Reply to the comments of Anonymous Referee

First, we would like to thank the referee for constructive and pertinent comments on the manuscript. Below, we will describe how we have address the general and specific comments.

Reply to specific comments

The main concern of the referee was the lack of substantial new results. We have tried to address this important criticism in in several ways. To begin with, in the revised manuscript we have now included results from the PMIP3 mid-holocene and pre-industrial simulations. Including the PMIP3 model results provides an opportunity to examine not only the model-model variation, but also differences between the new and previous generation of climate models. In the revised manuscript, we have included several new and more quantitative analyses of how the sea-ice cover is affected by the orbitally-induced insolation change. This includes a more detailed analysis of changes in the seasonal cycle of the Arctic sea-ice extent. Further, we have also examined the relation between changes of the annual-mean ice extent and the summer surface air temperature north of 60 N. We find a correlation between the summer warming and sea-ice extent decrease in the PMIP simulations, which is not surprising. What is perhaps less expected is how strong this correlation is for the majority of the models.

To provide more substantial and process-based analyses, we have in the revised manuscript examined the physical reasons for differences in the model responses to the orbital forcing. For this purpose we have focused on two selected models, which have similar sensitivities of the sea-ice extent in global warming scenarios but yield different sea-ice responses to changes in the orbital forcing. In particular, we have examined the interplay between the shortwave radiation and clouds, focusing on the shortwave cloud radiative forcing at the surface and cloud fraction. For the two selected models, differences in the Arctic cloud distribution in the background pre-industrial climate can essentially account for the model differences in response to orbital forcing. Further, we discuss this issue in broader terms as well pointing out the radiative forcing of CO2 should be less dependent on the background climate, whereas the effective forcing due to orbital changes depends strongly on cloud distributions and surface albedo of the background climate.

Another concern of the referee was that the paper was too repetitive. To address this, we have followed the suggestions of the referee and removed some of the figures and tables that are found in published work. Further, we have completely revised section 3 that describe the simple thermodynamic ice model. Here, we have completely removed the qualitative four-step model (old sec 3.1). We have removed the detailed description of the Thorndike model, only keeping the parts essential for investigating the response to orbital forcing. What builds on Thorndike's and others work is essentially presented in sec 3.1, whereas in sec 3.2 the model is applied to study the effect of orbital forcing; a straightforward but new application of the simple sea-ice model.

We were also criticized on grounds that there were errors in the analysis of the simple ice model. However, the original statement concerning increased melting in response to a reduction of the melt season is correct. The trouble seems to be that we poorly explained the underlying physics and that we also not clearly enough emphasized that this statement only concerns the situation when the time-integrated insolation *remains constant* (as it roughly would if only the precessional cycle changed). Clearly, if the number of Joules of solar radiation is fixed, then a shorter melt season decreases the number of Joules that is lost from the ice through long wave radiation (the ice melt being proportional to the absorbed solar radiation minus the emitted longwave radiation). We have now put an effort in explaining this somewhat counterintuitive feature in a clear and transparent manner. Overall, we have made a considerable effort to make section 3 clearer and more readable.

We have also made an effort to better connect the model analyses with the story of the Arctic sea ice that begin to emerge from sea-ice proxy studies. In particular, we discuss the relic beach ridges found along the coast of Northeast Greenland in relation to the PMIP mid-holocene simulations.

Further, we have included several new reference of relevance to the present study, including the ones that the referee pointed as being overlooked.

Reply to smaller comments

Due to the character of the major referee comments, the manuscript has been subjected to major revisions. Due to this some of the more specific comments are no longer applicable, as the parts of the old manuscript are removed or significantly changed.

1. p.3446, l.14 and others: There is a clear convention as to the usage of the term ”sea-ice area” versus the usage of the term ”sea-ice extent” in the published literature, with the former being the size of the actual ice cover and the latter being the size of all grid cells with more than 15

   A: In the revised manuscript we consistently use ”extent”, not ”area”.

2. p.3446, l.20: By definition, a reduction in extent is very unlikely to happen in the ices interior.

   A: Removed in the revised manuscript

3. p.3448, l.7: This refers to CMIP3 models. This section should be updated with recent papers referring to CMIP5 models (which, I understand, only became available after this version of the paper was submitted).

   A: We refer to results from CMIP5 data in the revised manuscript.

4. p.3451, l.12: I did not see a difference between ”acronym used in this paper” and ”model name in PMIP2 database” in table 3

   A: To avoid being repetitive we have removed table 3 in the revised manuscript.

5. p.3452, l.6ff: The description of Thorndike’s rectangle by referring to a figure that does not show a rectangle is confusing.

   A: As mentioned earlier, the four-step model is no longer included in the paper, so this comment is no longer relevant.

6. p.3453, l.5ff: As outlined above, this model is well known and has already been presented twice in detail in earlier publications. Hence, I suggest to leave out the models details. If you decide to keep them, you would have to also explain A, B, L, and should change the notation of FLW in eq. 1 and 2 to become consistent with Eq. 4 and 5.

   A: We have stripped the description of the Thorndike model, but all symbols of used in the ”reduced” model are defined.

7. p.3454, l.11: What is a steady heat conduction? The ice-thickness dependence of surface temperature is an obvious feature of all sea-ice models just because thicker ice isolates the ice surface more efficiently from the warm ocean.

   A: This comment has been taken into account in the revision of section 3.

8. p.3454, l.12: The surface temperature decreases with increasing ice thickness. Ice growth decreases because of the larger ice thickness and hence lower heat conduction.

   A: This comment has been taken into account in the revision of section 3.

9. p.3455, l.1ff: This analysis seems to be flawed. I expect you have a sign error somewhere in your analysis. If you reduce m in Eq. (1), M is reduced. Since smaller M refers to less melting, a reduction in m reduces melting.

   A: It is in fact fully correct, see the comments above.

10. p.3457, l.27: The inclusion of Fig. 5 is unnecessary. It would be more helpful to include some estimate of pre-industrial ice edge (or e.g. 1980s mean ice edge) in Fig. 4.

    A: Fig. 5 is no longer included

11. p.3458, l.19: You do not analyse the ice albedo feedback in this paper. However, this would be interesting. How do you know that this is primarily driven by albedo?

    A: In the sea ice margin, with thinner sea ice and lower sea ice concentration (hence more open water) the albedo of the ice will be lower. This would result in enhanced warming and more melting of ice. The problem with this sentence may be that that many of the PMIP2 models did not include sea ice models with appropriate sea-ice albedo parameterization. This statement is no longer included in the revised manuscript.

12. l.3458, l.27ff: The models do not lose or gain more ice to maintain a certain equilibrium thickness, but simply because physics dictates

such loss or gain. The loss or gain then defines the equilibrium ice thickness.

A: This was an unfortunate formulation, and is removed in the revised manuscript.

13. l.3459, l.10: How can I see from Fig. 8 that the reduction is strongest in summer?

    A: This could not be seen from Fig. 8, which regrettably included some serious flaws. In the new analysis this figure is no longer included.

14. p.3459, l.15: Why do models have less thinning in winter than in summer?

    A: The MH forcing is positive in summer and negative in winter, this could lead to lower temperatures in winter, which could counteract the thinning of the sea ice that has occurred during summer.

15. p.3460, l.20ff: This subsection is one example where further analysis would have been helpful. What drives the sea-ice loss in the models? Changes in winds? Temperature? Currents? How do these changes relate to proxy records of atmospheric parameters?

    A: In the revised manuscript we have included a more thorough analysis of two selected models, to address the question about drivers of sea-ice loss in the model, but the focus have been on clouds, hence we have not done any comparison of the features in the models to proxy data.