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Climate of the Past
Discussions

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

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1 General comments

Why should the Cenomanian-Turonian thermal maximum be of more relevance than that of the PETM, MECO, etc. to the future. Worthwhile to flesh out why this particular time period is deemed important.

→ We did not mean to imply that the Cenomanian-Turonian thermal maximum was of more relevance to the future than other greenhouse intervals of the deep-time past. However, the CT thermal maximum can be of particular interest (as can be the PETM) in that it is a time interval with elevated atmospheric CO_2 levels, probably similar or even higher than those expected in the future under a business-as-usual carbon emission scenario. In this manuscript, we present a CT simulation, which has been performed as part of a greater project on OAE2, but we note that Early Eocene and PETM simulations have also been performed by the same group and are part of the DeepMIP project (Lunt et al., Clim. Past 2020, in review). We have clarified the text as follows: "The Cretaceous period is of particular interest to understand drivers of past greenhouse climates because intervals of prolonged global warmth (O'Brien et al. 2017, Huber et al. 2018) and elevated atmospheric CO_2 levels (Wang et al., 2014), possibly similar to future levels, have been documented in the proxy record. The thermal maximum of the Cenomanian-Turonian (CT) interval (94 Ma) represents the acme of Cretaceous warmth, during which occurred one of the most important carbon cycle perturbation of the Phanerozoic, the oceanic anoxic event 2 (OAE2). Valuable understanding can hence be drawn from investigations of the mechanisms responsible for the CT thermal maximum and carbon cycle perturbation."

There will also be non-linear feedbacks that cannot be deconvolved with the current methodology. I.e. The proportion of landmass at different latitudes between the preindustrial and Cretaceous at Preindustrial CO_2 levels (and topographic height differences) will have a dissimilar impact on the vegetation response and in turn the warming response from increased CO_2.

→ We agree and note that referee #1 also raised this point. We agree that repeating the experiments with a different sequence of changes would be desirable to fully assess the shortcomings of the linear factorization method. It indeed has been suggested that a different sequence of boundary condition changes would probably give different results (Lunt et al., 2012). However, these additional experiments using the IPSL-CM5A2 earth system model require a computational cost that we cannot afford. We have added the following discussion about linear factorization in the revised manuscript:

"The choice of applying a linear factorization approach was made for problems of com-

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puting time and cost. Changing the sequence of changes or applying a symmetrical factorization as in Lunt et al (2012) would require too many supplementary simulations. However, such a method is very dependent on the sequence of changes, and results would be probably different if boundary conditions were change in a different order (Lunt et al., 2012)".

I did sometimes get a bit confused with the methodology section. It was sometimes hard to see what models were initialised with what initial conditions. How were some of the boundary conditions treated. i.e. what were the actual albedo prescribed? Was the vegetation uniformly applied even to mountain regions where it may not grow? Might be worth being a bit more concise in describing them individual boundary conditions and how they were implemented for clarity and reproducibility.

→ The description of boundary conditions will be reorganized to be clearer.

2 Minor points.

Page 5 – Line 40. It looks like only the 1X-NOICE is in equilibrium in the deep ocean. The other four still appear to be trending. I think for clarity this should be stated or changed to "near-surface equilibrium". I certainly sympathize as some of my own simulations can take up to 10,000 model years to reach equilibrium. However, I do not think this will change the overall results, but for clarity it should not be stated as complete equilibrium. Gregory plots may also be another useful diagnostic to see if you have an energy imbalance otherwise. → Simulations are indeed not perfectly equilibrated and we have clarified the text: Page 5, line 40: "The piControl simulation was run for 1800 years and the five others for 2000 years in order to reach a near-surface equilibrium (Fig.1)".

Ice sheet removal impact. I agree with your assessment of the regional impact, however it might also be that you get a pseudo ice sheet in the 1X-NOICE simulation with perennial snow cover over the soil surface, just a low elevation one. I suspect this is the case as in the 4X-NOICE you get a much large response in the change in surface

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and planetary albedo. Did you ever run a 4xCO_2 experiment with ice sheets in the pre-industrial to see the relative impact of just the CO_2? → Unfortunately, we did not perform a 4X CO_2 simulation with prescribed preindustrial ice sheets. Indeed, we have a snow cover in the 1X-NOICE, but it is also the case in the 4X-NOICE, and the snow cover is even larger over the Antarctic in the 4X-NOICE than in the 1X-NOICE because of a larger amount of precipitations. The large decrease of surface albedo seen between the 1X-NOICE and the 4X-NOICE is due to the decrease of sea ice. We will add figures of surface albedo in the supplementary figures.

See Figures 1, 2, 3 of the response.

Do you see any change in ocean circulation patterns from removal of the ice sheet and increased CO_2 in the preindustrial?

→ The global meridional stream function keeps the same structure when removing ice sheets or increasing CO_2 in the preindustrial. However, the intensity of ocean circulation changes. In the simulation without ice sheets, NADW is slightly weaker whereas AABW is slightly stronger relative to the preindustrial. The 4x CO_2 simulation predicts stronger AABW and NADW with a shallowing of the NADW.

See Figure 4 of the response.

We can see also some local changes of surface circulation around the Antarctic when removing the polar ice cap, that seems to be related to changes in winds. These changes are probably driving the cooling patch that we can see on fig. 4b, that corresponds to a local increase of sea ice. We didn't want to detail this in the manuscript as it is regional changes.

See Figure 5 of the response.

3 Minor comments.

Page 1 & 2 – Line 17 & 33. 'period' not "era"

→ Modified.

I think it is worthwhile to define what you mean by 'high' and 'low' latitudes as you often see different values purported in different studies for clarity.

→ Done: "high-latitudes (> 60° of latitude)"; "low-latitudes (< 30° of latitude)"

Throughout – put references in chronological order. Page 2, Line 60. Define "P.A.L." here. This is the first instance in the ms rather than on page 6- line 50. Page 3 – Line 79. Change sentence to "the primary driver of Cretaceous climate has been suggested". Page 3 – Line 83. Delete erroneous "s".

→ Done.

Page 4 – Line 96. Probably more accurate to say "We performed six simulations using both Pre industrial and Late Cretaceous boundary conditions where we incrementally modify the Pre-industrial boundary conditions to that of the Late Cretaceous for: : :: : :1,2,3,4".

→ Thank you for this suggestion. The sentence was rephrased as: "We performed six simulations, using both preindustrial and Late Cretaceous boundary conditions where we incrementally modify the preindustrial boundary conditions to that of the Late Cretaceous for the following…".

Page 4. Although stated that IPSL-CM5A2 has been used for contemporary and future climate simulations it would be worth adding a line that states how well the model performs in simulating a modern-day climate. → The performances of the IPSL-CM5A2 are fully described in (Sepulchre et al., 2019), we added the reference to the manuscript: "Building on technical developments, IPSL-CM5A2 provides enhanced computing performances compared to IPSL-CM5A-LR, allowing thousand-years long integrations required for deep-time paleoclimate applications or long-term future projections (Sepulchre et al., 2019). IPSL-CM5A2 reasonably simulates modern-day and historical climates (despite some biases in the tropics), whose complete description and evaluation can be found in Sepulchre et al., 2019."

Page 5 – Line 20. Repetition of "long" in the sentence. Remove one of them. Page 5 – Line 37. "retreat" to retreat. But I think 'removed' would be more accurate. → Done.

Page 6 – Line 53. Is that from the 4X-NOICE simulation? → We will give more precisions in the manuscript (see also reponse to comment from reviewer #1): we have adapted the Lunt et al., 2017 formulation has the Cenomanian-Turonian ocean was warmer (see also Sepulchre et al., 2019) :

If depth $\leq$ 1000 m :

T=10+((1000-depth)/1000)*25 cosâĄą(latitude)

if depth > 1000 m :

T = 10

Page 6 – Line 218-222. Did you look at atmospheric stability arguments in relation to this?

→ Indeed, we should look at atmospheric stability to explain the observations made in relation to these processes that are quite complex. We would need to go into details regarding the changes in water content, relative/specific/absolute humidity as well as atmospheric dynamics to explain the observed changes. We do not want to go into such details in the manuscript which is already long and which not specifically focus on the impact of a CO_2 increase, so we finally decided to remove this paragraph.

Page 6 – Line 223. Do you mean greater season ice melt or there being less sea ice area in the 4X-NOICE compared to the 1X-NOICE simulation?

→ Less sea ice in the 4X-NOICE: "The sea level pressure decrease is possibly a feedback driven by reduced sea ice cover and associated higher temperatures."

Page 12 – Section 3.5.1. The percentage change adds to 99%. Rounding error?

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→ The 30% was actually a 31%, but we finally decided to remove the percentages as it was confusing and to keep only raw values (see also comments from reviewer #1).

Page 12 –Line 321-324: There does not appear to be any change in the N.Hem SST gradients (0.45/lat). Any idea why? You attribute the Greenland ice sheet/sea ice for less sensitivity in the atmospheric gradient of the N.Hem. Something similar here?

→ Indeed, by calculating the gradient with the temperature value at 80°N it seems that there is no flattening. However, if we look at the figure 9b, we can see that the gradient is flatter until 70° of latitude North and is very steep beyond. It light be because the arctic ocean is very isolated due to the paleogeographic configuration, which doesn't allow heat transport beyond 80°N.

Page 14 – Line 370. Do you mean that you used the Tabor, et al. dataset and adding more data points

→ Yes, we clarified it : "Our SST data compilation is modified from Tabor et al (2016), with additional data from more recent studies (see Supplementary data)."

Page 14 –Line 388. Only if you suggest there is a seasonal proxy bias. This is mentioned later on, but might be worth a few ref's that show there are seasonal bias in some proxies.

→ Done: "This congruence would imply that a seasonal bias may exist in temperatures reconstructed from proxies, which is suggested in previous studies (Sluijs et al., 2006; Hollis et al., 2012; Huber, 2012)."

Page 16 – Line 448. Do you mean 'more complex' rather than "large"?

→ Modified sentences: "The signal is notably due to a 9°C warming in response to the fourfold increase in pCO2, which converts to an increase of 4.5°C for a doubling of pCO2 (assuming that the response is linear). This sensitivity agrees with the higher end of the range of values in the investigations mentioned above. However, the sensitivity of IPSL-CM5A2 in our simulations could be slightly lower as the simulations are not completely equilibrated – see Figure 1).”

Page 17 – Line 480. “cloud” not “clouds”.

→ Modified.

Page 17 – Conclusions section. This is a tad bit repetitive of the results/discussion. Perhaps broaden this out with respect to the discussion in your introduction.

→ We will arrange the conclusions to be less repetitive and to fit with raised questions in the introduction

Figure 1, 5,6, 8, 9, 11 Captions. Add ‘mean annual’ to caption.

→ Done.

Figure 2. Is that the model resolution geographies?

→ Bathymetry was shown at the model resolution but not the topography that is shown with a bilinear interpolation. We will change the figure to show the model resolution for the topography also.

Figure 11b. Appears to be some modelling studies missing? E.g. HadCM3L 2xCO_2 data point and others. Or was I interpreting this wrongly? Quite possible!

→ Indeed, some points were missing! We added them.

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Snow mass annual average (kg/m2) over the Antarctic for1X-NOICE and 4X-NOICE simulations and corresponding anomaly.

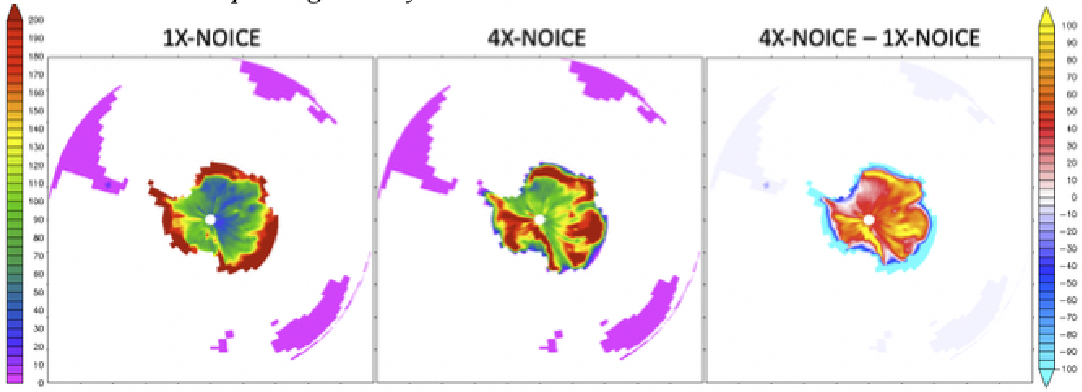
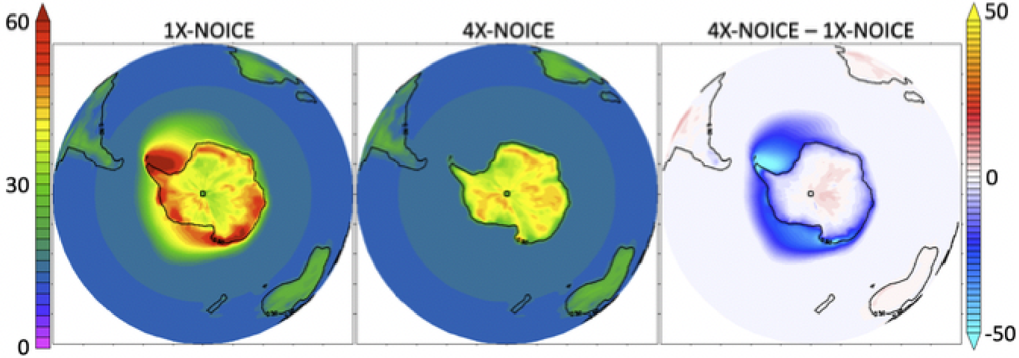


Fig. 1.

Surface albedo average (%) in the Austral ocean for1X-NOICE and 4X-NOICE simulations and corresponding anomaly.



Surface albedo annual average (%) in the Arctic for1X-NOICE and 4X-NOICE simulations and corresponding anomaly.

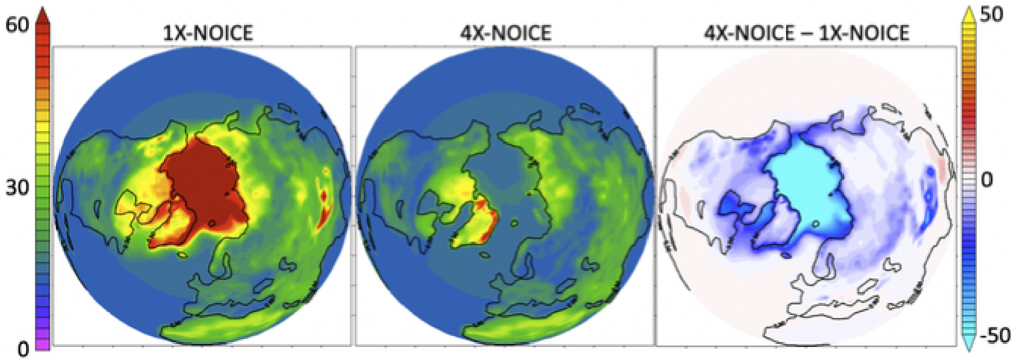


Fig. 2.

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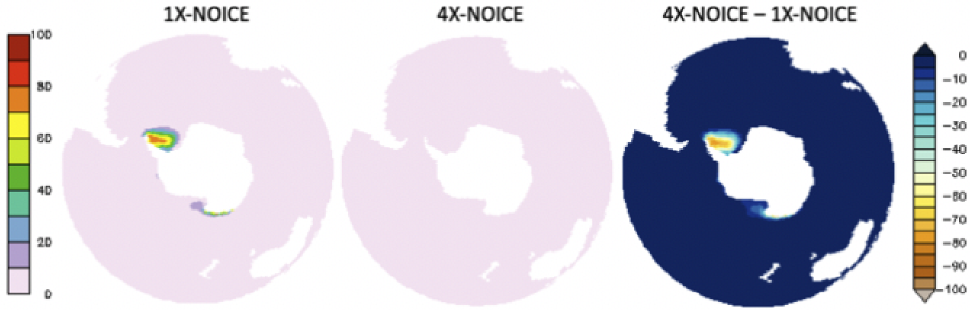
Sea-ice annual average (%) in the Austral ocean for1X-NOICE and 4X-NOICE simulations and corresponding anomaly.



Sea-ice annual average (%) in the Arctic for1X-NOICE and 4X-NOICE simulations and corresponding anomaly.

Fig. 3.

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Global meridional stream function (Sv) for piControl, 1X-NOICE and 4X-NOICE simulations. Positive values (red) indicate a clockwise circulation and negative values (blue) an anticlockwise circulation.

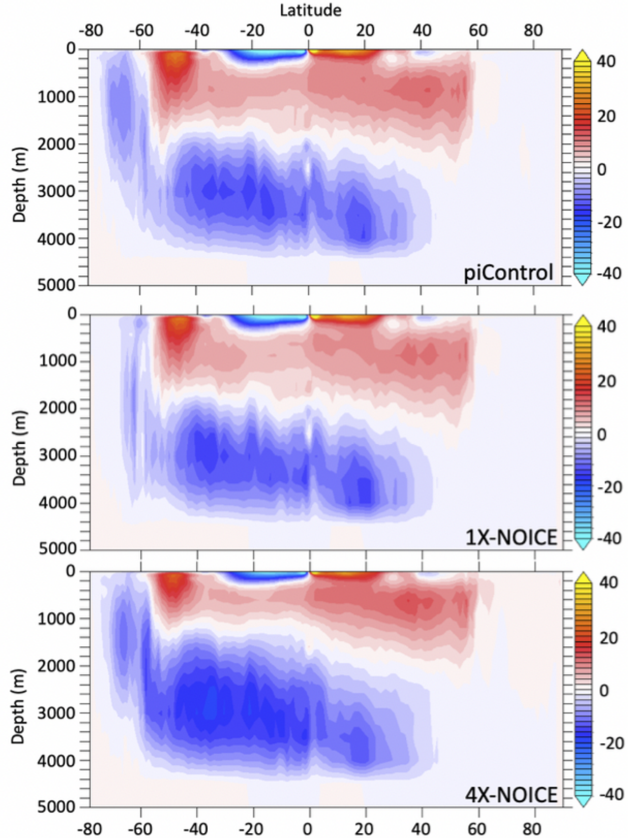
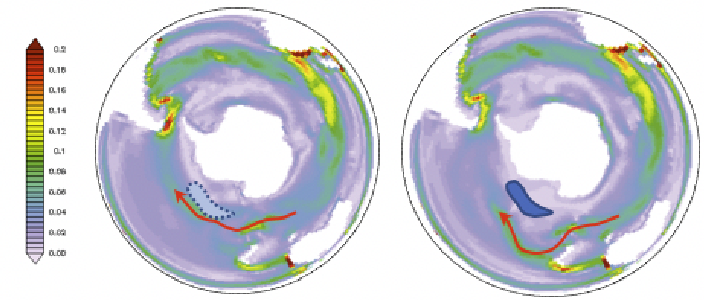
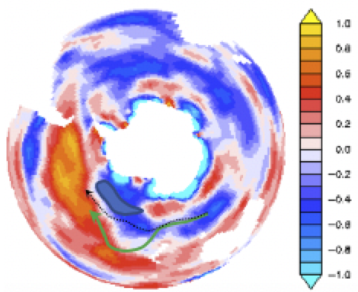


Fig. 4.



Currents velocity for piControl (left) and 1X-NOICE (right) simulations. The red arrow shows the ACC position. The blue box shows the position of the sea ice increase (=cooling area).



Sea surface wind speed anomaly during winter (1X-NOICE-piControl). The polar ice cap removal causes a displacement of winds and thus ACC to the North, which allows an increase of sea ice in the 1X-NOICE simulation. Blue box: Sea ice anomaly, Black arrow: ACC position for the piControl simulation, Green arrow: ACC position for the 1X-NOICE simulation (after polar cap removal).

Fig. 5.