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Climate of the Past Discussions

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

Anonymous Referee #2

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———————- General comments ———————-

This is a nice paper that proposes to explain regional SST patterns from the Holocene thermal maximum and in particular the zonal summer SST gradient in the Nordic Seas. For this purpose, the authors use a climate model of intermediate complexity (LOVE-CLIM) with the best-known forcings for the Holocene. They focus on the impact of the Greenland ice sheet (GIS) melting. With the help of lots of sensitivity experiments, they show that a moderate GIS melting increases the east-west SST gradient in the Nordic Seas, which, they claim, is in better agreement with reconstructions. This is a valu-

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able attempt to explain precisely the pattern of observed changes thanks to a climate model.

Nevertheless, I have the feeling that this paper needs a little bit more materials to be convincing. In its actual form, it is only composed of five figures, and I believe that a few more figures will help the reader to better understand and evaluate what has been really done in this study. Indeed, I believe that showing map of sea surface salinity (SSS), sea ice, and convection changes in a similar manner as Fig. 2 will be very interesting to evaluate the impact of freshwater input in this model. Indeed a strong limitation of the present study is that it is based on only one model. Since lots of literatures exist on climate models response to freshwater input, I think the authors should at least discuss their model results in regards to other models responses. In particular a recent paper (Swingedouw et al., published on line in Clim. Dyn.) discusses the fingerprints, in terms of SSS and SST notably, of 40 years of freshwater released around Greenland with a rate of 0.1 Sv in state-of-the-art AOGCMs. Although the rate is larger than what is presented here, I believe that the experimental design share similarities at least in term of location of freshwater release. I have the impression that the main fingerprints found in Swingedouw et al. are quite different than the ones simulated in LOVECLIM. In particular most of the models from the Swingedouw et al. study find a warming in the Nordic Seas in response to freshwater input around Greenland, which is not the case here. The authors should consider including at least discussions on the sensitivity of their results to the model used or to the mean state (does a similar experiment of freshwater release around Greenland has been made under present-day conditions?).

More generally, I think the authors should try to better explain the different responses to freshwater input that they observe in their simulation. Are they really robust (no statistical test are made to see if the anomalies are larger than internal variability)? Which changes in the dynamics can explain them (stratification, convection, sea ice, heat transport etc.)? In particular, when inspecting Fig. 2d, it is very spectacular to see how the additional GIS melting input in OGGIS leads to a large gradient in SST across

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the Nordic Seas, while it was not really the case in 9kOGx1,2, and 4. Can the authors tell a little bit more on potential processes to explain that? Are the SST anomalies related to SSS ones, and thus to rthe spread of freshwater input? The authors try to give pieces of answer to these questions in section 3.1.3, but as it stands, this section is not clear at all. This is why I believe a few more diagnostics (at least, SSS, sea ice and mixed layer depth) will really help to better understand the impact of the different freshwater input in the different experiments and also help to better describe what is happening. I encourage the authors to rewrite the section 3.1.3.

I have also another important concern about the comparison with the data, which are never shown. I believe it will be very enlightening to try to obtain a few data in order to compare them with the model output more directly, and not only saying that a given experiment seems to be in better agreement with the data. In particular, the authors should at least show the location of the data they are discussing on their Fig. 2 for instance. I also believe that reconstruction estimates should appear in Fig. 3 and that the east-west gradient should be computed on the locations of the available data. Last but not least, in numerous places, there are problems with the notations or the colour scale (I hope!). For instance in Fig. 4.c in the early Holocene (before 7 kyr BP) I see a larger gradient in OGMELTICE and a very low in OGGIS. The authors claim the opposite in the text. Is there a problem with line colours? There are also problems in the table etc. (see below for precise comments).

———————— Specific comments ————————

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

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

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).

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

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

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

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.

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.

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

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.

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

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they discuss the changes in the AMOC while it is not stated here. Please be more precise.

p. 5275, l. 4: Add a "(not shown)" before the point or show the map of changes in sea ice.

p. 5275, l. 11: It is stated 21.6 Sv rather than 22 Sv in Table 2.

p. 5275, l. 15: Map of mixed layer depth changes will help to prove what is said here.

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.

p. 5276, l. 18: "Inducing..." Please consider rephrasing this sentence.

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!

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.

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.

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

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

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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.

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.

——————— Technical corrections ———————

p. 5268, l. 25: replace “sea-ice” by “sea ice” as well as “land-surface” by “land surface”

p. 5276, l. 25: replace “stronger” by “strongly”

p. 5277, l. 3: delete “so” after “less”

———- References ———-

Kanzow T, Cunningham SA, Johns WE, Hirschi JJ-M, Marotzke J, Baringer MO, Meinen CS, Chidichimo MP, Atkinson C, Beal LM, Bryden HL, Collins J (2010) Seasonal variability of theAtlantic meridional overturning circulation at 26.5_N. J Clim 23:5678–5698. doi:10.1175/2010JCLI3389.1

Swingedouw D., Rodehacke C., Behrens E., Menary M., Olsen S., Gao Y., Mikolajewicz U., Mignot J., Biastoch A. Decadal fingerprints of fresh water discharge around Greenland in a multi-models ensemble. Climate Dynamics DOI: 10.1007/s00382-012-1479-9

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