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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.

Anonymous Referee #2

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The authors describe centennial-to-millennial-scale AMOC (Atlantic meridional overturning circulation) variability in an earth system model of intermediate complexity ("LOVECLIM"). This variability occurs only when very low obliquity values (< 22.4°) 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 the Labrador Sea" is the reason for the shutdown of convection in the Labrador Sea. The 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.

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.

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.

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.

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.

6) In their explanation of the mechanism behind the AMOC variations the authors focus on the OBL22.4 experiment and sell this mechanism as « universal ». 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.

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 mask). In 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 experiment, 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.

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 artifacts without any counterpart in the real world. Without a clear discussion, the reader is completely

lost.

Minor comments

Section 2: "Bering Strait is closed in our simulations". Why? This doesn't make any sense to me.

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

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

Fig. 3d: Missing SSS contour labels.

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

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

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

Conclusion

In summary, the mechanism proposed for centennial-to-millennial-scale AMOC variations is not convincing. There is a lack of an in-depth analysis and a discussion that involves proxy evidence for climate variability under low obliquity values. As a reader, I got the impression that modeled climate variations are described which have no counterpart in the real world. I do not recommend publication of the paper in "Climate of the Past".

Interactive comment on Clim. Past Discuss., 5, 2019, 2009.