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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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General comment This is an interesting modeling contribution to the challenge of trying to understand the complex set of ocean climate interactions that resulted in the abrupt formation of the Antarctic ice cap 34 millions ago at the Eocene-Oligocene boundary. It explores the possibility of a major change in meridional overturning circulation (MOC) from a unipolar Southern Ocean mode to a bipolar mode similar to modern. This is something that has been alluded to in sedimentary and geochemical proxy studies (Miller and Tucholke, 1993, Debbie Thomas and co workers as cited, Davies et al., 2001) but not modeled, as for as I am aware and in this respect it is a new and useful contribution.

Like all modeling studies of ancient climate states that we have only limited understand-

ing of, there will have been an element of tweaking and tuning the model to produce a satisfactory result, not necessarily the correct the result, but the model and method (I am not a modeler and cannot comment in detail on this part) appear to be mostly appropriate for addressing the question of changes in MOC during the E/O climate transition. The outcome is a set of focused questions that can be targeted with future proxy studies. There are however a number of major issues that I am concerned about including; 1) lack of discussion about constraints on northern hemisphere E/O ocean salinity, 2) the mismatch between ocean temperature proxy records and the surprising deep sea and sea surface temperatures modeled for the northern hemisphere, 3) the large mismatch between the magnitude of the modeled d18O, 3) the lack of coverage of some significant areas of the existing literature, and 4) a lack of consideration for the role of tectonic changes in the north Atlantic in facilitating deep water exchange at the E/O boundary. Another issue is that the paper fails to comment on the possible significance of the proposed change in MOC in driving or contributing to Antarctic glaciation and E/O climate change.

In summary, this modeling study at least suggests that a change in the MOC is plausible and presents a set of scenarios to be tested or explored with proxy data, although the timing of events is still unknown. However I think there are problems with the model at that need attention before this is can be published. The manuscript is mostly well written and structured, although some areas need reworking. The figures are of good quality and easy follow.

Specific comments The first thing I would say is that the authors slightly misrepresent what they are saying and showing: lines 14-15 Abstract "Here a new interpretation of the δ18O signal is presented, based on model simulations using a simple coupled 8-box-ocean. . .. . . It is argued that the first step in the d18O represents a shift in meridional overturning circulation from a Southern Ocean to a bipolar source of deep-water formation, which is associated with a cooling of the deep sea." ALSO p. 1396, line 7 "Here a new interpretation of the δ18O record will be presented, in which several parts of the climate system are involved and switches in the MOC play a dominant role." -These statements are a bit misleading. A signal for colder water first, then ice is fairly widely supported by proxy data, it's the mechanism for introducing the deep sea cooling that is new here. The statements above suggest that paper is going to say that d18O isn't related to ice. . .or temperature, and that no one has considered that ocean/deep water cooling contributes to the well documented d18O increase before. In fact at least 3 papers (Lear et al., 2008., Katz et al., 2008; Liu et al., 2009) presenting geochemical proxy data attribute the first step of the two-step d18O shift largely to cooling, the second step to ice growth. Similarly changes in deep water circulation, although have been suggested to play a role in E-O climate back to Shackleton et al., 1975. This should be acknowledged. What the Tigchelaar et al. study does that is new and a development in the field is to attribute the cooling to a specific mechanism, i.e. initiation of bi-polar deep water formation, and that the addition of deep water formed in the northern hemisphere that is colder than the Southern Ocean sources is what is showing up in the deep sea records. The text should be changed to reflect this.

Main concerns This paper provides suggests a scenario whereby the first step in d18O recorded in deep sea records represents deep sea cooling caused by a switch in the MOC to a bipolar deep water source. In the model the step pattern occurs because the pCO2 threshold at which the MOC switch (a density perturbation in the northern hemisphere) occurs is higher than that for glacial inception. This is an interesting result but I have some major issues with the model results. 1)What are the constraints for changes in fresh water budgets and buoyancy flux for the Eocene compared to the Oligocene? This needs more discussion (currently merely attributed to Oberhuber, 1988), especially since the proxy temperatures thus far disagree with the model. Additionally, how do the results fit in with the possibility of a warm salty equatorial deep water source to help explain the warm deep water temperatures?

2) In the simulation the deep sea experiences E-O cooling because the newly initiated northern component water is cooler than southern ocean deep water. This is not con-

sistent with organic biomarker proxy records that suggest: 1) very mild surface ocean temperatures (12-18°C) in the high northern latitudes (where deep water is likely to have formed; Norwegian Greenland Sea) in the early Oligocene, although seasonality likely increased with winter temperatures falling to 0-2°C across the E-O transition (Eldrett et al., 2009) and 2) even warmer surface waters, 18-26°C, in the late Eocene (Liu et al., 2009). Similarly, microfossil records are not consistent with sub-zero temperatures in the high north Atlantic during the studied interval (Eldrett et al., 2009; ODP Site 647, Shipboard Scientific Party, 1987). Moreover there is no indication that Southern Ocean surface waters were warmer than the high northern hemisphere as suggested here, instead the Lui et al. (2009) data set suggest that Southern Ocean waters were 4-5°C cooler than the Norwegian Greenland Sea both before and after Antarctic glacial inception, although temperatures in both regions may have cooled by 5-8 °C across the EOT. I can't see how the model results can be trusted if this basic parameter that is the 'thermal' part of the thermohaline circulation development is not realistically represented.

3) The size of the d18O isotopic excursion produced in the model -why precisely is this is so much greater than other models 6.00 ‰ compared to 1.0 ‰ elsewhere (e.g. DeConto and Pollard, 2003a, 2003b)? The existing explanation is not satisfactory.

4) Some important elements of the literature very relevant to this work that have not been cited specifically, and review papers are cited instead a few too many times (e.g. papers relevant to evidence for northern hemisphere climate, deep water circulation Davies et al., 2001; Eldrett et al., 2007; 2009 and references therein).

One significant aspect concerning MOC changes at the E-O has been completely omitted, that is the role of tectonic changes in the north Atlantic in facilitating deep water exchange. A body or work indicates that in the early Cenozoic, exchange between the Artic and North Atlantic basins was inhibited by the Greenland-Scotland ridge, that prevented deepwater exchange. The timing of subsidence of this ridge and gateway opening has been debated (Miller and Tucholke, 1983; Davies et al., 2001) but this consideration warrants discussion.

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. Modeled pCO2 forcing, very low resolution pCO2 records, impossible to prove cause and effect. DeConto & Pollard did consider circulation in terms of ocean heat transport in their model. Modeling thus far may have primarily focused on the Southern Ocean changes but make sure this is made clear.

Summary and discussion -It would be helpful here to put the order over 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 pCO2 causes a switch in the MOC first and THEN glacial inception?

Technical corrections: typing errors, etc. Note. Suspect use of language has also been highlighted in yellow on the attached PDF.

Abstract p.1392, line 7 "Simultaneous changes in the δ13C record are indicative of a greenhouse gas control on climate." -δ13C change may be suggestive but not indicative.

line 9-should be "Previous MODELING studies show that a decline in pCO2 beyond a certain threshold value may have initiated the growth of a Southern Hemispheric ice sheet. -Add 'modeling' to this sentence.

Reference correction p. 1393, line 24, Use Eldrett et al., references for N. hemisphere IRD in place of Coxall & Pearson, 2007

Include E-O circulation references: Sijp and England, 2004, (plus other papers by the author(s) on Southern Ocean E-O circulation). Also Cramer et al., 2009.

p. 1400, line 11, add 'continental. Line 18, is this consistent with DeConto et al., 2008? Line 14, over what time period was pCO2 falling? Line 16, "The threshold CO2 value for Antarctic ice growth is 270 ppm, at which the ice sheet starts to grow exponentially

and reaches its full size within 100 kyr." . . .. The Antarctic ice sheet starts to melt at 605 ppm, a value well above the CO2 concentration of ice inception. Explain how this compares to DeConto et al., 2003 and 2008?

Figure 2. Add 'SPP' to the Figure caption text, in the same way as TH and NPP are shown in figs 4 and 5 respectively, to highlight that this is the mode of MOC operation that has been reconstructed for the Eocene climate state.

p. 1403, line 25, why is the d18O shift ∼6 per mil?) p. 1404, line 9, even 3.5‰ is much too large. Why? lines 15-18. Should also consider topography. Explanation for the lack of ice in the NH. p. 1405, lines 2-3 I don't understand what this last sentence means. What is spontaneous?

Summary and discussion section p. 1405 "Thus far, the remarkable two-step profile of the d18O record at this boundary has remained unexplained." -Rephrase. That there are ice volume-increase AND deep sea cooling components to the two step d18O profile is not new. It's the nature of the cooling that is the unknown and the subject of this study.

p. 1406, line 18- "In our model simulations, the Southern Hemispheric ice sheet was allowed to grow to its full size, 9X1016 m3." What do you mean by full size? Describe the potential accommodation space available.

p. 1407, line 13, we don't have any good northern hemisphere deep sea temperatures based on reliable data yet.

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Eldrett, J. S., Greenwood, D. R., Harding, I. C. & Huber, M. 2009 Increased seasonality through the Eocene to Oligocene transition in northern high latitudes. Nature 459, 969-973. Eldrett, J. S. & Harding, I. 2009 Palynological analysesof EocenetoOligocenesedimentsfromDSDPSite338, OuterVøringPlateau. Marince Micropaleontology 73, 226–240. Eldrett, J. S., Harding, I. C., Wilson, P. A., Butler, E. & Roberts, A. P. 2007 Continental ice in Greenland during the Eocene and Oligocene. Nature 204, 91–127, doi:10.1038/nature05591. Katz, M. E., Miller, K. G., Wright, J. D., Wade, B. S., Browning, J. V., Cramer, B. S. & Rosenthal, Y. 2008 Stepwise transition from the Eocene greenhouse to the Oligocene icehouse. Nature Geoscience 1, 329-334. Lear, C. H., Bailey, T. R., Pearson, P. N. P., Coxall, H. K. & Rosenthal, Y. 2008 Cooling and ice growth across the Eocene-Oligocene transition. Geology 36, 251-254. Liu, L., Pagani, M., Zinniker, D., DeConto, R., Huber, M., H., B., Sunita, R., Shah, R., Leckie, R. M. & Pearson, A. 2009 Global Cooling During the Eocene-Oligocene Climate Transition. Science 323. Miller, K. G. & Tucholke, B. E. 1983 Development of Cenozoic abyssal ciruclation south of the Greenland-Scotland Ridge. In Structure and development of the Greenland-Scotland Ridge (ed. M. H. P. Bott, J. Thiede, S. Saxov & M. Talwani), pp. 549-589. New York: Plenum Press. Shipboard Scientific Party,1987 Site 647. In Initial Reports of the Ocean Drilling Program (Pt. A), 104, vol. 104 (ed. S. P. Srivastava, M. Arthur, B. Clement & e. al.), pp. 675-905. College Station, TX: Ocean Drilling Program. Sijp, W. P. & England, M. H. 2004 Effect of the drake passage throughflow on global climate. Journal of Physical Oceanography 34, 1254-1266.

Wright, J. D. & Miller, K. G. 1996 Control of North Atlantic Deep Water circulation by the Greenland-Scotland Ridge. Paleoceanography 11, 157–170.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/6/C716/2010/cpd-6-C716-2010-supplement.pdf

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