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Comment

Interactive comment on “Effects of melting ice sheets and orbital forcing on the early Holocene warming in extratropical Northern Hemisphere” by Y. Zhang et al.

Anonymous Referee #2

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The study uses the LOVECLIM earth system model of intermediate complexity in a series of Holocene-long experiments with differing specifications of ice sheet impact on the system. One experiment employs constant present-day ice sheets while two other experiments employ time-varying reconstructed ice sheet configurations with different specifications of the resulting freshwater input to the ocean. This allows the authors to separate the purely greenhouse gas and orbital forcing induced changes from those induced by the ice sheet changes. They find that existence and timing of a Holocene thermal maximum are conditioned on the inclusion of ice sheet effects and have distinct spatial patterns.

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The study is interesting and could potentially contribute to furthering our understanding of the complex picture that emerges from the paleoclimatic proxies regarding the sequence of events during the final deglaciation. However, in its current form the manuscript is too lacking in its presentation to properly bring home the main points.

Three major concerns arise: 1) The paper is much too long and there are way too many repetitions. This goes for the abstract (in particular), the introduction, the results, the discussion and the conclusion. 2) The language is not good enough and needs to be gone over carefully. 3) There are a number of issues regarding the analyses on model-data comparison and figure presentations that need tightening. See below.

Major concerns

Length of the paper: The paper is too long. There are too many details and too many repetitions. This goes for the abstract (which is particularly long), the introduction, the discussion and the conclusion. The results section could also be shortened if you didn't have separate subsections for each region. In doing that you force yourself to write more than perhaps was necessary. For the paper as a whole and for each part of the paper individually, decide what the main points should be, how they fit into the big picture, and work up the paper or section around that.

Language: The language really needs to be looked at closely by a native English speaker. Each page has multiple examples of strange-sounding sentences, surprising wordings and grammatical errors. It is too large a task to correct all of these in a review, so below I give some examples from the first couple of pages. Please do not just correct these and leave the rest alone. The entire paper needs to be gone over carefully.

Analyses: Below I list a lot of detailed concerns. There are very few major issues, but the volume of seemingly minor issues leaves the reader with a sense that the paper does not strongly enough argue for its conclusions.

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How much of the cooling in a region-box (for instance your Scandinavian box) is due just to the lapse rate effect associated with the high-altitude ice sheet points? And if this effect is a major part of the signal in that box, is it then fair to compare the box-wide temperature signal to that in a proxy, which in many instances (presumably) records a low-altitude signal with much less of an effect due to changed elevation? As an example, your Scandinavian box-wide average temperature is influenced by the higher-than-present surface elevation. If you compare this box-wide average to a lake sediment then the proxy location has not undergone any surface elevation change.

Given the rather smooth forcing applied in the ORBGHG experiment, the wiggles on the red curves in Figures 5-9 suggest some level of internal variability. Do you have a way of gauging (in all your experiments) the importance of forced vs. internal variability? This could be important because the paper does tend to focus quite a bit on pretty small wiggles. If you want to compare such wiggles to proxy data, you should be pretty confident that they are forced variations, such that you are not just interpreting noise.

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 the 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

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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.

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?

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.

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.

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

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).

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

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.

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.

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?

5358.21 “0.28” The figure says 0.29.

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

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?

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

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?

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.

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.

5367.1 Same as above: you say that the summer climate over Siberia is cooler, but to

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me the area seems mostly red in the area in Fig 4b.

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?

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.

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.

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.

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.

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,

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then your albedo match will be coincidental and not worth mentioning.

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?

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.”

Figures

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

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.

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.

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?

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

Figs 5-9: Legends are too small.

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

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Fig 16: Colorbar has too small font.

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

Language examples (first few pages only)

First of all, the title: "Effects of melting ice sheets and orbital forcing on the early Holocene warming in extratropical Northern Hemisphere". I would move the "the" further back in the title to read: "Effects of melting ice sheets and orbital forcing on early Holocene warming in the extratropical Northern Hemisphere". I might even consider dropping the entire last part to just read "Effects of melting ice sheets and orbital forcing on early Holocene warming".

5346.6: "decaying of ice sheets in cryosphere" add "the" in front of cryosphere. In fact, you don't need the "cryosphere" part. Just say "decaying ice sheets". Completely the same considerations for "biosphere" in the next line.

5346.7: "over the high latitude" -> "over high latitudes"

5346.19-20: "reaching the maximum cooling as here the climate was strongly influenced". The "as here the" part sounds strange.

5347.13 "LIS" has not yet been defined.

5347.13 "till" is used many times throughout. I believe this is informal. I'd suggest using "until".

5347.15-16 "with only exception in Arctic's winter". I'd have written "with the only exception being the Arctic winter" or "with the only exception being winter in the Arctic".

5347.17-18 "as the response to". The whole sentence has a strange feel to it, but you could save it by just changing this bit to "in response to". Also "diverse mechanisms" in the end of the sentence is too vague. Rephrase or specify.

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5347.26 “for instance” seems a poor wording here: is the warming an example (signaled by “for instance”) or is it, in fact, what the isotopic shift is interpreted to correspond to?

5348.11 add “the” before “Northern Hemisphere”

5349.9: “topography of ice sheets generated” -> “ice sheet topography generated”

5349.11-12 “under the LGM context” sounds strange. I’d have chosen “during the LGM”

Interactive comment on Clim. Past Discuss., 11, 5345, 2015.

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11, C2694–C2702, 2015

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