

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

**Masakazu Yoshimori and Marina Suzuki**

masakazu@aori.u-tokyo.ac.jp

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Thank you very much for carefully reading the manuscript and for pointing out some of the messages that need to be sharpened. Perspective on the revision is provided below.

As to the differences between MH and future, and to the implication for the scientific community, we will add some discussions and crystalize the message. As the main goal of this paper is to show the link between MH and future Arctic warming, there are less focus on the differences as they are naturally expected from the different radiative forcing patterns. What was not pointed out in previous studies, to the extent analyzed here for multi models, is the transfer of extra energy absorbed in the ocean during

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summer to the heat release from the ocean during winter, and consequent amplified warming occur in a similar way in response to different types of radiative forcing. This notion is also valuable to understand the Arctic response in much wider paleoclimate conditions. On the other hand, the reviewer's point is valid in that the difference is less emphasized: early spring response is particularly distinct between MH and future forcing cases. This needs to be mentioned even though the current multi-model analysis does not identify the exact mechanism. As in the response to the reviewer 1, we will sharpen the conclusion so that new findings and implication become clearer. We do not claim any new 'emergent constraints' in the current study although that would offer more practical implication. We believe that the application of such constraint should go hand in hand with mechanism understanding, statistical identification of the link between the past and the future (e.g., Schmidt et al., 2014), and paleoclimate proxy searches suitable to constrain the link. In our view, the community is not ready to apply such an integrated approach using the MH Arctic state with confidence. Nevertheless, our study suggests that proxy records quantitatively measuring winter Arctic warming in MH (relative to the preindustrial) would have a potential as a constraint, based on our mechanism understanding of how winter warming is amplified commonly between MH and future. We hope the current study provides a step towards such an ultimate goal for the community.

Reply to general comments:

1. We apologize for the confusion between PMIP3 and CMIP5. The MH experiment was designed by PMIP3, and that was endorsed as a part of CMIP5. All the data were downloaded from CMIP5 data base. We will clarify this point.
2. The negative ice growth-ice thickness feedback is not quantified explicitly in the current analysis. Therefore, it does not appear in the decomposed terms in Equation (2) although they are closely linked to the sea ice related terms including the magnitude of albedo feedback (a function of ice cover among others) and heat release from the ocean (a function of ice thickness among others). Our analysis is based on the

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surface energy balance as in many other previous studies. The quantification of ice thickness feedback would require energy budget analysis for sea ice itself and probably for mixed-layer of the ocean as well. This does not mean that we think the feedback is unimportant. We will mention this point.

3. Thank you for pointing out uncited references. We will cite them in the revised manuscript.

Reply to specific comments:

>19-01: "indirect atmospheric stratification" might be unclear to many. Please rephrase or explain.

>06-02: "time periods" → "periods" (a period is always referring to time)

>07-02: "discouraged general comparisons": do you mean that the studies found that comparisons were not simple to make? Please rephrase.

>23-02: "time periods" → cf. 06-02.

>26-03: "effect" → effects

We will rephrase/change these expressions as suggested.

>24-02: The last sentence of the paragraph is not quite clear; consider removing it.

We will remove it.

>22-04: "ts" should be  $T_s$  in mathematical form.

We will correct this.

>07-05: Why using the  $\Lambda$  sign for temperature differences, and not  $\Delta T$ ? It is not clear how  $\Lambda$  relates to Eqs (6) and (4).

We will replace  $\Lambda$  by  $\Delta T$ .

>28-05: Can you elaborate on how the ERF was computed precisely? It is said that an

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AGCM was used, but which one? What was the exact setup? It would be impossible to reproduce your results if the readers do not have this information.

The model information was only given in the figure caption. We will move this into the text, and also add more precise description as to the setting of the ERF computation.

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-175>, 2018.

## CPD

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