Reply to comments by anonymous referee #1

We would like to thank anonymous referee #1 for his/her time and effort in reviewing our manuscript (cp-2020-05). The comments raised in the review are highly appreciated and have helped us to clarify our statements and to further improve our manuscript. In the following, we respond to comments raised in the review.

MAJOR COMMENT(S)

* I have one major comment on your sensitivity tests to changing CO_2 from 405 to 400 ppm, and the large difference in the North Atlantic SST due to this change : as you state in the discussion, this is probably not a 'real' signal. It is, as you state, either due to longwave oceanic variability, or to the fact that one of your simulations is not in equilibrium regarding NADW formation. Could you please check your NADW formation or mixed layer depth for the two simulations concerned (Eoi400 and Eoi405), across the whole integration period ? How long was your integration time for both these simulations?

We thank the reviewer for pointing this out, both simulations were integrated for 1500 years as shown in Figure S1.

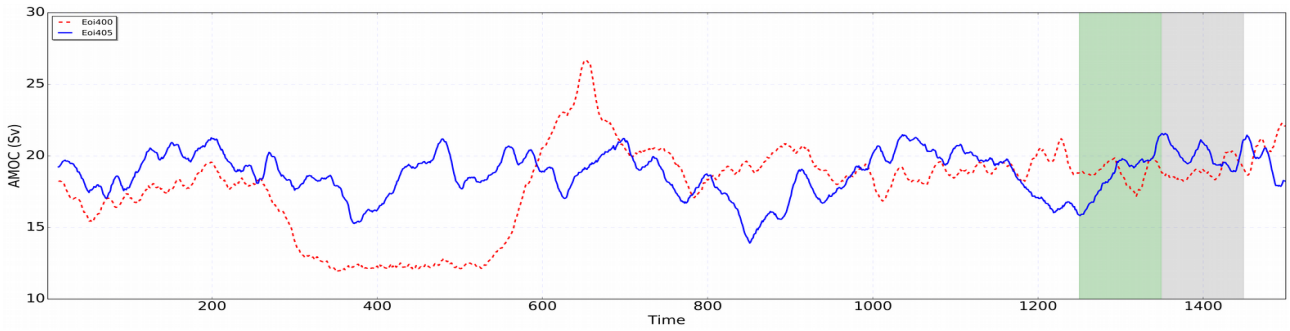

Figure S1: Time-series of AMOC index across the whole integration period of 1500 years for simulations Eoi400 (red) and Eoi405 (blue). The AMOC index is defined as the maximum in the stream function below 500 m and polewards from 20°N in the North Atlantic, smoothened using 12-year moving average to reduce inter-annual variability. The green shading shows 100-year period used in calculating SST anomalies shown in the manuscript, while the grey shading shows the 100-year period added to the analysis of both simulations to get rid of the cold pool in the North Atlantic. The results and discussion section will be updated in the revised manuscript.

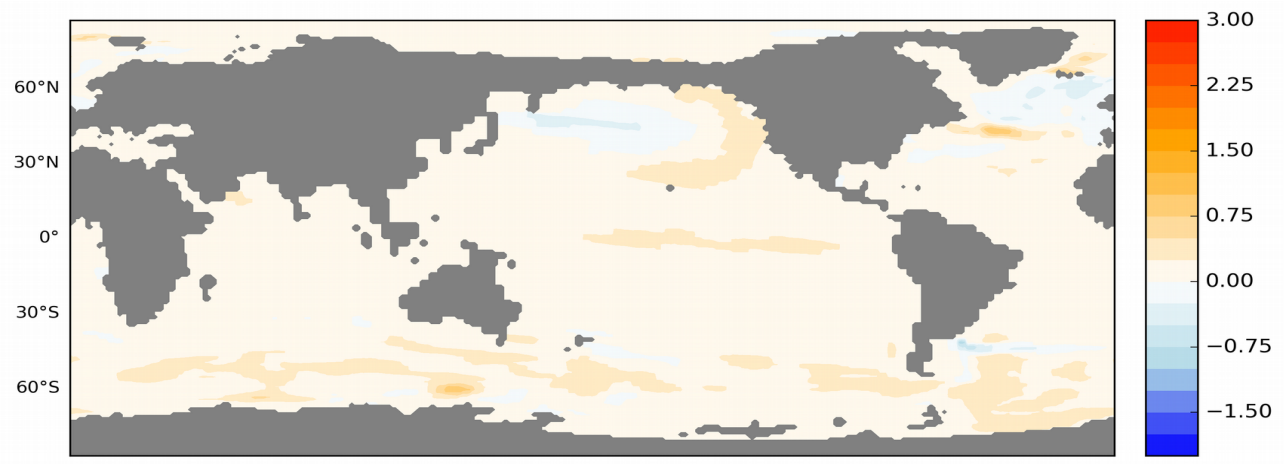

Figure S2 (Figure 9a in manuscript): Annual mean SST (◦C) anomalies between mid-Pliocene simulations Eoi405 and Eoi400, quantifying anomalies due to changes in mid-Pliocene CO_2 from 405 to 400 ppmv, as utilized for PlioMIP1 and PlioMIP2 respectively.

** To make the manuscript easier to follow, in my opinion, the authors should refer more frequently to the name of each simulation they're describing rather than describing which simulation they are talking about (i.e. writing 'PlioM1' rather than 'the PlioMIP1 simulation' That would make the manuscript more concise and easier to follow (at least for me). Also, in the Discussion section, please refer to the figures.

We agree with the reviewer that when referring to simulations in some parts of the manuscript, they are described again. We have fixed this by calling the simulations by their ID as shown in Table 1 of the manuscript.

*** I also think that for the sake of clarity and answering more properly to the scientific question raised, the Results section could have been organized by forcing, rather than by climatic variable (i.e impact of changing CO2, impact of MIS K1 orbit, etc. rather than 'SST', 'SAT' etc.).

We have revised the result section and separated it into different subsections according to the contribution of different forcings.

- Impact of Changing CO_2
- Effect of MIS K1 orbit on PlioMIP2 simulations
- Contributions of PlioMIP's Palaeogeography

**** Curiously you do not show a single precipitation map. Did you look at them and see that only very minor changes appeared? Please explain the reasons for this choice, as precipitation is an important component of climate, especially at low latitudes.

The main idea going into this study was to infer the major driver of the mid-Pliocene warmth, hence the sensitivity studies by changing different boundary conditions and analyzing SAT and SST. According to your suggestion, We have added some precipitation analyses showing the difference between PlioMIP1 and PlioMIP2 core simulations in this supplement (Figure S3). Does the editor suggest we add any of this to the revised manuscript?

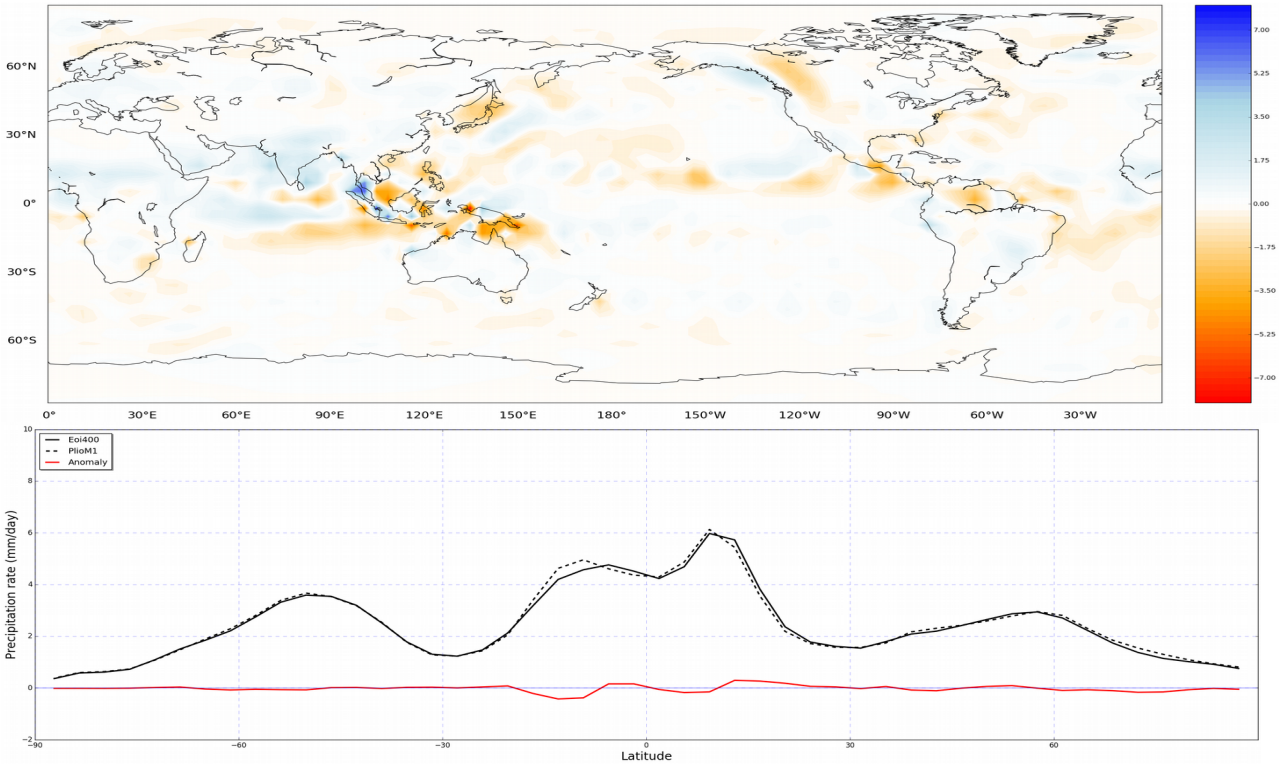

Figure S3: (a) Annual mean anomalies of precipitation (mm/day) between Eoi400 and PlioM1, while (b) shows zonal averages, where the solid black line shows Eoi400 while the dashed line denotes PlioM1. Furthermore, the red line represents of the anomaly between both simulations.

SPECIFIC COMMENTS

Abstract

1. Do not detail the minor changes in boundary conditions here. (page 1, line 5 to 10)

We have removed the details of minor changes in boundary conditions from the abstract.

2. It looks like the abstract was written before the paper was really finished. Some of the conclusions of the abstract are in contradiction with the conclusions of the paper, for example, page 2 : "The difference in prescribed CO_2 accounts for 1.1K of warming in the Arctic, leading to an ice-free summer in the PlioMIP1 simulation and a quasi ice-free summer in PlioMIP2" → where do you get that information ? from figure 4a and figure 9a there are only «0.5◦ C changes in the Arctic. The big signal is in the North Atlantic, but is probably not robust. Second, you conclude in your conclusions that CO_2 change is likely not the cause of the changes between PlioMIP2 and PlioMIP1, the factor of change being mostly paleogeography changes.

We agree with the reviewer, that the initial draft of the manuscript compared the core PlioMIP1 and PlioMIP2 simulations to account for CO_2 difference but we later realized that other boundary conditions could also contribute in this respect, hence the implementation of Eoi405. We have made the necessary correction and the abstract has been re-written based on the comments.

3. Page 2, line 25 Consistency : use mid-Pliocene not Mid-Pliocene and Plio-Pleistocene instead of Pleistocene-Pliocene

Mid-Pliocene changed to mid-Pliocene and Pleistocene-Pliocene deleted based on RC2.

Experimental design

1. Please provide integration length for all simulations.

We have added the integration length for all simulations to the experimental design section of the manuscript.

Results

1. As I said in the General comments I think this Section would be clearer if it was organized in terms of forcing rather than in terms of variable. Also, please provide temperatures in degrees Celsius rather than in K, because few people speak in K and your figures are in degrees C. Please refer to the figures whenever necessary, it's not always the case in particular in the SAT section.

- The results section has been re-organized into different subsections according to the contribution of different forcings.
- Temperature unit K has been changed to degree Celsius to ensure consistency.
- We have referred to figures whenever necessary.

2. Page 1, lines 10 to 20: comparing the SST dataset of Dowsett et al., 2013 to PlioMIP2 results is irrelevant. The Dowsett 2013 dataset includes data spread over a large amount of time, and peak-averaging. You have to compare to the new dataset by Foley and Dowsett 2020. Also, you here

speak of RMSD between this dataset and several simulations but you should provide a table for the reader to refer to.

We thank the reviewer for pointing this out. The SST dataset of Foley and Dowsett (2019) was not available during the preparation of this manuscript. However, we have now compared our results with the reconstructions of Foley and Dowsett (2019) and the result is shown in the table below. Further comparison with the reconstructions of McClymont et al., (2020) will be added in the revised manuscript.

Table S1. Root mean square deviation between Atlantic sea surface temperatures of selected simulations and the alkenone U^{k}_{37} based reconstructions by Foley and Dowsett referring to time windows 10ka and 30ka.

Exp. ID	U^{k}_{37} (10ka) **Foley and Dowsett (2019)**	U^{k}_{37} (30ka) **Foley and Dowsett (2019)**
Eoi400	3.90	3.72
PlioM1	4.30	4.25
Eoi400_K1	4.11	4.05

Discussion

1. Please also refer to the figures whenever necessary in this section.

We have revised the discussion section and have referred to figures whenever necessary.

2. Page 13, line 1 “effects of changes in boundary and initial conditions”. I did not see that you had changed initial conditions, and if you have an effect from a change in initial conditions that means your simulation has not reached equilibrium, doesn’t it?

All our simulations are well equilibrated and there is clearly a misunderstanding due to unprecise formulation on our side. We have accordingly rephrased the sentence from “effects of changes in boundary and initial conditions” to “effects of changes in boundary conditions”.

3. Please revise the discussion regarding the effect of 5 ppm CO_2 change on the SST after you have found a way to remove the artifact of the cold signal in the North Atlantic, either by averaging on longer time period, or by running the model to full equilibrium regarding the NADW formation.

We thank the reviewer for suggesting possible ways to remove the supposed cold signal in the North Atlantic. We have averaged the simulation over a longer time period and the discussion regarding the effect of 5 ppm CO_2 has been changed.

4. Page 16 discussion on sea-ice : “may tell very different stories about the evolution of sea-ice”. Certainly, different models lead to different sea-ice simulations. However, what I conclude from your results is, a small change in forcing leads to small changes in sea-ice, but the big story in the same in all you PlioMIP simulations. With COSMOS, in Pliocene conditions, you have strongly reduced sea ice with almost sea-ice free summers in PlioM1, Eoi400, Eoi405, Eoi400_ORB and Eoi405_ORB, and a remarquably similar winter sea-ice extent for all these simulations and

Eoi400_K1. Slightly more Arctic sea ice in summer with Eoi400_K1. To me, all these simulations, except maybe Eoi400_K1 which has slightly more ice, tell the same story of sea-ice. But these changes are anyway much smaller than the precision that sea-ice proxies can provide.
We agree with the reviewer that the changes are small, and this part of the discussion section on sea ice has been removed.

5. By the way what is sea-ice compactness? Did you mean sea-ice thickness? I have never seen sea-ice compactness before.

The analyzed model variable is sea ice compactness. It is the fraction of sea ice covered to sea ice free ocean surface at any grid cell. We have changed "sea ice compactness" to the more widely used term "sea ice concentration".

Conclusion
1. Page 17 Please update the conclusions regarding the effect of 5 ppm CO_2 change in the North Atlantic, according to the comments above.

Updated.

I hope that my comments are helpful to the authors. Sincerely

Yes, they have been extremely helpful in improving our manuscript. Thanks for your time and best regards.

References

Foley, K. M., and Dowsett, H.J.: Community sourced mid-Piacenzian sea surface temperature (SST) data: U.S. Geological Survey data release, https://doi.org/10.5066/P9YP3DTV, 2019.

McClymont, E. L., Ford, H. L., Ho, S. L., Tindall, J. C., Haywood, A. M., Alonso-Garcia, M., Bailey, I., Berke, M. A., Littler, K., Patterson, M., Petrick, B., Peterse, F., Ravelo, A. C., Risebrobakken, B., De Schepper, S., Swann, G. E. A., Thirumalai, K., Tierney, J. E., van der Weijst, C., and White, S.: Lessons from a high CO2 world: an ocean view from ~ 3 million years ago, Clim. Past Discuss., https://doi.org/10.5194/cp-2019-161, in review, 2020.