Reply to the Anonymous Referee #2

We would like to express our thanks to the reviewer for these comments that help us to improve the presentation and the clarity of the manuscript. We provide our point-to-point reply to these comments below (indicated by blue color).

Reply to the ##Major concerns## (we do not include the original comments to limit space): We will go through the manuscript to improve the language and to remove redundancy. However, we are considering keeping the subsection for each region, since we would like to discuss the different responses to the forcings in these regions.

As for the lapse rate effect, we will calibrate the surface temperature to sea level temperature by applying a lapse rate of 6.5 °C/km when comparing them with proxies.

We agree that the model potentially could induce internal variability that could be evaluated by comparing simulations starting from the different initial stages. However, in this paper we apply a 500-point running smoothing that is supposed to rule out the short-term internal variability. Thus, we focus on the general temperature evolution instead of the small wiggles. Considering the wiggles of the red curves, they mainly appear in the winter temperature, implying the relatively larger variability than other seasons. If the reviewer means the difference between the two freshwater forcing scenarios by the words “we focus on small wiggles”, we can associate this temperature difference with variations in deep water formation.

Detailed comments

Abstract: Extremely long. I would suggest to seriously trim this to something like 50%. The first paragraph, for instance, starts with a lot of background, which, in my opinion, is unnecessary in an abstract. In that first paragraph 5346.2-13, keep only the last sentence 5346.10-13. In general, you do not need to give the entire conclusion section in abstract. Tell the overall story, keep only the few numbers that are really necessary and boil it down to a couple of key points.

Introduction: This is too long. While I appreciate the enormous amount of work that went into covering a huge body of literature, there are simply too many details. You risk burying the reader in details so that he/she loses track of the points that you actually want to make. Take a step back and think about what the main points you want to make are, and then write the introduction around this. Also, as a detail, the paragraph spanning 5348.15 to 5350.12 is way too long. It corresponds in length to about three named subsections in Section 3. It covers many different things so it should be pretty straightforward to split up if you choose to keep it in some form. Finally, and this is probably a matter of taste, I like to see the questions/hypotheses tackled by the paper moved up much earlier in the introduction.

REPLY: Thanks for the advice on the abstract and introduction. We will shorten these parts as suggested.

5349.9 “the glacial anticyclone”. This makes it sound as if there is one single glacial anticyclone. Is this what you mean? If so, which ice sheet are we talking about?

REPLY: It should be “a glacial anticyclone” instead of “the glacial anticyclone”, as this glacial anticyclone appeared not only over the LIS, but also over the FIS at a smaller scale, and is also well known from other modelling studies. We will correct this in the revised version.”

5352.10-24 While I appreciate your desire to showcase the merits of the LOVECLIM system, this list of previous results becomes too long. Particularly because much of it more or less repeats stuff from the introduction. Make it short and sweet. Again, figure out what the key points should be and give those – and feel free to keep all the references, because this does actually add weight to it.

REPLY: We will adjust the text accordingly. At the beginning, we try to say that LOVECLIM has been applied to simulate multiple climate periods. However, we agree with the reviewer and will shorten the text.

5353.1-3 Are the Martinez-Boti and Palaeosens references really suitable to back your claim that an equilibrium

climate sensitivity of 2 K is in the lower range of GCMs (a claim that I, by the way, do not at all contest)? As far as I know, those references don't work on GCMs.

REPLY: These papers discuss the climate sensitivity in general terms, instead of only for models (GCMs). The climate sensitivity of GCMs is given in Flato et al. (2013). We rephrased it and replaced the reference: "The model's sensitivity to a doubling of atmospheric CO2 concentration is 2 K, which is in the lower end of estimations in GCMs (Flato et al. 2013)."

5353.11- How do you estimate the GHG forcing in W/m2 from the concentration changes?

REPLY: This estimation was based on the reference of Ramaswamy (2001), who calculate the radiative forcing according to the deviation of GHG concentrations from the pre-industrial level. We included the reference and it now reads: "the radiative GHG forcing anomaly (relative to 0 ka) in Wm^{-2} calculated from the GHG concentration (Ramaswamy, 2001), representing the overall GHG contribution"

5353.22-23. This shouldn't be the sort of place where you want to use the word "likely". This is easy stuff, and you need to know it and tell the reader. Finally, the 10 ka change seen in Fig 1 clearly says precession is playing a role (at least in high-latitude summer insolation).

REPLY: We agree and rephrased it to: "At the same time, the global annual-mean insolation stayed at almost the same level (not shown). Both changes in obliquity and precession are resulting in the insolation variations on the multi-millennial time-scale of the Holocene."

5354 It is not clear exactly whose time-dependent ice sheet reconstructions you use in the model.

REPLY: Spatial extents of ice sheets are based on a number of previous reconstructions. To be more specific, we checked each grid with reconstructions and decided whether it was covered by the ice sheet according to the majority of these estimations. The elevations of the ice sheets are based on Ganopolski et al. (2010). This will be clarified in our revised version.

5354.27-28 Do you have a reference for the claim in the second half of the sentence? I.e. that we know the LIS deglaciation well enough to not warrant that to enter into the sensitivity experiment.

REPLY: No reference yet. As we explain in the text, the LIS deglaciation is relatively well constrained with the help of proxy reconstructions and modelling studies.

5356.8 "indicating lower sensitivity of oceans to the prescribed forcings than continents" To me it looks more like the damping effect of large ocean heat capacity on a seasonal signal.

REPLY: We agree with the reviewer and we rephrased this to: "The warming over the oceans is less conspicuous with a 1.5 °C warmer climate."

5358.21 How are these rates calculated? As least squares fits? Over which period? Same period in all plots? Please specify. Do you have uncertainty ranges on these slopes?

REPLY: These current slopes are calculated by simply dividing the temperature variations by the length of corresponding period from the onset of Holocene to the highest temperature, which means varying periods over different regions. However, as suggested, we have also done the least squares regression and the new results turn out to be slightly smaller in most cases. Since these slopes are only used as the overall estimations, we did not provide the uncertainty ranges for them.

5358.21 "0.28" The figure says 0.29.

REPLY: It should be 0.29 instead of 0.28. So there was a mistake in the text and we will correct it.

General reply regarding comments on model-proxy comparison: We will rewrite this part by shortening the description and adding one extra figure of summarized model-data comparison.

5362.7 "climate reconstructions based biological proxies" You make it sound like the biological proxy is based on climate reconstructions. Rephrase.

REPLY: It was rephrased to: “Climate reconstructions based on biological proxies”

5363.2 “By contrast” Here you are contrasting an annual mean cooling of 1.5C in the model to a roughly unchanged summer temperature in the proxy. Why not compare the summer proxy to your model’s summer response – which, in fact, is weakly red (slightly positive) in the region (Fig 4b). Or am I misunderstanding something?

REPLY: We agree with the reviewer that the reconstructed summer temperature should have been compared with simulated summer conditions instead of annual mean temperature. We will correct this in the revised version.

5363.28-5364.2 Why compare proxy-based sea ice to modeled temperature? Why not compare with modeled sea ice changes?

REPLY: Considering the lack of the temperature reconstructions in the Arctic, the sea ice only served as an indirect proxy since we focus on temperatures. Therefore, by referring to the sea ice concentration, we try to have a general comparison with proxy data to loosely validate our results.

5364.2 “if we assume that negative sea ice concentration anomaly could extend back to 11.5 ka.” Given your transient results, do you have reason to expect this assumption to hold? Why not compare 9-6 ka proxy-based seas ice with 9-6 ka modeled sea ice?

REPLY: As explained above, we try to explore the temperature pattern at the onset of Holocene. However, we agree and will modify this model-data comparison.

5365.17: I may be mistaken, but it appears that Fig 11 is mentioned before Fig 10 in the text. If so, consider swapping them.

REPLY: We agree and have now swapped Fig 10 and Fig 11.

5366-top-of-page: You talk of a bunch of ways that an ice sheet can influence surface temperatures. Locally and remotely. But not until the bottom of the page (5366.22-24) do you mention what I find the most obvious (at least for a local effect), namely the elevation-lapse rate effect.

REPLY: We were trying to explain all ice sheet related cooling effects in this paragraph. However, we agree with the reviewer and will modify this paragraph so that it will focus only on the ice sheet effects, and try to deal with the lapse rate effect elsewhere in the text.

5367.1 Same as above: you say that the summer climate over Siberia is cooler, but to me the area seems mostly red in the area in Fig 4b.

REPLY: We agree with the reviewer. The comparison should be between the simulation OG11.5 and OGIS11.5 (with and without ice sheet) instead of OGIS11.5 vs. PI. It was corrected to: “If we take Siberia as an example, the slightly warmer summer climate in OGIS11.5 than OG11.5 is the consequence of the overall higher summer albedo (Fig 10). This higher albedo can be attributed to the feedbacks between boreal forest and tundra or/and bare ground.”

5367.14-16: How fair is it to compare Arctic Ocean and Fram Strait heat transports with North Atlantic AMOC strength. How tightly are these two quantities coupled in the real world?

REPLY: We agree that these ocean systems depend on the specific local situation, even though they are connected as a whole system. So we rephrased to: “coincides with the foraminifera records from the Arctic Ocean and Fram Strait”

5367.21-23 “Actually, sea ice coverage in OGIS11.5 is much more extensive over Davis Strait (N Labrador Sea) than in OG11.5 (Fig. 13) implying that positive feedbacks involving sea ice were active (Renssen et al., 2005).” I think I understand what you are trying to say, and it might actually be true. But I am not convinced that an increased or reduced sea ice cover in some climate perturbation experiment “implies” that a positive sea ice

feedback is active. Sure, it might be (and likely is) but it might also just be responding passively to whatever other things are changing in the system.

REPLY: We try to stress that the extended sea ice was partially caused by the feedbacks since the magnitude of the sea ice expansion was much greater than passive responses only. We modified it into: "Actually, sea ice coverage in OGIS11.5 is much more extensive over Davis Strait (N Labrador Sea) than in OG11.5 (Fig. 13). This profound extension seems beyond the one-way response to the reduced heat transport, thus implying that positive feedbacks involving sea ice were active (Renssen et al., 2005)."

5367.25-27: This enhanced convective activity is caused by the shift of deep water formation from the east Greenland Sea to the west, which is induced by the freshwater discharge from ice sheets melting." From which melting ice sheet? Aren't you getting FW input from both FIS and GrIS? Your sentence appears to indicate that the FIS is winning over the GrIS in this respect.

REPLY: The freshwater discharge implies the combined effect of the freshwater fluxes, suggesting freshwater fluxes from both the FIS and the GIS. For this region, the FIS freshwater forcing was stronger than that for the GIS, however this situation might be changed in other places.

5368.5-10: This paragraph is a great example of the way I think you should be writing your paper. Short, to the point, giving the big picture.

REPLY: We will improve it accordingly in the revised version.

5368.5: Why do you consider the ORBGHG experiment the "control". I would rather think of it as an idealized breakdown aiding the interpretation of the basic set of experiments, OGIS_FWF-v1/2.

REPLY: Initially, we considered ORBGHG as our reference state without the impact of early Holocene ice sheets, since it did not include additional freshwater fluxes and modifications in ice sheet configuration. However, we agree with the reviewer and changed in into: "Comparison of the full forcing simulations (OGIS_FWF-v1/2) with the reference run (ORBGHG)"

5369.19-21: "the albedo in the model has been checked and it turns out that the albedo value in the model is consistent with the vegetation of dwarf shrub and herb over Scandinavia at that time". What does this mean? Do you use your modeled albedo as a proxy for your modeled vegetation? Why not compare vegetation with vegetation? If the reason is that the model's vegetation types aren't comparable to those observed, then your albedo match will be coincidental and not worth mentioning.

REPLY: Yes, we use the albedo as a proxy for simulated vegetation, and agree that this is a rather indirect way of comparing with data. Hence we will remove this sentence.

5370.24-26 "For Siberia, a small summer temperature anomaly in response to positive insolation anomaly was surpassed by the cooling effect of the high albedo" Again, isn't Siberia mainly red in Figure 4b?

REPLY: The word "surpassed" should be replaced by "offset". Correspondingly, the sentence will be: "For Siberia, a small summer temperature anomaly is simulated due to the fact that positive insolation anomaly was partially offset by the cooling effect of the high albedo"

5371.8-9 "The summer temperature was similar to the preindustrial when these two factors were in similar magnitude". I think you mean something like "During summer, these two factors were similar in magnitude, and the temperature was similar to that in the preindustrial."

REPLY: Yes, and we will use the suggested expression in our revised version.

Figures

Generally: can you reduce the number of figures? Can Figs 5-9 be combined somehow? Are Figures 2 and 16 necessary?

REPLY: We are considering moving some of them to the online supplementary information. However, we think

that it is important to keep the figs 5-9 since this geographic variability is one of our main points.

Fig 2: What do the black contour lines show. If they just show the same as the color shading, why are they necessary? Also, I would suggest you use a white-in-the-middle colorbar. It makes this kind of plot much more easy to read.

REPLY: Yes, both contour lines and the colours show the insolation anomaly for the clarity. We will modify the colour bar in the revised version.

Fig 4+12+14: The latitude, longitude and depth labels are small, stretched and of very low resolution, so you basically can't read them. If you think they are necessary, make them readable.

REPLY: The label texts are now enlarged. For the figures, we think they are worth showing, at least figs 4 & 14, since they present spatial patterns of early Holocene temperature change (Fig 4) and the ocean response to ice sheet melting (fig. 14).

Fig 5b (and perhaps other of the line plots in Figs 5-9): In the text (5358.1) you say that you applied a 500 yr running mean. But Fig 5b doesn't look like a curve that has been smoothed by 500 yr. Did you really do that also here?

REPLY: We applied a 500-year running mean smoothing. The larger variation in winter (Fig 5b) seems to be caused by a larger variability of our model during winter than during summer.

Figs 5-9: What are the units on the indicated slopes? And over which x-range were they fitted? Uncertainties?

REPLY: The slope's unit is °C/ka. These slopes were calculated by dividing the temperature variation by the length of the corresponding period, implying varying periods. No uncertainties were provided here since it only give us rather general estimations.

Figs 5-9: Legends are too small.

REPLY: The legends were enlarged.

Fig 15: Did you really apply a 100 yr smoothing?

REPLY: There is a small mistake in the explanation of the smoothing technique. The red and green lines are filtered by a 100-point running mean, while the black line was only a 30-yr mean. We have already corrected this.

Fig 16: Colorbar has too small font.

REPLY: The colourbar fonts were enlarged.

Is the supplementary figure necessary? Couldn't these boxes be drawn onto one to the existing maps? If you want to keep it, you need to make it look nicer. It is stretched and of low resolution.

REPLY: We initially tried to mark these domains on the last map of fig 4, but it did not work well visually. Therefore, we decided to put these domains in the supplementary information. However, we will modify its appearance accordingly.

Language examples (first few pages only)

REPLY: We corrected them, and the manuscript's language will be fully revised by a native English speaker.