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Climate of the Past Discussions

Interactive comment on "Two-signed feedback of cross-isthmus moisture transport on glacial overturning controlled by the Atlantic warm pool" *by* H. J. de Boer et al.

Anonymous Referee #2

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The authors analyze the response of the Atlantic-Pacific cross-isthmus moisture transport to AMOC collapse and associated changes in AWP (Atlantic Warm Pool) area. The cross-isthmus moisture transport regulates Atlantic salinity and may therefore play a key role in stabilizing or destabilizing the AMOC. To address this issue, a series of experiments with a relatively simple and coarse-resolution atmosphere model (PUMA) was carried out. Even though the manuscript presents some interesting modeling results I do not recommend publication in Climate of the Past in its current state. The main message of the study, i.e. a two-signed feedback of the cross-isthmus moisture transport, is based on highly speculative assumptions regarding the dynamics of the AWP during glacial slowdowns of the AMOC. The calculation of moisture transport is

flawed and the discussion is confusing.

Specific comments:

1) PUMA is a relatively simple coarse-resolution atmosphere model. The model's skill in simulating the (present-day) regional precipitation and wind systems (in particular the Caribbean Low Level Jet) needs to be assessed before any conclusions for glacial climate dynamics can be drawn.

2) The final conclusions, i.e. the existence of a two-signed feedback, relies on highly speculative assumptions regarding changes in AWP area during Heinrich events. What is the reason for assuming that the AWP was 1/3 smaller than the average modern AWP? In this respect, Section 2 is very confusing since the presented proxy records do not provide any support for this assumption. Moreover, why are records from the Iberian margin and the tropical east Atlantic presented? These sites are far beyond the reach of the AWP.

3) The calculation of the atmospheric moisture transport is wrong. Equation (3) does not make any sense if the authors want to calculate the moisture flux through the given line segments. This is easy to see even for a nonmathematician as the integrand is always positive, independent of the wind direction. Moreover, it is unclear why the vertical integration stops at 700 hPa and whether the moisture flux calculation is based on 6-hour, daily, monthly or whatever model output. Also note that dP is not a pressure difference but a differential, and g is not the gravitation constant but the acceleration due to gravitation.

4) Why does extratropical cooling of the North Atlantic result in a weakening of the CLLJ? Usually, models predict a strengthening of the northeasterly trade winds in response to North Atlantic cooling.

5) The authors discuss the case where the extratropical Atlantic cools without major changes in the AWP. This situation is actually hard to imagine given that the modern

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AWP correlates with the relatively small (compared to glacial millennial variability) variations of the Atlantic Multidecadal Oscillation (Wang et al., 2008).

6) Section 5.2 is very confusing. The authors argue that proxy evidence suggests reduced cross-isthmus moisture transport during Heinrich events and that this would agree with their modeling results. On the other hand, they argue that the AWP was strongly affected during AMOC collapse resulting in enhanced cross-isthmus moisture transport.

7) The "statistics" presented in Fig. 7 is wishful thinking. Firstly, the correlation coefficient in Fig. 7c is statistically not significant at the 0.05 significance level; secondly, there is obviously no temporal correlation between the Gulf temperature and the salinity records presented in Fig. 7a, which would be expected from a significant influence of Gulf temperature on cross-isthmus moisture flux.

8) Boundary conditions for ice-sheet cover and orography are taken from Paul and Schafer-Neth (2003)? This is probably not the right reference.

Interactive comment on Clim. Past Discuss., 7, 3859, 2011.