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Climate of the Past Discussions

Interactive comment on "Orbital modulation of millennial-scale climate variability in an earth system model of intermediate complexity" *by* T. Friedrich et al.

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Anonymous Referee 1 Received and published: 15 September 2009 The authors describe results of climate model simulations focusing on the effect of changes in obliquity on the Atlantic Meridional Overturning Circulation (AMOC). For present day (interglacial) boundary conditions the model exhibits centennial to mil- lennial time scale oscillations for low obliquity values. These model oscillations are analyzed in some detail, leading to the conclusion that freshwater fluxes from Hudson Bay into the Labrador Sea are important. For glacial boundary conditions, presumably due to the removal of Hudson Bay, these oscillations are not observed. The authors conclude therefore that

these model oscillations could not represent the millennial oscillations observed in the paleoclimate record (Dansgaard/Oschger oscillations). Obviously this conclusion is correct. I also think that this conclusion is one of the few useful aspects of this paper that might be publishable.

AUTHORS' REPLY Recent studies by Rial and Yang [2007] and Rial and Saha [2008] left the impression that millennial-scale, D/O like AMOC oscillations can be triggered in the LOVECLIM model whose pacing is related to the background climate. Our manuscript presents a thorough analysis of those oscillations that clearly disproves the conclusions of the papers mentioned above.

As a reader of the manuscript, however, one is disappointed and has the impression of having wasted time by going through all the model analysis of these oscillations only to learn that they have no relation to the real climate system and are pure model fiction.

AUTHORS' REPLY The authors understand that one might feel disappointed to learn that the observed oscillations have in fact nothing to do with D/O oscillations. (We as authors were disappointed when we found out.) However, the bottom line of our analysis and conclusion (that we did not find D/O oscillations) is already pointed out in the abstract. Moreover, we believe that our analysis helps to assess previous claims that these model solutions are real. To make a convincing case we have decided to present a thorough description of the model physics.

The analysis of the impacts of these oscillations on global temperature, precipitation, oxygen and carbon cycles is cursory at best. Many recent studies analyzing the impact of AMOC changes are not cited.

AUTHORS' REPLY Our findings with respect to global temperature, precipitation changes reflect well known characteristics of D/O oscillations that have been published before. Thus the authors refrained from presenting a more detailed analysis that would only be repetitive. If the reviewer wants us to include specific

references he/she may want to offer suggestions. The literature about AMOC impacts is vast. With respect to the changes in marine, terrestrial and atmospheric CO2, our study is (at least to our knowledge) the first to present and analyze the impact of internally generated millennial-scale AMOC oscillations on the carbon cycle. However, results with respect to millenial-scale oxygen variations have been removed from the 'impact' part.

I'm also concerned about the freshwater flux correction the authors are using. suggests that an important process affecting the AMOC is not (or not adequately) represented in the model. The usefulness of such a model for AMOC studies is therefore questionable. The authors do not seem to be worried about this because I find no discussion about it in the manuscript. An easy way to address this would be to repeat the simulations without the freshwater flux correction.

AUTHORS' REPLY The simulated tropical trade winds in the ECBilt model are too weak which results in a too small moisture transport from the Atlantic to the Pacific. In order to generate an Atlantic salty enough for an AMOC, we apply a freshwater correction. Such an approach is quite typical for EMICs which can not resolve all features of the atmospheric circulation realistically. This is now mentioned in the 'Model configuration' section of the revised manuscript on page 4. The freshwater flux correction used in the model helps to create a stable AMOC in the model. The re-distribution of freshwater that makes the North Atlantic saltier and the Pacific fresher does not contribute to the oscillatory behavior of the AMOC found in the simulations, but makes the AMOC in fact less vulnerable to the stochastic excitations described in the manuscript.

Overall the paper is substandard and I recommend rejection. I could see a much shorter paper possibly publishable, concentrating on the analysis of the oscillations that have already been found by other users of that particular model and showing that they go away for glacial boundary conditions. The impact section should be completely left out.

AUTHORS' REPLY In spite of the unphysical trigger mechanism, the teleconnection response to the internally generated millenial-scale AMOC changes is well reproduced by our model. Hence we have decided to keep these relevant results in the paper.

Anonymous Referee 2 Received and published: 18 September 2009 The authors describe centennial-to-millennial-scale AMOC (Atlantic meridional over- turning circulation) variability in an earth system model of intermediate complexity ("LOVECLIM"). This variability occurs only when very low obliquity values (< are applied to the model. The authors suggest that stochastic disruptions of deep convection in the Nordic Seas induce reorganizations of the atmospheric surface wind pattern which in turn favor the flow of fresh water from Hudson Bay into the Labrador Sea. As a result, deep water formation in the Labrador Sea ceases (or is at least substantially reduced), thus amplifying the total weakening of the AMOC. Moreover, the authors describe the marine and terrestrial carbon cycle response to the AMOC variations. The authors conclude that the mechanism of the simulated AMOC variations is "fundamentally different from the one that triggered real Dansgaard-Oeschger events during the last glacial period.

Major problems 1) The authors argue that a "flush of fresher water from the Hudson Bay into Labrador Sea" is the reason for the shutdown of convection in the Labrador Sea. freshwater flush in turn is triggered by a change in wind direction over Hudson Strait. This process, however, is not convincingly shown in the model analysis. The authors should show a timeseries of the freshwater flux through Hudson Strait along with the timeseries of the AMOC.

AUTHORS' REPLY Figure 3c,d of our manuscript show the wind field, sea surface height (SSH) and sea surface salinity (SSS) anomalies in the vicinity of the Hudson Bay. The anomalously low SSH in the Hudson Bay associated with an anomalously high sea surface in the Labrador Sea and the related SSS anomaly with a maximum in the Hudson Bay clearly provide evidence for a fresh water flush being responsible for the shutdown of convection in the Labrador Sea. In

the revised manuscript we also show and discuss now a timeseries of SSS and meridional wind speed at the Hudson Strait for the entire model simulation (new figure on page 20, discussion on page 8). From this additional figure it becomes apparent that flushes of fresher water from the Hudson Bay into the Labrador Sea are the reason for the shutdown of the Labrador Sea convection for all individual AMOC events.

2) The authors argue that a deep-decoupling mechanism is responsible for the recovery of the AMOC. This is a mechanism of multicentennial timescale. The much shorter weak-AMOC events in experiment OBL22.4 around years 3300 and 4250 do not support the notion of an important role for deep-decoupling in reanimating the AMOC.

AUTHORS' REPLY Deep-decoupling mechanism involves a threshold in vertical stratification. The timescale of this process depends on initial perturbation of the stratification (e.g. the strength of the halocline) and the advective and/or diffusive timescale which erodes the vertical stratification. Thus the timescale of deep-decoupling can be a few decades to several centuries.

3) Another comment on the proposed deep-decoupling mechanism: I understand why there is a sub-surface warming when convection is stopped or at least substantially reduced (Fig. 3f). I also expect a concurrent increase in sub-surface salinity (unfortunately, sub-surface salinity changes are not shown). However, when both sub-surface temperature and salinity increase, why should there be a significant decrease in sub-surface density? A concurrent sub-surface salinity increase would tend to stabilize the water column.

AUTHORS' REPLY The reviewer is absolutely right. Part of our analysis shows that the concurrent increase in sub-surface salinity reduces the destabilizing effect of sub-surface warming on the vertical density gradient. Yet the thermal effect turns out to be stronger than the haline, eventually leading to a complete erosion of stratification. We have added a few sentences in the revised

manuscript describing this effect (page 7 and 8).

4) The authors try to underpin the deep-decoupling argument with an additional experiment in which sub-surface temperatures are kept constant (Fig. 7). The whole concept would be more convincing if both sub-surface temperature AND salinity were kept fixed in the experiment (see my argument above). The experiment (as it has been carried out) provides no insight into the mechanism of AMOC recovery.

AUTHORS' REPLY The experiment shows that even if salinity is exclusively allowed to increase and to stabilize the water column for 150 years, sub-surface warming will lead to an erosion of the stratification. We believe that the design of the experiment is actually pretty good to elucidate the role of deep-decoupling. If salinity was kept fixed as well the recovery would only be faster.

5) Why does it take a hundred years for Labrador Sea convection to switch on again after deep water formation in the Nordic Seas has recovered (phase g in Fig. 3). This timescale is not consistent with the proposed mechanism.

AUTHORS' REPLY After the resumption of the overturning in the Nordic Seas the recovery of the AMOC starts immediately. To gain full strength again anomalously fresh surface water needs to removed from the North Atlantic. The timescale of this process determines the timescale of the complete AMOC recovery.

6) In their explanation of the mechanism behind the AMOC variations the authors focus on the OBL22.4 experiment and sell this mechanism as Âń universal Âż. However, in experiment OBL22.1, the relationship between GSOC (GIN Seas overturning circulation) and Labrador Sea convection (reflected in total AMOC) is not as evident as in experiment OBL22.4 (see Fig. 2, bottom). There are phases with strong GSOC but weak AMOC (i.e. Labrador Sea convection), for instance between years 1000 and 1500 or after year 4500. This clearly contradicts the mechanism proposed by the authors for AMOC variability

AUTHORS' REPLY The reviewer is right in the way that the connection between GIN-sea overturning and AMOC is actually hard to see in Figure 2d,h. As stated in the reply above, the recovery for the AMOC takes longer than for the GIN-sea overturning. Thus, the observed shorter-term variability of the GIN-sea overturning is not reflected by a complete recovery of the AMOC. A very close look at Figures 2d and to 2h however reveals that an increase in GIN-sea overturning leads to an increase in the AMOC. But the latter is interrupted by the subsequent shutdown of the overturning in the GIN-sea.

7) To underpin the role of Hudson Bay freshwater in perturbing Labrador Sea convection, the authors perform an additional sensitivity experiment with LGM boundary conditions. However, this experiment is badly designed to support a role for Hudson Bay, because the setup includes changes in runoff mask as well as "drying" of the Barents Sea and Siberian shelves (due to the application of a LGM land-sea m other words, the effect of a removed Hudson Bay is not studied in isolation. As a consequence, ocean dynamics in the Nordic Seas changes fundamentally: As shown in Fig. 11, GSOC no longer reaches the 4-5 Sv that are typical in the other OBL22.1 ex- periment, but fluctuates between 1 and 2.5 Sv. What is the reason for this suppression of GIN Sea convection? Probably not the removal of the Hudson Bay.

AUTHORS' REPLY: We share the reviewer's concerns that using a LGM-bathymetry does not study the role of the Hudson Bay in triggering the observed AMOC events in isolation. Thus, for the revised manuscript we added a sensitivity run in which temperature and salinity was kept fixed in the Hudson Bay. This run provides great support for our mechanism. Even though the initial GSOC weakening still occurs in the manipulated run, no AMOC shut-down is observed. This is now discussed in our manuscript on page 9. A figure was added at page 23.

8) The most disturbing aspect of the manuscript is the lack of a Discussion section. The authors describe centennial-to-millennial-scale climate variations in a model that

only occur when the obliquity is sufficiently low. Is there any proxy evidence for the occurrence of such oscillations in the real world? Obviously, the authors do not describe glacial Dansgaard-Oeschger oscillations. So what else are they describing? Interglacial Bond cycles? But these appeared in the Holocene, i.e. a time when obliquity was relatively large. It is very likely that the oscillations are pure model artefacts without any counterpart in the real world. Without a clear discussion, the reader is completely lost.

AUTHORS' REPLY: As already pointed out in the reply to referee 1 the authors intended, inter alia, to present a thorough analysis of the millennial-scale AMOC oscillation found by Rial and Yang [2007] and Rial and Saha [2008]. In contrast to their conclusions we show that the observed oscillations are no D/O oscillations and can be regarded as a model artefact.

Minor comments Section 2: "Bering Strait is closed in our simulations". Why? This doesn sense to me.

AUTHORS' REPLY: The Bering Strait was closed in MIS 3 when the majority of D/O events occurred. In order to carry out a simulation as realistic as possible, we decided to run the model with a closed Bering Strait.

Section 4.5: "Hence weak AMOC states are accompanied by an overall increas atmospheric CO2 by about 10 ppm." Fig. 10a suggests typical CO2 variations of 6 ppm.

AUTHORS' REPLY: The reviewer is right. The given number was too high and has been corrected.

Fig. 3: The maps display anomalies against which interval?

AUTHORS' REPLY: The anomalies are shown against the strong overturning state (years: 2400-2450). This is now stated in the manuscript.

Fig. 3d: Missing SSS contour labels.

AUTHORS' REPLY: There are contour labels given for SSS in our version. Maybe a pdf problem?

Fig. 3 (caption): "wind speed (arrows)". Wind speed is a scalar quantity, the magnitude of the vector of motion! Shown is wind v

AUTHORS' REPLY: The reviewer is right. This has been corrected, according to the reviewer's suggestions.

Fig. 7a,b: What is shown on the y-axis? I assume depth in units of metres.

AUTHORS' REPLY: Yes, the y-axis is depth in units of meters. This has been corrected, according to the reviewer's suggestions.

Fig. 8: Only the global response for experiment OBL22.1 is shown. How do the patterns look like for OBL22.4?

AUTHORS' REPLY: The bi-polar seesaw and the changes in precipitation scale with the strength of the AMOC reduction. The pattern for OBL22.4 looks the same.

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