

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 pointing out some of the messages that need to be sharpened. In the following, reviewer's comments are indicated by [RC]. Response to the comment and perspective on the revision are indicated by [AC].

[RC] In this study, the authors conduct a diagnostic surface balance analysis based on output from the PMIP3 and CMIP5 simulations. They wish to test the extent to which past Arctic warming could be used as an analogue for future warming, which could then make the basis for a more objective model selection. The authors find that despite different forcing mechanisms, several common feedbacks operate between the two

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periods, making the case that these periods can indeed be compared to one another.

The paper is interesting to read but is quite descriptive: the differences between the MH-PI and RCP4.5-Historical simulations are stated and described, but not a lot of attention is given to try to explain why patterns differ and, more importantly, why this would have implications for the scientific community.

[AC] In the original manuscript, the similarity in feedbacks in particular season (autumn) might have been too emphasized, and less attention was paid to the difference between the MH and future. We will enlarge the discussion and conclusion with emphasis on the relevance in the Arctic response between the MH and future (RCP4.5). 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] The conclusions fall a bit short, for example. The authors explain that the MH period could be used to evaluate the models, but they do not state what type of constraints could be applied. Based on their results, can the authors make a step forward and come with recommendations on such constraints?

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

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(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 manuscript).

[RC] A few general comments: * The analysis relies on four types of runs: Mid-Holocene (MH), Pre-industrial (PI), Historical and RCP4.5. I understand that MH simulations are taken from PMIP3, and so are PI simulations. I understand that Historical and RCP4.5 simulations are taken from CMIP5. Is that correct? Something confusing is that the authors write that "For the MH and PI simulations, we use monthly climatological data averaged over periods longer than a century, which were archived as part of the CMIP5 dataset" but also write that MH simulations were taken from PMIP3: "The MH simulation was designed and coordinated by the PMIP3 project". Could the authors clarify this at p. 2, line 30 (I did not find the explanations very clear).

[AC] We apologize for the confusion between PMIP3 and CMIP5. The MH experiment

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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.

[RC] * There is an important negative feedback that is not mentioned in the study: the negative ice growth-ice thickness feedback, which states that sea ice grows faster when it is thin. The existence of this feedback is a safeguard for sea ice, which would otherwise disappear much faster due to the positive albedo feedback. I'm unclear if the aforementioned negative feedback is covered at all by the authors and if so, to which term of Eq. 2 it belongs.

[AC] 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 Eq. (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 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 as a future perspective.

[RC] * There are two references missing that deal with high-latitude changes and the role of feedbacks, that I think should appear in the text: - DOI:10.1038/s41598-017-04623-7 - DOI: 10.1038/s41467-018-04173-0

[AC] Thank you for pointing out uncited references. We found these references useful and cite them in the revised manuscript.

[RC] Specific comments (Syntax: 22-03 = line 22, page 3) 19-01: "indirect atmospheric stratification" might be unclear to many. Please rephrase or explain.

[AC] We will rephrase the word.

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

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[AC] Will be corrected.

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

[AC] We meant that the studies generally refuted to regard past warm periods as the analogue. We will rephrase it.

[RC] 23-02: "time periods" -> cf. 06-02.

[AC] Will be corrected.

[RC] 26-03: "effect" -> effects

[AC] We will change it as suggested.

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

[AC] We will remove it.

[RC] 22-04: "ts" shoud be T_s in mathematical form.

[AC] We will correct it.

[RC] 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).

[AC] We will replace Lambda by T.

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

[AC] The model information was only given in the figure caption. We will move this into the text, and also add more detailed description as to the setting.

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

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