

Author's response to peer review 2

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Response to reviewer 1

- Line 211: The word 'effects' should be changed to 'affects'.
AR: corrected.
- Figure 10: According to SMB3 and SMB4, both temperature and precipitation contributed to the increase of SMB for the Dronning Maud Land ice sheet, correct? This analysis does not rule out the possibility that other regions may develop ice sheets if incepting ice sheets are seeded, is that correct?
AR: that's not correct per se. We have looked specifically at places where our ISM forms ice sheets given a strong perturbation of the system — that has been the basis for our seeding. We cannot exclude other areas / regions (especially when they are smaller / more subtle than the areas we considered), but our method already implies a large forcing. Based on these simulations, large glaciated regions other than the ones we analysed are therefore rather unlikely to occur.

Response to editor review

- Sustainability vs stability: This popped up in response to comments by one of the reviewers, to the point where the title now features the term "sustainability" and the term is also used in a section heading. However, throughout the manuscript the term ice-sheet "stability" is still used frequently: this leads to potential terminological confusion for readers. It may be useful to explicitly state somewhere (introduction?) what you mean by these two different terms, and then carefully go through the manuscript to confirm that the terms have been applied as you intended.
AR: thank you, we agree that we must choose one term. We picked sustainability, and changed it throughout the manuscript.
- Throughout: "early" "middle" and "late" modifiers for the Eocene are not formal chronostatigraphic terms and thus should not have leading uppercase letters.
AR: corrected.
- Line 31 and first paragraph in general: What is a "balanced" $\delta^{18}\text{O}$ value? Can you describe the excursions more directly in terms of direction and magnitude?
AR: changed this line to include indeed the direction and magnitude of the two excursion phases.
- Lines 31/32: "...it is commonly interpreted as..." Worth slight revision here to make it clear that "it" refers to the EOT.
AR: corrected.
- Line 36: re: point 2 about "level fall in Antarctic coastal sediments"... Can you rephrase for increased clarity/nuance? My very quick reading of the two citations suggests that it's the combination of isostatically-induced relative sea-level *rise* along Antarctic coast close to the new ice sheet—with far-field and core evidence for sea-level drop elsewhere—that is the collective evidence for ice-sheet formation.

AR: indeed, that's correct. We changed it in the manuscript.

- Lines 51-53: Consider using the new CenCO2PIP compilation of Cenozoic CO2: <https://www.science.org/doi/10.1126/science.adi5177>.

AR: thank you for the suggestion. We included the reference and changed the values accordingly.

- Fig 7: As suggested by the reviewer of the revised ms, please re-examine the caption and figure (units on color ramps, etc).

AR: units on the color ramp were already included, but we changed the caption to match the panels shown.