Climate of the Past Discussions

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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 #2

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-RC2, 2021

Toumoulin and colleagues present a valuable model-data comparison of how seasonal temperatures across the Eocene-Oligocene Transition developed. Toumoulin et al model different mechanisms (e.g. a decrease in $pCO_2$, the development of the Antarctic ice sheet, and a decrease in sea level) and how these forcings affected concomitant changes in global temperature seasonality. The models are compared against known proxy data, primarily from paleobotanical sources, that quantitatively demonstrate temperature and temperature seasonality. Although the authors are not able to resolve the demonstrated mismatch between proxy data and the models, they are able to provide a new perspective on how and why temperature seasonality likely evolved during the EOT. Furthermore, they offer some ideas as to why the proxy data and the models do not correctly align. I think the conclusions they draw are appropriate and interesting. The manuscript is well-written and researched, the topic of study and the methods employed are appropriate for *Climate of the Past.* I have a number of mostly minor comments and suggestions regarding certain aspects of the manuscript. Following some suitable revision to address these comments, I feel the manuscript should be published.

My suggestions and comments are detailed below:

#1 I was not wholly satisfied with the introduction. The themes and content of the introductory sections are generally appropriate, however, I feel that their organization and connectivity could be improved. For example, I felt the context of the EOT as provided in section 1.1 was a bit shallow. The chance to set the stage of the EOT is somewhat lost as the authors transition very quickly into how temperature seasonality can be quantified. I think there is an opportunity to offer more to the reader about our current understanding of the EOT and the significance of the event as a potential analogue with respect to our modern climate. Some of these ideas are presented at the end in the conclusions, but I think they could be presented earlier.

The aims of the study are provided in section 1.4; however, the overall placement of this section feels late. I was left wondering very early as I was reading through sections 1.1 through 1.3 what the authors were planning to accomplish. I think presenting this earlier will provide better context to the reader for what the authors goals are as they read through the following sections. I would suggest the authors to consider revising the introduction to improve some of these shortcomings.

#2 In section 1.2 the authors list a number of plant genera and family, however, only in a couple cases are a more common or generalized named provided. Not all readers may be familiar with the plant genera or families listed and thus some quickly communicated information about the type of habitats that these plants represent is lost. This becomes especially problematic when plant families that are no longer formally recognized, such as Flacourtiaceae, are used. This makes it especially difficult if a reader tries to discover more. I would recommend the authors provide the common names for the listed genera and families as this can only help the botanically unfamiliar reader.

#3 In figure 5 panels g-h the model simulations show changes in primary productivity. These panels as ordered imply to me that the model is suggesting that primary productivity increased in the northing latitudes during the summer (JAS). I am not sure if there is a convention here that is being used that I am unfamiliar with, but if this is not the case and model does show a decrease in net primary productivity then this would be very counter-intuitive to what is expected and requires some explanation. This also seems contradictory to what is stated in the text in section 3.1.3, where the authors state that conditions favour primary productivity in the summer.

#4 In table 1 the authors defined MAT as the Mean Annual global 2-meter air Temperature, which appears to add an additional layer of complexity to the well-known definition of MAT. Although this is a relatively minor point, I would suggest better to call it Global MAT or devise a different acronym for this purpose. This usage is also different to how MAT is defined by the authors in supporting table S1. For Table S1 MAT is defined as the average Mean Annual Temperature changes. I think it would be better for this table S1 to be labeled as ΔMAT. There needs to be consistency between definition used in both the manuscript and the supplemental information.

#5 There is not much discussion about the paleogeographic position of the proxy data used to compare against the model simulations. The locations of the fossil proxy localities are important to the context of the changing sea level. If the forests that the plants were growing in were affected by a coastal climate, then a reduction in sea level would have greatly influenced seasonality and promoted a more continental climate. However, if some of these localities were already far away from a coastline, they may not have experienced a significant increase in seasonality. Coastal influence is discussed briefly, but a greater context I feel is absent and think would add to the authors discussion.