

## ***Interactive comment on “Large-scale features and evaluation of the PMIP4-CMIP6 midHolocene simulations” by Chris M. Brierley et al.***

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We would like to thank you for your kind, yet thorough, comments about our manuscript. We believe that the revisions we plan to implement should satisfactorily address your comments.

The first main comment in this review related to our decision not to present findings about sea ice cover changes. There are two main reasons that we did not include sea ice in this manuscript. Firstly, we wanted to constrain the scope of this manuscript to a manageable amount of analyses. It already feels possibly too long with its current 11 figures. The mid-Holocene sea ice story can support a whole manuscript on its own, as demonstrated by the submission of a paper on the lig127k sea ice to this special

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issue (Kageyama et al., 2020). Secondly, there are technical issues around the calendar adjustment using the Bartlein and Shafer (2019) software. It has been tested and evaluated on surface temperature and precipitation. The software development required for it to adjust fields on rotated grids has been completed, but it has not yet been scientifically validated for sea ice coverage. Three of the models have, however, provided daily fields that avoid such issues. As a supplement to this Author Comment, we include figures of the composited patterns at the day of the annual maximum and minimum of the Arctic sea ice coverage for this small subset and our preliminary analysis of the role of calendar adjustment on one of them. Given the two reasons it may be best to leave the presentation of sea ice coverage changes at the mid-Holocene for a subsequent more-detailed manuscript (as happened in PMIP3). Conversely, the Last Interglacial equivalent to this manuscript (Otto-Bliesner et al., cp-2019-174) already includes mid-Holocene sea ice coverage changes in its Fig 17. We will explore both approaches before committing to a course, and take guidance from the Editor upon this question.

The second main comment discusses the AMOC. A sub-group of our authors have already initiated a detailed analysis of the mid-Holocene AMOC and deep-water formation, but this is planned as a separate paper to allow the analysis to sufficiently investigate the mechanisms at play. The review also suggests a subtly different choice to the AMOC latitude. We have tested the alternate definition on a subset of the models (see supplement) and find that it results in variations in the midHolocene AMOC percentage change with a magnitude of at most 0.85

We shall detail retrospectively how we have revised the manuscript in light of each specific comment in the review. However, we feel it would be instructive to respond to two particular ones at this early stage.

Firstly, it is possible to create a version of Fig. 5 that includes the observations/reanalysis, and we include it in the supplement to this Author Comment. However, given that the boundary of the domain in the observations/reanalysis is already

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marked in two panels, we question how much more information such an additional panel conveys.

Secondly, the review posits an interesting method to estimate the size of the interpolation error from the PaleoCalAdjust routine – namely by looking at the changes in the annual mean surface temperatures. We include also this figure in the supplement to this Author Comment, and it is up to 0.8oC in magnitude. However, it is important to stress that this difference really arises from assumptions in the subsequent workflow, rather than the PaleoCalAdjust routine itself. We have relied upon the Climate Variability Diagnostics Package (Phillips et al, 2013) to compute the bulk of the fields presented in the manuscript. Within this package, the annual values are calculated as the unweighted average of the 12 monthly values. This results in minimal errors under present orbital configuration yet allows the package to readily handle many different calendars efficiently. Unfortunately, such an assumption is not appropriate once PaleoCalAdjust has been implemented on the Mid-Holocene monthly output, because that software intentionally adjusts the data to represent the different, non-equal length months.

Please also note the supplement to this comment:

<https://www.clim-past-discuss.net/cp-2019-168/cp-2019-168-AC4-supplement.zip>

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-168>, 2020.