

## ***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***

### **Anonymous Referee #2**

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This is an interesting study that tackles an important question in paleoclimate; that of whether or not the abrupt transitions between interstadial and stadial conditions during Marine Isotope Stage (MIS 3) were forced by the addition of freshwater to the North Atlantic. Although the study does not provide a definitive answer to this question, it leaves open the possibility that these major transitions could have been spontaneous (self-generating). Although the model used is not extremely high resolution and the background conditions employed in the ‘non-hosed’ simulation are unrealistic, I think that the results are sufficiently novel and important enough to warrant publication in CP

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after some adjustment.

This study joins a number of others to show spontaneous (i.e. without external forcing) transitions in Atlantic Meridional Overturning Circulation (AMOC) using a complex coupled climate model. Combined with recent data-based studies, there is mounting evidence that the long-standing paradigm in explaining so-called Dansgaard-Oeschger (D-O) variability (i.e. freshwater hosing across the North Atlantic) may not survive the final analysis. The paper describes the results of several hosing experiments using the coupled ocean-atmosphere model CM2Mc performed with different background conditions (ice sheets, CO<sub>2</sub>, orbital configuration), which broadly agree with other studies using a variety of different models. In addition, the authors have found a combination of settings (low CO<sub>2</sub>, pre-industrial ice sheet configuration, low obliquity and weak boreal seasonality) which give rise to spontaneous transitions between weaker and stronger modes of the AMOC. Of particular note is that changes in various output parameters (when normalised to a common AMOC weakening) are very similar between all of the experiments (both hosing and non-hosed scenarios), with orbital configuration often more important as a controlling variable. An important implication of this study is that hosing experiments are still a valuable means of assessing the remote implications of AMOC variability.

Comments in order of appearance:

P4675 line 8: The un-hosed oscillations are different from those of Peltier and Vettoretti (2015) in that theirs were more symmetric (roughly similar duration for strong and weak mode) whereas these are biased towards the cold mode. Please comment?

P4675 line 9: The authors use a highly unrealistic model setup for their un-hosed experiment and I presume that they do not find the same transitions when using more realistic conditions like Peltier and Vettoretti (2015)? The authors should comment more on this and add a line about the ‘observed’ occurrence of D-O variability being limited to conditions of intermediate ice volume. This is one of the weaker parts of the

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paper and should be treated with more detail to avoid dismissal by a section of the readership.

P4675 line 11: Why not use the same flux of FW as the other hosing experiments?

P4675 line 20: Can the authors comment on the apparent lack of sensitivity to orbital configuration for the pre-industrial experiments?

P4679 description of Fig. 5: Why do we see such pronounced changes in intermediate depth [O<sub>2</sub>] in the un-hosed experiments, particularly the Equatorial Atlantic?

P4680 Section 3.4: For the un-hosed experiment the final 300 years or so seem to enter a 'third state', with a strong AMOC and high salinity North Atlantic but intermediate values for Greenland temperature and N. Atlantic ice free area. Can the authors comment?

P4681 Line 10: I note that the authors do not normalise for the difference between hosed and un-hosed scenarios (okay) but then they compare the difference to 'absolute' values shown in Figs. 3-7. But this is not a real comparison because those values have been normalised.

Figs 3 and 4: I can't make out the contoured values. Perhaps add 2 additional plots in each Fig to show std dev separately?

Minor comments:

P4670 line 1: Better to stick with data-based studies for this set of citations, rather than model studies using freshwater.

P4670 line 2: by 'early on' you mean in early studies (or early in the D-O cycle?)

P4670 line 2: layers can't be dramatic – you mean that the layers imply that a dramatic event must have occurred.

P4670 line 11: Meaning of 'plumbed' here?

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P4672 line 24: IRD is associated with most if not all stadials (e.g. Bond and Lotti, 1995).

P4680 line 3: Add refs – e.g. Hendy and Kennett (1999, Geology)

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

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