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 mechanism leading to warm interglacials during the deglaciation they start by setting up a series 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 reproduce 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 reasoning. 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 literature 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 mentioned 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 T1 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 seasonal 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 and 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.
3 Literature cited

P. Huybers and G. Denton, Antarctic temperature at orbital timescales controlled by local summer duration, Nature Geoscience 1 (2008), pp. 787–792.

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