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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "Constraining atmospheric CO_2 content during the Middle Miocene Antarctic glaciation using an ice sheet-climate model" *by* P. M. Langebroek et al.

Anonymous Referee #2

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Conceptually, this is a very interesting paper. However, I do have a number of serious concerns that should be addressed before publication. The manuscript is quite promising, but significant revision will be required before I can recommend it for publication. My comments and questions are meant to be constructive and I do hope that the authors (and editors) find them helpful.

As the authors acknowledge, the stepped isotope shift ~14 Ma that paleoceanographers have always assumed represents the "expansion of Antarctic ice" is far too big to be accommodated by Antarctica alone... and must include some deep sea cooling. Sure, the EAIS might have become bigger at this time (as supported by indirect sea

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Interactive Discussion

Discussion Paper

level estimates), however, proximal Antarctic records suggest the presence of significant Antarctic ice before this time (Pekar, Naish, etc...), so the stepped isotope shift can't simply be explained by expanding Antarctic ice, unless i) Antarctica was essentially ice free at the onset of the event, and/or ii) the isotopic composition of the growing ice sheet was lighter than -100 per mil- which isn't likely. Some additional review of what's known about the Antarctic environment before and after this event would be helpful, and would justify the attempt to simulate what is assumed by the authors to be a continental glaciation event. There is a nice paper by Shevenell et al., 2007 that should be cited. This paper gives a somewhat different (and more complicated) perspective on the Miocene climate transition than the one described here. They describe the transition as a protracted, stepwise, and orbitally paced event beginning about 15 Ma (a million years prior to the event described here), with the initial glacial advance occurring during a time of apparent warmth in the Southern Ocean. The reader needs more confidence that the authors have a firm sense of the event they are trying to simulate with their model. At the very least, it would be nice to see more discussion on how the model results fit into (or don't fit into) the up-to-date, data-driven understanding of the event.

I do appreciate the application of simple ice and climate models to problems like this and there's a lot one can do with axi-symmetric ice sheet models. However, there appears to be a problem with the simple 3-box EBM. It is tuned to yield a southern hemispheric sensitivity to a doubling of CO_2 of 2.8°C, which is certainly reasonable, but the climate sensitivity of the high latitude box appears to be too big. In section 2.2, the authors describe a CO_2 sensitivity of 11.6°C. Presumably this occurs in latitude bands where the snow/ice albedo feedback is greatest, however, not enough details are given to determine if this exaggerated response is widespread, how it impacts the ice sheet ablation zone, or how it compares with more spatially resolved 2x CO_2 GCM results. The extreme CO_2 sensitivity in the polar box results in a much lower CO_2-glaciation threshold (400 versus ~750 ppm) than shown in prior GCM-ice sheet modeling studies (Pollard, DeConto, etc.). Hysteresis in this model relative to CO_2 (~20 ppm CO_2) is also much smaller than in GCM-ice sheet models (>200 ppmv), simplified plastic ice sheet models (Pollard and DeConto, 2005), and standalone ice sheet models driven by prescribed climatologies (Huybrechts, 1993). The high sensitivity of the model to prescribed CO_2 forcing relative to orbital forcing appears to make the timing of glaciation somewhat arbitrary. In this case, it's all about the prescribed CO_2 forcing regardless of orbital configuration, which as far as I know, is contrary to most evidence from Cenozoic deep sea isotope records which suggest many Oligocene-Miocene glaciation events occur during nodes of reduced obliquity variance (Palike, Pekar, Zachos, Shackleton, etc.).

Interestingly, one of the fixed CO_2 simulations (400 ppm CO_2 in Fig. 3) does glaciate during a period of relatively low summer insolation, but this orbital event occurs $\sim$0.4 myr after to the event being considered here. What makes the orbital sequence at 13.4 Ma special and how does the one at 13.8 Ma compare? How does the 400 kyr orbital pacing between 15 and 14 Ma noted by Shevenell fit into the story? At the very least, issues such as these need to be better discussed and/or considered in the context of both prior modeling work by Huybrechts, Ritz, Pollard, DeConto, etc., and available Miocene proxy records including those from Antarctica (not just the deep sea).

As currently worded, the title implies that the model is capable of determining absolute values of Miocene CO_2 at the time of glaciation. This is somewhat misleading, considering the general lack of model validation, clear model-data comparison, or calculated CO_2 sensitivity over critical Antarctic accumulation and ablation zones. In my opinion, the title should be changed to better reflect the limitations of the study, which should stay focused on general model behavior rather than absolute levels of CO_2.

Given, the small hysteresis window of this particular model, figure 2 Implies that the EAIS will begin to rapidly disappear at levels of CO_2 that will be reached later this century! That maybe the case for WAIS, but most ice sheet modelers would agree that this is unlikely for the EAIS. Again, how does this model's behavior compare with prior 3-D modeling studies suggesting that $\sim$20K of warming is needed for major retreat of the EAIS (e.g., Huybrechts, Pollard, etc.)? Furthermore, contrary to the Shevenell and Holbourn data, the results are only weakly influenced by orbital forcing. Again, this maybe an artifact of unrealistic CO_2 sensitivity relative to orbital forcing. Tuning the model to yield a hemispheric average sensitivity of 2.8°C may not be sufficient. Perhaps some care should be given to compare the model's polar climate sensitivity (especially in the critical latitudes for ice sheet growth) with more sophisticated GCMs results. This issue could be explored in a future version of the manuscript.

Some more specific comments/editorial recommendations:

Abstract, line 10. Grammar: Remove "and" after "forcing".

Section 3.2, line 22, first word. Spelling: "Antarctica" not "Antarctic"

End of section 3.2: Why is the orbital lag for small ice sheets (5-6 ka) bigger than for large ice sheets (2 ka)? Shouldn't this be the other way around? I see that it's stated the other way around in the conclusions. Please check.

The results showing the greater importance of summer versus annual insolation is significant, and may bear on the recent discussion between Raymo and Huybers as to the bipolar influence of precession forcing (summer intensity) versus obliquity (summer duration).

Discussion. In the calibration between apparent sea level (100m) and ice volume ($33\text{x}10^{15}$ m^3), the equivalent ice volume looks a bit too small.

Section 4.3. CO_2- orbital sensitivity tests show glacial-interglacial variability that again implies surprisingly limited East Antarctic ice sheet hysteresis.

Section 5, line 5, "In the case..." not "In case..."

Section 5, line 16, "with a rate" not "with a speed"

The model description is clear and well written.

CPD

4, S523–S527, 2008

Interactive Comment

Page 873. Eqn. A1. Shouldn't there be a minus sign in front of the divergence term?

Page 875. Line 12. I think the author's mean to say that the "Latent heat flux due to evaporation is parameterized as ..." rather than "The latent heat of evaporation is parameterized as..."

Page 879. The authors crudely represent water vapor feedback by simply adding it to the CO_2 sensitivity term- rather than including a separate temperature-dependent water vapor feedback parameterization. Is the lack of explicit WV feedback contributing to the small response to orbital forcing relative to CO_2?

In summary, I really like the general concept and this work holds great promise. However, there appear to be too many fundamental model deficiencies at this time to allow the results from being relatable to a specific climate event or from "constraining" actual Miocene CO_2 levels as implied by the title. In my opinion, much more model validation and comparison with Antarctic climate sensitivity in more complex, spatially resolved models (like IPCC-class GCMs) is required before any such claims can be made. I'm not saying that these results are worthless, but I do think it's a stretch to consider them representative of what happened specifically at 13.8 million years ago. At the very least, the basic behavior of the model should be discussed in the context of prior modeling work on Antarctic glaciation, which from what I have read, show very different CO_2 sensitivities and hysteresis. The authors should also consider some of the new information coming from more direct, proximal Antarctic drilling records regarding the Cenozoic evolution of the Antarctic environment, and new information from deep sea drilling that complicates the notion of a simple Miocene glaciation event (e.g., Shevenell et al., 2007). I realize this will require some work on the part of the authors, but it will greatly improve the paper.

Interactive comment on Clim. Past Discuss., 4, 859, 2008.

CPD

4, S523–S527, 2008

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