

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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The manuscript by Friedrich et al. presents in depth analysis the mechanisms of millennial scale variability which has been previously "observed" in the ECBilt-CLIO model by a number of workers. The authors found that simulated abrupt climate changes are associated with the flushes of low salinity water from the Hudson Bay which suppressed deep convection in the Labrador Sea. Since Hudson Bay was covered by thick ice sheet during glacial times, the authors concluded that this specific mechanism is unlikely to be the right one for explaining Dansgaard-Oeschger (DO) events observed

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during glacial times. I believe, this is an interesting and useful paper and I am only a bit surprised that the manuscript was submitted to GMD rather than to CP, for example.

General comments

1. The authors proposed the following chain of events during the transition from the warm to the cold climate states: random reduction of the inflow of the Atlantic water into GIN seas → surface cooling in GIN seas → changes in the atmospheric circulation over the northern N Atlantic → enhanced inflow of Hudson low salinity water into the Labrador sea → suppression of the deep convection in Labrador sea → reduction of the AMOC. While such chain of events makes perfect sense to me, I am not fully convinced that the change in the atmospheric circulation (wind stress over the Hudson Strait) is the primary cause of the lowering of Labrador Sea salinity and shoaling of convection. The authors argue their case by using Fig. 3b (unfortunately, this figure is of very low quality). However, this figure shows also a strong cooling over the Labrador Sea which implies that convection was already considerably weakened there at the given time interval (denoted as the interval “c”). This makes it hard to determine what is the cause and what is the consequence. It would be more logical to show changes in the atmospheric circulation for the interval “b” prior to convection change in Labrador Sea. In this case, I guess, the atmospheric circulation changes will be more alike Figure 7b. However, in the later case, changes in the wind over the Hudson strait seem to be opposite to that shown in the Fig. 3b. In short, I do not doubt the role of enhanced flow of low salinity water through the Hudson Strait into Labrador Sea as the cause for suppression of convection there but I am not sure what triggers this enhanced flow.

2. If abrupt climate changes simulated in the ECBilt-CLIO model are indeed caused by variations of freshwater flux through the Hudson Bay, then they hardly can be considered as the direct analogy for the glacial DO events. Still, I would not rule out the possibility that a number of mechanisms involved in the simulated abrupt climate changes can be relevant for the understanding of the real DO events, such as strong climate impact of a relatively modest reorganization of the AMOC, changes in the deep

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water formation areas, the role of the sea ice and the role of subsurface warming in the abrupt resumption of the AMOC. Note, that the late issue has been already discussed in detail in Mignot et al. (2007).

3. I would like to comment on the interpretations of my own works given in the manuscript. Firstly, the authors cited the wrong paper (Ganopolski et al., 1998). This paper presents simulations of the LGM climate but not the stability analysis or simulation of DO events. The right citation would be another Nature paper - Ganopolski and Rahmstorf (2001). Secondly, I would respectfully disagree with the authors' interpretation of our concept of the DO events. The CLIMBER-2 model does possess hysteresis behavior under present day climate but, as it was shown in Ganopolski and Rahmstorf (2001), the hysteresis essentially disappears under glacial climate conditions and we explained DO events as the transitions between two strong modes of the AMOC which differ between each other primarily by the location of the deep water formation areas. We do not consider DO events as the transitions between "on" and "off" modes of the AMOC which many workers still use to explain and simulate (e.g. Liu et al., 2007) abrupt climate changes. Therefore the modes of the AMOC operation which we invoked to explain DO events are "fundamentally different from the multiple equilibria" of the Stommel's model. On the other hand, our concept is not fundamentally different from the Winton's "deep decoupled oscillations" since it invokes both advective and convective instability. The latter, as in the Winton's case, occurs through the development of subsurface warming. The only difference is that in some models (usually hemispheric) these oscillations occur within some (usually very narrow) parameter space in the noise-free case whilst in the CLIMBER-2 model, with the standard set of parameters, the noise-free oscillations do not occur. However, adding a weak forcing or a random noise leads to the development of millennial scale variability, as was shown in Ganopolski and Rahmstorf (2001, 2002). Since ECBilt is a "noisy" model and this noise cannot be easily switched off, it is not possible to conclude whether the simulated variability represents deep decoupled oscillations (in the original sense of this term, i.e. self-sustained oscillations in the noise-free system) or they are noise-induced

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oscillations (e.g. coherence resonance).

Specific comments

Page 275, first para. I agree that “the jury is still out” in respect of the existence of the AMOC hysteresis. But I do not understand how Liu et al. (2007) paper is related to the mechanism of DO events, in particular, that proposed in Ganopolski and Rahmstorf (2001). I guess, the authors are also aware that the “abrupt warming” simulated by Liu et al. (2007) is in fact almost hundred (!) times slower than that occurred in reality at the onset of the Bolling event. In this respect, the jury is definitely in.

Page 276, first para. I do not understand the meaning of the sentence “in the presence of external periodic forcing SR and CR behave very differently”. SR occurs only in the case of periodic forcing and CR in the absence of periodic forcing. If the authors are talking here about the difference between bi-stable system and the system with one stable and one excitable state, then both systems still can behave similarly under applied periodic forcing. Namely, the stochastically excited oscillations can be synchronized with the external forcing in both cases.

Page 276, second para. “Orbital forcing is a likely candidate..” I would assume that the ice sheets and CO₂ are even more likely candidates in the view of their much stronger impact on climate than the orbital forcing alone.

Page 278, second para. I see no sense in such lengthy description of the LOCH model since the reader can find it in the “neighboring” GMDD paper.

Page 281, line 19. What is “surface boundary layer high pressure anomaly”?

Page 281, last line. “wind stress near Hudson Strait changes its direction”. Firstly, I would suggest to specify the Hudson Bay area for which the wind is shown in Fig. 4. Secondly, Fig. 4a shows that the wind direction over the Hudson Bay remains negative (southward) even during cold events and therefore Ekman transport cannot explain the “flush of freshwater from the Hudson Bay”.

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Page 284, line 6. What is “prevailing obliquity”?

References

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