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Climate of the Past Discussions

Interactive comment on "A new interpretation of the two-step $\delta^{\vec{18}}O$ signal at the Eocene-Oligocene boundary" *by* M. Tigchelaar et al.

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First of all, we would like to thank the referee for her thorough reading of the paper and the many stimulating comments. Below, we comment on the main concerns. Furthermore, all other (technical) comments will be corrected in the revised manuscript.

As a general remark, we would like to emphasize that the model used in this study is conceptual, and therefore direct comparisons with proxy data may be problematic. The main point, we show in this manuscript is that a switch in the MOC pattern may be a possibility to explain the deep-sea cooling in the first $\delta^{18}O$-step at the Eocene-Oligocene transition as the modeled temperature change is of the right order of magnitude. The trigger for such a switch is not necessarily decreasing CO_2 levels, but can rather occur spontaneously due to a (random) perturbation in the freshwater flux.

The second step, instead is linked to decreasing CO_2 levels, as it contains mainly ice growth. To our knowledge this possibility has not been considered before, and of course needs to be tested with more sophisticated models in the future.

1 Response to main concerns

1. *Constraints on NH E-O ocean salinity* To our knowledge there are not so many reconstructions of freshwater fluxes or North Atlantic ocean salinity across the Eocene/Oligocene boundary available. For the early to middle Eocene, both proxy and climate model studies indicate an enhanced hydrological cycle ((Speelman et al., 2010) and references therein, (Manabe, 1997; Huber et al., 2003)). This means in particular, that there is more excess precipitation at high latitudes, only weak changes at midlatitudes and more evaporation in the tropics and subtropics. This general feature is reproduced by our model (see Figure 3d). For the modern day case, the comparison against the Oberhuber climatology was mainly an order of magnitude check to validate the buoyancy flux calculations. More specific comparisons are not possible in this conceptual model.

   Under Eocene boundary conditions we have not found MOC states with warm salty equatorial deep water, although this possibility cannot be ruled out completely. In any case here, the modeled warm deep sea temperatures are not caused by warm, salty equatorial deep water. Due to the limited number of boxes in the ocean, these type of models tend produce too warm deep-sea temperatures.

2. *The mismatch between ocean temperature proxy records and the surprising deep sea and sea surface temperatures modeled for the northern hemisphere* The mechanism we propose works as long as on average Northern high latitude SSTs are colder than Southern high latitude SSTs *before* the transition. Proxy data

(Eldrett et al. 2009, Liu et al. 2009) are somewhat inconclusive about a North-South high latitude temperature difference. For example the Liu et al data indicate very warm SSTs at sites 277 and 1090 in the Southern Hemisphere, while the data from site 511 show SSTs similar to Northern Hemisphere SSTs. Eldrett et al. 2009 suggest that colder winters and cooler temperatures at high northern latitudes occur *before* the Oi-1 event. As deep water at high latitude most likely forms in winter, Northern Hemisphere temperatures may indeed be colder than Southern Hemisphere temperatures. Again, a direct comparison is difficult with this conceptual model, but on average we think that the model data mismatch is not so large.

Furthermore, our model results suggest even another scenario for a MOC transition, as pointed out by the second referee: A transition from an NPP state to TH would cause only a slightly smaller deep-sea cooling than a transition from SPP to TH and shows SSTs which are warmer in the NH than in the SH. However, such a transition would involve a warming in the Southern Hemisphere and would imply North Atlantic deep water formation in the Eocene. This is not consistent with proxy data suggesting that NADW formation has first occurred around the Eocene/Oligocene boundary possibly due to subsidence of the Greenland-Scotland ridge (see also comment to Dr. Abbot).

Therefore, we have chosen the SPP state as the Eocene reference circulation pattern. We will discuss this issue in more detail in the revised manuscript.

3. *Mismatch between measured and modelled 2nd $\delta^{18}O$ peak* There are two main reasons why the 2^{nd} δ^{18}O peak turned out to be too large: first of all, the maximum size of the Antarctic ice sheet in the model is set such that it is $9x10^16m^3$. This value comes from assuming a perfect plasticity ice sheet on a surface with no bottom topography that has a full meridional extent and has a parabolic profile in zonal direction. This is of course much more than Antarctica can actually accommodate. By imposing a maximum ice sheet size on Antarctica, this could

be easily overcome within the model. We will do this in the revised version. Secondly, once the CO_2 concentration drops below a certain threshold value the ice sheet starts to grow and there is no mechanism present that stops its growth. It will therefore grow to its full size. As discussed in the article, one can think of several factors that would have stopped ice growth. For instance, the pattern of CO_2 decrease was definitely not linear and can also have changed temporarily to CO_2 increase. Also there might have been phase locking with the obliquity, causing the ice growth to slow down. The value that we found should not be seen as the result of an accurate attempt to model the second δ^{18}O peak; rather it should be interpreted to show that the second δ^{18}O peak can be attributed to rapid growth of the ice sheet. We will add more discussion in the manuscript concerning this point.

4. *Lack of coverage of some significant areas of the literature* We fully agree, and will add more specific references in the revised manuscript.

5. *Lack of consideration for the role of tectonic changes in the North Atlantic in facilitating deep water exchange at the E-O boundary* Indeed, subsidence of the Greenland-Scotland Ridge would facilitate deep-water formation in the North Atlantic, and we should have discussed this in the manuscript. If timely, the subsidence of this ridge may be another cause for a switch between SPP and TH state.

6. *Misrepresentation of main point: not a new interpretation but a new mechanism* We agree with this comment and will change the manuscript accordingly. The new title could be 'A new mechanism for the two-step δ^{18}O signal at the Eocene-Oligocene boundary'

7. *Other scientific issues: Abstract: Line 11 "Furthermore, they did not address the potential role of changes in ocean circulation in the E-O transition." -Set the scene more accurately.* We agree and will change the revised manuscript accordingly.

8. *Summary and discussion -It would be helpful here to put the order over of events as the authors see it in perspective, thereby providing comment on what the significance the hypothesized change in the MOC played in the EOT. Gradual declining pCO_2 causes a switch in the MOC first and THEN glacial inception?*

   The switch in the MOC may have occurred due to (random) fluctuations in the freshwater flux, not necessarily due to a decreasing CO_2 level. A switch from SPP to TH is accompanied by slight cooling of the southern polar box (decrease in transport) and large heating of northern polar box (strong increase in heat transport). Therefore the switch will probably have caused the Antarctic ice sheet to form at a CO_2 concentration a little bit higher than it would have without the switch. It will have made NH ice sheet formation more difficult. It is not possible for us to elaborate on detailed effects of MOC on Antarctic glaciation and E-O climate change because the atmosphere is non-dynamic and effect of MOC on Antarctic meteorology is not resolved in this model.

2 Response to questions with text

1. *p1395 l15* We will add more references.
2. *p1399 l3 What about altitude?* In our model, each box only has a fraction of land or ocean. No altitude effects are considered.
3. *p1399 l16-20 Temperatures not in agreement with Bijl et al. 2009* It is difficult to compare the model results with specific proxy data sites, as there is only one box for the entire Southern Ocean, see also discussion above (Section 1, item 2).
4. *p1400 l4 Freshwater flux comparison of reference state against Oberhuber climatology: Is this North and South?* The comparison against the Oberhuber climatology was mainly an order of magnitude check to validate the buoyancy flux

   calculations. It would not make sense to compare modern-day measurements against Eocene model results to great detail. So indeed this comparison involved both North and South and as can be seen from Fig. 2 the Northern freshwater flux is not very different from the one in the South.
5. *p1400 l14 Over what time period was CO_2 falling?* From 1500 to 100 ppm at a rate of 750ppm/200kyr, so total time of decrease was ~373kyr.
6. *How does lack of ice in NH and growth pattern of SH ice sheet compare to DeConto 2003 & 2008? (p1400 l16,18)* In DeConto 2008 SH ice sheet starts to grow at ~750ppm and NH ice sheet at ~280ppm. In our model SH ice sheet starts to grow at 270ppm in SPP state and lower than 400ppm in TH state. No NH ice sheet forms. The discrepancy between the DeConto study and our study is due to the fact that in our model radiative forcing is represented in a very simplistic way (see Dr. Abbot's comment) so CO_2 concentrations should not be compared directly. Rather, we can conclude from our study that SH ice inception starts at a higher CO_2 concentration than NH ice inception and that this is in agreement with more sophisticated modelling studies. Nothing more about the pattern of SH ice sheet growth in our model can be said than that it is exponential. There is no topography so there are no locations where ice growth starts and from which it spreads out.
7. *p1403 l25 Why is the $\delta^{18}O$ shift 6 permil? Even 3.5 is too large.* Total shift consists of shift due to change in ocean circulation (0.5 permil), ice growth (3.5 permil of which 2.6 due to ice and the rest due to temp decrease) and background decrease of ocean deep sea temperature because CO_2 is continuously decreasing. The first shift has an OK magnitude, for discussion on why the second shift is too large see above. The background decrease in deep sea temperature is there because we assume a linearly decreasing CO_2 concentration. In reality CO_2 did not necessarily have to continue to decrease in between the steps. Fur-

thermore, the total CO_2 decrease in our conceptual model does not necessarily correspond to an equal CO_2 decrease in 'real life' (see Dr. Abbot's third comment) so background change in deep sea temperature as a result of decreasing CO_2 was probably less. See also above, section 1, item 3.

8. *p1405 l 2-3 What is spontaneous?* What we mean here, is that if the SPP state (or NPP if we chose that as Eocene reference state) ceases to exist at a certain CO_2 level, the system switches to another MOC state at that CO_2 level spontaneously.

9. *p1406 l18 What do you mean by full size of SH ice sheet? Describe potential accommodation space available.* See point on δ^{18}O excursion above.

10. *p1407 l13 No good northern hemisphere deep sea temperatures based on reliable data exist yet.* Even without reliable data of northern hemisphere deep sea temperatures it becomes obvious that our modelled deep sea temperatures are too warm, for instance by comparison against the Liu et al. 2009 model results.

References

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