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Climate of the Past Discussions

CPD

Interactive comment

Interactive comment on "Impact of melt water on high latitude early Last Interglacial climate" *by* E. J. Stone et al.

Anonymous Referee #2

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Overview

The recent review of high latitude climate records by Capron et al., (2014) showed that the early Eemian (ca 130 ka) was anomalously cool at northern high latitudes and anomalously warm at southern high latitudes (when compared to model simulations forced with orbital and GHG variations). In the current manuscript Stone et al conclude that this data versus model discrepancy is explained by meltwater discharge into the North Atlantic. To justify their conclusion they present global climate model simulations showing that the temperature discrepancy is reduced when 0.2Sv of freshwater is applied to the North Atlantic.

I congratulate the authors on their synthesis of observations and model results. Once some weaknesses are addressed I think the manuscript will make an important contribution to our understanding of Eemian climate.

I have two main concerns. First, I agree that the model results support the hypothesis that freshwater fluxes to the North Atlantic reduce the data model discrepancy. However, I do not think that this *proves* that freshwater fluxes are the explanation. Second, the authors invoke the 'bipolar seesaw mechanism' without defining what they really mean. It is not the authors fault that the bipolar seesaw mechanism is loosely defined in the literature, but I think it would help in this case to be more clear about the main process they have in mind. I expand on these points below. If the authors can address these and also the minor technical points that I raise further down then I am very happy to recommend publishing the paper in *Climate of the Past*.

Major points

- *Fresh water fluxes*: Freshwater fluxes (FW) are commonly applied to suppress deep water production and AMOC strength in climate models. In this case the size of the perturbation, 0.2 Sv, is supported by data. The 0.2 Sv FW flux is plausible, but I think the authors should be a little more cautious in their conclusion (e.g. in the abstract) that FW release is what 'accounts for' the observed temperature anomalies. Recent work on the timing of IRD layers, AMOC changes and temperature anomalies provide a good lesson on why such caution is advised. Barker et al., (2015) and Alvares Solas et al., (2013) have shown that the Heinrich Event 1 freshwater release into the North Atlantic and marginal seas comes *too late* to have caused the AMOC shutdown seen in proxies during the early part of the last deglaciation. Furthermore, recent papers have presented alternative triggers for AMOC changes, such as salt oscillator in the North Atlantic (Peltier and Vettoretti, 2014) or changes in Laurentide ice sheet height affecting wind-stress over the sub-polar gyre (Zhang et al., 2014). Climate changes at northern high latitudes due to shifts in modes of atmospheric circulation also remains a possibility (Kleppin et al., 2015), as appears to be the case in the NorESM simulation cited in the text (p5l21). All this is to say that while FW forcing reduces the data model discrepancy it does not rule out alternative mechanisms for triggering millennial-scale cooling of the NH; the authors need to acknowledge this in the revised version. Some discussion of alternative mechanisms would strengthen the paper.

- The HadCM3 simulations are run for 200 years. I doubt that this is long enough to see the final result of changes in ocean heat transport on Antarctic temperature. The recent work by the WAIS Divide Project Members (2015) shows that during MIS3 the *onset* of the bipolar seesaw signal in the WAIS ice core systematically lags Greenland transitions by ca 200 years (i.e. they report not seeing any signal for the first 200 years). The Antarctic warming in response to FW discharge in the North Atlantic appears to be arriving sooner than this in the HadCM3 simulations - which begs the question: how is the signal propagated so quickly to the southern high latitudes? I don't think the answer to this question is needed in the current manuscript, but the authors should at least acknowledge that Antarctic and Sth Ocn temperatures have probably not completed their adjustment to the change in ocean heat transport.

- p5l18: Stocker's (1998) perspective covers several possible mechanisms for out of phase climate changes in Antarctica and Greenland. The authors do not spell out which of these mechanism they are referring to. Is it the concept, mostly attributed to Crowley (1992), of a change in northward heat transport in the Atlantic? Or is it Broecker's (1998) idea of competition between NADW and AABW production? Some more discussion is needed here and some additional references.

Technical and minor points

1. Figure 4 and p4l18: It's counterproductive to begin the results section by comparing the 130k time slice with the Turney and Jones (2010) data. Three reasons: (1) TJ2010 is not the new result here so why put it first. (2) As is pointed out, the TJ2010 assumptions of synchronous temperature changes across the Eemian and of annual mean temperature estimates are flawed. (3) In any case, it It doesn't make sense to compare their 116-130ka slice to your 129-131ka mode time slice (as you say, any similarities are misleading!). I suggest to cover TJ2010 in the introduction and perhaps later in the discussion, but remove from Figure 4 and remove from the start of the results section.

2. In all figures the temperature anomalies that are not significant according to a t test need to be masked out.

3. p2 l17: ..early *onset of* warming..

4. p4 l32-p5l11: The flow of the results section is interrupted by the digression to talk about two methods of calculating RMSE. I would help the reader to focus on the results if the RMSE methods were moved to a subsection of the methods.

5. It appears that the RMSE is being calculated without including the uncertainty in the observations. Since observational uncertainties are provided by Capron et al there is no excuse not to make full use of them here. The observational uncertainty should be listed each time an RMSE is given for the data vs model comparison (or the equivalent data should be tabulated). Better still would be to give the data vs model RMSE in the form of a 95/

6. Figure 4 and 6: Please state in the caption how the anomalies are calculated. Compared to present day control HADCM3 run?

7. p4l2: Simulations are mentioned with FW varying from 0 to 1 Sv 'to determine the sensitivity of the model to FW forcing under the LIG climate regime (Fig 3)'.

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But Fig 3 just shows where the FW was applied. Reading on I see that the results of the sensitivity study come up near the end of the Discussion. The choice to focus on the 0.2Sv forcing is an essential part of the experimental design and so should be justified early on. I would suggest to move these details on the model's AMOC sensitivity to the methods section and also to include a reference to the current Fig 9 in the methods section.

8. p5l12: You should mention here the 12Sv reduction in the AMOC.

9. p5l14: Is 3.3C still a significant discrepancy considering the observational uncertainty?

10. p5l30: I can not find where Lunt et al (2008) discuss the influence of AMOC changes on Sth Ocn SSTs and I can not find where Vellinga and Wood (2002) discuss changes in advective heat transport to Antarctica. Please expand or revise. Pedro et al., (2016), goes into some detail on how AMOC variations may affect Antarctic and Sth Ocn temperatures and should be cited here; they emphasise the importance of sea ice changes.

11. p6l6:'only modest'. Rephrase, since the upper estimate of 4.3m is equivalent to a rather immodest 70% of the 6 m estimate.

12. p6l16: Some more discussion of the results compared with Steig (2015) would be useful. For example, do Steig's results lend support to a collapse of WAIS already by 130ka?.

13. p6l24: The decision to replace WAIS with shrubs comes with no reference or argument about why shrubs are an appropriate land cover compared for example to bare ground (as in the Dry Valleys today). Please either justify this choice or revise, also consider whether Figure 8c is really necessary.

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14. p7l6: Now it becomes more clear that changes in northward heat transport within the AMOC are what you propose explains the North Atlantic cooling. Hence the Crowley (1992) mechanism should be cited earlier.

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