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Interactive comment on "Hosed vs. unhosed: global response to interruptions of the Atlantic Meridional Overturning, with and without freshwater forcing" *by* N. Brown and E. D. Galbraith

N. Brown and E. D. Galbraith

nic.brown0@gmail.com

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We thank Dr. Arzel for taking the time to read our paper and provide us with comments. The comments bring up a number of excellent points, which we address in a point-by-point form below.

"This manuscript presents results of millennial abrupt climate changes simulated by a global coupled model in hosed and unhosed situations. The idea is to compare the climatic impact of a collapsed AMOC in forced (hosing expt) and unforced (where intrinsic

variability emerges) simulations. The paper is relevant, and certainly deserves publication in CP, because internal variability of the coupled ocean-atmosphere system on millennial timescales is often overlooked when interpreting the paleo record. However the description of the unhosed variability (evolution of ocean/climate variables during an oscillation cycle and primary mechanism of the variability) is not convincing, and this would deserve further analysis.
Introduction (p4671). "most importantly through the existence of multiple stable states". I would add "and to the existence of *unstable* states". Mechanisms like stochastic resonance, noise-induced transitions do require multiple stable states (e.g. Timmermann and Lohman, 2000) but other mechanisms of abrupt climate changes are jumps between *unstable* states of the circulation. The example of deep-decoupling oscillations (Winton, 1993) is just like that (see Colin de Verdière, 2007)."

This is a good point. We will modify the phrasing of that sentence in the revision.

"Experimental design (p4674). The boundary forcings used by the authors seem to have been chosen to obtain intrinsic millennial variability in the model. Why have the authors applied a preindustrial ice-sheet topography in the unhosed experiment combined with a glacial value for the CO2 concentration ? Does the intrinsic variability emerge when all the boundary forcings are imposed to glacial values ? This should be discussed here or elsewhere in the paper. It would be useful that the authors provide the values of the main parameters used in their study (horizontal/vertical turbulent diffusivities at least) so that other groups of scientists can replicate or at least compare their results to the present ones."

Indeed, the context of the unhosed simulation was not properly explained. Essentially, this was one of about 50 'equilibrium' model simulations spanning a matrix of CO2 (120, 147, 180, 220, 270, 405, 607 and 911 ppm) orbital configurations (varying obliquity and precession) and with either pre-industrial or full glacial maximum ice sheets. Of all these simulations, only the one with a

CO2 of 180, pre-industrial ice sheets, low obliquity, and near-present precessional phase showed clear millennial AMOC variability. The main reason behind this appears to be that under these boundary conditions, the salinity difference between NADW and AABW is small, facilitating an oscillation between them, whereas with full glacial ice sheets, altered wind patterns strengthen the AMOC. Obviously, the real AMOC oscillations occurred under a broader spectrum of boundary conditions, which may reflect model inaccuracy and/or the importance of external drivers (such as variable freshwater input). Nonetheless we think this is a useful illustration of the general sensitivity of AMOC oscillations.

A separate manuscript is currently in preparation discussing the full suite of simulations. However, given that both reviewers also requested further clarification of this point, we see that it is worthwhile expanding on here. Therefore, in the revision we will add a figure, and some text, briefly relating the unhosed simulation to the full suite of simulations.

"Simulated changes in the North Atlantic (p4676). The authors present a description of the unhosed variability that is not supported by any analysis in the paper. There is no heat or salt budgets below the thermocline or in polar regions that are presented that would support the chain of events proposed by the authors. One figure presenting such budgets appear necessary at this point."

Given that the focus of the paper is on comparing the response of global climate to AMOC weakenings, hosed or unhosed, we did not go into detail regarding the mechanisms behind the unforced oscillations. However, we can rewrite this paragraph and add one figure in the revision.

"Simulated changes in the North Atlantic (p4677). Here the authors suggest that the mechanism is similar to that proposed by Kaspi (2004). This is very surprising since the mechanism proposed by Kaspi (2004) relies on an interactive ice-sheet component, but

ice-sheet topography is prescribed in the model used by the authors... So how could the present mechanism be the same as that proposed by Kaspi (2004) ?"

Thank you for pointing this potential point of confusion. We had intended to refer to the idea, discussed by Kaspi et al., that sea-ice can amplify relatively small AMOC changes to produce large variations in Greenland temperature. But, it is true that the mechanism proposed by Kaspi2004 includes ice-sheets that discharge freshwater. In light of this fact, we will rewrite the relevant sentences in the revision with better use of citations.

"Similarly to my previous comment, the authors here do not bring any material to the reader to support their claim (differentiate one mechanism rather than another). Determining the origin of the existence of variability in a coupled model is not an easy task. Sensitivity experiments such as the ones performed by Arzel et al. (2010) and Arzel et al. (2012) are necessary to extract this kind of information. In these studies the origin of intrinsic millennial oscillations (similar to deep-decoupling oscillations) was ascribed to the reduced oceanic poleward heat transport in the North Atlantic (which weakens the negative temperature-advection feedback and thus decrease the stability of the circulation)."

We agree that causality is difficult to show unambiguously. Since we have not performed such sensitivity experiments to determine the origin of the variability, we would rather keep the focus of the paper on the comparison between hosed and unhosed rather than speculating on details of the unhosed simulation.

As mentioned above, we propose that the revision include a rewrite of the paragraph explaining more thoroughly the mechanism of the millennial variability (p.4676), and leave a full understanding of the dynamics behind the oscillations to further work.

"Conclusions (p4683). Existence and properties of intrinsic millennial oscillations are sensitive to vertical mixing (Peltier 2014) but also to the latitudinal profile of freshwater forcing (Colin de Verdière, A., 2007), CO2 levels and ice sheet topography (Arzel, O. et al. 2012). In summary such oscillations are sensitive to the background climate state (see also Wunsch, 2006 who proposed an alternative view). Some references should be added here."

Thank you for the suggestions, we will add these references.

Interactive comment on Clim. Past Discuss., 11, 4669, 2015.