

Clim. Past Discuss., referee comment RC1  
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## **Comment on cp-2021-27 "Evolution of continental temperature seasonality from the Eocene greenhouse to the Oligocene icehouse - A model-data comparison"**

Anonymous Referee #1

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Referee comment on "Evolution of continental temperature seasonality from the Eocene greenhouse to the Oligocene icehouse –a model–data comparison" by Agathe Toumoulin et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-27-RC1>, 2021

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This is my first review of the manuscript by Toumoulin et al., titled "Evolution of continental temperature seasonality from the Eocene greenhouse to the Oligocene icehouse - A model-data comparison". The authors use a series of model simulations to investigate changes in seasonality (Mean Annual Temperature Range, MATR) across the Eocene-Oligocene transition (EOT). They also compile published estimates of temperature seasonality proxies and compare those to their model simulations. I think the manuscript provides a significant contribution providing a new view on the changes taking place across the EOT. The manuscript is also well written, mostly easy to read, and contains a wealth of background information. My main critique concerns the presentation of the results, as I think the authors could make their arguments stronger with a bit more analysis and/or by better acknowledging the limitations of their study. I suggest a major revision, but they should be quite straightforward to do. After a revision, I believe the manuscript would be worthy of prompt publication in the Climate of the Past. Please find my detailed comments below:

Major comments:

#1 Overall, I think 3.1.3 is cutting corners, it might well be that some of the stated mechanisms are true, but it is difficult (if not impossible) to confirm the mechanisms based on the evidence presented. For this type of paper, it is not crucial to identify the exact mechanism, although it is valuable of course. I would suggest the following 1) to be able to make a bit more robust statement, the authors could check the correlation between surface air temperature change and latent heat flux change/P-E change/Primary prod. Change. 2) I would change the language towards 'we suggest that this phenomenon could be explained by...' rather than 'this phenomenon is well explained by'.

#2 Especially the argument of increasing cloud cover is not very convincing to me. In western Europe, there is a 10-20% increase, but that is not really seen in the southern hemisphere (small patches of 10% increase in austral summer). However, Fig 4b and magenta contours in Fig 5 seem to suggest that the negative MATR changes take place at the edge of the Hadley cell (and the associated ocean gyres/fronts) and the changes would be consistent with an equatorward/poleward shift of the Hadley cell – which would also impact the oceanic subpolar gyres. The Hadley cell extent has been well studied and can be related to changes in a latitudinal temperature gradient, which is clearly changing in these simulations. I would encourage the authors to rethink their results in this context.

#3 In relation to comments #1-#2 I would encourage the authors to check the relative change in MATR. Since the MATR is usually small over the ocean, I would think that some of the signals would be emphasized, and maybe easier to appreciate, if one would look at the change relative to the baseline (i.e change in percentage).

#4 To me the proxy-data comparison mainly demonstrates that the simulations and proxies do not match in several locations in the 35-60N latitude band. I agree with the authors that especially in Europe the changing sea-level in the complex topography might be important (changing from sea to land would increase seasonality), and I wonder if it would be possible to 1) indicate which locations are in Europe in Fig. 7 and/or 2) provide a figure like S2 showing also the MATR difference in the proxy locations (coloring the dots accordingly).

Minor:

#5 I think it would be easier to see that the MATR change is due to cool summers if the authors would show [2X-3X (JFM)]-[2X-3X (annual)] in the second row, and [2X-3X (JAS)]-[2X-3X (annual)] in the third row. At the moment one needs to do this comparison by eye, which is not optimal.

#6 L424, L488: The authors write “The best representation of the temperature seasonality evolution from Priabonian to Rupelian arises when sea level drop is taken into account...” and “Europe stands in an intermediate position between North America and Asia with generally weaker changes in MATR.”. It is unclear if these statements are based on the model results presented in this study (if yes, then please refer to figure/section in the manuscript) or is there some proxy/literature support as well (if yes, please provide references here).

Language/Typos:

#7 L135 ‘the’ instead of ‘a’

Figures:

#8 Fig. 1: The authors might want to check how they save the image. In the pdf version, it seems that there are some longitudinal stripes that I believe are not realistic. This is not a huge issue, but it could be due to an artifact of switching between ps/pdf or something similar, so maybe worth checking if it can be easily fixed.

#9 Fig. 2: I would suggest adding 3X shoreline contour to panels using 2X-ICE\_SL (d,h,i). I was a bit confused first about the large positive temperature differences, but then realized that those are in regions where the land-sea distribution has changed.

#10 Fig 5. in panels e-f most of the latent heat flux change is negative, but in the text, the authors talk about an increase. I understand that this apparent contradiction can be simply due to a sign convention (negative down), but I would suggest flipping the sign (so positive anomaly implies an increase), and also define the sign of the fluxes in the caption. The same is true for other figures as well, I would ask the authors to use positive for an increase and negative for a decrease.

#11 Fig 6: L295, I believe the authors mean 'low-level cloud fraction changes'.