Reply to anonymous Referee #2, 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 referee for the positive and constructive review, which helps to improve the manuscript and its comparison to other modelling studies.
We think it is a very valid and constructive suggestion to include maps with more surface properties (SSS and MLD) to better understand our results and evaluate with proxy reconstructions or other modelling studies not necessarily included in this study. Our initial focus on just SSTs might have been an unnecessary step in simplifying our results. Referee #2 mentions that there are lots of studies investigating freshwater hosing experiments in numerous time periods and that recent results from Swingedouw et al. (2012) find a somehow different fingerprint of GIS melt water. They simulated the impact of 0.1 Sv of melt water from the GIS in the period of 1965 to 2004 with 5-6 AOGCMs (one OGCM) and they find a "surprising" warming in the Nordic Seas, which they attribute to an emergence of Atlantic subsurface waters that are not influenced by mixing of the subpolar gyre due to capping of the surface by freshwater. We think that this study does not disagree with our results because the seasonal difference in the early Holocene is increased compared to the present-day one. We investigate melt water impact on higher summer SSTs in a warmer climate (as we compare 9kOGx1 to 9kOG) and in a next step to a climate cooled by the impacts of the LIS (9kOGGIS to 9kOGMELTICE). Although the melt water induced in their simulations (0.1 Sv) and in our final simulation (9kOGGIS, 0.103 Sv) are indeed very close and even without the LIS ice sheet (9kOGMELT, 0.09 Sv) the two studies show some common patterns in SST cooling as a response to freshwater, but regarding the difference in time period and summer versus annual temperatures the two studies are different from each other. We find their results interesting in context to the proposed increased subsurface Atlantic water advection seen in proxy reconstructions (Risebrobakken et al., 2011) that is not evident in our model for 9ka BP. Therefore it might be useful to include this into the discussion in section 3.1.2 together with the discussion of proxy-based reconstructions. We further agree that the manuscript will improve by a more comprehensive description of the mechanisms (3.1.3) and propose to expand this in combination with changes from the other Referees, as well as previously mentioned new figures. In this context it is also viable to include locations and mentioned values in the text to figures, for instance figure 5. We sincerely thank the reviewer for noticing the mix up in Figure 4 with the colours and not resigning on our explanations and interpretations, that might have been quite doubtful due to this confusion. Thanks again.

1. p. 5265, l.6: please explain why the Nordic Seas is an “important” region.

   Reply: *we add the following sentence:*

   The causes of this spatial and temporal complexity of the HTM have not been resolved for all regions, for instance the Nordic Seas. This region is the gateway between two major ocean currents, the North Atlantic Current (NAC) to the east, transporting warm and saline waters to the North and the East Greenland Current (EGC) to the west, transporting cool and less saline waters to the South. In this paper we therefore evaluate the characteristics of the HTM in this important region by analysing the impact of potential forcing factors in numerical climate model simulations and by comparisons with available proxy evidence.

2. p. 5266, l. 7: “quite some discussion”. It would be nice if the authors can summarize in more details these discussions.

   Reply: *The following paragraph addresses this discussion in quite some detail. The use of this phrase is rather narrative than avoiding discussion. We hope the reviewer agrees that the following paragraph deals with that discussion and agrees to a rephrase of that sentence to: However, it has been argued by Koc et al. (1993) and other studies (Kaufman et al., 2004; Jansen et al., 2008; Risebrobakken et al., 2011) that these warmer temperatures cannot be due to insolation alone.*

3. p. 5266, l. 25: “more forcings”. The authors should better define what they mean by forcing. It would be nicer to be more precise (horizontal heat advection).

   Reply: *corrected to: horizontal heat advection*

4. p. 5266, l. 26-28: This sentence is not very clear nor logical. Please clarify.

   Reply: *We can rephrase in combination with Referee #1 (#5):*

   The reconstructed non-uniform response across the Nordic Seas from Andersen et al. (2004) seems to be a robust feature in palaeoceanographic reconstructions, thus challenging the question of the origin of this zonal difference, despite the fact that eastern SST reconstructions give a broader range of warmer SSTs.

5. p. 5267, l. 2: Once more, I believe it will be better to clarify what is meant by forcing (cf. “forcing factors”)

   Reply: *We rephrase: ... the impact of possible forcings like increased heat advection by the NAC or Greenland melt water are still unclear.*

6. p. 5267, l. 26: Please convert the figures from Rignot in Sv to have the same unit to compare with.

   Reply: *The unit used by Rignot et al. (2011) refers to an acceleration, and not to a volume flux. Therefore a direct conversion is not possible, and that's why we use the example of 80*

mSv after 100 yrs.

7. p. 5268, l. 5-7: The last sentence of this paragraph is not clear. "comparison" of what? Reference to "warm past climates" while this paragraph is only discussing present day and future. Please clarify.

   Reply: *Maybe "comparison" is misused here. Rephrase: Although these estimates assume a fixed acceleration rate, the impact on the AMOC highlights the importance of the GIS in future and in warmer past climates*

8. p. 5269, l. 16-17: I do not understand why the authors cite here Schmittner et al. (2005) for comparing their model. This paper is a very small paper that looks into the response of the AMOC in the future. Please be more specific. In any case, I believe it is better to compare their model with observation rather than with other models. Nowadays, there exists estimates of AMOC strength (Kanzow et al. 2010 for instance) and there are also observations of convection in the North Atlantic in the Nordic Seas, the Labrador Sea and the Irminger Sea. Indeed the authors discuss convection in the Irminger Sea later (p. 5274, l. 25), but do not discuss it here. . .. A map of convective area in the model as well as the differences in the different sensitivity experiments will be helpful.

   Reply: *We agree with the referee that a comparison with modern-day values is useful (Kanzow et al., 2010). Our citation to Schmittner et al. (2005) is however valid in terms of comparison to other models. In section 3.1.3 we refer to AMOC values and shall include a reference + present-day values for comparison reasons there.*

9. p. 5271, l. 10: "OGMELTICE" does not exist in Table 1. I assume it is OGICE?

   Reply: *We apologize for this confusion, our mistake. It has to say: OGMELTICE in the table.*

10. p. 5273 sec. 3.1.2: I think it is necessary to show where are the cores you are referring to and try to stick your computation on them. Following what is said in the conclusion, I have the impression the authors tried and it did not work very well. It should be clearly stated here and the location of the cores as well as the estimates should appear in the different figures. Or else, it is very difficult to really agree with the "better model-data fit" claimed l. 25.

    Reply: *To clarify this point we will include values mentioned in the text from proxy reconstructions to relevant figures, as well as the core locations.*

11. p. 5274, l. 10-17: These lines present interesting processes to be explored. The authors should try to refer to them more clearly in the following of this section. Moreover they discuss the changes in the AMOC while it is not stated here. Please be more precise.

    Reply: *The referee is correct, we do not explicitly refer to this list in the text below. We propose to do so in the revised manuscript. The AMOC is not mentioned here directly,*

because it is a prominent response, but not a forcing, as are the others in the list.

12. p. 5275, l. 4: Add a "(not shown)" before the point or show the map of changes in sea ice.
    Reply: *Added a "(not shown)".*
13. p. 5275, l. 11: It is stated 21.6 Sv rather than 22 Sv in Table 2.
    Reply: *Corrected.*
14. p. 5275, l. 15: Map of mixed layer depth changes will help to prove what is said here.
    Reply: *Noted. We agree, changes will be made in response to the referees general comments.*
15. p. 5275, l. 21: I do not understand why the authors look at the transport at 30 S here. This is quite far from the North Atlantic. . . Moreover, it is hard to make a link between this transport and the Northern Hemisphere cooling as it is claimed l. 23.
    Reply: *The meridional heat flux at 30S shows the bigger impact of changes in the North Atlantic. It is also a standard benchmark parameter that is used in modelling studies for heat transport. This will be clarified in the revised text.*
16. p. 5276, l. 18: "Inducing. . ." Please consider rephrasing this sentence.
    Reply: *We rephrased: LIS melt water in OGMELT has a minor effect on the Nordic Seas (less than 0.5K difference).*
17. p. 5277, l. 5-10: As stated before, this is not what I see on Fig. 4.c where the gradient is stronger in OGMELTICE than OGGIS around 8 kyr BP. This really needs to be clarified!
    Reply: *We apologize for the mix-up of colours in the scale and clarify that OGGIS is indeed stronger than OGMELTICE, as it says in the text.*
18. p. 5277, l. 16-17: The legend of Fig. 5 is really unclear so it is hard to follow this section 3.2.2. How is computed this timing? Please also try to include available data on this figure 5.
    Reply: *As explained in section 3.2.2, we calculated the timing of the HTM (maximum temperatures) from 1000yr-running-mean-filtered August SSTs. We will add the mentioned estimates inside the figure for easier comparison and will include more description inside figure caption.*
19. p. 5280, l. 1-8: You compare a summer change of 42 W/m2 at 65◦ N with a global radiative changes of 8.5 W/m2. I think this is a bit confusing and you should discuss it. Indeed, if stated like this, it does not really support your assertion that "it is not surprising that Rignot et al. (2011) report and acceleration of GIS melt" since apparently the forcing is far lower for present day as compared to the early Holocene. So please clarify what are the arguments here.
    Reply: *In combination with our reply to Referee #1 (#41) we rephrased that part, which we hope resolves the issue:*

4. We find in our experiments that GIS melt plays an active role in the Nordic Seas environment and GIS evolution therefore has to be considered in the evolution of the early Holocene climate and future melting scenarios.

In the context of future climate change this study underlines the importance of GIS melt water and its active role in the climate system. Compared to today, in the early Holocene the annual and summer insolation forcing at 65N (i.e. in S Greenland) was 2.4 and 42 Wm^2, respectively (Berger and Loutre, 1991). This corresponds closely to the annual radiative forcing of proposed future anthropogenic emission scenarios, that vary between 3 and 8.5 Wm^2 (Meinshausen et al., 2011). Therefore it is not surprising that recently Rignot et al. (2011) reported an acceleration of GIS melt rates that could soon outrange those of the early Holocene, and thereby stressing the importance of GIS melt for Nordic Seas future climate evolution.

20. p. 5285: Table 1: Why is LIS melt equal to 0 in the line corresponding to OGMELT? Also indicate the length of the simulations.

    Reply: *Mistake corrected.*

21. p. 5286: Table 2. Why the authors do not make a t-test for the H0 hypothesis that their change in mean is significantly different from zero (in place of considering what is outside the 1 STD). Moreover, the authors should define what is their "NAC strength" and specify if the 30◦ S flux is taken only in the Atlantic. They should also state that the East-West gradient is for the Nordic Seas.

    Reply: *We agree that this is unclear. We will change the boldness of numbers to be consistent with a H0 test. The AMOC strength is given by the NADW exported southward in the Atlantic ocean at 20 S. The 30 S heat flux is only taken in the Atlantic.*

22. p. 5288: Fig. 2: average over the 500 year of simulations? Any t-test to test the significance of the difference? Same comment for the other figures.

    Reply: *The last 100yrs of each simulation is used for averaging.*

New References:

Swingedouw, D.; Rodehacke, C.; Behrens, E.; Menary, M.; Olsen, S.; Gao, Y.; Mikolajewicz, U.; Mignot, J. & Biastoch, A., Decadal fingerprints of freshwater discharge around Greenland in a multi-model ensemble, *Climate Dynamics, Springer-Verlag,* **2012**, 1-26