

Clim. Past Discuss., author comment AC2
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Reply on RC2

Agathe Toumoulin et al.

Author 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-AC2>, 2021

Dear reviewer,

We sincerely thank you for reviewing our manuscript and for recognizing the value of our work. We carefully accounted for all your comments and questions (here in bold italic), and provided detailed answers here.

In addition, while correcting our manuscript, we felt it was unfortunate not to include data from the compilation of Pound and Salzmann (2017), given the small number of data available. We propose to increase the number of data and thus update the results accordingly as presented in a dedicated section after answering your comments. We are aware that this kind of practice is not usual, and we apologize for the extra work it may require, but we believe it will give more representative results of the changes of the Eocene-Oligocene Transition. The message of the paper and the conclusions remain the same.

Overall, we feel the manuscript is greatly improved by these substantial revisions.

Best regards,

Agathe Toumoulin on behalf of all co-authors.

#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.

Thank you for this comment. We agree the paleoclimatic context was a bit too short. We went back to the introduction to better contextualize our study and now announce research questions earlier in the text, as you suggested.

New text at the end of section 1.1 : "By providing paleoclimate modelling simulations compared to a short synthesis of indicators of seasonality changes (Table S1), our study attempts to reconstruct the evolution of seasonal temperature contrast from the middle Eocene to the early Oligocene. We assess global patterns of temperature seasonality change and their main mechanisms through a set of five simulations taking into account the three-major climate forcings described through this time interval: pCO₂ drawdown, AIS formation and concomitant sea-level lowering."

Regarding the association of our results with the current climate deterioration context, we kindly disagree and would prefer not to do so. You are right, the study of ancient warm climates is often justified by the current climate crisis, the Miocene Climatic Optimum has also been called a potential model for our future world. Yet, although pCO₂ already reached values reconstructed for the Oligocene we consider highly speculative that we may face a change back to the Eocene world if we reach late Eocene pCO₂ values, notably because of different geographies. Thus, we do not believe that our research can be related to current climate change. We do not claim the immediate usefulness of our research but believe that the interest of our research lies in its exploratory aspect, which we hope opens new avenues of reflection concerning the understanding of the Eocene-Oligocene transition. We thus would prefer to conservatively restrict the scope of our study modeling the Eocene climate to the mechanisms of greenhouse climate in line with previous studies on that period.

#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.

We agree on providing common names to simplify. For the beginning of the sentence, we modified the order of the words within the sentence to provide common names first, following your view. However, we kept some of them in the end of the sentence because there is not always a common name for families and genera. Apart from laurel, common names may not be particularly enlightening to non-botanist reader anyway (e.g. one could talk about "annonaceae" by saying "annonas" and "myrtaceae" by saying "myrtles"). Also, since Flacourtiaceae was divided into various different families, we deleted it.

Original text (lines 64-69): "[...] species characteristic of warm paratropical to temperate environments such as conifers *Doliosstrobos* sp. (conifers), *Nypa* sp. (palms), *Rhodomyrtophyllum* sp. (Myrtaceae), and some families with tropical elements such as Annonaceae, Lauraceae, Cornaceae, Flacourtiaceae, Icacinaceae, Menispermaceae, and, depending on bioclimatic zones, the expansion of temperate to boreal vegetation through the increase of deciduous and / or coniferous species (Eldrett et al., 2009; Kunzmann et

al., 2016; Kvaček, 2010; Kvaček et al., 2014; Mosbrugger et al., 2005; Utescher et al., 2015; Wolfe, 1992).

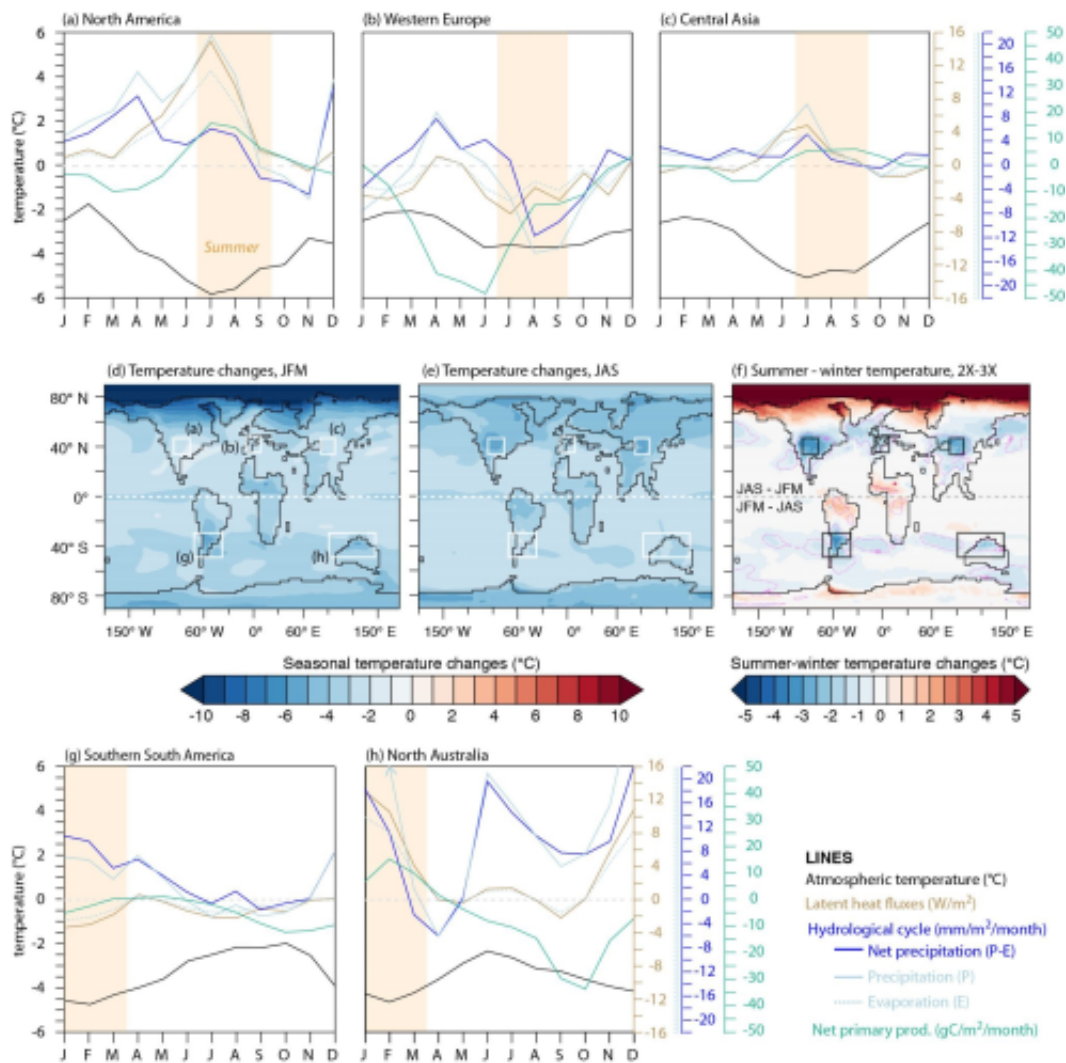
New text (lines 64-69): “[...] species characteristic of warm paratropical to temperate environments such as mangrove fern (*Acrostichum* sp.), conifers (e.g., extinct *Doliosstrobos taxiformis*, plum yew *Cephalotaxus* spp.), palms (e.g., mangrove palm *Nypa* sp., rattan palm *Calamus daemonorops*), plants from oak family (Fagaceae, e.g., trigonobalanoid clade species), plants from the myrtle family (Myrtaceae, e.g., *Rhodomyrtophyllum* sp.), and some plant families with tropical elements (e.g., Annonaceae, Lauraceae, Nyssaceae (mastixioids), Icacinaceae, Menispermaceae), and, depending on bioclimatic zones, the expansion of temperate to boreal vegetation through the increase of broadleaved deciduous and / or coniferous species (Eldrett et al., 2009; Kunzmann et al., 2016; Kvaček, 2010; Kvaček et al., 2014; Mosbrugger et al., 2005; Utescher et al., 2015; Wolfe, 1992).”

#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.

Thank you for your comment, our figure was indeed confusing. In fact, we are talking about the increase in primary productivity within the areas of decrease in MATR, which are framed by the pink dotted lines in the original Figure 5.

Since both of the reviewers found this figure unclear, we suggest dividing Fig 5 into two figures by:

- Restricting Fig. 5 to subfigures (a-d) and
- Making a new figure (see below) showing temperature, latent heat, hydrological cycle (precipitation / net precipitation / evaporation), and net primary productivity changes between 2X and 3X, with regional plots (one for each zone in which MATR decreases) instead of maps (as in the original Figure 5). It is now easier to identify eventual correlations between the different parameters. We propose to add this Figure to the supplementary material since it provides information on specific climate mechanisms that are not necessary for the understanding of the manuscript.



Additional diagnostics - Annual variability of multiple climate parameters within the different seasonality lowering terrestrial zones between 3X and 2X (a-c,g,h): surface atmospheric temperature (black), latent heat flux (soil to atmosphere; brown), hydrological cycle (incl. precipitation, evaporation and net precipitation, different shades of blue), and net primary production (green). (d-f) Temperature changes and Δ MATR between the simulations. Rectangles contour terrestrial zones (ocean zones are not included) analysed in subfigures (a-c,g,h).

#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 Δ MAT. There needs to be consistency between definition used in both the manuscript and the supplemental information.

Thank you for noticing. We simplified the caption of Table 1: "MAT: Mean Annual Temperature (°C)" and used Δ MAT for Table S1. In the same way, we added a " Δ " to the headings of the other columns of the table s1

#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.

The effect of transgressions/regressions on the change in oceanity/continentality of regional climate and thus regional vegetation is an interesting point.

In the original version of the MS, we suggested this effect in the section 4.1.2: "Interestingly, the combination of the three forcing mechanisms also lead to a better agreement of modelled Δ MATR and middle to late Eocene data, especially in coastal areas of Kamchatka, and South China (triangles, Figure 4). Although the 70-m sea level decrease from the 2X-ICE-SL simulation is too important for the late Eocene, the better data-model agreement when both AIS and sea-level decrease are considered suggests that small ice-sheet development before the EOT may have played a significant role in driving the middle to late Eocene Δ MATR."

Even if we agree with you on the principle that the vegetation of coastal areas has certainly been more affected by the drop in sea level than continental areas, which we suggest in our article, it could also be that in some areas, even coastal ones, the drop in sea level is not systematically recorded by the vegetation considering that we have only 10-20% of the original vegetation (woody species) preserved as fossils in macro-floras. However, and this is important, what paleobotanists have recognized is that floristic composition could indeed be markedly different between neighboring lowland coastal plain regions depending if they are influenced by different-warmed seas/oceans. So, we could hypothesize that it is particularly the disappearance of temperate to warm shallow basins (and epicontinental seas) that should be recorded more frequently. However, a much larger number of points would be needed to confirm this.

We completed our discussion with the following sentences:

- section 4.1.2: This better agreement with these coastal sites can be explained by the fact of a possible greater sensitivity of the vegetation of the coastal zones, which are generally not very seasonal and with mild winters.
- Section 4.1.3: Note that our results are very dependent on the paleogeography used in the simulations and of the location of the data we compared to our modelling results.
- Section 4.1.3: This disparity could be due to differences in recording, the fragmentary nature of the fossil record could induce differences in the quality of the recording of MATR changes, but also to differences in the temperature of marine/oceanic zones before regression. It can be assumed that depending on their volume, these areas played a more or less important buffering role on temperature variations, and therefore their disappearance has affected the MATR more or less significantly.

DATA ADDITION

In correcting our manuscript, we felt it was unfortunate not to include data from the compilation of Pound and Salzmann 2017, given the small number of proxy-data available. We propose to increase the number of data and thus change the results accordingly as presented hereafter. We selected data from Pound and Salzmann, 2017 to retain (1) the best dated data according to the dating quality indicator used by their study (data Q1 to

Q3), (2) sites with temperature estimates for the Priabonian and Rupelian, or at least one nearby locality that could be compared. No Eocene-Oligocene site was selected for more clarity. This allowed us to add 18 data points (to the 17 points present in v1 of our publication). In an effort to limit the addition of overly uncertain ΔMATR data, we chose not to include data with a range of CMMT estimates ($\text{CMMT}_{\text{max}} - \text{CMMT}_{\text{min}} \geq 10^\circ\text{C}$ (either for Priabonian or Rupelian sites). Of these new sites, 14 are located on the continents and enable a direct comparison to model ΔMATR values, the others from marine cores using pollen of uncertain provenance, are shown in the new Figure 4 but are not used in the statistical analyses.

For greater realism, we also changed the way we calculated the differences in $\Delta\text{MATR}_{\text{min}}$ and $\Delta\text{MATR}_{\text{max}}$ (i.e., the negative and positive error associated to ΔMATR from the data), which did not sufficiently reflect the possible extent of ΔMATR . $\Delta\text{MATR}_{\text{min/max}}$ are now calculated from the average prediction error of the coldest (CMMT) and warmest (WMMT) months, instead of simply the difference between $\Delta\text{MATR}_{\text{min}}$ and $\Delta\text{MATR}_{\text{max}}$ (see below).

- In the submitted version of the manuscript

$$\Delta\text{MATR}_{\text{min}} = \text{MATR}_{\text{min}}(\text{recent}) - \text{MATR}_{\text{min}}(\text{old})$$

$$\Delta\text{MATR}_{\text{max}} = \text{MATR}_{\text{max}}(\text{recent}) - \text{MATR}_{\text{max}}(\text{old})$$

- In the new version

$$\text{Error } \Delta\text{MATR} = \text{average}((\text{CMMT}_{\text{max}} - \text{CMMT}_{\text{min}}) + (\text{WMMT}_{\text{max}} - \text{WMMT}_{\text{min}}))$$

RMSE analysis - The addition of these data decreases the average model-data difference and leads to better RMSE scores as well (see Table 2). It is nevertheless necessary to specify that, for the RMSE, this low deviation is partly due to the sometimes-wide prediction ranges of ΔMATR (difference between $\Delta\text{MATR}_{\text{min}}$ and $\Delta\text{MATR}_{\text{max}}$). The trends described in the first version of the paper remain the same with a slightly reduced prediction when the Antarctic ice-sheet alone is added, but the best-one when the Antarctic ice-sheet and sea level decrease are added together.

In addition, a better agreement between data and simulations without sea level drop is also observed, as visible with the percentage of sites where the direction of ΔMATR is adequately modelled (Table 2 below, line "%"). This is due to data points from Pound and Salzmann (2017) predicting decreases in ΔMATR in areas where the model also predicts a decrease in seasonality (which is based, as explained in v1 of the manuscript, on the lowering of $p\text{CO}_2$). As before, agreement is better when the least warm Eocene simulation (3X) is used as the reference point for the model's ΔMATR calculation (right part of Table 2).

Table 2 – Grey values are from the original manuscript, bold values are new values calculated after adding new data from Pound and Salzmann, 2017.

	2X - 4X	2X-ICE - 4X	2X-ICE-SL -4X	2X - 3X	2X-ICE - 3X	2X-ICE-SL -3X
Mean ΔMATR	5.3 °C	5.8°C	3.9°C	4.6°C	5.1°C	3.2°C

(model - data)

NEW Mean
 Δ MATR

-3,52°C -3,91°C -1,92°C -2,81°C -3,20°C -1,20°C

RMSE

5.0°C 5.3°C 4.1°C 4.8°C 5.0°C 3.8°C

NEW RMSE

3,06°C 3,38°C 2,49°C 2,91°C 3,15°C 2,35°C

%

5,8 % 5,8 % 35,3 % 0,0 % 0,0 % 58,8 %

NEW %

19,35% 19,35% 41,94% 22,58% 16,13% 45,16%

rho

0.21 0.35 0.57** 0.20 0.37 0.56**

(p = 0.45) (p = 0.20) (p = 0.02) (p = 0.47) (p = 0.17) (p = 0.03)

NEW rho