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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

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.

→ The results presented here are representative only for the Cenomanian-Turonian. Simulations for Early Cretaceous or Late Cretaceous (Maastrichtian) would imply different boundary conditions (e.g. paleogeography). It may be not clear enough in the

manuscript, we will clarify it by changing "Cretaceous" to "Cenomanian-Turonian" in the title and in the manuscript.

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.

→ Indeed, the boundary conditions are different for the Eocene and the comparison of gradients cannot be done directly. The idea was more to have a discussion on the temperature gradients simulated by Earth System Models in general.

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.

→ You are correct. 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 computing 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)".

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 CO2 versus paleogeography (introduction, paragraph starting on page 3 line 78)?

→ We will rearrange the conclusion in order to be less repetitive with the previous sections, but also to be more coherent with the questions raised in the introduction.

2 Minor / specific comments Abstract, page 1 line 14: Please replace "suffered" by something more objective, such as "experienced".

→ Done.

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

→ We replaced Âń era Âż by Âń period Âż where needed throughout the revised manuscript.

Page 2, line 55: What is the "conundrum of Cretaceous pCO2 question"?

→ We meant that atmospheric pCO2 levels are not well constrained in the Cretaceous with a large spread in the values inferred from proxy data (e.g. Wang et al. ESR 2014, Foster et al. Nat Comm. 2017). The sentence was rephrased as follows: "Modelling studies have also focused on estimating Cretaceous atmospheric CO2 levels (Barron et al., 1995; Poulsen et al., 2001, 2007; Berner, 2006; Bice et al., 2006; Monteiro et al., 2012) in an attempt to refine the large spread in values inferred from proxy data (<900 to >5000 ppm)."

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 pCO2 increase would certainly also affect the ice sheets.

→ PISCES is also running but results will be presented in another paper in the near future. Based on previous published results by members of our group (Ladant and Donnadieu, 2016), we have considered that no ice-sheet could be formed at 4-fold pCO2 during the C-T.

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 4xCO2 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? → We will rearrange the description of boundary conditions to improve clarity and readability. In simulations with a preindustrial geography, polar ice sheets were removed by setting the fixed land ice fraction to 0 in the model. The topography has been changed accounting for isostatic rebound resulting from the loss of the land ice beneath the Greenland and Antarctic ice sheets. In place of the ice, soil and vegetation parameters were prescribed to brown bare soil (no vegetation), for which the albedo is derived from the soil color (Krinner et al., 2005). River routing stays unchanged. We will add supplementary figures of the new topography without ice sheets and of the temperature change due to the lapse rate effect. Oceanic initial conditions in simulations with 4x CO2 concentrations are adapted from Lunt et al., 2017. In fact, the constant initial salinity field (to 34.7 PSU) is identical to that proposed by Lunt et al. 2017 but the initial ocean temperature field is slightly different from that of Lunt et al. 2017 because of stability issues in the model.

We therefore use the following temperature conditions (see also Sepulchre et al., GMD 2020, in review) :

If depth $\leq$ 1000 m: T=10+((1000-depth)/1000)*25 cosâĄą(latitude)

if depth > 1000 m : T = 10 In our simulations, vegetation in ORCHIDEE is semi-dynamic in the sense that PFTs are prescribed and cannot change but plant phenology is predicted by the model based on surface climate conditions.

Page 6, line 61: TSI in SOLAR-experiment 1351 or 1353W/m2 as in Table 1?

$\rightarrow$ Corrected to 1353 W/m2.

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

$\rightarrow$ Indeed, the change in solar forcing induces a cooling as indicated on line 195. The general global warming mentioned here refers to the global temperature change in the last simulation (4X-Cretaceous) relative to the first simulation (preindustrial), that is, the summed effect of all boundary condition changes.

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, CO2 contributes 9°C or about 80% to this total warming (and not 61%).

We agree that this section was confusing and we will change it in the revised manuscript. To include the impact of solar forcing (cooling), which is opposite to those of other boundary condition changes, we had chosen to describe the temperature changes in terms of percentage of the cumulative absolute temperature change. However, as it is confusing, we decided to remove the percentages and to keep only the raw temperature values.

Page 8-9, Table 2: The global anomaly rows are somewhat confusing with the different units (_C versus %). Respective rows showing the relative contributions of the changes (ice, CO2, ...) to the temperature, albedo, emissivity would be useful. That would also help to double-check if the contributions add up to 100%.

$\rightarrow$ We removed the percentages that were expressed in terms of initial value percentage and keep only the raw values to avoid confusions.

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

$\rightarrow$ Modified sentence: “First, a decrease of sea ice and snow cover (especially over Northern hemisphere continents and along the coasts of Antarctica) leading to surface albedo decrease, explains the warming amplification over polar oceans and continents.”

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?

$\rightarrow$ This part of the manuscript is indeed not very clear. 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 CO2 increase, so we finally decided to remove this paragraph.

Page 9, lines 220-226: Are the prescribed processes speculative, or have they been identified in the presented simulation? $\rightarrow$ The processes are identified in the simulation, however, as explained for the previous comment, we finally removed this paragraph from the manuscript.

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? → We do not detail here the processes responsible for the contrasted climatic response to the ice sheet absence (or presence) as the simulated impacts are mostly regional, and because the climatic consequences of the absence/presence of ice sheets at the poles are already the focus of other studies (Goldner et al., 2014; Knorr and Lohmann, 2014; Kennedy et al., 2015; Kennedy-asser et al., 2020). In these studies, as well as in our simulations, local changes in winds are observed, probably due to topographic changes linked to the existence of the ice sheet. These changes in wind may locally impact oceanic currents, deep-water formation and oceanic heat transport, and thus explain the regional contrasts in temperature changes around the Antarctic continent. We added some precisions in the manuscript to briefly describe these processes:

"These contrasted climatic responses to the impact of ice sheets on sea surface temperatures have been documented in previous modeling studies (Goldner et al., 2014; Knorr and Lohmann, 2014; Kennedy et al., 2015). Their origin in the Southern Ocean is still unclear but, in these studies as well as in our simulations, it seems that local changes in winds, due to topography change after polar ice cap removal, are impacting locally oceanic currents, deep-water formation and thus oceanic heat transport and temperature distribution. In the Northern Hemisphere, the observed cooling over Eurasia could be linked to stationary wave feedbacks due to the change in topography after Greenland ice cap removal (See supplementary information; see also Maffre et al., 2018)."

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.

→ Supplementary figures of planetary and surface albedo as well as emissivity will be provided.

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

→ The increase in albedo, which drives cooling during summer, is compensated by a strong emissivity decrease during winter, leading to winter warming. Modified sentence in the manuscript: "This increase in albedo is compensated by a strong atmosphere emissivity decrease during winter but not during summer, which leads to the seasonal pattern of cooling and warming (See Supplementary Figure S5)."

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?

→ Indeed, the Drake Passage is not completely closed but is very shallow (<300 meters of water depth). This configuration essentially shuts down the ACC, as observed on figure 7.

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?

→ Modified sentence: "This high-altitude cloudiness increase is consistent with the extratropical increase in OHT. . ."

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.

→ The 30% is wrong and should be 31%. However, we finally decided to remove the percentages as it was confusing.

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-simulation (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.

→ The cooling patch observed in the Northern part is located over the Greenland and is due to ocean becoming land in the CT simulation. The cooling patch observed in the Arctic (North of Greenland) could be indeed due to changes in circulation in the North Atlantic. However, this is a regional effect that does not dominate the global signal. The Cretaceous simulation is globally warmer at these latitudes regarding the latitudinal temperature gradients for both winter and summer seasons (fig 6). We do not want to go into details for such regional effects, as they are too numerous to explain, and we want to stay focus on the global signals.

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?

→ The meridional gradients are indeed calculated by doing (T(30)-T(80))/50. No average is applied, the values 30° and 80° where chosen to be coherent with the Upchurch et al., 2015 study.

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

→ Modified sentence: "The progressive flattening of the SST gradient can be visualized by superimposing the zonal mean temperatures of the different simulation and by adjusting them at the Equator (Fig 9b)."

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?).

→ Indeed, the observed cooling can be due to modifications of stationary waves, at

least for the continental part of Eurasia (see figure below). This cooling is however very weak (<1.5°C) compared to the warming observed over Greenland (up to 20°C). We added the precision to the manuscript and will add the following figures to supplementary information:

"In the Northern Hemisphere, the observed cooling over Eurasia could be linked to stationary wave feedbacks due to the change in topography after Greenland ice cap removal (See supplementary information; see also Maffre et al., 2018)."

See Figure 1 in the author response.

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.

→ Indeed, the warming is more pronounced over ocean than over proto-Australia. This warming can be explained by the increased southward oceanic heat transport (cf Results - p.11 – line 277). We modified the paragraph to include this result: "In the mid- to high-latitudes of the Southern Hemisphere, the increase in poleward OHT and in the continental area fraction in the CT drive a reduction in the steepness of the atmospheric gradient."

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).

→ Here, we analyze the global signal, and do not look individually at the different basins. Considering that the global ocean heat transport stays stronger for the Cretaceous (even without Gulf Stream - figure 8c), we don't rely changes in circulation to a lack of paleogeography impact on atmospheric gradient flattening.

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.

→ New title : 4.1 ABOUT THE CENOMANIAN-TURONIAN CLIMATE : MODEL/DATA COMPARISON

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

→ Modified sentence: “The results predicted by our CT simulation are here compared to reconstructions of atmospheric and oceanic paleotemperatures inferred from proxy data (Fig 10a,b).”

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

→ Most of the data used here for the comparison come from Tabor 2016, and we added some other points from more recent studies. All the references are indicated in the supplementary data. → Modified sentence: “Our SST data compilation is modified from Tabor et al (2016), with additional data from more recent studies (see Supplementary data).”

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

→ Yes

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.

→ We are working here in terms of latitudinal gradients, that’s why we didn’t want to make the 2D map that would give another direction to the manuscript.

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

→ Yes

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.

→ Modified sentences: “The simulated annual SSTs reach a monthly maximum of 28°C around the location of site DSDP 258. We speculate that a seasonal bias in the foraminiferal record may represent a possible cause for this difference, as may local deviations of the regional seawater d180 from the globally assumed -1‰ value. The same trend is observed for atmospheric temperatures with data indicating higher temperatures than the model at high latitudes in both the Southern and Northern Hemispheres. This inter-hemispheric symmetry in model-data discrepancy could indicate a systematic cool bias of the simulated temperatures.”

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.

→ The gradients are calculated based on the slope of the linear regression line (see supplementary data 1). We added in the supplementary file the standard error for the regression line.

Page 15, lines 424-425: "... with the lowest latitudinal gradients being obtained for the highest pCO2 values." This is not correct according to Fig. 11. The lowest gradient seems to be computed for the 6xCO2 CCSM3 simulation. Also, for example, the 2xCO2 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 CO2 play a large role and should be discussed in detail.

→ Indeed, this observation is only true when considering the same model and boundary conditions (except for the SST of the HadCM3L), but is not correct when considering the different boundary conditions (i.e Eocene vs Cretaceous for the ECHAM5 and CCSM3). We also have different meridional gradients for the same model/boundary conditions but with different model parametrization (e.g. altered cloud physics). We made the correction in the manuscript: “However, when comparing different studies with the same model (Cretaceous vs Eocene for ECHAM5 and CCSM3) it is not the case, indicating the major role of boundary conditions in defining the latitudinal temperature gradient. For instance, the South Hemisphere gradient obtained for the Eocene with the ECHAM5 model is always lower than those obtained for the Cretaceous with the same model, regardless of the pCO2 value (See Fig. 11 and Supplementary Data).”

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

→ Modified title: 4.2 PRIMARY CLIMATE CONTROLS

Page 16, lines 446-448: 2000 years are not very long for equilibrium climate sensitivity simulations, and the 4xCO2 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.

→ We have clarified this point: - 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)”. - Page 16 line 448: “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 16, line 469: Please rephrase, "reduced gradient was amplified"?

→ Modified sentence : It has been suggested that high latitude warming was ampli-

fied in deep time simulations by rising CO2 via cloud and vegetation feedbacks (Otto-bliesner and Upchurch, 1997; Deconto et al., 2000) or by increasing ocean heat transport (Barron et al., 1995; Schmidt and Mysak, 1996; Brady et al., 1998), in particular when changing the paleogeography (Hotinski and Toggweiler, 2003), thus contributing to the flattening of the meridional SST gradient”.

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).

→ Ok

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

→ Modifications: “The presence of such a type of vegetation could change the albedo of continental regions, but also heat and water-vapor transfer by modified evapo-transpiration processes, thus leading to a warming amplification at high-altitudes and thus a reduced temperature gradient (Otto-bliesner and Upchurch, 1997; Hay et al., 2019). Based on these studies and on our results, we cannot exclude that this kind of high latitude vegetation can give more weight to the role of paleovegetation in reducing the temperature gradient”.

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.

→ Deleted.

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

→ Yes, they are, we added the precisions to the manuscript.

Figure 1: Labels are too small. Figure 2: Ticks and labels of oceanic area anomaly plot do not line up.

→ Modified. Figure 4: 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.

→ Labels and contour intervals modified. We also added in the figure legend that the white color (which is not in the colorbar) corresponds to areas where the anomaly is not significative regarding the student test.

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.

→ We added in the figure legend and in the text, the level pressure for low-altitude cloudiness (>680 hPa) and high-altitude cloudiness (<440 hPa). The anomaly was not the absolute value but the change of cloudiness in % of the 1X-NOICE value. We changed the figure to be clearer and stay coherent with other anomaly figures, and plotted the absolute change.

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

→ Modified.

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 (_CO2, ...).

→ This could be an alternative, yes. However, we wanted to keep the shifted curve, as it is very visual to represent what is the 'gradient flattening", what would not be the

case with anomalies.

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

→ We forgot to remove this from the figure legend. We finally didn't plot the regression line, not to overload the figure. The regression line is visible in the supplementary data file.

Figure 11: Labels are too small.

→ Modified.

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)

→ Corrections done in the revised manuscript

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Geopotential height (m) at 850 hPa for piControl and 1X-NOICE simulations and corresponding anomaly.

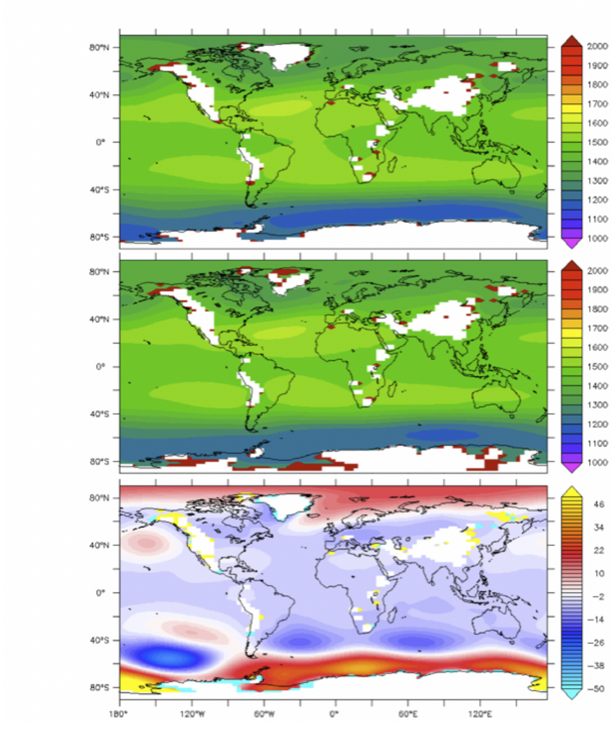

Fig. 1.