

Interactive comment on “Internally generated millennial-scale climate variability in an earth system model of intermediate complexity: sensitivity to ocean bathymetry and orbital forcing” by T. Friedrich et al.

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[10pt]article

Response to Reviewer's Comments

Anonymous Referee #2

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General comments:

The authors investigate simulated low-frequency AMOC oscillations in detail. The description is clear and relatively easy to follow. I appreciate the sensitivity experiments in Sect. 4.4 that attempt to test individual pathways of a suit of oscillation processes. I found two points that make me difficult to recommend this manuscript for publication in the GMD in the current form. The first point is that I have a doubt that this paper matches to aims and scope of the GMD (see the major point 1). The second point is that the experimental design precludes addressing relevance to DO events that is the focus of this paper (see the major point 2). I think the first point should be examined carefully by the editor (and it may not be my position to judge). As to the second point, I would like to add that the results of the sensitivity experiments are of theoretical interest and useful.

Note that negative line numbers indicate counting from bottom of the page in the following.

Major points:

1. I do not see a reason why this paper is suitable for the GMD. The contents are not really "description", "development", or "evaluation" of numerical models. The purpose of this study is to test a hypothesis in paleoclimate. This study deals with rather idealized theoretical experiments, and may be somewhat related to paleoclimate. Other journals whose focus includes paleoclimate modeling may be recommended although the relevance to the actual paleoclimate is still weak to my view. According to the GMD web page, the followings are the aims and scope of the GMD: a) Geoscientific model descriptions, from box models to GCMs; b) Development and Technical papers, describing development such as new parameterisations or technical aspects of running models such as the reproducibility of results c) Papers describing new standard exper

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iments for assessing model performance, or novel ways of comparing model results with observational data; d) Model intercomparison descriptions, including experimental details and project protocols. I do not find any of these points apply to the current manuscript.

In the revised version, the manuscript has a stronger focus on the evaluation of the model's behaviour and the mechanism that triggers low-frequency AMOC oscillations in ECBilt-CLIO. We do not wish to link the characteristics of this variability to the background climate, but to inform the (paleo-)modelling community about the artefact that seems to be responsible for having generated centennial-to-millennial scale AMOC oscillations in previous publications.

2. This study examines the AMOC oscillations with three different obliquity values: 22.8, 22.4, and 22.1, and oscillatory solutions are found with the two small values. The obliquity was larger than 22.8 between about 62 and 36 kaBP when the DO events were pronounced. Therefore, the experimental setup does not allow us to answer whether the actual DO events were modulated by obliquity. The results have some theoretical interest, but the context in ways that the results are presented is confusing. For this reason, it is not convincing that OBL22.4 experiment that is well described in the paper has something to do with DO events in addition to the suppression of oscillations with more realistic ocean bathymetry.

The authors agree with the reviewer that our experimental setup does not allow us to answer whether the actual DO events were modulated by obliquity. As mentioned above we focus in the revised manuscript on the mechanism behind the low-frequency AMOC variability rather than on its relation to obliquity. We

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show now that the centennial-to-millennial scale AMOC oscillations occur for a wide range of parameters and that they are caused by a model artefact.

3. In the introduction, the authors contrast 'flip-flops' (between multiple equilibria) and deep decoupling mechanisms, or stochastic resonance and coherence resonance in the presence of external noise as a possible mechanism of the DO events. In the conclusion, however, there are not many discussions on that.

The concept of our paper does not allow for verifying or disproving existing hypotheses about DO mechanisms. Thus, the authors refrain from discussing those hypotheses in the revised version of the manuscript.

4. The title of Sect. 4.2 says "millennial-scale", but the oscillations look more "centennial-scale". Remarks from spectral, autocorrelation, or wavelet analysis would be helpful here. If such analysis is not suitable to the simulated pulse-pause oscillations, a little more explanation/description of the time scale is useful. In addition, the authors should state clearly which process really determines the time scale of these oscillations. Why do these oscillations, arising from the combination of advective and convective feedbacks, exhibit such "millennial-scale" low-frequency variability? Another important point that is not mentioned is that the simulated oscillations do not exhibit a feature of the DO events: gradual cooling and abrupt warming.

We added two more model simulations to Figure 2 (and removed one) to show

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that the low-frequency AMOC variability can be observed for a wide range of background climate conditions. As indicated by Figure 2, the pulse-pause ratio differs significantly for the different simulations. In the revised manuscript we refer to them as “centennial-to-millennial scale” oscillations or “low-frequency” AMOC variability. Furthermore we clarified that we clearly distinguish them from DO oscillations.

Minor points:

1. p.274, l.-5: This is just a comment: my impression is that "bipolar see-saw" behavior is more suggestive of AMOC relevance to the DO events, rather than "abruptness" as wind-driven circulation and atmospheric processes can be as abrupt as AMOC.

We agree with the reviewer. The text was changed accordingly and refers now to the "bipolar see-saw" behavior.

2. p.275, l.5-8: What do you mean by "hysteresis behavior...is weak"? Please clarify.

The paragraph was removed

3. p.280, last paragraph: While the contrast between SR and CR is highlighted in the

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introduction, the reason why the authors consider the oscillation in Fig.2 reflects CR and not SR is not discussed upfront. It would be useful to describe the evidence on which the authors conclude that the simulated oscillations reflect CR and not hopping between multi-stable solutions.

The paragraph was removed

4. p.281 and p.285: Atmospheric processes that establish ridges over Greenland and Hudson Bay from cold anomaly in GIN Sea is poorly described in the text while oceanic processes are described rather in detail.

The authors explain and show through a sensitivity experiment that the atmospheric anomaly pattern establishes in response to ocean SST anomalies triggered by the weakening of the overturning. Since the mechanism we describe has to be regarded as a model artefact we do not believe that there is the need for a more detailed description.

5. p.284, l.-2: typo: "a lower boundary conditions"

The typo was corrected.

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6. Figs. 3 and 7: Please improve the visibility of contour lines.

The Figures have been improved.

7. Fig.7: “apply SAT climatology” should be “apply SST climatology”.

The typo was corrected.

8. Fig.8: vertical axes say “OBL22.8” but the caption says “OBL22.4”.

The label for the vertical axes was corrected.

9. p.285: I like Sect. 4.4, but would like to comment two points. 1) It would be helpful to add another figure of climatological SSS and surface ocean current anomaly vectors to Fig.7; and 2) If the wind anomaly is causing the flush of freshwater from Hudson Bay to Labrador Sea, why prescribing SSS in Hudson Bay changes the result? Is climatological low salinity in Hudson Bay already enough to cause the ‘flush’ as long as the wind anomaly is generated by atmospheric teleconnection?

Our explanation of the sensitivity study using prescribed temperature and salin-

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ity in the Hudson Bay was somehow misleading. Prescribing both variables suppresses the flush of freshwater triggered otherwise by the surface wind stress anomaly. In our description of the triggering mechanism we make clear that it is the combination of the freshwater flush and the increase in snow fall that causes the reduction of the Labrador Sea overturning.

10. p.286 last paragraph: The authors should also mention the quantitative aspect of temperature variations over Antarctica as EPICA Community Members (2006) is cited.

We added a sentence mentioning that the simulated warming over Antarctica during the weak AMOC state accounts for ~ 50 % of the increase in SAT estimated by the EPICA Community Members (2006).

11. p.289: I do not see the evidence that indicates the importance of existing noise in the simulated oscillations, which are mentioned in a couple of places in the text. I understand the concept, but it would be nice if the demonstration is more easily identified. Is it possible to point out a figure or elucidate it in the text?

As mentioned above, in the revised version of the manuscripts the authors would like to focus less on discussing DO concepts as they cannot be verified by our simulations. Thus the sentence was removed from the “Conclusions” section. However, the simulated reduction of the GIN Sea overturning is generated through a sea-ice-overturning feedback that is triggered by noisy variations in

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the sea ice coverage and the strength of the overturning.

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