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Discussions

Interactive comment on "Arctic sea ice in the mid-Holocene Paleoclimate Modelling Intercomparison Project 2 simulations" *by* M. Berger et al.

Anonymous Referee #1

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In this manuscript, M. Berger and co-authors present results from PMIP2 simulations of Arctic sea ice during the mid-holocene and compare these results with estimates from a simple, conceptual model. They find a decrease in sea-ice coverage for mid-holocene conditions compared to a pre-industrial control run in all simulations, with some quantitative difference between individual models.

General comments The authors of this paper did a very thorough job in presenting output from all individual models that participated in the PMIP2 project. While such very broad data base should allow for some interesting science, the analysis presented here unfortunately remains rather superficial and is quite repetitive. In addition, the

results presented here are not put into sufficient context of existing literature. Finally, some of the analysis seems flawed. **As such, this paper requires a very substantial revision to become suitable for publication.** Such revision would, in particular, need to address the specific comments below.

Specific comments

My main concern with this paper is the lack of substantial new results. Many of the results which the authors derive from the Thorndike model have been known from Thorndike's and Bitz and Roe's paper, in particular the dependence of the seasonal cycle on ice thickness and the stronger sensitivity of thicker ice to warming. As such, much of section 3 (incl. table 2) is only a repetition of earlier work. Most of table 3 is included in other PMIP2 papers, in particular in the standard reference of Bracconot et al., 2007, Part I. Figure 3 is similar to Fig. 5 of Bitz and Roe. An analysis similar to fig. 6 is included in Bracconot et al., 2007, Part II (not referenced here), and the maps shown in Fig. 8 are available directly from the PMIP2 homepage. What is new is a somewhat more detailed analysis of the response of ice thickness in PMIP2 models, but the relevance of this finding is not made sufficiently clear by the authors. As it stands, the analysis is primarily a description of PMIP2 results, with very little insights that would carry beyond the PMIP2 project itself. This is the more of concern as the PMIP2 project was finished about 5 years ago, with PMIP3 output now being available. The paper would gain by a broader interpretation of PMIP2 results. Other modelling studies on sea ice during the mid-Holocene indeed provide insight that does not focus on particular models. See, for example, the contribution of Goosse et al., Clim. Past, 2007 (http://www.clim-past.net/3/683/2007/cp-3-683-2007.html, not referred to in the current paper).

Hence, a revised version would require a more substantial progress beyond what is known, and, if possible, a broader interpretation of the results. The authors should also strive to leave out repetitions of earlier work.

In a revised version, the results presented in section 3.2 must be revised, too: on p. 3455 the authors state that a shorter melt season leads to increased melting. This simply cannot be the case, and I expect the authors to have a sign error somewhere in their calculations. This error becomes obvious by reducing the melt season to zero length: If the authors were right, the sea ice would then melt most. Note that the sign convention in the original papers by Thorndike and Bitz and Roe is positive for both M and G, while the current paper has a negative convention for G (where a large positive heat flux from the water increases G, but physically reduces the ice growth).

Throughout the paper, some more substantial analysis would be desirable. These model results provide a unique opportunity to understand and to analyse differences between different models. What drives, for example, the changes in sea-ice coverage? Changes in wind? In temperature? In currents? What are differences in these drivers between individual models, etc. This kind of analysis would then allow for insights that would carry beyond PMIP2.

Smaller comments:

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 % ice coverage. Throughout, your reference to sea-ice area should become a reference to sea-ice extent to avoid confusion.

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

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).

p.3451, l.12: I didn't see a difference between "acronym used in this paper" and "model

name in PMIP2 database" in table 3

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

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 model's details. If you decide to keep them, you would have to also explain A, B, L, and should change the notation of F_{LW} in eq. 1 and 2 to become consistent with Eq. 4 and 5.

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.

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.

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

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

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

l.3458, l.27ff: The models don't 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.

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

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

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?

Technical corrections There is a number of smaller typos and error in grammar throughout this manuscript. However, given the substantial revision that is required anyway, it does not seem to make much sense to list those for the time being.

Interactive comment on Clim. Past Discuss., 8, 3445, 2012.