The Cryosphere Discussions

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Comment on tc-2021-309

Anonymous Referee #2

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-RC2, 2021

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.

(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.

(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.

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.

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.

(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?

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/CO2 points for each discrete steady state simulation.

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?

Specific comments

Title: 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.

L76: 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?

L52: Consider citing Pollard & DeConto 2005 (Hysteresis in Cenozoic Antarctic ice-sheet variations) as a seminal paper plotting hysteresis as CO2 vs ice volume.

L96: What are 'exposed' vs 'deep' shelf environments that the M_{expo} and M_{deep} melt rates are respectively applied to?

L101: 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.

L170: 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.

L197-198: I don't understand this sentence – what are quantitative vs qualitative 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.

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.

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).

L380: 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.

L385: Likely tectonic evolution as well as glacial erosion