This submission examines CLIMBER-2 AMOC response to FWF (freshwater forcing) scaled to inferred global ESL changes starting at the Heinrich Event 1 interval. The main conclusion is that that such a forcing would not induce an AMOC response consistent with paleooceanographic inferences. Aside from the issues raised by the anonymous reviewer (especially in the context of the “strawman” nature of this submission) another fundamental problem of this study is that the relevant FWF is that which makes it as a low salinity plume to the sites of NADW formation (as already raised by Tarasov and Peltier, 2005, especially in their supplement, among others) and/or to other relevant regions considering some of the other proposed mechanisms involved. This will depend on regional discharge (In additions to Anders Carlson’s comments consider model based studies of Tarasov and Peltier, 2006 and Wickert, 2016) and subsequent advection by ocean currents (cf Condron and Winsor, 2012). Another major flaw is an insufficient consideration of relevant literature, again as detailed below. This study also needs a critical self assessment of the CLIMBER-2 ocean model (which is not a primitive equation model and only models 2 dimensional flow in 3 basins and yet none of this is even mentioned in the submission) and the extent to which CLIMBER-2 AMOC response to FWF is realistic. Given it's simplifications, I can't see how CLIMBER-2 could even plausibly assess sensitivity to regional (eg Gulf of St. Lawrence versus Hudson River) FW injection.

These flaws can in part be addressed by: 1) a more careful and thorough review of relevant litterature, and 2) clarification of the difference between scaled global ESL changes, regional FWF, and the fraction thereof that makes it to the sites of NADW formation and thereby making clear what the submission actually examines as opposes to what it claims to examine, 3) critical reflection on what CLIMBER-2 can and can't assess, and thereby 4) more accuracy and clarity in what the study is actually assessing. The authors also need to make clear that they are not addressing the "question as to whether the FWF applied in these studies is realistic or not".

detailed comments

However, the question as to whether the FWF applied in these studies is realistic or not has never been assessed in detail.

I partially disagree with this statement. One need only compare the traditional hosing approach to the study of Tarasov and Peltier (QSR, 2006) that rigorously inverted deglacial FW drainage from North America along with that of Condron and Winsor (2012) that assessed where this FW would end up. There is still the problem that actually regional FWF will also depend on changes to regional precipitation and not change regional ice mass loss, and this is much harder to constrain.

More to the point, this submission is examining CLIMBER-2 response to scaled ESL FWF. It provides no evidence that this FWF is realistic nor does it assess this realism.

coupled ocean-atmosphere oscillatory mechanisms ((Peltier and Vettoretti, 2014)) related to the formation of super polynias (Vettoretti and Peltier, 2016),

or climate-related feedbacks favouring the existence of a glacial oscillator (Arzel and England, 2012; Dokken et al., 2013; Banderas et al., 2015; Zhang et al., 2017b).

It should be made clear, that none of these mechanisms are exclusionary of the other.

Ice-sheet reconstructions such as ICE-5G (Peltier, 2004b), ICE-6G (Peltier et al., 2015) or GLAC-1D (Tarasov et al., 25 2012) are based on past sea-level changes and isostatic adjustment of the Earth's crust to ice unloading during deglaciation, and have yielded objectively constrained deglacial meltwater histories. However they are poorly constrained due to their coarse time resolution (between 1 and 0.1 kyr) resulting from the large depth and age uncertainties in individual sea-level proxies, and to uncertainties in isostatic rebound and drainage reconstructions

This is an incomplete description of constraints and sources of uncertainty. It's also problematic to group ICE-5G in this group as it is based on Lego-like manipulation of ice blocks (except for the Greenland component which is based on my glaciological modelling) with no glaciological physics. All the reconstructions also depend on C14 dated constraints on ice margin positions along with other constraints for ICE-6G and GLAC1-D.

GLAC-1D has limitations due to limited climate forcing degrees of freedom (and general uncertainties in climate but has the strength of Bayesian inversion, while ICE-* rely on hand tuning and therefore lack any uncertainty assessment.

furthermore, "poorly constrained due ... uncertainties in .. drainage reconstructions" is incorrect. ICE-5,6G do not use drainage as a constraint. GLAC1-D did use Mississippi drainage as a constraint, but that (approximate changes in outflow) is reasonably well constrained from GOM paleo-oceanographic records. GLAC1-D also used inferred opening of major choke points as a constraint, but again that is based on well-dated lacustrine shorelines. Yes there are uncertainties (especially related to the position of the ice sheet margin in Keewatin during the Younger Dryas interval), but much smaller than the ones you are invoking by assuming regional FW output is proportional to change in global ice volume.

re-analysis of the dating of glacial retreat evidence (Clark et *al., 2009) nevertheless support the idea that a large component of MWP-1A originated in the AIS, and coupled climate model studies have shown that if this MWP had been sourced from the AIS*

What do you mean by "large"? And why do you not cite the glaciological Bayesian inversion for North American deglaciation of Tarasov et al, 2012 that found the "North American contribution to mwp1a was likely between 9.4 and 13.2 m eustatic over a 500 year interval", which along with Eurasian contributions (3-4 m) would not require any significant Antarctic contribution. You should also mention that the more recent work of the RAISE consortium of glacial and marine geologists found no evidence for major Antarctic retreat during MWP1-a (special issue, QSR, 2014)

The first scenario, based on Barbados Uranium/Thorium-dated fossil coral records, 30 place it around 14.2 ka BP or earlier (Fairbanks et al., 2005; Peltier and Fairbanks, 2006; Stanford et al., 2006).

The second scenario, based on radiocarbon dating from the Sunda Shelf, suggests the MWP-1A started 300-500 years earlier (Hanebuth et al., 2000).

I don't see the contradiction. In GLAC1-D, mwp1a starts about 400 years earlier than 14.2 ka and ends around 14.1 ka.

Stanford et al. (2011) have ...
An additional pulse (MWP-1B) is identified just after the end of the Younger Dryas (11.3 ka BP).

That has been identified well before the Stanford et al study, eg already described in Peltier and Fairbanks, 2006

Finally, because Stanford et al. (2011) represents global ESL, the source of MWP-1A is not well established.

This statement requires consideration of the relevant literature and not just a single study that was focused on inferring global ESL changes.

Finally, a new ensemble of simulations with FWF forcing in the Southern Ocean (FSO) was carried out as well. The forcing imposed in this case was specifically chosen so as to guarantee compatibility with both the ice-sheet based reconstructions and Stanford et al. (2011).

A more complete description of the experimental setup is needed. What model parameters were varied across the ensemble?

However, the critical field here is not the ESL but the actual FW forcing, which is the time derivative of ESL

This is incorrect. regional FWF is not the time derivative of ESL, only global FWF is. And AMOC response will depend on where that FW enters the ocean and how it is advected to sites of deep convection.

Because the ESL is the time-integral of the freshwater forcing, abrupt changes in freshwater forcing can go unnoticed in the ESL.

This claim makes no sense to me, wrt basic calculus if you have a continuous record. As such, this statement has to be much more carefully qualified

The former results highlight an inherent unsolved feature of the evolution of the climate system within glacial climates; namely, how can we reconcile a major meltwater pulse (the MWP-1A) concomitant or slightly lagging an AMOC resumption (the BA)? How can FWF forcing then possibly be the trigger of such a resumption?

You need to refer to Roche et al, climdyn 2010.

et al. (2012) or Peltier et al. (2015), ... The same occurs if the more recent reconstruction by Stanford et al. (2011) ESL is used (Figure 3).

How is 2011 more recent than 2012 and 2015. Need to be more precise as to what you mean by more recent.

antarctic -> Antarctic

However, according to Peltier (2004b), Tarasov et al. (2012) or Peltier et al. (2015), this is far from being the case since actually no FWF decrease can be inferred from these sea-level records after 17 15 ka BP

Why are you not referring to any study that actually extracted freshwater discharge into each major drainage basin, such as Tarasov and Peltier, QSR, 2006 and Wickert, 2016?

Note Golledge et al. (2014) simulated a maximum contribution to MWP-1A from 10 the AIS of ca. 2m provided an abrupt warming in the Southern Ocean is able to trigger destabilization of the AIS.

Should also mention other major AIS reconstructions (Whitehouse et al, 2012, and Briggs et al, 2014) also fail to get an AIS contribution to MWP1-a of even 2 mESL (even considering a 500 year window in the case of Briggs et al).

Considering FWF as the major forcing for deglacial abrupt climate changes would thus require a deeper understanding of the role that the location and distribution of FWFs have on the oceanic response

should cite Condron and Winsor, PNAS, 2012 in this regard

Despite the recognition of some of the authors of the disagreement with the sea-level reconstructions (Liu et al., 2009; Carlson et al., 2012) a clear comparison of the FWF forcing with the ESL-implied FWF has never been shown until now.

Which "the FWF forcing" are you talking about?

We have shown here that when such an exercise is conducted, the timing of the abrupt events during deglaciation is completely incompatible with proxies

No you have not since that would require the regional FWF reaching the sites of deep convection

FWF derived from sea-level records

This is the fundamental flaw given claims made

Regardless of the reconstruction used, the FWF required to simulate an AMOC evolution that is consistent with pale-oceanographic reconstructions (McManus et al., 2004) is inconsistent with the available ESL reconstructions

Again incorrect statement for previously stated reasons.

additional references

@Article{levg8,
  author =   {L. Tarasov and W. R. Peltier},
  title = {A calibrated deglacial drainage chronology for the {North American} continent: Evidence of an {Arctic trigger for the Younger Dryas}},
  journal =   qsr,
  year =   2006,
  volume =   25,
  number =   {7-8},
  pages =   {659-688}

A systematic study of the impact of freshwater pulses with respect to different geographical locations

Didier M. Roche , Ane P. Wiersma , and
Hans Renssen
Clim Dyn (2010) 34:997–1013
DOI 10.1007/s00382-009-0578-8

Reconstruction of North American drainage basins
and river discharge since the Last Glacial Maximum
Andrew D. Wickert
Earth Surf. Dynam.., doi:10.5194/esurf-2016-8, 2016