Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-166-RC1, 2020

Climate of the Past Discussions
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EGU

Interactive comment on "Stripping back the Modern to reveal Cretaceous climate and temperature gradient underneath" *by* Marie Laugié et al.

Anonymous Referee #1

Received and published: 25 February 2020

1 General comments

The authors present a set of six IPSL-CM5A2 Earth System Model simulations, in which the boundary conditions are progressively changed from pre-industrial values to estimates for the Cenomanian-Turonian (94 million years ago). Using these simulations, the authors aim to better understand which of the boundary condition changes caused the reconstructed warm and equable (reduced temperature gradient between low and high latitudes) Cenomanian-Turonian climate.

They find that most of the simulated *global* warming is due to the prescribed 4-fold (rel-

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ative to pre-industrial) increase in atmospheric pCO_2, and that the *temperature gradient* is reduced mostly due to 1) the removal of the Antarctic ice sheet, 2) polar amplification in response to higher pCO_2, and 3) enhanced ocean heat transport in response to the different continental configuration.

Unfortunately, the authors do not quite succeed to simulate the reconstructed equable climate, suggesting that the presented list of boundary condition changes and processes that contribute to the more equable climate is not complete or that their contributions are not represented correctly in IPSL.

Although the authors thus do not solve the long-standing problem of simulating equable past climates, the presented results are still interesting and relevant to the field, they are within the scope of CP, and should be published.

However, I do have some corrections and suggestions to improve the manuscript prior to publication, as detailed below.

First, it is a bit unclear to me whether the presented results are supposed to be representative for the Cretaceous in general (as suggested by the title of the manuscript), or for the Cenomanian-Turonian, or for the Oceanic Anoxic Event 2. In the discussion section, due to a lack of other Cenomanian-Turonian simulations, the results are also compared to results for the Eocene, which occurred much later in time than the Cretaceous; but the boundary condition differences between these different time slice simulations are not discussed appropriately.

While the applied approach of successively changing the boundary conditions is appreciated and useful here, and a lot of extra work, the authors do not appropriately discuss any potential shortcomings of the applied linear factorization approach. For example, in a warmer climate like that with $4xCO_2$, a reduction of the surface albedo due to the removal of continental ice sheets would probably have a larger effect, because the surface albedo effect would be masked to a lesser extent by snow cover. I.e., the contributions can depend on the sequence of the changes.

As the conclusions (Section 5) are written now, they are a summary (repetition) of the previously presented results (except for the last two sentences, which to me sound like general speculative remarks). I think the conclusions could be worked out more clearly and in more detail, in particular with the questions in mind that are raised in the introduction. For example, do the results contribute to a consensus on the relative importance of CO_2 versus paleogeography (introduction, paragraph starting on page 3 line 78)?

2 Minor / specific comments

Abstract, page 1 line 14: Please replace "suffered" by something more objective, such as "experienced".

Abstract, line 17: As far as I understand the Cretaceous is a period and not an era.

Page 2, line 55: What is the "conundrum of Cretaceous pCO_2 question"?

Page 4, line 10: Are the carbon and biogeochemical cycle models / is PISCES also running in the presented simulations? Will the results be described elsewhere? In this context, any missing feedbacks could be discussed. E.g., in reality, a 4-fold pCO_2 increase would certainly also affect the ice sheets.

Page 5-6, experimental design: The prescribed boundary condition changes could be described in more detail. How exactly is the ice removed? What happens to the water? Is the ocean salinity adjusted in any of the simulations? What are the properties of the bare soil (e.g., albedo)? What happens to the river routing? To understand the simulated surface warming, an estimate of the pure surface height / lapse rate effect would also be helpful. What are the initial conditions for the $4xCO_2$ runs, why are they only "similar" to those described by Lunt et al.? It is not clear to me how ORCHIDEE works in the presented simulations; are the PFT distributions prescribed and do they

stay the same thereafter in all simulations?

Page 6, line 61: TSI in SOLAR-experiment 1351 or 1353W/m^2 as in Table 1?

Page 8, line 189-190: ...progressive change... induces a general global warming...: this is not true, solar forcing causes cooling.

Page 8, line 191 and Table 2: It does not make any sense to me to describe the warming in terms of the percentage of the initial temperature. The total warming should be set to 100% here. The following contributions should be described accordingly, too. That means, for example, CO_2 contributes 9$^\circ C$ or about 80% to this total warming (and not 61%).

Page 8-9, Table 2: The global anomaly rows are somewhat confusing with the different units ($^\circ C$ versus %). Respective rows showing the relative contributions of the changes (Δice, ΔCO_2, ...) to the temperature, albedo, emissivity would be useful. That would also help to double-check if the contributions add up to 100%.

Page 9, lines 214-215: A decrease of the snow cover over continents probably does not explain the warming over the polar oceans.

Page 9, line 218-220: "The relative humidity decrease can be driven by the temperature rise...". Maybe this is just my lack of meteorological knowledge; references?

Page 9, lines 220-226: Are the prescribed processes speculative, or have they been identified in the presented simulation?

Page 10, line 241: "These contrasted climatic responses to the impact of ice sheets on sea surface temperature have been observed in previous modelling studies but their origin is still unclear..." A discussion of the different suggested mechanisms compared to the processes at work here would be interesting. Is that possible?

Page 10, lines 259-260: "These trends are linked to a general decrease of planetary albedo and/or emissivity, ..." Maybe maps of the prescribed surface albedo, actual

surface albedo (with snow and ice), and planetary albedo for some of the different simulations would be helpful to follow the argumentation in this section.

Page 10, line 261: Which trend is compensated exactly?

Page 11, line 267: The Drake Passage still looks open to me in Fig. 2. Maybe it is better to plot the geographies on the model grid, also showing the sea-land mask?

Page 12, line 299: "...increase is due to...", it seems more appropriate to write that it is "consistent with" those previous findings by Rose and Ferreira; or is this evident from the presented simulations?

Page 12, lines 310-314: The warming due to the different boundary condition changes does not add up to 100% (49% + 30% - 16% + 4% = 67%). Please check the numbers.

Page 12, lines 314-317: "... the increased contribution of paleogeography in the simulated sea surface warming compared to the atmospheric warming, which is probably driven by the major changes simulated in the surface circulation (Fig. 7)." I think this is an interesting hypothesis that could be elaborated in more detail, because the results to test this hypothesis are available here. Regarding the present-day North Atlantic, I suspect that the large cooling patch in that area in the CT-simiulation (Fig. 4f) could be due to the lack of a Gulf Stream or North Atlantic current equivalent. I.e., in that case, the changed circulation leads to a high-latitude cooling rather than to a warming.

Page 12, line 320: How exactly are the meridional temperature gradients calculated? (T(30°)-T(80°))/50°, without any averaging applied? Why between 30° and 80°, and not between equator and poles?

Page 12, line 325: ...the SST gradient can be visualized (not explained) by...

Page 13, line 355: To me it looks like the local warming due to the removal of the Greenland ice sheet would have a visible effect on the meridional temperature gradient, but this effect is somewhat counterbalanced by the cooling in Europe and Siberia. As mentioned earlier, I think it would be interesting to elaborate on the processes at work

here (e.g., stationary wave feedback?).

Page 13, lines 360-362: I do not agree. Looking at Fig. 4f, the warming at 40-70°S appears to be more pronounced in the proto-Indian Ocean than over proto-Australia, i.e., the enhanced warming is probably not due to the larger continental area.

Page 13, lines 362-363: Again, I don't agree that this should be only an effect of the ocean area (but also, for example, due to the lack of a Gulf Stream or North Atlantic current equivalent).

Page 14, Section 4.1 A large part of this so-called discussion section is a comparison of the presented model results to previously published proxy data – the section should probably be titled accordingly.

Page 14, line 369: This is not really a prediction, but rather a hindcast; "were compared" meaning "are here compared"?

Page 14, line 370-371: What exactly does "essentially based on Tabor et al. 2016" mean? What are the differences?

Page 14, line 383: Comparison shown in Fig. 10, not 9a?

Page 14, lines 393-394: It could make the discussion of the data-model fit and of the wide spread of temperatures in one latitudinal band easier if the proxy data was also shown on a 2D map (like Fig. 4f). The DSDP locations could then also be marked.

Page 14, line 400: Between 25 and about 28 (not 30)°C according to Fig. 10a?

Page 15, lines 407-409: This is a main point and conclusion of the study, and should be worked out in more detail. "...we observe the same underestimate...", same as whose / same as what? The temperatures are compared to proxy data, not observations.

Page 15, lines 410-416: How exactly are the gradients computed from proxy data? Please add a measure of uncertainty, the range of the reconstructed temperatures for most latitudinal bands is very large.

Page 15, lines 424-425: "... with the lowest latitudinal gradients being obtained for the highest pCO_2 values." This is not correct according to Fig. 11. The lowest gradient seems to be computed for the $6xCO_2$ CCSM3 simulation. Also, for example, the $2xCO_2$ Eocene simulation with ECHAM5/MPIOM shows a lower northern hemisphere temperature gradient than any of the 2, 3, 4, 5, or 6x Cretaceous simulations with ECHAM5/MPIOM. Note that the simulation denoted here as ECHAM5 is actually also an ECHAM5/MPIOM run. This illustrates that the differences in the applied boundary conditions between the simulations other than CO_2 play a large role and should be discussed in detail.

Page 16, line 441: The title of the subsection is a bit misleading, since not only Cretaceous studies are discussed (also e.g. Eocene).

Page 16, lines 446-448: 2000 years are not very long for equilibrium climate sensitivity simulations, and the $4xCO_2$ simulations do still show a significant trend at the end (SST cooling by about 1K/thousand years; Fig. 1); this should be pointed out here.

Page 16, line 469: Please rephrase, "reduced gradient was amplified"?

Page 17, line 473: Please rephrase, "is the primary control" -> "is a primary control"; other similarly large effects may be missing in the model (given the model–proxy data mismatch).

Page 17, lines 484-488: What would be the probable effects of such a vegetation change, and what the implications for this study?

Page 18, lines 512-513: "Such modelling efforts would probably even more increase the equilibrium climate sensitivity, ..." This is interesting but speculative. Please delete or discuss in more detail / add references.

Page 18, data availability: Are the model source code and the applied boundary condition files also available?

Figure 1: Labels are too small.

Figure 2: Ticks and labels of oceanic area anomaly plot do not line up.

Figure 3: Labels are too small. Fewer contour intervals might be better, also to identify the zero-line. Maybe this is just my printer, but I don't see white in the colorbar.

Figure 5: Labels are way too small, especially in the lower panel. I do not know by hard what the usual cloud cover is at what pressure level. Plots of the total cloudiness from observations and from PI and maybe also 1x-NOICE would be helpful. Is the anomaly really plotted as the absolute difference in cloud cover, as suggested in the caption? That would indicate that the top of the simulated atmosphere is totally covered by clouds!?! Please also define in the text what is meant by high and low clouds.

Figure 6: Caption: "4xNI ... SOLAR-SOLAR" typo experiment name.

Figure 9b+d: Instead of shifting the curves to the same equatorial temperatures, it might be better to plot the anomalies due to the individual boundary condition changes (ΔCO_2, ...).

Figure 10: The red dashed regression line is not visible in my copy.

Figure 11: Labels are too small.

3 Language / typos

There are numerous typos and little grammatical errors, as well as some unclear sentences; I would hence recommend copy editing.

For example: *Page 3, line 85:* ...latest "work" are divided... *"studies" instead?*

Page 4, line 96: ...a set of simulation run... (missing plural s)

Page 9, lines 208-209: The whole surface is warmer... and which is generally larger over continents... (sentence structure)

Page 10, line 251: ...data ant temperature... (and)

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