

Interactive comment on “Arctic sea ice in the PlioMIP ensemble: is model performance for modern climates a reliable guide to performance for the past or the future?” by F. W. Howell et al.

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My co-authors and I would like to thank the reviewer for their review of the paper. We have addressed the comments as detailed below:

(Reviewer 1) This study examines the sea-ice simulations from the eight models that constitute the PlioMIP ensemble, aiming among others to gain insights regarding model reliability. Based on its analysis, this study suggests that sea-ice models tuned for pre-industrial climate conditions might not be suitable to simulate sea-ice conditions under different climate conditions.

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Unfortunately, in my opinion this paper is not suitable for publication in its current form and requires a major revision before it might become acceptable for Climate of the Past. This is because this paper does not reflect our current physical understanding of sea ice as discussed in the wealth of literature analyzing the modeled past and future evolution of sea ice for example from the CMIP5 ensemble. This physical understanding strongly suggests that some of the major findings of this paper are not supported by the evidence presented here. An analysis of the PlioMIP sea-ice ensemble is clearly worthwhile, and I believe that interesting insights can be gained from such analysis, but such work would have to reflect what we already know about sea ice. Re-writing this paper along these lines, rather than primarily presenting a statistical analysis, would make for an interesting paper that could then indeed offer insights also regarding the future evolution of sea ice.

A new version of this paper would have to reflect at least some of our current knowledge of sea ice, including:

The areal extent of thin ice is more sensitive to warming than that of thick ice, because thin ice can more easily melt completely for a given warming. In contrast, thick ice simply becomes a bit thinner for some warming, which then does not lead to a substantial areal change. Hence, the main finding of this paper that pre-industrial sea ice is less sensitive to temperature changes than the much thinner ice of the PlioMIP ensemble is neither surprising nor new.

We accept that the discussion of sensitivity of sea ice extent to temperature changes should have taken into account the thicker pre-industrial sea ice. The discussion will be amended to note that the stronger correlations between Pliocene Arctic sea ice extent and temperatures may be as a result of the thinner Pliocene sea ice. In addition, sea ice volume results will be shown, in addition to sea ice extent, which will reflect the total amount of sea ice melted, rather than just the change in areal extent.

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The thickness of sea ice that is output by climate models is usually the average thickness that the ice would have if it were to cover the entire grid cell while conserving volume. To obtain actual thickness which then could be compared with satellite observations, one simply has to divide this so-called equivalent thickness by sea-ice concentration. This is apparently not done here (at least it is not mentioned), making the comparison to IceSAT simulations somewhat hard to interpret. It also renders some of the other discussion of sea-ice thickness hard to interpret, since this discussion seems to be based on the equivalent thickness but interprets it as if it were actual thickness.

Calculations of sea ice thickness will be amended to take into account the sea ice concentration. In addition, as mentioned in the response to the first point, values for sea ice volume and changes will be shown.

The paper suggests a number of times that areal patterns of sea-ice thickness can be tuned for. However, I do not know of a single modeling group that would know a reasonable way of how to achieve this. Tuning of sea-ice models usually only involves a very simple metric, like for example March mean sea-ice thickness or the like, but not a tuning of any patterns. I also find the discussion of the tuning of CICE to most likely not reflect the reality at climate modeling centers. I would expect the developers of NorESM-L to tune CICE according to their needs. CICE itself cannot be tuned meaningfully, because it is a stand-alone sea-ice model that requires a given forcing to produce tunable results. The entire discussion of tuning also fails to appreciate the fact that tuning is necessary for any large-scale model, simply because necessarily the parameterizations cannot fully reflect the physical processes that occur on smaller length scales.

We respectfully disagree with the statement that the paper suggests a ‘number’ of

times that areal patterns can be tuned for. The possibility of the influence of tuning is mentioned once (pg 1281, line 25), but is dismissed later in the paragraph as a likely contributing factor to the difference in the similarity of the models' thickness patterns to the observations.

However, the discussion will be amended so that it reflects the necessity of tuning in models. We feel it is still important that the potential influence on the sea ice from tuning still be discussed, but will cite previous studies of tuning effects (e.g. Eisenman et al., GRL, 2007, DeWeaver et al., Comment on GRL, 2008) to help put it into greater context.

Throughout, this paper seems to assume that it is primarily the formulation of the seaice model that is responsible for the resulting sea-ice evolution. It fails to acknowledge that in all coupled climate models, it is by far more important to expose the sea-ice model to realistic oceanic and atmospheric forcing to obtain reasonable sea-ice results.

We agree that more emphasis on ocean and atmosphere forcings are needed, and they will be included in the revision. However, in section 4.3 (pg 1285), we make the point that there does not appear to be a strong link between the sea ice rheology or dynamics scheme and the nature of the sea ice produced by different models, so we do not state that the sea ice model is the primary driver of sea ice. We will emphasise more clearly that the sea ice model is not the primary influence on the state of the sea ice, but it is important to include discussion of the potential effect which they may be having.

We have pretty reliable observations of sea-ice concentration from 1953 onwards, which should be much closer to pre-industrial sea-ice conditions than those of the past three decades. It would be helpful to compare the simulations against this earlier data

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set to obtain more robust insights into model quality compared to the recent period with its rapidly changing sea-ice conditions.

We accept that comparison of pre-industrial simulations with observations since 1979 is unsuitable, but for similar reasons comparison with observations from 1953 would not be much of an improvement. We will remove this comparison, but will refer to comparisons of observations with CMIP5 simulations (e.g. Shu et al., The Cryosphere, 2015), in which most of the PlioMIP models have representation.

For sea-ice thickness, once it is correctly divided by sea-ice concentration, again the comparison of pre-industrial thickness to single-point observations from two months of satellite observations in 2009 is not very meaningful. Over the past decades, summer sea-ice thickness in the Arctic has decreased by roughly 50%, and it will be very hard to gain robust insights into the quality of a pre-industrial simulation based on satellite observations from 2009.

We also accept that for the same reasons as the extent comparisons, the comparisons made in the paper with the sea ice thickness are not optimal, and so we will remove these comparisons from the paper, and instead refer to the comparisons made between CMIP5 sea ice thickness and observations in Stroeve et al. (The Cryosphere, 2014), in which most of the PlioMIP models are represented.

The discussion of albedo vs. warming vs. sea-ice evolution remains unclear. Why should the ice-albedo feedback lead to a stronger relationship between T and extent during the Pliocene? The same ice-albedo feedback acts during the pre-industrial period as during the Pliocene, suggesting that the relationship between a change in T and a change in extent should be similar in both periods if the ice-albedo feedback

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was indeed the driving mechanism.

Our suggestion was that if a GCM has been tuned to achieve a particular sea ice coverage, then the influence of the tuning may have resulted in a diminished influence of other climatological factors, such as the surface temperatures, but this may not be the case when simulating a climate state for which the model has not been tuned.

As mentioned previously, we will reduce the emphasis placed on the influence of tuning on differences in Arctic sea ice extent, and the stronger correlations between Pliocene sea ice extent and temperatures. Discussion will look at other atmospheric and oceanic influences, and look at the changes in sea ice volume as well as extent. We will alter the line on page 1288 which says that the albedo feedback is likely to have contributed to the stronger Pliocene sea ice-temperature relationship.

I strongly recommend to focus less on statistical relationships, or to at least try to interpret those based on physical grounds. For example, the higher value of CV for Pliocene sea-ice extent is probably simply a reflection of the thinner and smaller mean ice cover, but is geophysically in my opinion not relevant. Geophysically, the actual areal change is much more relevant than the percentage change relative to some mean sea-ice cover.

We believe CV is a useful metric for measuring variability between datasets with different means. It has been used in other studies, (e.g. Stroeve et al., The Cryosphere, 2014) described as a 'normalised variability measure'. Whilst there are many other measures of the changes in sea ice cover, which may have greater relevance, we do not believe that CV is then irrelevant.

Many of the insights found here for the PlioMIP period have been found before by

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existing studies that deal with CMIP-type ensemble simulations of future sea-ice evolution. These studies should be cited here, and the progress made relative to these studies should be discussed.

Papers discussing CMIP5 Arctic sea ice output (e.g. Stroeve et al. (The Cryosphere, 2014), Shu et al. (The Cryosphere, 2015), Semenov et al. (The Cryosphere Discussions, 2015)) will have results relevant to the discussion in this paper, and these will be discussed, as well as other relevant papers on Arctic sea ice simulations.

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