

Response to Reviewer 1

We are very grateful to the reviewer for the time he/she has taken to review our manuscript. The comments and suggestions have enabled us to further improve the clarity of the manuscript in such a way as to make the work more accessible to our readers.

Pg. 2 last paragraph and the subsequent climate sensitivity assessment — that aerosols and their indirect effects are assumed to have been similar to modern while in fact the proxy data currently provides little constraint on this. Some mention should be made of this caveat.

We thank the reviewer for this comment and have added an appropriate remark regarding aerosols in the revised version of the manuscript.

Pg. 3 third paragraph & Pg 4 second paragraph & Fig. 9 — While the mid-Pliocene simulation “reasonably accurately capture features of the proxy-inferred enhanced warming in the high-latitudes during the mid-Pliocene.” it does not appear to be capturing the structure and amplitude tropical to subtropical warming as is the case for most models and also the zonal SST gradient along the equator e.g. see Fig. 1 in *Brierley et al.* [2015]. What is amazing in Fig. 9 is just how uniform the zonal mean warming is equatorward of 45N&S and we know from the data this is not the case e.g. *Dowsett et al.* [2013] Figs 2 & 3 Fig OR Fig. 3b in *Haywood et al.* [2016]. Some mention should be made of this shortcoming and the fact that the result are perhaps more relevant to high latitude Pliocene climate than tropical and subtropical Pliocene climate.

We thank the reviewer for this very pertinent comment. In our previous paper *Chandan and Peltier* [2017], when discussing data-model comparison we should have specifically noted the shortcoming that our simulated results have in common with results from the initial phase of the PlioMIP program in that the model is unable to simulate the proxy-inferred negative SST anomalies at tropical sites characterized by strong equatorial divergence, and at Mediterranean and Atlantic coastal sites. This remaining issue was regrettably overlooked. Although our current paper does not deal with data-model comparison, we have inserted a brief comment in the introduction of the revised manuscript regarding this shortcoming of our results.

Pg. 18 lines 29–30 — This weak cloud forcing outside of the high latitudes is the reason why there is no weakening of the meridional SST gradient between the mid-latitudes and the deep tropics and the zonal SST gradient along the equator, e.g. see *Fedorov et al.* [2015], *Burls and Fedorov* [2014]

We have added a comment in the revised manuscript regarding the findings of *Fedorov et al.* [2015], *Burls and Fedorov* [2014] which show that the meridional cloud albedo exerts a direct control on the zonal SST gradient, and which could be applicable to the mid-Pliocene, with the

caveat that the question of the existence of a reduced zonal SST gradient along the equator during the mid-Pliocene is currently debatable. This importance of cloud albedo has also been discussed in some detail in the analyses of *Yang, Peltier and Hu* [2016] in the context of an effort to determine the dependence of the zonal tropic SST gradient upon the atmospheric CO₂ concentration.

Pg. 19 lines 5–7 — This result is consistent with *Feng et al.* [2017].

We thank the reviewer for bringing this reference to our attention and have included it in the revised version of the manuscript.

Pg. 19 lines 30–32 — Another noteworthy reference that highlights the potential importance of mixed-phase clouds is *Sagoo and Storelvmo* [2017]

We also thank the reviewer for bringing this reference to our attention, and have also included reference to it in the revised version of the manuscript.

Pg. 7 end of paragraph two — Does this mean that the Eo experiments represents not only the effects associated with changing to Pliocene orography but also bathymetry? In which case changes in ocean-gateways? This needs to be clarified.

On a related note some discussion is needed of the recent paper by *Otto-Bliesner et al.* [2017] pointing to changes in Pliocene gateways as a mechanism supporting Arctic warmth during the Pliocene. Could this help explain some of the hemispheric asymmetry discussed in paragraph 1 on page 17?

1. Yes, the Eo experiments include all non-ice sheet “physical” changes to the planet’s surface. We do mention in the second paragraph on page 7 of the original version of the manuscript that there are also changes to bathymetry, vegetation and river routing in this configuration. What we have done in the revised manuscript is to add a further comment concerning this in section 2.2.2 where the notation used to refer to our experiments is first introduced.
2. *Otto-Bliesner et al.* [2017] do not discuss the impact of Pliocene gateways on Arctic warmth; rather they discuss the impact on warming in the North Atlantic region. Although there would be other impacts in the high latitudes of the NH from the closure of the gateways, those impacts are not explicitly presented in their paper. We have discussed their findings in our previous paper *Chandan and Peltier* [2017] when explicitly performing data-model comparisons, specifically, we’ve noted that the significantly improved data-model comparison that we have obtained for the high latitudes could in fact be partly due to the closure of the Arctic oceanic gateways. In this manuscript however, we have not included this reference because there is no discussion in this paper to which their findings are directly applicable.

With regards to whether the asymmetry could be understood through the results of *Otto-Bliesner et al.* [2017], it is important to be reminded that the asymmetry being discussed

is in the surface air temperature residual $\Delta T - (dT_{CO_2} + dT_{orog} + dT_{ice})$. Since this is a residual it is therefore difficult to say which boundary condition(s), or which higher-order interaction between boundary conditions is responsible for the asymmetry (specifically, for the excess positive temperatures in the North Atlantic). It is more natural to expect that the origin for this could be in the asymmetry in the physical meridional transport of heat and in the deep water formation process (which is what we have done in the original manuscript).

Pg. 26 lines 17–18 — Any ideas of the mechanisms behind why the situation is the opposite with SH sea ice displaying a greater response under higher CO₂? This is an interesting result.

We also think that this is interesting. At this point, however, and in the absence of the further analyses that will be required to provide a definitive explanation, we prefer not to speculate.

The second last sentence on page 30 is unclear.

The second last sentence on page 30 currently reads “The higher pCO_2 is found to contribute 33% to the warming, followed by ice sheet contribution at 13%”. Since the paragraph in which this sentence appears is summarizing the warming induced by planetary albedo change, and the contributions of the boundary condition changes to this warming, we have revised this sentence slightly to make it clearer as: “The contributions from higher pCO_2 and ice sheet changes to the planetary albedo change induced warming are found to be 33% and 13% respectively.”

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