

Review Shi et al.

Minor comments:

Lines 26-29: I would suggest to remove the last point from the abstract (“One important.....and night on our Earth”). This is important information and should be provided in the manuscript, but it makes the abstract a bit too long. Furthermore, you mention this point already briefly on line 14.

Line 441: In figure S11 only the model-data comparison is shown for the classical calendar definition. Combining this with information from figure 4 the authors continue to argue that in some regions the model-data match is improved by using the angular-seasons, while in others it is deteriorated. This is all very descriptive and qualitative, why not actually show this in a figure?

Lines 452-458: Here LIG annual mean temperature anomaly reconstructions are compared with simulated summer temperature anomalies. I understand that the authors wish to present both model-data comparison for the MH and the LIG, but to me this LIG model-data comparison makes little sense. As the authors mention, annual mean temperature anomalies should not be impacted by the calendar definition. By comparing them with modelled summer temperature anomalies, a calendar effect can be shown, but is that sensible? Arguably, LIG annual mean temperature anomaly reconstructions include a seasonal bias, perhaps towards summer, but to me that seems outside of the scope of this manuscript. Perhaps simply leave out this section and concentrate on the MH model-data comparison?

Lines 459-464: It is not clear to me why ‘bioclimatic indicators’ would be less dependent on the calendar definition. With respect to the example shown here, I would expect that the leaf area index is strongly impacted by temperature, and since the authors show large SON changes in temperatures depending on the calendar definition, I would expect impacts on leaf area index as well. Shown here (fig S13) are percentages changes between the two calendar definitions for different periods and those are indeed small. But perhaps more importantly, how large are these differences compared to the differences between the different climatic periods? So for instance the SON leaf area index difference between PI and LIG? Are they of similar magnitude? In general, if an example like this is given, then sufficient information and discussion should be provided for the reader to follow and judge the line of reasoning.

Conclusion section: Given that this manuscript really covers all aspect and choices that need to be made when dealing with calendar issues in paleoclimate modelling work, the conclusion section seems to me to be a great place to clearly outline some “best practices”. Which calendar-related adjustments should palaeo modellers do and which ones are of second-order importance?

Figure 11: There are some important differences between the seasonal cycles based on daily versus monthly precipitation data. This is not unexpected, but should be mentioned in the main text.

Technical comments:

Line 56: Perhaps “larger seasonal temperature contrast”? If it is not temperature but insolation that you are referring to, then I don’t understand the remark about “which holds true for both hemispheres in most model simulations”, because TOA insolation should indeed be nearly identical in models.

Line 105: “and Kepler’s 2nd law states”

Line 183: “more than”

Figure 11: some labels seem missing.