

## ***Interactive comment on “Constraining atmospheric CO<sub>2</sub> content during the Middle Miocene Antarctic glaciation using an ice sheet-climate model” by P. M. Langebroek et al.***

**E. Wolff (Editor)**

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Both reviewers agree that this study covers an interesting topic, and that the approach could yield useful results. However, both of them are concerned that the simplicity of the model does not allow such definite results (such as an estimated CO<sub>2</sub> concentration) to be declared. Based on these comments (but pending the authors' reply), I have to conclude that this work is unable to deliver what the title claims, and is therefore not likely to be acceptable as a CP paper in its present form. However, I think there is a CP paper that can be made from this one, if the authors are willing to alter the thrust of the paper to make its conclusions defensible. If they do wish to submit a CP version,

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the authors should carefully address all the reviewer comments. In particular:

1. Unless the authors can convincingly argue against the referee comments, they should frame the work as an exploration of the CO<sub>2</sub> and insolation properties for a stable Antarctic ice sheet, rather than a specific exploration of exactly the 13.9 Ma period. I agree they can use this event as an example however, hence the next point.
2. The authors should certainly do more to present the context to the reader. I would specifically like to see an isotope curve that indicates what the behaviour being simulated is (eg Zachos curve). The authors will then be in a position to carefully explain which aspects of the Miocene (eg insolation) they are including, and which (eg palaeogeography) they are not. It will also allow them to discuss more of the previous work, as recommended by referee 2.
3. The authors should also discuss what experimental CO<sub>2</sub> data are out there for this period, and why they might not be entirely reliable, in order to give a context for the work.
4. There is a clear need to discuss further the climate sensitivity of the model, and in particular the apparent oversensitivity in the Antarctic region. If this high value is a valid criticism, then as a further sensitivity study the authors should repeat some of their runs with a climate sensitivity that will give more appropriate values in the Antarctic.
5. Once these issues are taken care of, the authors should consider carefully how their study adds to previous ones. My feeling is that it does not give definitive answers about CO<sub>2</sub> concentration at 13.8 Ma; however the authors have been able to explore a range of time-dependent sensitivities, including appropriate oscillations of forcings, that may allow them to point out some of the issues that may not be well-discussed in other studies.

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