Clim. Past Discuss., 11, C3044–C3052, 2016
www.clim-past-discuss.net/11/C3044/2016/

Climate of the Past Discussions
Open Access
EGU

Interactive comment on "Palaeogeographic controls on climate and proxy interpretation" *by* D. J. Lunt et al.

Anonymous Referee #1

Received and published: 26 January 2016

This is a review of "Palaeogeographic controls on climate and proxy interpretation" by D. Lunt et al. In this contribution, the authors carry out a large ensemble of simulations with the HadCM3L GCM. The 19 simulations span the Cretaceous and Paleogene (1 simulation per stratigraphic stage) using palaeogeographic maps provided by Getech Plc. They find that inter-stages global mean surface temperatures are remarkably similar but can display strong regional differences, mainly attributed to transitions in ocean circulation modes. Although the comparison between different stages can provide very valuable insights, this is not the primary focus of the study. Indeed, these simulations are the basis to reconstruct an "adjustment factor", which integrates palaeogeographic and solar forcings and that can be used to correct proxy records of (surface) temperature all over the globe and crop up the component associated with carbon cy-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

cle changes. An application of their method to the Falkland plateau records over the Cretaceous-Paleogene is then presented.

The approach presented in this manuscript is interesting and would certainly be useful to a large part of the paleoclimate community, as it may help revise how much of the Cretaceous and Cenozoic warming or cooling seen in the proxy records can be attributed to CO2 variations. As such, it certainly is in the scope of Climate of the Past.

The manuscript is clear and well written and I think it deserves publication provided that the following remarks are taken care of. Essentially, it is of upmost importance to me that the authors discuss the numerous caveats more thoroughly. Especially as I have little doubt that the data community will extensively use the adjustment factors provided here.

The experimental protocol is sound and fixing the CO2 to 1120 ppm seems reasonable. I think that the simulations would need to be run for a longer period, as indeed some seems not quite equilibrated. Yet I am aware of how complex and computationally expensive it is to carry out that many simulations at a time and I also believe that the main message presented in this manuscript – that evolving palaeogeography may critically impact proxy records – should be published. However, an additional word of warning regarding the equilibrium states of the simulations would still be in order in the conclusion. It has to be clearly stated that intermediate to deep waters temperature records cannot be corrected with the adjustment factors proposed in this manuscript.

In my opinion, three major caveats related to the adjustment factor are missing in the discussion of this manuscript. This is somewhat problematic because although an interesting idea, the adjustment factors proposed in this study should be considered with extreme caution. An incorrect adjustment factor (e.g. due to incorrect palaeogeography) would indeed lead to misinterpretations of the CO2 component in the proxy records.

1) The uncertainties in the palaeogeographic reconstructions. Stage-stage regional

CPD

11, C3044–C3052, 2016

variations in sea surface temperatures (SSTs) or surface air temperatures (SATs) are critical in the construction of the adjustment factor. However, small corrections to the palaeotopography and/or palaeobathymetry and/or land/sea mask may lead to huge differences in the local SST/SAT variations and this is not at all discussed in the manuscript. For instance, a closed Drake Passage during the Early Cenozoic (e.g., Eagles et al. 2006) would alter the ocean circulation and likely the distribution of SST/SAT, especially in the Southern Ocean (e.g. at Falkland Plateau, a record of which is used as a highlight to demonstrate the method in the manuscript). I therefore need to see a paragraph discussing this caveat.

2) Some of the authors have been involved in Modelling Intercomparison Projects (e.g., Lunt et al. 2012). Given the sometimes large inter-model spread, it is legitimate to wonder how much would the adjustment factor vary if another model was used. Model-dependence of the results should also be carefully discussed.

3) Similarly, while the authors claim that they keep the CO2 constant to disentangle the non-CO2 from the CO2 component of proxy records, I question the robustness of the adjustment factor to varying CO2 levels. Lunt et al. 2010 have indeed demonstrated that the ocean circulation response to CO2 is non-linear. This would indicate that the stage-stage local variations in SST/SAT depend also on the background CO2 values. Therefore what if the stage-stage SST distribution is different at 560 ppm than at 1120 ppm? How different would the adjustment factor derived from the 19 simulations carried out at 560 ppm compared to the one derived from the simulations at 1120 ppm? I think it is crucial to also discuss this, as presumably the results obtained with another background CO2 would be different. Accordingly, the last sentence of the abstract should be watered down to acknowledge the numerous underlying uncertainties.

Finally, I think the figures should be thought over again. The stage-stage climatic variations are mainly due to changes in the ocean, yet not a single figure in the main text does indeed show ocean variables. It would be nice to at least display 2 or 3 figures in the main text to avoid jumping back and forth to the supplementary figures. Space can

CPD

11, C3044–C3052, 2016

easily be made by 1) merging Figs. 2 and 3 of the main text into one and displaying it in the Supp Info (also why is the scale so large on Fig. 2?) 2) merging Figs. 5 and 7 Also, it is hard to see anything on Fig. 6 once it has been printed. I suggest showing a larger version of Fig. 6 displaying only the stage-stage transitions discussed in the main text, plus a couple of figures showing the ocean changes argued for in the text. The other stage-stage transitions can be displayed in the supplementary material.

Typos and minor comments

P 5684, L 15 “paleogeographic” should be “palaeogeographic” to be consistent Also on P5687, L 24, “paleoclimates”

P 5686, L 25 Change “could itself be” to “is”

P 5687, L 1 Missing bracket after “Lauderdale et al. 2013”

P 5687, L 27 and following OK but I also suggest mentioning important work on the Late Cretaceous (e.g., Otto-Bliesner et al. 2002, Upchurch et al. 2015). The study of Donnadieu et al. 2016 could also be added.

P 5690, L 1-5 Some of the processes described later in the manuscript (Section 3.3) are high-latitude processes. Could the polar smoothing and/or flattening explain part of the stage-stage temperature changes?

P 5690, L 6 Fig. S2 and S3 should be switched since in the main text Fig. S3 is quoted before Fig. S2.

P 5690, L 12 Could the ice caps be removed? I doubt that they influence the results whatsoever since the ice caps seem really tiny, yet I wonder why these were included.

P 5691, L 1 What is the purpose of comparing the evolution of the solar forcing computed in this manuscript with that of Caldeira and Kasting 1992? All the more as both seems near identical on Fig. 2.

P 5694, L 26 and following I somehow disagree with this statement and think it should

CPD

11, C3044–C3052, 2016

be removed. To me, the first-order trend is not really well reproduced (e.g., the Late Oligocene warming ∼25 Ma or the cooling after the MMCO ∼15 Ma) plus I think it adds confusion to the approach because CO2 concentrations are not kept constant between the Neogene simulations (according to Fig. 3) and ice sheets are taken into account.

P 5694, L 11 Should change "The Hansen et al. 2013 record" to something more accurate, like in the caption of fig. 5: "the record produced by applying the methodologies of Hansen et al. 2013 to the Cramer et al. 2009 data". As far as I know, in their 2013 paper, Hansen et al. did not apply their methodologies to the Cramer et al. 2009 dataset. Also on P 5684, L 13 and 21.

P 5694, L 29 Is a correlation coefficient of 0.42 really meaningful?

P 5695, L 2 Is there any reason to exclude the Berriasian stage?

P 5697, L 1-2 It seems unclear to me if a correlation coefficient of 0.47 can really imply a correlation between global temperatures and continental areas. Same with the correlation between mean orography and land surface temperatures (0.49).

P 5697, L 15-16 This is somewhat unclear and/or contradictory. An influence on global temperatures from the relative albedo of land (therefore surface albedo?) compared with ocean is found (P 5697, L 1-2 and Fig. 8b) but on L 15-16 and on Fig. 8e, no correlation is found between global temperature and (planetary?) albedo. It is said that no partitioning is possible between surface and cloud albedo so how can any conclusion be firmly drawn?

P 5697, L 23 "Aptian-Aptian" should be "Aptian-Albian".

P 5698, L 13-14 Fig. S10 and S9 should be switched since Fig. S10 is quoted first. It can be sloppy to discuss ocean overturning since the mid to deep ocean is not fully equilibrated. Inferences related to mid to deep ocean processes could be reformulated to reflect that until equilibrium they remain hypotheses (although unlikely to change much)

CPD
11, C3044–C3052, 2016

P 5698, L 20 “Fig. S12” should be “Fig. S11”.

P 5698, L 25 “60 and 80° N” should be “70 to 80° N” according to the fig. S12r.

P 5699, L 14-22 Similar to previous comment.

P 5700, L 8 What is the warming associated with then?

P 5700, L 17 “Fig. S12” should be “Fig. S12h”.

P 5701, L 3 As it would appear that the albedo feedback is the primary contributor to the stage-stage warming/cooling in roughly half of the cases shown on Fig. S12, it is unclear to me why the emissivity should be the most consistent feedback? Is is because of the good correlation seen on Fig. 8d?

P 5701, L 6-9 All the records cited here focus on the Cretaceous. It would be worth citing some Paleogene studies as well, as the manuscript focuses on the CPE.

P 5702, L 5 Please give the references associated with each of the 7 records used.

P 5702, L 14 Are the numerical values correct? Max. minus Min. temperature at Maud Rise (Demerara Rise) across the CPE does not seem to be equal to 12.3 (5.6) °C on Fig. 9b.

P 5702, L 20 A small suggestion here: for coastal areas (e.g. Tanzania), it could be useful to provide two different curves (one assuming land, the other ocean) when occurs ambiguity in the model because of its resolution? For instance, by using the temperature estimate of the closest ocean (land) grid cell. Depending on the type of the records, the marine (continental) estimate of the adjustment factor would be used.

P 5703, L 7-9 Rather unclear. For instance for Tanzania (which I guess is part of the equatorial sites mentioned), the amplitude of change due to changing palaeogeography and solar constant (Fig. S11c) is similar if not greater than the amplitude of change due to plate movement (Fig. S11a) if we exclude the shift from a marine to continental site in the Eocene (which is, if I am correct, due to the model resolution). However I

CPD
11, C3044–C3052, 2016

might have misunderstood something here.

P 5703, L 25-26 Fig. 11 is really nice and informative, but could be quite different if other palaeogeographic reconstructions of the CPE were used, or even with the same palaeogeographies but subtle changes in freshwater routing and/or in gateways and/or in local palaeobathymetry/palaeotopography. This is why I think it is critical to clearly highlight these uncertainties, especially if, as proposed in the manuscript, these maps were to be used to help find future drillings or proxy studies sites.

P 5705, L 4 What does Phase 5 consist in? Is it only a further spinup of all the simulations shown here, with the refinements discussed in P 5705 and 5706? Or will new palaeogeographies be included (like Neogene simulations)?

P 5706, L 22 and following Same comments as above.

Fig. 4 If I am correct, according to the initial distribution of temperature in the ocean (P 5692), the temperature of the deepest layer of the ocean model should not be below 10 °C regardless of the longitude/latitude. How is it possible that the mean zonal global temperature at 2700 m is initially at a value of about 7 °C for all the simulations? Is the equation correct? It would also be nice to add small letters to facilitate the identification of each curve with the corresponding simulation, at least for the 670 and 2700 m temperatures. Finally, if the figure can be enlarged, please do it.

Fig. 11 In the caption, “Ypersian” should be “Ypresian”.

Fig. S2 “bartonian” should be “Bartonian”

Fig. S3 “blue cicles are inland endorheic nodes” should be “red circles are inland endorheic nodes”.

Fig. S4 What do the different colours stand for?

Fig. S7 The colorbar is inverted. I haven’t checked every figure but at least it is the case for the Valanginian-Berriasian figure (S7r) and the Albian-Aptian figure (S7n). Fig.

CPD
11, C3044–C3052, 2016

S7r indeed shows a topographic increase in the highest Arctic during the Valanginian whereas it should be a decrease as the highest Arctic becomes oceanic (P 5697, L 28; and Fig. S1).

Fig. S10 “ciculation” should be “circulation” “grescale” should be “greyscale”

Fig. S11 On every subplot, the labels of the periods/epochs (i.e., Eocene, Pal., Late and Early Cretaceous) are shifted. Also, in the (c) and (d) caption, there are missing “and” in the sentences.

Fig. S12 It would be nice if the numerical values on the subplot were aligned. Also it is stated in the caption that the values are expressed in degrees C and as percentage of the total change. The latter is missing.

References

Eagles, G., Livermore, R. & Morris, P. (2006). Small basins in the Scotia Sea: the Eocene Drake passage gateway. Earth and Planetary Science Letters, 242, 343-353.

Lunt, D. J., Dunkley Jones, T., Heinemann, M., Huber, M., LeGrande, A., Winguth, A., Loptson, C., Marotzke, J., Roberts, C. D., Tindall, J., Valdes, P. & Winguth, C. (2012). A model–data comparison for a multi-model ensemble of early Eocene atmosphere–ocean simulations: EoMIP. Climate of the Past, 8, 1717-1736.

Lunt, D. J., Valdes, P. J., Dunkley Jones, T., Ridgwell, A., Haywood, A. M., Schmidt, D. N., Marsh, R. & Maslin, M. (2010). CO2-driven ocean circulation changes as an amplifier of Paleocene-Eocene thermal maximum hydrate destabilization. Geology, 38, 875-878.

Otto-Bliesner, B. L., Brady, E. C. & Shields, C. (2002). Late Cretaceous ocean: Coupled simulations with the National Center for Atmospheric Research Climate System Model. Journal of Geophysical Research, 107.

Upchurch, G. R., Kiehl, J., Shields, C., Scherer, J. & Scotese, C. (2015). Latitudinal

temperature gradients and high-latitude temperatures during the latest Cretaceous: Congruence of geologic data and climate models. Geology, 43, 683-686.

Donnadieu, Y., Pucéat, E., Moiroud, M., Guillocheau, F. & Deconinck, J.-F. (2016). A better-ventilated ocean triggered by Late Cretaceous changes in continental configuration. Nature Communications, 7.

Interactive comment on Clim. Past Discuss., 11, 5683, 2015.