

# ***Interactive comment on* “The Holocene thermal maximum in the Nordic Seas: the impact of Greenland Ice Sheet melt and other forcings in a coupled atmosphere-sea ice-ocean model” by M. Blaschek and H. Renssen**

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Reply to anonymous Referee #3, interactive discussion (C2747-C2755, 2012) on "The Holocene thermal maximum in the Nordic Seas: the impact of Greenland Ice Sheet melt and other forcings in a coupled atmosphere-sea ice-ocean model" by M. Blaschek and H. Renssen:

We thank the reviewer for the positive and constructive review, which helps to improve the manuscript and its use to fellow researchers. We would like to reply to the gen-

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eral comments first before replying in detail to the other comments and questions. We agree that the manuscript will benefit from a more detailed discussion on mechanisms and improved versions of figures (included mentioned proxy estimates from the text). Concerning the GIS vs. LIS impact: we would argue that the impact of the GIS is actually quite clear, as the impact remains similar between simulation 9kOGx1, it cools the south-eastern side of Greenland, and simulation 9kOGGIS compared to 9kOG-MELTICE. The anomalous warmth in 9kOGMELTICE in the western Nordic Seas can be attributed to an increased convective activity there, as a response to enhanced atmospheric cooling transported east. We agree that the part (3.1.3) where this is explained has to be improved and will hopefully resolve your concerns. A further suggestion is to include into our study another transient simulation with only GIS melt water included, but we would argue that this would not improve the results or the discussion. Please note that our setup can be seen as an update of previous work (Renssen et al., 2009) that involved a stepwise addition of forcings applied to efficiently distinguish impact factors. Additionally this hypothetical simulation would not improve the comparison with proxy data, as the proxies reflect the response to ALL forcings at once (i.e. the separate GIS impact cannot be inferred from the proxy data). Therefore we suggest not to include this additional simulation. We agree that the LIS has a strong influence on the early Holocene climate and therefore its uncertainty is relevant to this study. We propose to expand the experimental design section (2.2.1) of the paper by a summary of what is included in the supplementary of Renssen et al. (2009), stating changes in the atmospheric circulation imposed by the LIS. These changes are similar to the results of Justino et al. (2006), who investigated the atmospheric response in the LGM climate due to changes in ice-sheet topography with a former version of our model, namely ECBilt-Clio. Their setup includes simulations with the older ICE-4G and newer ICE-5G topography, as well as only albedo effects. Given the quasi-geostrophic approximation used in the ECBilt-atmosphere and its low vertical resolution of only three layers, the cooling patterns of North America and in SSTs over the North Atlantic are similar to results obtained by Kitoh et al. (2001) using the MRI CGCM. Therefore we

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think that our model, within the limitations set by its idealized setup, gives a reasonably response to topographic changes and produces an atmospheric circulation consistent with these topographic changes. Following these conclusions we find that our model depends strongly on topography, as it should be, and thus that uncertainties in topographic reconstructions are passed on to our modelled climate (c.f. Shinn and Barron, 1989; Pausata et al., 2011).

For the comments please see the supplement.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/8/C3414/2013/cpd-8-C3414-2013-supplement.pdf>

Interactive comment on Clim. Past Discuss., 8, 5263, 2012.

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