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Interactive comment on "The relevance of mid-Holocene Arctic warming to the future" *by* Masakazu Yoshimori and Marina Suzuki

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Thank you very much for carefully reading the manuscript and for various helpful suggestions which would improve the manuscript and pointing out places that need to be clarified or discussed. In the following, reviewer's comments are indicated [RC]. Response to the comment and perspective on the revision are indicated by [AC].

[RC] General comment

This manuscript proposes an in depth analysis of the different radiative and turbulent latent and sensible heat fluxes terms that constraint the seasonal changes in surface temperature in the Arctic. The analysis considers the mid-Holocene climate and the RCP4.5 scenario for the future, with the objective to derive emerging constraints from

the mid-Holocene climate that can be used to assess the results of future climate projections. This analysis is interesting, but the conclusion is not strong enough about the analogies between the two periods and what can be done out of it. It is only during the ice melting period, when albedo decreases and water vapor increases in the atmosphere, that similar feedbacks occur. The forcing factors are very different between the two periods. Even though the different elements are found in the text, similarities and differences could be better discussed.

[AC] The objective of the current study is not to derive a specific emerging constraint but to reveal similarities and differences in processes, based on the detailed diagnosis, which are not obvious from the forcing and response patterns alone. The derivation of emerging constraints is one of ultimate goals beyond the scope of current study, and that would require several steps from statistical approach, mechanism understanding, and proxy searches. Even if similarities are found to be weaker or limited and they are unfavorable signs to find the specific emerging constraints in some cases, we do not think that would be fundamentally critical. The mechanism understanding of different periods under the same framework is really the most important aspect of the current study (which have not usually been done). For that, as the reviewer pointed out, it is also important to discuss differences between MH and future and the limitation of the use of MH climate information as well. As the differences were expected from the beginning due to different radiative forcing patters, we placed less emphasis on the differences. This might have led the impression that we were trying to stress the similarities. We will add more discussion on the differences.

[RC] The abstract could also be more informative on the results and better stress the role of the clear sky long wave radiation.

[AC] We will make the abstract (and conclusion) more specific so that the messages become clearer. The main points will be:

(1) It is found that many of the dominant processes that amplify Arctic warming over the

ocean from late autumn to early winter are common between the two periods, despite the difference in the source of the forcing (insolation vs. greenhouse gases).

(2) A chain of processes responsible for the warming trend from summer to autumn is elucidated by the decomposition to factors associated with sea surface temperature, ice concentration, and ice surface temperature changes.

(3) The downward clear-sky longwave radiation is one of major contributors to the model spread throughout the year. Other controlling terms vary with the season, but they are similar between the MH and the future in each season.

(4) The MH Arctic change may not be directly relevant to the future in some seasons (spring in particular) when the temperature response differs, but it is still useful to constrain the future Arctic projection (partly new addition to the original manuscript).

(5) The significant cross-model correlation found between summer albedo feedback and autumn-winter surface temperature response in both forcing cases suggests that feedbacks in preceding seasons, sea ice cover in particular, should not be overlooked as a constraint (new addition to the original mansucript).

[RC] The different figures are difficult to follow, because there is no direct relationship between the names of the different terms plotted in figure 5 (a key figure in this manuscript) and the decomposition done using equation 2 to 7. I therefore consider that this manuscript is worth publishing, but that an effort should be made to clarify the expression of the different terms and better explain their role.

[AC] We will change following points to improve the readability associated with Fig. 5 and equations.

(1) We will write terms in Eq. (4) explicitly after combining with Eq. (2), so that each term corresponds exactly to the description in Table 3 and each component in Figs. 5 and 10.

(2) We replace Lambda in Eq. (7) by T so that it is obvious that the symbol represents

temperature.

[RC] The discussion should also be enlarged, so that the paper more clearly address the point listed in the title.

[AC] The discussion will be substantially enlarged with separate points (1) in terms of the ensemble mean response, and (2) in terms of the model spread. We will also discuss not only the similarities but also for the difference between the MH and future (when and how). A particular attention will be paid to spring when the ensemble mean response differs between the two periods.

[RC] Other comments:

- P2 make sure you properly refer CMIP or PMIP everywhere.

[AC] We will revise the mixed use of terms, “CMIP” and “PMIP”, and avoid confusion.

[RC] - P3 l 15. And section 4.3. The comparison of the MH results with observations is not fully used in the manuscript. Is there a way to go one step further by provided an evaluation that could really inform on the relevant processes between past and future?

[AC] It would be ideal to derive a specific emergent constraint and apply that to the model selection or to narrower quantitative uncertainty range, but the practical application is beyond the scope of the current paper. Nevertheless, we will add “recommendations” that the seasonal evolution of surface temperature response (cold season in practice) and likely summer sea ice cover are likely useful constraints based on the current analysis.

[RC] - P3 bl29. Could you provide an order of magnitude of the uncertainty related to emissivity for models that have a variable emissivity?

[AC] As shown by the good match of superimposed black solid and blue dashed lines in Fig. 5, the simulated temperature change and the sum of partial temperature changes calculated with unit emissivity are very similar for the ensemble mean, and also for all individual models (not shown). For both annual and October-November-December means averaged over the Arctic ocean, for example, mismatches for all models are smaller than 0.06°C. Therefore, we think it is safe to assume the constant emissivity as in many previous studies.

[RC] - P4 2 Is the equation correct for S?

[AC] Thank you for catching this typographical error. In the second and third terms on the right side of Eq. (2), Delta alpha should be alpha, and S should be Delta S. The analysis was made correctly and the results are not affected. We will correct them.

[RC] - P4 l15 what do you call sect. 3a?

[AC] We will correct it to "Sect. 3.1".

[RC] - P4 end of section 3.1. It could be worth mentioning that the approach is direct because there is no change in land-sea mask between the different simulations.

[AC] Thank you for the suggestion. We will add this point

[RC] - P4. L 27 are your referring to ice concentration or to ice fraction?

[AC] As in the original manuscript, it is ice concentration, and it also represents fraction of ice cover for each grid cell. To clarify the procedure, we will add that the analysis of Eq. (6) is applied for each grid and each month.

[RC] - P5 Would it be possible to rewrite equation 7 so that there is a more direct link with temperature ? or use one example to fully explain what is done and the strength of the diagnosis. This could also be needed to present the different terms of equation (4) and make sure there is no ambiguity on global or local anomaly (or their relative strength).

[AC] As to the Eq. 7, we will replace the symbol Lambda by T so that it becomes clearer that the equation directly evaluates the temperature change and avoid any potential confusion. As written above, we will write terms in Eq. (4) explicitly after combining

with Eq. (2), so that each term corresponds exactly to the description in Table 3 and each component in Figs. 5 and 10.

[RC] - P4 l 8. May be you could site Hewitt and Mitchell 1997 for the definition of the MH insolation forcing.

[AC] It is nice to cite one of the earliest MH simulations in this context. We believe that it is Hewitt and Mitchell (1996, not 1997) in Journal of Climate. We will add it.

[RC] - P7 There is a large emphasize on clouds before showing the effect of lw_clr. This later term reflects both changes in water vapor and in atmospheric lapse rate. The cloud cld_effect arrives later (in season) compared to albedo and lw_clr. I would suggest reconsidering the way the whole section is written, to better discuss the relationship between the different terms and their monthly evolution.

[AC] In Sect. 4.3 in "Results", we will describe the results season by season first, and then state important points afterward so that the reader can grasp the overall results in the sequential order first.

[RC] - P8 section 4.5. I am not entirely convinced that OND are the best months to look at to infer model spread. Sea-ice and temperature result certainly of what happens during the preceding months in terms of forcing and feedbacks. This needs to be clarified.

[AC] We agree with the reviewer that the Arctic warming processes are not independent during each season. As briefly stated, we do not eliminate the possibility of links between feedbacks in other seasons and OND, for example: "this result does not mean that the summer albedo feedback is irrelevant to the OND model spread." Without numerical experiments, it is difficult to entangle the feedback links across seasons. We will discuss the inter-seasonal linkage between summer albedo feedback and OND response to the result which adds the important point.

[RC] - P9 section 4.5 I am lost in the call to the different figures.

[AC] We will add explanation on the relevance of these figures in the discussion here.

[RC] Figure 10 also show a large model spread in the lw_clr, not only in clouds. This should be highlighted. The cloud cover is important but results certainly from the other conditions: sea-ice fraction, temperature, lapse rate, water vapor, changes in atmospheric convection or large scale condensation. This should be discussed, at least to tell when there is an analogy or not between the different feedbacks between mid-Holocene and future climates.

[AC] We will stress the role of lw_clr throughout the paper. As stated in Sect. 4.5, the dominance of lw_clr was expected as most of downward longwave radiation comes from near-surface, and the near-surface temperature is thermally coupled with the surface temperature. Therefore, constraining lw_clr is equally difficult to constrain the surface temperature change. We will add the statement on lw_clr in abstract and conclusions so that the paper does not give false impression that the term is small or unimportant. We will mention that the cloud feedbacks are related to other feedbacks as pointed out by the reviewer. The discussion will be substantially enlarged with separate points (1) in terms of the ensemble mean response, and (2) in terms of the model spread. We will also discuss not only the similarities but also for the difference between the MH and future (when and how). A particular attention will be paid to spring when the ensemble mean response differs between the two periods but factors for the model spread are similar. Consequently, as the reviewer suggests, discussion on the existence and non-existence of analogy in feedbacks between MH and future for different seasons will be added.

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