

## ***Interactive comment on “Sensitivity of mid-Pliocene climate to changes in orbital forcing, and PlioMIP’s boundary conditions” by Eric Samakinwa et al.***

**Anonymous Referee #1**

Received and published: 3 March 2020

General comment : This manuscript presents the results of simulations using the COSMOS model and different boundary conditions in the frame of PlioMIP. PlioMIP1 and PlioMIP2 simulations are compared, minor differences in the boundary conditions are also investigated (small changes in orbital configuration for the preindustrial, 5 ppm difference between the CO<sub>2</sub> prescription from PlioMIP1 to PlioMIP2), and finally a simulation using the orbital configuration of MIS K1 is carried out. The goal is to understand 1/how some minor changes in the boundary conditions, both for the PI and the Pliocene simulations, and 2/ a change of orbit, can impact the results of PlioMIP2.

The scientific question is understandable and the manuscript is relatively easy to fol-

C1

low. The figures are clear. The manuscript is generally well written. Overall it is an interesting contribution to the Special Issue of PlioMIP2 as the scientific question is relevant for this topic. I recommend that it is published after some modifications have been made, notably on one major comment.

I have one major comment on your sensitivity tests to changing CO<sub>2</sub> 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 ?

In any case, because you state yourself that this cold SST anomaly is certainly an artefact, you have to get rid of this artefact otherwise you cannot discuss the results of this sensitivity test. You can try to overcome this artefact by integrating each simulation on 200 years, or by continuing your simulations if they were in fact not in equilibrium regarding NADW.

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.

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 CO<sub>2</sub>, impact of MIS K1 orbit, etc. rather than ‘SST’, ‘SAT’ etc.).

Curiously you do not show a single precipitation map. Did you look at them and see

C2

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.

#### Specific comments

##### Abstract

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

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<sub>2</sub> 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<sub>2</sub> change is likely not the cause of the changes between PlioMIP2 and PlioMIP1, the factor of change being mostly paleogeography changes.

So please rewrite the abstract carefully.

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

Page 3, line 18 “all model data...”. This sentence is confusing to me, and could lead a non-specialist reader to think that you feed the model with the same data that you compare it to. Please be more specific like “Boundary conditions of the Pliocene simulations are directly based on PRISM”

Page 5 line 10 “we create model setups where the prescribed PlioMIP2 model setup...” → we carry out an additional simulation using PlioMIP2 boundary conditions except for orbital configuration which is representative of MIS K1.

#### C3

##### Experimental design

Please provide integration length for all simulations.

##### Results

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.

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.

##### Discussion

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

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 ?

Please revise the discussion regarding the effect of 5 ppm CO<sub>2</sub> change on the SST after you have found a way to remove the artefact 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.

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

#### C4

in sea-ice, but the big story is the same in all your 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 remarkably 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.

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

Conclusions Page 17 Please update the conclusions regarding the effect of 5 ppm CO<sub>2</sub> change in the North Atlantic, according to the comments above.

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

---

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2020-5>, 2020.