity of the AIS to a subsiding bed has been recently explored in depth (e.g., Colleoni et al., 2018; Paxman et al., 2019, 2020). The experiments presented here and corresponding discussion (L306 onwards) are interesting and relevant, but does not seem to me to produce a novel conclusion given that marine ice advance is mostly absent in these simulations and ice sheet response to a subsiding bed (and therefore increasing ice-ocean interactions) is heavily parameterized. This paragraph could be moved to the Discussion.

In the discussion section, we compare our results to the work of Colleoni et al. (2018) and Paxman et al. (2020). Our steady-state results indeed concur with their earlier findings. However, we additionally perform transient simulations. We find that the subsidence of land below sea level during the early- and mid-Miocene reduces transient 40-kyr AIS variability by 10% in amplitude. Although maybe not groundbreaking, this is to our knowledge a novel result and therefore worth mentioning in the conclusion section as well as in the abstract.

This sentence will be removed from the manuscript.

REFERENCES:


