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Climate of the Past Discussions

Interactive comment on "The impacts of Meltwater Pulse-1A in the South Atlantic Ocean deep circulation since the Last Glacial Maximum" *by* J. M. Marson et al.

Anonymous Referee #2

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This submitted manuscript started with a good introduction, but then clearly fell short in the execution. The results presented here are from a transient paleoclimate simulation performed with NCAR's CCSM3 model (Liu et al, 2009, He, 2011). The hypothesis in the introduction is formulated imprecisely and is in fact not really at the center of the discussed results. It is not so much about the timing of the onset of present-day like conditions, but more about the differnces between two simulations with different freshwater input. Similar things have been done before (Roche et al., 2010 for the North Atlantic; Menviel et al, QSR, 2011 for a transient simulation LGM-Pre-Industrial).

The transient simulation (TraCE-21K) was 'constructed' to match best the proxy data. In particular the simulation was very carefully designed with regards to the timing and

amplitude and region of freshwater input into the ocean. This was done based on the excellent expertise and careful analysis of proxy data sources where they compared the modeled climate with proxy records. But once such a simulation was achieved to represent successfully the proxy evidence, one has to be careful in the use of the model results to make new inferences. I have the feeling that most of the discussion here is a circle-argument, in particular after screening the details about the freshwater setup in Dr. He's Dissertation. I suggest the authors use their gained insight to explain in some more depth how the freshwater forcing in the Southern Ocean spreads in the oceans, and how it controls differences in ocean and atmosphere, and as suggested in Stouffer et al. (2006) to explore in more depth if 'fingerprinting technices' can help to constrain the meltwater contribution in the Southern and in the North Atlantic Ocean using a network of proxies.

Also I found the figures were not really giving support to the propositions in the result section and conclusions.

Perhaps, age tracers and oxygen concentrations are not available from this run, but they would be very helpful to identify the ventilation of the deep and intermediate ocean layers, marking of NADW, AAIW etc.

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More Specific comments:

Introduction:

p.6376 l17. The last deglaciation started around 21,000 yeasr ago, when orbital changes led to increase in northern hemisphere summer insolation that prompted an initial melting that started a chain of feedback processes that amplified the global deglacation trends (Denton et al. 2010, Clark et al. 2012).

Need to acknowledge the alternative hypothesis that SH insolation forcing may have played an active role, in particular to help to raise CO2 levels and all-seson global

temperature increase [Stott et al, 2007, Timmermann et al. 2009, Huybers and Denton, 2008].

At least it should be mentioned that CO2 is also important in the whole deglaciation (Kerr, Science, 223, 1053-1054, 1984).

Model description:

p.6379: When was Bering Strait opening? This affects the salt export and overturning in the simulations and should be discussed for the establishment of AMOC to present-day values. (Dr. He's disseration has information on the timing, I think it is after the BA in the YD)

(Hu et al. 2012, PNAS, 109, 6417-6422, doi: 10.1073/pnas.1116014109)

What is meant by several meltwater schemes: Meltwater input into NAtl and SO were tried in different proportions? What proxy data information was used to select from the various forcing experiments. Was it based on reproducing MWP1a/YD sequences of climatic signals in ocean and atmosphere. Was any weight given on the Holocene climate, too? This is worth noting, because the main hypothesis and conclusion in the end are focusing on the Holocene circulation onset (p.6378 l. 10-13). In other words the results from freshwater experiments not shown here, do they all end up in a state with low Overturning in the Holocene?

p.6379 Equation: Integral has upper and lower bounds. Please add them.

Results:

I would prefer to start with the 'typical' depiction of the 2-d structure of the overturning circulation and then go into the description of the representative grid box average etc.

p 6380, l. 2-3: "In contrast with the other oceans, [...]" Well there are only two other oceans: The Pacific, and the Indian Ocean. The Indian Ocean has no polar Northern Hemisphere extension and is not really comparable to the situation in the Atlantic.

Leaves one other ocean, the Pacific.

p.6380 l10-13: This statement is way too much simplified: (a) it does not match the anomalies during LGM (Fig.2 which would be a 'warm NH' relative to the Holocence. The problem is that the authors assume that the total NH temp (anomalies) are explained by the ocean heat transport in the overturning in the Atlantic. The atmospheric fluxes must be included of course (see for example the recent paper of Donohoe et al., J. Clim, Vol 26, 3597-3618, 2013). (one p. 6381 l.4-8 the authors describe and try to explain the LGM exception, so I assume they want to stress the H1-BA-YD time, i.e. the statement works for millennial-scale variability, not orbital-scale variability)

p. 6381 the descripition of the water masses:

two deep water masses can be distinguished in a T-S phase space by the end members and mixing of them would be along a line between them. Now looking at Figure 4b one could argue, that due to the much more salt release into the AABW during brine rejection in the sea ice formation process the LGM AABW has been extremely salty. And if AAIW did not change that much in temperature and only more or less in the same way as the global average salinity decreased, one could at any point in time define the mixing line of these two water masses. The NADW may have experienced some changes in temp and salinity 'physically independent' from AABW and AAIW formation processes, such that is was lying always between AAIW and AABW in terms of temperature, and shifted in salinity at a rate between the AAIW rate of change and AABW rate of change. At a certain point in time the connecting mixing line AAIW-AABW could line up with the NADW. Now does that mean that the NADW water mass not formed or formed at a lower rate or replaced in its depth range by the AAIW-AABW mixing? Additional tracers (age tracers, Epsilon Nd., d18O,d13C) would could help to more firmly support the water mass formation, spread and mixing.

However, Fig. 5 gives a better dynamical perspective, but this follows after this parapgraph and that's why it is probably better to start with the 2-d structure of the overturning

and water masses before showing Fig. 3, in my opinion.

In addition, snap shots of the mixed layer depth, convective regions, sea ice formation or concentration could help to support the described changes in NADW

(p.6381 l.22-29: again the most convincing argument comes with Fig. 5 in my opinion)

p.6382 l4-5: rewrite this sentence ("does not only impact in reducing the entire water" (?) and "along the Atlantic" (?))

p. 6382 l.12-13: Denton may not be the best reference for the attribution of the H1 event to the first meltwater input into the N Atlantic. I am not aware of the this new idea that the 19ka freshwater forcing directly caused H1. Please be specific as to the causes of the early sea-level rise (i.e. meltwater input and implications for AMOC). Clearly in the simulation the H1 event is forced by the later freshwater input.

p.6382 l. 25-28. Stouffer et al describe and illustrate the surface spread of the salinity anomaly, but details of the vertical advection/mixing processes that eventually mix the anomalies into all depths must be explained here more clearly, since the stratification is a stabilizing anomaly.

p.6383 l. 22 This is an example where the statement is correct but the information is conveys is misleading: "The TraCE-21 model results are consistent with the idea of a weaker NADW during YD [...]" Because the model simulation was 'designed' to be consistent with this YD reduced AMOC / NADW. So, it should be put not into the results section but the model description.

Conclusions:

p. 6384 l.6-16 The summary should follow the temporal order from LGM to PD. Confusings otherwise.

p.6384 l.17-19: This conclusion goes too far! In his dissertation Dr. He describes clearly that Holocene AMOC levels are sensitive to freshwater forcing following during

YD and Holocene. Your statement suggests a delayed effect of more than 4000 years are more important (is that what 'much' means suggests ?) than the direct forcing control? Well, yes the legacy of SH vs NH may have an important role in the further evolution, but from previous experiments (Stouffer et al, 2006 for example) and the old debate about multiple equilibria and hysteresis effects I would argue that the experiments and the analysis are not sufficient to support such a conclusion.

p.6384-6385: That part of text in a conclusion? I have hard times to 'transfer' deglaciation processes and AMOC of millinnial time scale into time periods, where glacial cycles were absent or completely different than in the last 1,000,000 years. Not needed.

Last paragraph: Repeated content. What's the point? Not always do we need a connection to the future climate change debate. Understanding past climate is enough of a scientific challenge. If you want keep it, it must be written in a more convincing way that the BA period analyzed here gives more information than Stouffer et al. (2006) could have 'extracted' from their study, for example.

Figures: Suggest to add anomaly plots to Figure 5

Make Figure 5 Figure 1

Enhance font size in figures such that labels can be read with ease!

Suggested References:

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