

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

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Review of

The relevance of mid-Holocene Arctic warming to the future

by M. Yoshimori and M. Suzuki.

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 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. 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?

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).

* 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.

* 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

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Specific comments (Syntax: 22-03 = line 22, page 3)

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. 24-02: The last sentence of the paragraph is not quite clear; consider removing it. 26-03: "effect" → effects 22-04: "ts" should be T_s in mathematical form. 07-05: Why using the Λ sign for temperature differences, and not ΔT ? It is not clear how Λ relates to Eqs (6) and (4). 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.

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

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