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Climate of the Past Discussions

Interactive comment on "Interhemispheric coupling and warm Antarctic interglacials" *by* P. B. Holden et al.

Anonymous Referee #3

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This manuscript by Holden and co-authors focusses on the question of the warm interglacials preceding our own and more specifically on the interglacials that were warmer than our own.
They aim, as explained in the abstract, at understanding why such warmth then existed in the Antarctic (as shown by the ice-core record) in contrast to our interglacial. Their claim is that it is linked to the bipolar See-Saw mechanism during the deglaciation.

The manuscript is, in my opinion and in its present form, far from being satisfactory. For the reasons given below, I think it is not acceptable in its present form for publication in climate of the Past and requires major revision. There is need to further review a revised version.

1 Major points

A first consideration has to do with the very setup of the manuscript. While the authors want to investigate the mecanism leading to warm interglacials during the deglaciation they start by setting up a serie of transient experiments for the last 800 kyrs. The only merit I see in those experiments is to show that the GENIE-1 model forced as it is fail to reproduced any warmer that present conditions in Antarctica over the last 800 kyrs (cf. manuscript figure 1.b). And this results is achieved with freshwater fluxes included, therefore proving that the mechanism suggested by the authors in the abstract do not enable to achieve the required warmth. Having said that, the authors still pursue their analysis invoking a reduction of the west Antarctic ice-sheet to achieve the Antarctic warming, though they acknowledge that it is an uncertain mechanism. Finally, as the GENIE-1 model even without any WAIS fails to reproduce an Antarctic warmth comparable to the data, they complement with a GCM study that achieve the required temperature change. This set-up gives the feeling that the study is build on a statement (warm interglacial are due to thermohaline circulation changes and FWF) which does not prove to be correct (no significant warming in Antarctica) and is therefore kept but with some additions (WAIS retreat) which end up to be far more important.
I recommend to reconstruct the manuscript without the 800 kyrs simulations that cloud the reasonning. Setup would be: 1/TII simulations (discussing the set-up and results) with FWF leads to no significant changes in Antarctica 2/ TII simulations without WAIS are closer to the data but still insufficient and unrealistic 3/ use of GCM simulations on the other hand provide a more that sufficient temperature response (discuss why in much more details) 4/ discuss the discrepancies and where this leads us.
A second major point is the lack of proper citation of previous work on many topics. The authors are presenting the idea that the warming in Antarctic interglacials is due to thermohaline circulation changes as new but this is not truly the case. I recommend that the authors review litterature cited in the Stocker & Johnsen paper they cite but also paper like Ganopolski & Rahmstorf 2001. On the discussion of Antarctic tem-

perature Huyber & Denton (2008) should be mentionned among others. The authors should also discuss their results with respect to those of Ganopolski & Roche (2009) who show that freshwater forcing is enough to account for the difference of Antarctic behaviour between TI and TII, in clear opposition to their findings.
A third major point is that the author do not discuss at all their model with respect to their aims. They use the GENIE-1 model without discussing whether its very simplified atmospheric component (an EMBM) is proper to evaluate something as subtle as temperature and precipitation changes in Antarctica. The authors already note that much more complex models (GCMs) fail to reproduce Antarctic temperature changes during glacial times. Is their model more appropriate? I think not, and the physical meaning of the experiments should be thoroughly detailed in that respect. Even more when considering the GCM experiment they produce (but do not explain in details) where they show that the seasonnal distribution of precipitation is crucial in Antarctica (an impossible task for an EMBM). Similarly, the choices for the model set-up with respect to timing issues (sea-level & topographic forcing timescales w.r.t. ice-cores) should be discussed in details.
Fourth point is the data model comparison: the use of a comparison for only one oceanic core in one location does not prove anything, unless you can show that this coherence is also true in other basins an depths. Also, the comparison to oceanic cores is not independent from your forcing (LR04) and thus you should compare to records that are as different as possible in "shape".

2 Some detailed comments

Title. Should the authors revise the manuscript as suggested, the title should reflect something related to "Freshwater forcing" and "WAIS melting" with "Antarctic warm interglacials". The authors do not discuss interhemispheric coupling from the physical mechanisms.

Abstract. As expressed above the abstract do not reflect the content of the manuscript, neither the conclusions reached. Please rewrite to conform to the content.
pp. 2558, lines 10-16: this statement is a good start, why don't you construct a simple experiment that shows it rather than constructing a complex more realistic one?
pp. 2558, lines 20-24: I do not agree as mentionned above that the 800 kyrs experiments provide an assessment of the role of meltwater determining transient North Atlantic & Antarctic temperatures.
pp. 2559-2560, lines 24-8: the scaling of d18O to sealevel is not clear: what do you do if d18O > d18OLGM or d18O <d18Opresent ?
pp. 2560, lines 9-23: this developemnt is not particularly clear. Show the freshwater you constructed.
pp. 2560-2561, para. 2.1.1: this discussion is not self-contained and thus not clear. It would be better to discuss what are the effect of the different parametrization of concerned for what your study is concerned with.
pp. 2561, line 15: "SAT, expressed as sea-level equivalent throughout" This is an interesting statement. How do you convert ^{o}C in meters of e.s.l equivalent?
pp. 2562, line 26-27: what is the start state of the GCM you spin up the simulation from? 200 years is rather a short experiment for a different climate state.
pp. 2563, line 18: Why do you introduce DOME F? You are not using the difference between DOME F and DOME C in the discussion anyway ...
pp. 2564, first para: the fact that your model correspond very closely to spikes in DOME F does not prove anything, except that your scenario in FWF (which is not shown) have also the same variability. Moreover, the difference in timing that you mention is impossible to discuss physically as you do not have the time resolution for it between LR04 and DOME F.
pp. 2565, line 11-12: "Thus the possibility ... cannot be ruled out" you show precisely the opposite.
Figure1: This figure is quite complicated, and most of the facts inside are unused in the text. Please simplify. Also use a line to connect the blue dots in panel (b), second

part.
Figure 2: you temperature scale is not adapted to your discussion of 0.4^{o}C in Antarctica.

3 Litterature cited

A. Ganopolski and S. Rahmstorf, Rapid changes of glacial climate simulated in a coupled climate model, Nature 409 (2001), pp. 153–158
P. Huybers and G. Denton, Antarctic temperature at orbital timescales controlled by local summer duration, Nature Geoscience 1 (2008), pp. 787–792.
A. Ganopolski and D. M. Roche, On the nature of lead-lag relationships during glacial-interglacial climate transitions, Quaternary Science Reviews Volume 28, Issues 27-28, December 2009, Pages 3361-3378

Interactive comment on Clim. Past Discuss., 5, 2555, 2009.