

## **Anonymous Referee #1**

We thank the reviewer for their helpful comments and considerable time spent working on this manuscript. The contribution of reviewer has added substantially to the quality of the manuscript, this is greatly appreciated particularly by the lead author. The level of detail within the review was sincerely appreciated as was the significant amount of time that the reviewer spent on commenting. With regards to the comments, these are addressed in sequence by SJH with input from the co-authors. I (SJH) will start with the general comments and then finish with the line-by-line comments.

### **1. Remarks regarding the model and simulation description**

**Could the authors provide in their text on page 3 details regarding the employed model version of the oasis coupler and include the respective reference?**

I have removed the term “using the oasis coupler” as this was incorrectly stated for this version of the model. I am working on another project using the coupler so a bit of laziness crept in.

**Furthermore shortly after that: I think the description of coupling “every model day” is ambiguous. Do the authors mean that the coupling occurs exactly once per model day? Please clarify the text accordingly** I have clarified this sentence by changing it to “The model has a time-step of 30 minutes and is coupled to the ocean model (Section 2.2) at the end of every model day.”

**On page 3, lines 29/30, the authors state that MOSES2 introduced “improved representation of surface and land processes”. Could the authors please elaborate this statement in their text to make clear what kind (and to which degree of detail) respective processes are represented in their model? Giving some respective references would be appreciated.**

I expanded the text so that it now reads:

“The land surface scheme is MOSES 2.1 (Met Office Surface Exchange Scheme; Cox et al. (1999); Essery et al. (2003)) which principally deals with the hydrology of the canopy to the subsurface and the surface energy balance (including subsurface thermodynamics). Within the scheme there are 5 plant functional types (PFTs: broadleaf and needleleaf trees, C3 and C4 grasses, and shrub) as well as soil (desert), lakes and ice. Each non-glaciated terrestrial grid cell can take fractional values of each surface type.

The HadCM3 PliomIP1 study of Bragg et al. (2012) used an earlier version of MOSES (MOSES1) which treats each model grid cell as a homogeneous surface and uses effective parameters to calculate the grid cell’s energy and moisture flux. However, MOSES2 introduced subgrid (tiled) heterogeneity and improved representation of surface and plant processes such that hydrological partitioning and energy balance is computed for each subgrid tile. A comparison of MOSES1 and MOSES2.1 can be found within Valdes et al. (2017). In this study we incorporate a software update taken from the HadGEM2 climate model (Good et al., 2013) which corrects the temperature control of plant respiration and improves forest resilience to elevated temperatures (making the model MOSES2.1a in the nomenclature of Valdes et al. (2017).“

**The meaning of the statement “upper layer of ocean” on page 3, line 32 is not clear. Do the authors state that the runoff is somehow vertically distributed over the layers of the upper ocean, or is it given exclusively to the uppermost ocean layer? Please clarify the text accordingly.**

The phrase “the coastal outflow point in the upper layer of ocean” has been changed to “the coastal outflow point in the uppermost layer of ocean”

**On page 4 the authors describe that the ocean model employs z-type cells with bottom topography represented by “full” cells. Does this mean that bathymetry is adjusted so that at the border between ocean and sediment the lowermost “wet” ocean grid cell has always the standard**

**thickness defined in the model, rather than a thickness adjusted to represent bathymetry as closely as possible – an approach, that is employed in the case of the “partial grid cell” scheme applied by some other models? If indeed the layer thickness is not adjusted to bathymetry, I would imagine that, in addition to the various approximations involved in the generation of the Pliocene bathymetry, there is another substantial approximation in that for deeper regions of the ocean, where the layer thickness is assumedly relatively large, the modified Pliocene bathymetry is significantly changed to fit it to the layer thickness. Could the authors please explain this a bit more detailed in the text?**

You are correct in that the bottommost ocean grid cell has a standard thickness (1 of 20 standard thicknesses). When the Pliocene bathymetry anomaly is applied to the modern bathymetry, there will be circumstances when we see changes in the bathymetry (due to the discrete thickness of the ocean grid cells). A similar discretisation occurred when the pre-industrial bathymetry was originally generated for the model using the ETOPO5 data. Therefore, the representation of the Pliocene (and pre-industrial) bathymetry has lower fidelity at greater depths (where the layer thickness is greatest). I have added an additional sentence to the description of the ocean model

“The model uses z co-ordinate vertical layers with bottom topography represented by “full” cells. This leads to a discontinuous representation of the bathymetry which has poorer fidelity at greater depths (where the thickness of levels is greatest).”

**Could the authors please add a remark whether the ocean grid is aligned in such way that one atmosphere grid cell covers exactly 6 ocean grid cells (the term “exactly”) is not clear to me.**

I have clarified the sentence so that it now reads “Horizontal spatial resolution is  $1.25^\circ \times 1.25^\circ$  (288 x 144 cell geographic grid) and the grid is aligned so that there are six ocean grid cells to each atmosphere grid cell ( $3.75 \times 2.5^\circ$ ).

**Furthermore, does the statement “The land-sea mask is effectively  $3.75 \times 2.5^\circ$  resolution in the top 200 m, but beneath increases to  $1.25^\circ$  resolution.” imply that there is some kind of horizontal interpolation of vertical fluxes occurring at critical depths? If so, what is the nature of this interpolation?**

There is no horizontal interpolation of vertical fluxes as I was referring to the bathymetry. I meant that the land sea boundary (isodepth) at the ocean levels  $<200\text{m}$  was  $3.75 \times 2.5^\circ$  resolution and at  $>200\text{m}$  it is  $1.25 \times 1.25^\circ$  resolution. It is confusing to describe in text and superfluous to the description, so I have clarified the sentence and it now reads

“To simplify coupling with the atmosphere model, the ocean model's coastline has a resolution of  $3.75^\circ \times 2.5^\circ$ ”

**The authors describe that they employ a prescribed time-invariant freshwater iceberg field that is omitted for Pliocene simulations. What is not clear to me is whether such omission is also done for simulations E400 and E560, where the climate state is as well much different from the one simulated in E280, for which the modern iceberg conditions are probably optimized or derived. This could be explained, and the respective impact on the interpretation of results could be discussed later on.**

This is a good question. I did apply the time-invariant freshwater flux rate (fixed in intensity and geographic distribution) to the E<sup>400</sup> and E<sup>560</sup> experiments as it is commonly done when using models of this era within historical and future experiments (e.g. within CMIP3). As you correctly identify, this is not ideal. With increasing CO<sub>2</sub>, ocean currents, winds, and ocean and boundary-layer temperatures will change, which will modify the iceberg melt trajectories away from the pre-industrial (altering the geographical distribution of the fresh-water correction). In addition, precipitation patterns (wrt. terrestrial ice and inland drainage basins) will alter, which will subsequently change the extent to which the hydrological cycle requires closure. These components (ice-berg trajectories and precipitation patterns) will act to modify the geographic distribution and magnitude of the required

freshwater correction. I have clarified the text describing the time-invariant freshwater iceberg field as follows (note the emphasis is mine):

“The fresh water budget of the ocean is balanced by fluxes from the river routing scheme and a freshwater correction applied to the uppermost ocean level. Within the pre-industrial (and associated CO<sub>2</sub> sensitivity experiments) the freshwater correction field is prescribed (time-invariant). The correction field had been derived to provided closure of the model’s modern hydrological cycle and consists of a uniform background component correcting internal-drainage (Section 2.1) and an iceberg component whose geographic distribution is derived from modern observations (Gordon et al., 2000; Pardaens et al., 2003). Within the Pliocene experiments we omit the time-invariant correction (including the iceberg component) and instead use an annual model-derived geographically-invariant freshwater correction to reduce residual salinity drifts to zero. We justify this as we currently do not have a priori knowledge of the geographic distribution of iceberg melt consistent with the ice sheet distribution within the PlioMIP2 enhanced boundary conditions. In the Northern Hemisphere we do not expect significant iceberg calving given the configuration of the Greenland Ice Sheet and the lack of marine terminating margins specified within the PRISM4 boundary conditions. “

**Related to this topic, regarding the artificial closing of the water budget for Pliocene simulations: Is the artificial budget term somehow regionally distributed, potentially weighted with regard to (a modern) salinity distribution of the ocean? Or is it rather a globally distributed residual term? This should be explained in more detail as I expect that depending on how this correction is applied a significant impact on buoyancy-driven ocean circulation cannot be excluded. Furthermore, it may be interesting to state the amplitude of the freshwater flux that is applied in order to close the water budget.**

In the Pliocene the correction is applied as a globally distributed residual term. I have expanded the description from

“...use an annual model-derived freshwater correction to reduce residual salinity drifts to zero.”

To

“... use an annual model-derived **geographically-invariant** freshwater correction to reduce residual salinity drifts to zero.”

Although I recognise the that this correction will have an impact on buoyancy-driven flow, I think that it is beyond the scope of this manuscript to explore it further. The fresh-water correction is a historic-feature of the model and its impact on the ocean flow is a given

**At the bottom of page 4 the authors describe that “Ice drifts only by the action [of] surface ocean current.” Does this imply that wind stress has no direct influence on sea ice transport? Please clarify in the text.**

Within the model, wind stress acts indirectly on sea-ice drift via its action of the surface ocean current. I have clarified the sentence such that it now reads “Ice drifts only by the action of surface ocean current, hence within the model, surface wind stress indirectly influences sea ice drift via its influence on the surface ocean current.”

**On page 5, line 9, the authors write that “pre-industrial experiments are run at 280, 400, and 560 ppm”. I think this statement may be a bit misleading, as pre-industrial is characterized by CO<sub>2</sub> of around 280 ppm. Would it make sense to rephrase this pointing out that “simulations based on a pre-industrial geography” are run with differing levels of CO<sub>2</sub>?**

That is a very good point (and the confusion was also picked up by the second reviewer). As you have suggested I have clarified the terminology used to describe these CO<sub>2</sub> sensitivity experiments with “... Within the pre-industrial (and associated CO<sub>2</sub> sensitivity experiments) ...” etc within the manuscript.

**On page 6 the authors write that corrections were applied “using a model resolution river routing model”. Could details of this procedure be included into the text? Does this, for example, imply eliminating internal drainage basins?**

I have added the following text to the manuscript

Within Section 3.2.1 in which you refer to I have expanded the text so that it now reads “River basins and outflow points were derived from the pre-industrial routing scheme (Section 3.1) but corrected in regions of LSM, topographical and ice-bedrock change using a model-resolution river routing model based on the D8 method (Tribe, 1992) This was then followed by manual correction in regions when model resolution fails to capture important orography or where the regridded Pliocene orography is flat.”

And within Section 2.1 (Atmosphere model) I have expanded the description so that it now reads “Internally-draining basins are present but the associated water loss is not explicitly modelled within the routing scheme. Instead, the loss of freshwater in the hydrological cycle is corrected using an artificial freshwater correction field applied to the uppermost surface of the ocean (Section 2.2). This freshwater closure also acts to correct the freshwater loss due to terrestrial snowfall accumulation.

**The authors write on page 6 that a BIOME4-to-MOSES2 lookup table has been employed. I think it is important for the less experienced reader to point out that the PRISM4 boundary condition is based on BIOME4, if I am not mistaken.**

You are correct. I have clarified the sentence by expanding it to “The PRISM4 vegetation scheme (represented by BIOME4 biomes) was regridded by combining a BIOME4-to-MOSES2 lookup table with a bespoke LSM-guided regridding relying on an area-weighted survey of underlying biomes.”

**Could the authors give a reference that explain details of the xancil and um2nc tools mentioned on page 6?**

Very good point. I have added a reference to the website that provides access to these tools so that the sentence now reads “All boundary conditions were generated within a bespoke Matlab framework using the MOHC-developed and National Centre for Atmospheric Sciences, Computing Modelling Services (NCAS-CMS) supported xancil and um2nc tools (NCAS 2019).”

I have also subsequently moved this block of text to the acknowledgements.

**On page 7 it is described that the CO<sub>2</sub> is adjusted via a 1% CO<sub>2</sub> ramp like in the respective CMIP6 simulation. Is there a specific reason for this methodology of creating a Pliocene models setup? Please explain.**

The described spin-up methodology was implemented as it is consistent with other modelling we have done in the group. For the *modest* CO<sub>2</sub> values used within this manuscript (cf. deep time) we could have used instantaneous changes in CO<sub>2</sub>, the impact being on how the model then approaches a state of equilibrium towards the year 2500. Given that the CORE simulations reach a satisfactory state of equilibrium the implementation method for CO<sub>2</sub> change is somewhat arbitrary. Our high CO<sub>2</sub> experiments did have higher TOA radiative imbalances and may have benefitted slightly from an instantaneous CO<sub>2</sub> change (as they could have benefitted from a longer integration time). Nevertheless, all our experiments had TOA imbalance which compare favourably to previous Pliocene experiments (e.g. compare our Table 2 to the PlioMIP1 TOA summary provided by Haywood et al. (2013)<sup>1</sup> Table 2)

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<sup>1</sup> Haywood, A. M., Hill, D. J., Dolan, A. M., Otto-Bliesner, B. L., Bragg, F., Chan, W.-L., Chandler, M. A., Contoux, C., Dowsett, H. J., Jost, A., Kamae, Y., Lohmann, G., Lunt, D. J., Abe-Ouchi, A., Pickering, S. J., Ramstein, G., Rosenbloom, N. A., Salzmann, U., Sohl, L., Stepanek, C.,

**On page 7 the authors explain that the final 50 model years are used for computing climatological averages. Considering the potential presence of slow variability in the model simulations, could the authors state whether results would look different if instead 100 model years are employed? What is the official time period over which PlioMIP2 climatologies shall be aggregated?**

I am not sure why we didn't include this in the PRISM4 protocol paper (Haywood et al., 2016<sup>2</sup>), as we did specify a minimum integration length. The MRI-CGCM2.3 study of Kamae et al. (2016)<sup>3</sup> used a 50 year averaging period and the CCSM4 study of Chandan and Peltier (2017)<sup>4</sup> used 30 years. Internally we have looked at the difference between using 50 years and 100 years as averaging periods and it didn't make a big difference.

I have expanded the text within Section 4 to include

“We derive climatological averages from the final 50 years (model years 2450 through to 2499) and climatic oscillations from the final 100 years. The final 50 years of output is used for climatological averaging to remain consistent with the HadCM3 PlioMIP submission (Exp. 2 of Bragg et al. (2012)). The PlioMIP2 protocol (Haywood et al., 2016) does not state a standardised time length for climatological means although the PlioMIP2 website (USGS, 2018) does request 100 years of monthly climatology. We therefore make the 50 year climatological average and 100 years of monthly climatology available on the PlioMIP2 data repository.”

## **2. Remarks regarding derived results and interpretations made by the authors**

**Regarding Fig. 2: It is not clear to me which simulation is represented by the data – or is it an average over various simulations? Please add this information to the text.**

I have clarified Figure 2 caption so that it now reads “Time-evolution of the globally-integrated temperature for the ocean layers within Eoi<sup>400</sup> experiment.”

**On page 9, lines 4 and five one could add to the results of Climate Sensitivity (CS) the statement that due to the overlap of variability ranges there is no significant difference between the model CS for the different climate states. Furthermore, based on rough calculations of presented numbers: Should the result 2.9°C for Pliocene CS should rather read 2.8°C?**

This has been corrected.

**Significant digits: I think the ESS/CS ratio should be 1.9°C rather than 1.90°C to honour the limited precision of the value used to compute that ratio. I agree.**

This has been corrected

**On page 9 the authors write that they “neglect” changes in topography and land sea mask. Would the meaning of the sentence get clearer if it was changed to: “... hence assuming consistency of ice sheet topography and land sea mask with the (simulated or in the boundary condition assumed) climate state”?**

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Ueda, H., Yan, Q., and Zhang, Z.: Large-scale features of Pliocene climate: results from the Pliocene Model Intercomparison Project, *Clim. Past*, 9, 191-209, <https://doi.org/10.5194/cp-9-191-2013>, 2013.

<sup>2</sup> Haywood, A. M., Dowsett, H. J., Dolan, A. M., Rowley, D., Abe-Ouchi, A., Otto-Bliesner, B., Chandler, M. A., Hunter, S. J., Lunt, D. J., Pound, M., and Salzmann, U.: The Pliocene Model Intercomparison Project (PlioMIP) Phase 2: scientific objectives and experimental design, *Clim. Past*, 12, 663-675, <https://doi.org/10.5194/cp-12-663-2016>, 2016.

<sup>3</sup> Kamae, Y., Yoshida, K., and Ueda, H.: Sensitivity of Pliocene climate simulations in MRI-CGCM2.3 to respective boundary conditions, *Clim. Past*, 12, 1619-1634, <https://doi.org/10.5194/cp-12-1619-2016>, 2016.

<sup>4</sup> Chandan, D. and Peltier, W. R.: Regional and global climate for the mid-Pliocene using the University of Toronto version of CCSM4 and PlioMIP2 boundary conditions, *Clim. Past*, 13, 919-942, <https://doi.org/10.5194/cp-13-919-2017>, 2017.

I have made the sentence more specific so that it now reads

“It must be noted, however, that this calculation assumes that the PlioMIP2 enhanced boundary condition *represents* the equilibrated Earth System under a contemporary doubling of CO<sub>2</sub>, hence neglecting non-glacial elements of the PRISM4 retrodicted palaeogeography.”

**On page 11, line 28 it is stated that sea ice extent is significantly suppressed within the Weddell Sea – is the significance of the change really shown?**

I think Figure 10(f and h) shows adequately the reduction in sea ice in the Weddell Sea (to the East of the Antarctic Peninsula) due to the change in palaeogeography. I have enlarged Figure 10. It is untidy if subfigures are referenced within the text e.g. “..(Figure 10h vs.10d and 10f vs. 10b)..”

**In the context of Section 4.2.3 I believe Fig. 11 should be referred, otherwise the textual description of results is difficult to follow.**

This has been corrected so that the sentence now reads

“The mixed layer depth (MLD) for  $E^{280}$ ,  $Eoi^{280}$  and  $Eoi^{400}$  is shown within Figure 11.”

**The statement on page 12, line 16, that the difference in AMOC can be ascribed to the earlier use of HadCM3 MOSES 1 seems to be a bit uncertain. As there are no variability ranges given by Bragg et al. (2012), one can only speculate whether there are significant differences between the AMOC values, or whether there is an overlap of both results. The authors elaborate later on that the original time series of AMOC by Bragg et al. (2012) are lost, impeding the computation of the old error ranges. Yet, this problematic should be mentioned here when ascribing a change in a result to a difference in the model version.**

I agree this is unsatisfactory. The lack of temporal data from the Bragg et al., 2012 study did cause me problems. I have removed reference to the Bragg paper within this paragraph (Section 4.2.4) and the Discussion (Section 5)

**On page 12, line 17, it is stated that the maximum AMOC strength is at about 1000 m depth. By eye Figure 12 suggests a rather shallower depth. Please verify and correct if necessary.**

This has been corrected to ~650 m depth.

**In the same line the authors write about “Fluctuations of the order in the AMOC”, without specifying the order of the fluctuations. It is also not clear what the difference in Mid-Pliocene and PI fluctuations should be. I at least do not see an obvious difference from the presented results. Please clarify.**

My apologies for this omission. Currently I am unsure what is causing this difference in AMOC behaviour. Nevertheless, I have corrected and restricted the sentence so that it now reads “Multidecadal to centennial fluctuations, including a dominant ~225 year oscillation, within the AMOC<sub>max</sub> are present within the Pliocene experiment but not in the pre-industrial experiment.”

**On page 13, line 12, the authors state that ACC strength appears significantly reduced in Pliocene experiments. Looking at the conveyed data, I get the impression that also the variability over time is reduced in the simulations. Is this impression correct? If so, I would state that as well in the text, and maybe discuss the implications for the Pliocene circulation regime in the Southern Hemisphere.**

That is an interesting point. Table 8 does indeed show higher variability within the ACC for lower CO<sub>2</sub> levels. I am hesitant to discuss the ACC in more detail within the manuscript as I have written a lot about the ACC already. Also, there are some interpretational difficulties associated with the ACC model output (which I discuss) and so I think that discussing its variability (to CO<sub>2</sub> sensitivity) is beyond the sections scope.

In sections 4.3.1 and 4.3.2 the authors elaborate on the statistical significance of differences between simulations with differing orbital configuration and TSI. I have to admit that I got a bit lost here. While the statement seems to be that there are statistical differences, my impression from the values given in the various tables is that simulations with different orbital parameters and TSI indeed show different mean values of respective quantities, but that in many (if not all) cases there is an overlap of the given variability around the mean value. Based on this observation I would assume that there is no statistical difference. Could the authors please clarify this in the text? I might have misunderstood their reasoning, but the matter is not yet clear to me.

For orbit and solar insolation there are no statistical differences within the standard climatic fields (MASAT, MAP, MASST etc. and PMOC) I have clarified Sections 4.3.1 and 4.3.2 to make this point clear. The simulation data does suggest the possibility that there is statistical difference within the AMOC

In the discussion the authors state that the primary control on ESS/CS ratio is the reconstructed ice distribution and global vegetation coverage. Assuming a prescribed vegetation, this is certainly the case. Yet, there are also modelling groups that will likely provide simulations with dynamic vegetation. Hence, the statement made by the authors could be explicitly tested in PlioMIP2. I would add some according remarks to the discussion section.

I agree and have added the following sentence to the Discussion section "The implementation of dynamic global vegetation models by PlioMIP2 participant groups will allow investigation of the sensitivity of ESS/CS to vegetation-climate feedbacks."

On page 15, line 19 following. I am not sure whether details and results of simulation Ei280, that is not considered in the manuscript, should be discussed here. Either, the relevant results should be explicitly shown somewhere before, or the results and discussion should go into the follow-up manuscript. Similar statement holds for the mentioned simulation Eo400.

My sincere apologies for the confusion as these were simply spelling mistakes. These have been corrected to Eoi<sup>280</sup> and Eoi<sup>400</sup> respectively.

The statement on page 15, line 26/27, that the findings are in contrast to Zhang et al. (2013): Is there really a contrast? Zhang et al. (2013) shows various models that have a stronger Mid-Pliocene AMOC. So aren't your results somehow in line with findings by Zhang et al. (2013)? AND On page 15, line 30, you state that "looking at typical HadCM3 (MOSES2) AMOC variability within Table 7". Where do I find this information in Table 7 (or somewhere else)? The understanding of the whole sentence in reference to PlioMIP1 is lacking to me.

I agree, this was a co-authors suggestion for inclusion but they had incorrectly remembered the premise of this paper. I have removed reference to Zhang et al., 2013. As discussed previously I have also removed reference to Bragg et al., 2012 wrt. AMOC within the Discussion (Section 5).

### **3. Remarks regarding quality of the presentation of results**

Regarding labels (a, b, c, ...) of subfigures: All subfigures are clearly labelled, which is very good. Yet, in very few cases the caption clearly defines what simulation, time average, etc. a label refers to. Instead, in many cases a heading is given for the subfigure that illustrates that information. As far as I know the use of labels is the preferred option for publications in *Climate of the Past*, rather than a subfigure heading that often reduces the space available for the illustrations themselves. If the authors choose to keep subfigure labels (which I strongly support), I would make sure that the meaning of a label is clearly defined in the figure caption.

I have ensured that the Figure caption clearly identifies each subfigure. I have also left in the subfigure label. In accordance with the second reviewer I have also made the text larger within each subfigure graphic.

**There is a prominent switch in the terminology employed in the various tables of the manuscript. In Table 1, the first column is headed “ID”, but in following tables it is headed “model”. One may argue which is the better term (I would opt for ID to avoid potential overlap with the term ‘climate model’, which is consistently HadCM3 for all simulations) – but at least the employed term should be consistent across tables.**

I agree with you. To remain consistent with the PlioMIP2 protocol (e.g. Table 3. Within Haywood et al., 2016) I have used “ID” within all the tables.

**The authors state at the beginning of Section 4.1.2 that MAP is influenced principally by geography and land surface changes and is relatively insensitive to Pliocene CO<sub>2</sub> changes. Is this statement supported by the presented results (difference between results for Eoi<sup>280</sup> and E<sup>280</sup> is only 0.07 mm/d, which is somehow in the range of the change created by modifications in CO<sub>2</sub>)?**

I agree, a good point. I was originally trying to refer to the geographic distribution of the precipitation. I have rephrased the Section on precipitation so it now starts

“The globally integrated Mean Annual Precipitation metric (MAP; Table 4) is influenced by both Pliocene geography and CO<sub>2</sub> changes. Pliocene geography acts to increase globally integrated MAP although this appears sensitive to the background CO<sub>2</sub> level (e.g. Pliocene geography increases MAP by 0.07 and 0.05 mm day<sup>-1</sup> at 280 and 400 ppm respectively). The geographical distribution of MAP change can be seen within Figure 5.”

**In the same section, it is stated that regions with little change in precipitation are regions that receive little precipitation in E280. Isn't this statement in contrast to the results derived for the rather large region of Eurasia?**

Eurasia within E280 receives a fair amount of precipitation (it is not dry like the North Africa or the East Antarctic Ice Sheet). The statement therefore doesn't refer to Eurasia. The ordering of statements within my sentence was the source of confusion, so I have changed it from

“Regions that have little (<0.1 mm day<sup>-1</sup>) change in precipitation under increasing Pliocene CO<sub>2</sub> are regions that receive little precipitation within E<sup>280</sup> e.g. North Africa and the East Antarctic Ice Sheet”  
To

“Regions that receive little precipitation within E<sup>280</sup> e.g. North Africa and the East Antarctic Ice Sheet have little (<0.1 mm day<sup>-1</sup>) change in precipitation under increasing Pliocene CO<sub>2</sub>.”

**On page 9, at the bottom, and on Page 10 up, the authors describe the simulated monsoon. I have to admit that the statements were difficult for me to verify and to follow based on the presented anomaly results. Am I right that showing an additional Figure with (seasonal) absolute fields of precipitation (MAP) for E280 would help to solve this problem?**

We removed the monsoon text as we believe that this would be more appropriate in a separate paper that allows the complexities of monsoon systems to be fully articulated and investigated.

**The change in the northern cell (by +10.8%) is difficult to identify in Figure 7, even when zooming in on the screen. This should be fixed if possible.**

I have made the subfigure axis labels larger and have made this figure within the manuscript. I have also expanded the description of the figure by indicating the Polar, Ferrel and Hadley cells. Within E280 the stronger northern Hadley cell can be seen as a stronger shade of red.



**The stated moving of the jet stream mean path from northern to southern Europe is very difficult to see in Figure 8.**

Within the text I have pointed the reader to compare Figure 8b vs. 8f. I have also made Figure 8 a little larger within the manuscript.

**The statement on the more continuous counter current in the Pliocene (Fig. 14, stated on page 13, line 29) is difficult to interpret from the illustrations as individual arrows of the streamlines are difficult to see.**

I have enlarged slightly the figure so that the reader can see the enhanced Pliocene counter current between 90 and 180°E

**I would like to point out that in my opinion the discussion provided by you regarding uncertainties due to setup of the Pliocene boundary condition – despite a common modelling protocol – is very important. In addition, I think one could elaborate (a bit more than already done) why the question after analogy or non-analogy of the Pliocene climate to modern or future conditions is so important in the context of Pliocene for future (P4F). I think references could be cited, e.g. Hill (2015) may be of relevance here.**

I have expanded the within the final paragraph of the discussion.

Palaeogeographic induced changes in mean state, for example the path of the Antarctic Counter Current around the Peninsula island (Section 4.2.5) represent non-analogous characteristics imposed by the PRISM4 Pliocene reconstruction. Other potentially non-analogous changes are associated with palaeogeographical changes to the Maritime continent and subsequent changes in Indonesian throughflow configuration, the closure of the Bering Strait and Canadian Archipelago, and the withdrawal of the Baltic Sea and Hudson Bay. These palaeogeographical changes should be considered alongside those described within Hill (2015) such as the suggestion of extensive uplift in the Barents Sea (e.g. Knies et al. (2014)) and the rerouting of major rivers (e.g. within North American) which may be currently unrepresented within the model. These important regional changes must be considered when considering the KM5c time slice as an equilibrium state analogue to contemporary climate change (i.e. a 400 ppm world).

**Page 21, Figure 1: I would add the term “streamfunction” after (the non-capitalized) term “barotropic” for consistency with the heading of section 3.2.2.**

I have corrected the text within the Figure 1 description

**Page 22, Figure 2: It is not clear which simulation is shown here. Add space between the subfigures. Maybe enlarge them and put them on top of each other. Would it be possible to give the depth information in addition to the layer information? Regarding the caption: I would add a “various” before “ocean layers”, make the comma after “spin-up stages” a colon, change “souther” to “southern”, and remove the hyphen of “high-latitudes”. “Incorporation”, “Correction”, and “Ocean layers” should in my opinion not be capitalized.**

I have altered Figure so that only the first figure is shown. I have also changed the figure description so that it now reads

“Figure 2. Time-evolution of the globally-integrated temperature for the ocean layers within the Eoi400 experiment. Whole ocean volume indicated by the thick red line and the top 200 m indicated by the thick green line. Vertical lines indicate key spin-up stages; (a) adding the barotropic physics to the ocean model, (b) incorporation of barotropic streamfunction islands into the barotropic solver, and (c) correction to the barotropic streamfunction island in the southern high-latitudes and incorporation of full PRISM4 vegetation boundary conditions into the model. The mid points to the ocean layers are 5 m (L1), 15 m (L2), 15 m (L3), 35 m (L4), 48 m (L5), 67 m (L6), 96 m (L7), 139 m (L8),

204 m (L9), 301 m (L10), 447 m (L11), 447, 666 m (L12), 996 m (L13), 1501 m (L14), 2116 m (L15), 2731 m (L16), 3347 m (L17), 3962 m (L18), 4577 m (L19) and 5195 m (L20).”

**Page 23, Figure 3: The physical unit is not given. Should “MAT” read “MASAT”? Should “student” be capitalized? There is no reference in the caption to subfigures a) to f), and there is no information in the caption that also two different Mid-Pliocene realisations are shown here.**

**Page 24, Figure 4: May it be that the figure is not explicitly referenced and used in the text? Could it be that captions of Fig. 3 and Fig. 4 were mixed up? Please check and correct if necessary. The physical unit is missing. Add reference to subfigures.**

I have corrected the text within the Figure 3 description to read MASAT and capitalised “student”. I have added physical units to all Figure color scales. I have also edited the caption to refer to the subfigures. Note also the Figure 3 and Figure 4 graphics were incorrectly swapped over within the manuscript.

**Page 25, Figure 5: Add reference to subfigures in the caption and fix capitalization of “Mean Annual Precipitation”.**

I have corrected the text within the Figure 5 descriptions and references the subfigures within the Figure caption.

**Page 26, Figure 6: I noticed that there is a gap in the stipples around the 0°E meridian (also the case for at least Fig. 5). Are stipples shifted or is there a data gap, and what does that mean for the interpretation of stipples in comparison to the shaded values? Add reference to subfigures in the caption.**

I have added reference to the subfigures within the Figure caption. The small gap at the 0E meridian is a NCL problem and I am currently unable to correct this.

**Page 27, Figure 7: The plots are too small, maybe put on top of each other and enlarge. Add reference to all subfigures in the caption. Replace “every” by “shown for intervals of”. Is the statement “ascending air moves southward” only true for counter-clockwise flow?**

I have enlarged the axis text within each subfigure to make it more clear. I have also made the figure larger and identified the cells within the first subfigure (described within the Figure caption)

**Page 28, Figure 8: Is there any way to enlarge the figure a bit more? Some details are difficult to decipher from the rather small plots. Point out in the caption that left is Northern Hemisphere, and right is Southern Hemisphere. Remove space in the superscript of simulation E280. Employ the defined abbreviations consistently throughout the caption. Add “by” after “typology”, “the” after “Note”, and “but” before “instead”.**

I have made the Figure larger and have enlarged the text. In response to reviewer 1 I have made the latitudinal extent of the north pole plots the same (and similar for the south pole)

**Page 29, Figure 9: The physical unit is missing. Could you explain (and correct if necessary) why there are two definitions for the warm pools applied (28°C and 28.5°C), also with respect to Table 6? Remove the “s” from “indicates”, add a “the” before E280, capitalize “Pliocene”, and change “have contrasting land surface” to “have land-sea contrast”. Add information on the criterion for the decision on statistical significance of anomalies.**

Within the literature the global warm pool was defined as the 28.5°C isotherm whereas the regional warm pools were defined as 28°C. For internal consistency I have recomputed the global warm pool as 28°C (Figure 9 and Table 6)

**Page 10, Figure 10: I think the labelling and the respective reference in the caption is incomplete (that is certain) and potentially also wrong. Capitalize “Southern Ocean”.**

I have corrected the subfigure referencing within the caption and capitalized “Southern Ocean”. I have also increased the subfigure heading text size and increased the size of the Figure within the manuscript.

**Page 31, Figure 11: The unit is missing. Give details of subfigure-label relation in the caption. Could you please elaborate (in the main text) why March and September means where shown in the plots, rather than, for example, boreal spring and boreal autumn?**

I have added the unit to the colour scale. March and September were chosen to keep consistent with other modelling studies looking at the MLD that I had read. These months were chosen to correspond with the maximum in sea ice extent.

**Page 32, Figures 12 and 13: The figures are too small, maybe combine them on top of each other. Add an “N” to the x-axis labels. The units are missing. Please specify the time interval that the data average represents (100 yr multiannual means? Maybe also consider this for other figures). Note the flow direction (e.g. clockwise circulation given for positive values). Maybe put the subfigure captions at a different location, they are difficult to read for PMOC plots. Add a space after (PMOC). Add the term “Meridional” before “Overturning”.**

I have enlarged Figures 12 and 13 and modified them so superfluous axis elements are removed. I have added the units to the colour scale bar. I have corrected the text of the figure captions and included the definition of the flow direction. Within the introduction to the results I removed the “We derive ... climatic oscillations from the final 100 years” as this was now unused. This now reads “We derive climatological averages from the final 50 years of each simulation (model years 2450 through to 2499).” This clears up uncertainty associated with the averaging period.

**Page 33, Figure 14: The physical unit is missing (maybe cm/s?). Do not capitalize “Mean Annual”. Remove the “the” before (c).**

I have added the unit, decapitalized “Mean Annual” and removed the “the”.

**Page 34, Figure 15: Define abbreviation MAT, and do not capitalize the words for a), b); c), d); e), f). The units are missing.**

I have added units to the colour scale bars to each sub plot. I have also used the abbreviations MASAT, MAP and SST to remain consistent with the manuscripts

**Page 35, Table 1: There is a problem with the text below the table (“our standard a discussion ...”).**

I have corrected the Table 1 sub caption text.

**Page 35, Table 2: Do not capitalize “Climatological”. Consider to use “ID” as heading for the first column (also for all subsequent tables).**

I have decapitalised “Climatological”. I agree with the use of “ID” and so have corrected this within all the tables.

**Page 36, Table 3: Do not capitalize the m of “1.5 M”. Could you provide the definition of polar and tropical regions as used in the analysis? Do the terms follow standard definitions? Add the**

**physical unit to the third column. I am a bit puzzled that the standard deviation of the third column is 0.7 °C for all simulations. Is this correct or is this a mistake?**

I have corrected the table and added definitions to Polar and tropical MASAT within the table caption

**Page 36, Table 4: Do not capitalize “Annual”.**

I have corrected the Table 4 caption text.

**Page 36, Table 5: Define abbreviation StJ in the first line of the caption, and apply it in the second line of the caption.**

I have corrected the Table 4 caption text to include the abbreviation

**Page 37, Table 6: Fix the typo in “charactersistics”. Below the table, clarify why the warm pool criterion is 28°C (rather than 28.5°C as given for the respective SST figure). In the first line below the table, there is a word missing towards the end of the line (maybe 28°C-criterion?). In the third line, I think one should adjust the text to “mean area that is at 28°C or above”.**

I have corrected the typo. Within the literature the GWP had been defined by the 28.5°C isotherm. To avoid confusion within the manuscript I have made the definition of the warm pools consistent within the manuscript so that they all are defined by the 28°C isotherm. Tables and Figures have been recomputed to reflect this change.

**Page 37, Table 7: In the column headings, fix the superfluous space between “AMOC” and the subscript “max”, add °-symbols to “N” and “S”, and maybe change “>500 m” to “below 500m” . Specify the meaning of values given in rectangular brackets of the last column. Define +ve and -ve PDW. Fix typo in “meridonal”. Is the abbreviation MOI used? Link the incomplete sentence “Pacific Meridional Overturning Circulation (PMOC)” to the rest of the text.**

I have removed the superfluous spacing in the heading caption, corrected the symbology and changed “>” to “below”. I have removed the Delworth et al., 1993 citation as the MOI abbreviation is not used within the manuscript. I have rewritten the Table caption.

**Page 38, Table 8: Fix naming of the current (see my discussion at a different location). Do not capitalize “Mean” and “Barotropic”. Make sure that the plus-minus sign is not separated from the 50% value via a line break.**

I have corrected the naming of the ACC and the capitalisation within the Table caption

**Page 38, Table 9: Fix capitalization.**

I have corrected the capitalisation

#### **4. Referencing**

**There are various references in the text that do not appear in the list of references at the end of the manuscript, which makes it unlikely for the reader to find the referenced literature. Respective references are often also wrongly formatted (e.g. with respect to use of comma between authors and publication year). I have found at least the following references that definitely need to be added to the list of references:**

**Johns et al. (2001); Matthews et al. (2016); Levitus and Boyer (1994); Edwards (1989); Wilson and Henderson-Sellers (1985); Randall et al. (2007); Stachnik and Schumacher (2011); Archer and Caldeira (2008); Koch et al. (2006); McCarthy et al. (2015); Jackson and Vellinga (2012); Delworth et al. (1993)**

All the references have been corrected and doi’s have been included in as much as possible.

The Lie and Xie (2014) is now not cited.

## **5. Language- and nomenclature-related remarks**

The authors employ various abbreviations, which is fine. Yet, not all of the abbreviations are defined in the text, and respective definitions are even rarer in the captions to figures and tables. While it is difficult to decide which abbreviations can be assumed to be understood by the readership, I would suggest to strictly define them all – in particular to ease understanding of the work by non-experts of the subjects of Pliocene, PlioMIP, CMIP/PMIP, and IPCC, that hopefully will also be attracted to reading this work in the context of informing themselves about the potential relevance of Pliocene climate for projections of the climate of the future. I would suggest to make sure that the following abbreviations are defined: HadCM3, PRISM4, GCM, CMIP3, IPCC, AR4/5. I may have overlooked some more, so ask the authors to once more check the completeness of the definition of abbreviations used throughout the text.

Another important remark regarding abbreviations: Please define abbreviations at the first occurrence of the text and only there, and, once defined, use them in all cases. Exceptions are the abstract, figure and table captions where abbreviations used in the respective text unit should be redefined regardless of their appearance in the main text (the latter is not everywhere the case). One case, where abbreviations are not consistently used, are the terms Figure (also used as Fig. and Fig) and Table (also used as Tab.). Another example is the abbreviation LSM for land sea mask, that is defined for the first time on page 6, while the full term is used various times on preceding and following pages. Similar problems are with polar jet (PJ) and Subtropical Jet (StJ) as well as with the term sea surface temperature (SST).

I agree. At the first instance I have defined the abbreviation in full. Where required, I have also defined the abbreviations within the table captions, so that the tables can be considered stand-alone and self-describing.

**Nomenclature regarding simulations: It must be made more clear what the authors mean with the term ‘control’ Pliocene experiment. In the abstract that term is used without explanation. While I assumed that ‘control’ stands for ‘CORE’ (the Eoi400 simulation), and then was surprised by the apparently rather small difference in global mean surface air temperatures that the HadCM3 Eoi400 CORE simulation assumedly provides if compared to E280, digging deeper into the text reveals that ‘control’ rather refers to simulation Eoi280. This is confusing even if one has the list of simulations (and simulation names) as proposed by Haywood et al. (2016) at hand. Maybe avoid the term ‘control’ altogether to avoid confusion and rather refer to the standard simulation names. Or, if you intend to use the term, make sure that it is clearly defined.**

Within the Experiment Design section (Section 3), in which I use PlioMIP2 terminology, I tie the PlioMIP2 CORE experiments to the standard use of the term “control”

“These experiments are labelled the *control* Pliocene experiment Eoi<sup>400</sup> (PlioMIP2 CORE), Eoi<sup>350,450</sup> (Tier 1; P4F+P4P), and Eoi<sup>280</sup> (Tier 2; P4F).” [...] “These are identified as the *control* pre-industrial experiment E<sup>280</sup> (CORE), E<sup>400</sup> (Tier 2; P4F) and E<sup>560</sup> (Tier 1; P4F). “

I use the term CORE when in sections discussing the PlioMIP2 experiment but stick to “control” within the text describing the results.

It is not fully clear to me what climatic quantity the authors refer to when they talk about “air temperature”. At one point of the text an air temperature at 1.5 m height above the ground is mentioned, but it is not clear to me whether all results in the text, in tables, and all air temperature illustrations in the various figures refer to this height (or maybe to a different height, like 2 m, or even to the surface skin temperature). This could be clarified if the height above the ground was specified together with a definition of the term surface air temperature at the earliest convenient location of the text, and if subsequently that definition is consequently applied throughout the text.

There are various definitions of SAT (MAT, MASAT, SAT?) – my feeling is that they all refer to the same quantity – if so, please use only one abbreviation.

At the earliest opportunity I have defined MASAT as the mean annual 1.5 m surface air temperature (Section 4.1.1 Surface Air Temperature and Climate Sensitivity). The abbreviation MASAT is then used throughout the manuscript.

There is a problem with the term Antarctic Circumpolar Current (ACC) in Section 4.2.5 and related text. The section is headed “Antarctic Circumglobal Current”, and that term appears to me at least to be unusual. Furthermore, at some locations the term Antarctic Circumpolar Current is used then anyway, although not abbreviated. Last but not least, understanding the text becomes even more complicated due to the appearance of the terms Antarctic Counter current and counter current, the former one could equally be abbreviated as ACC. The latter terms are to my knowledge different from the former terms, and rather refer to the near-coast flow in opposite direction to the ACC. This section left me puzzled with regard to the currents that were referred to in the various locations of the text. May I kindly ask the authors to overhaul this part of the text in order to clearly define ACC, Antarctic Circumpolar Current, and counter current?

My embarrassing and somewhat random use of the term “circumglobal” in relation to the ACC is due to my focus on Cretaceous climates (as in the *Tethys circumglobal current*). I have corrected this within the manuscript. With regards to my use of the term “Antarctic counter current”, I have changed this to its formal name, “Antarctic Coastal Current”.

**Definition of time periods:** There are at least two versions of the term pre-industrial period employed in the manuscript (pre-industrial and preindustrial). I would use only one, and in addition define once that it refers to simulation E280 (this has not been done if I am not mistaken). Furthermore, within PlioMIP there are various terminologies regarding the Pliocene time slice: Mid-Piacenzian (e.g. Dowsett et al., 2016), Mid-Pliocene (e.g. Haywood et al., 2016), and Pliocene (employed by the authors of this manuscript). If the term is defined clearly in the manuscript at the earliest available convenience, then in my opinion all three are suitable choices. Yet, the reference to the alternative term in the discussion on page 15 is in my personal opinion a bit late.

All instances of “preindustrial” and “Pre-industrial” have been corrected to “pre-industrial”. I have ensured that the pre-industrial referred to the E<sup>280</sup> experiment only once. Within the introduction of Section 1 we now explain our use of the term “Pliocene” with the following:

“PlioMIP2 focuses on a ‘time slice’ centred on an interglacial peak (MIS KM5c; 3.205 Ma) within the mid Piacenzian, for convenience we refer to this as the *Pliocene*.”

In the results section I have then used the term Pliocene throughout. An exception to this is when there is a requirement to be more specific in a temporal sense (e.g. discussing uncertainty in the CO<sub>2</sub> record or orbital configuration).

There is quite a variety of the use (or non-use) of spacing of physical or geographical units from the respective value (X) and within the units themselves. I think the text would look much cleaner after a respective overhaul. I think that there should never be a space within

**physical units, also to avoid that part of the unit is separated and put into the next line in the proximity of line breaks. Here some of the examples that I found: [...]**

I have made the following corrections

All double spaces have been changed to single space

All instances of  $W\ m^{-2}$  changed to  $Wm^{-2}$

All instances of X % changed to X%

All instances of space between ~ and X removed

All instances of  $X^0$  corrected to  $X^{\circ}$

All instances of “-hPa” corrected to “ hPa”

All instance of “~ X” changed to “~X”

All instance of “> X” changed to “>X” and “< X” changed to “<X”

All instances of “preindustrial” and “Pre-industrial” changed to “pre-industrial”

All instances of “Core” or “core” in reference to PlioMIP2 experiments has been changed to “CORE”

Instances of “ice-sheet” changed to “ice sheet”

“Northern/Southern Hemisphere” capitalised

The “sea” in “Barents Sea” capitalised

The “south” in “South Pacific” capitalised

The “polar” in “Polar cell” capitalised

“warm pool” decapitalised throughout

“mixed layer depth” decapitalised throughout

“Nordic Seas” capitalised

“Drake Passage” capitalised

“barotropic/baroclinic” and derivatives decapitalised

Instances of “through flow” and “through-flow” have been changed to “throughflow”

<additional>

I have replaced all instances of “ppmv” with “ppm”

I have replaced all instances of “sub grid” with “subgrid”

I have replaced all instances of “hand corrections” to “manual corrections”

For consistency I have replaced all instances of “Tab.” With “Table”

**P1L1** Changed “athe” to “the”

**P1L4** Added a comma after “ocean state”

**P1L5** Added “and various related sensitivity studies” after “Pliocene experiments”

**P1L7/8** I have added “( $E_{o1}^{400}$ )” and “( $E^{280}$ )” to clarify the stated experiments

**P1L9** I have added a “of” between “ratio” and “~1.90”

**P1L10** The text “wet-get-wetter” has been removed from the manuscript.

**P1L15** Comma deleted after PlioMIP2

**P1L17** I have changed the occurrence of “the total solar irradiance choice” to “the choice in total solar irradiance value” [and also altered within **P6L13** and **P27L20**]

**P1L18** I agree with Reviewer1 here and have changed “climatic systems” to “components of the climate system”

**P1L20** I have removed the superfluous space and have changed the semicolon to a colon

**P1L21** I have changed “uses” to “use”, added a “as”, and changed “contemporary” to “future” so that the sentence now reads “...has dual focus: to serve as a means to improve understanding of Pliocene climate and also, through its use as a potential analogue for future climate, as a means to evaluate climate model uncertainty.”

**P1L22** This section has been rewritten.

**P1L24** I have removed the definitions of abbreviations T1 and T2 as the abbreviations are only used within Table 1 in which they are described within the caption

**P2L3** I agree. I have added “model” between “additional” and “sensitivities”

**P2L9** I agree. I have changed “This leads onto the ..” to “This leads onto to descriptions of the ..”

**P2L11** I have inserted “section” after “results” and rephrased “atmospheric and surface climatology” to “atmospheric circulation and surface climatology”. I have also inserted “focussing on”, so that the sentence now reads “...with the atmospheric circulation and surface climatology (Section 4.1 and then focussing on the oceanic realm (Section 4.2).”

**P2L14** I have capitalised “Model”

**P2L14,15,16** I have removed the sentence (referring to CMIP3 and CMIP5) as it was surplus to its two bracketing sentences.

**P2L17** I have added an apostrophe to “models”

**P2L18** I have removed “compared to similar generation models” . I have altered “...well suited to long-integration palaeoclimate studies.” to “...well suited to conduct long-term integration palaeoclimate studies.”

**P2L21** I have changed “..models, these..” to “.. models, and these..”

**P2L22** I have broken the sentence up after the initial “Pliocene”

**P2L24** I have capitalised “seaway”

**P2L27/28** I have added “in which” , so that the sentence so that it now reads “This body of work therefore represents the first published record in which HadCM3 has been reconfigured with a bespoke global Pliocene palaeogeography.”

**P3L7** I have added an “at” between “and” and “45°”, and a comma making “latitude, respectively”

**P3L9** I have added a space between “CH<sub>4</sub>” and “760”

**P3L10** I have added “as reference” after “PMIP2”

**P3L13** This is correct, your replacement is more factually complete. I have replaced “based upon the modern” with “based upon modern climatological conditions”

**P3L18** My preference is not to use a comma in these circumstances as it looks messy when the TSI is initially presented to 1 d.p (1365.0 rather than 1,365.0)

**P3L19** see **P1L17**

**P3L22/23** I have reworded “..may depend upon if the group is participating within CMIP6.” to “..may depend upon if the group is a participant of CMIP6.”

**P3L24** I have added a comma after “..AMOC strength”

**P3L25** I have kept the sentence as “The land surface scheme is MOSES 2.1 (Met Office Surface Exchange Scheme; Cox et al., 1999)

**P3L26** I have decapitalised “Broadleaf” and “Needleleaf”

**P3L28** The reviewer is correct in that this sentence belongs in the description of the experiment design. I have therefore removed the sentence “We hold vegetation fixed through the entirety of each experiment.”

**P3L29** The comma was removed within “..subsurface, and..”



**P4L5** as suggested by the reviewer I clarified the sentence by changing “..giving 6 grid cells per..” to “...equivalent to six grid cells per ..”

**P4L9** “archipelago” was capitalised

**P4L10** A comma was added after “...region”

**P4L11** Changed “...mixing; important for the ..” to “..mixing that improves the..”

**P4L12** “A similar scheme is not present for Antarctic Bottom Water.” was changed to “ The scheme is not used for Antarctic Bottom Water.”

**P4L17** The “..(specifically, virtual -ve salinity fluxes)..” is superfluous and so was removed.

**P4L19** “artificiality” was corrected to “artificially”

**P4L25** I removed the hyphen in “high-latitudes”

**P4L27** I added “on the other hand” to make “An advantage of the rigid lid scheme on the other hand..”

**P4L30** I agree. I added a hyphen to make “Observation-derived upper-boundaries to..”

**P4L33** I have kept it within “Sublimation is represented ..”

**P4L34** I really failed here. I’ve corrected the spelling of “parameterisation”, and “of” was added to make “.. action of surface ..”. An “a” was added to “..by a parameterisation ..”. I changed “The effects of snow age and melt pond formation on surface albedo is ..” to “The effects of snow age and melt pond formation on surface albedo are ..”

**P5L1** corrected.

**P5L4** “experiment” changed to “experiments”.

**P5L4/6** The sentence “specified by the PlioMIP2 protocol, and a 3.205 Myr orbit consistent with the KM5c time slice.” changed to “specified by the PlioMIP2 protocol. A second set of Pliocene experiments were run with identical CO<sub>2</sub> values but with a 3.205 Myr orbit consistent with the KM5c time slice.”

**P5L8** I added “consideration of” to make “..the o and i indicate consideration of PRISM4 orography..”

**P5L9** New sentence started, to make “The former (o) includes PRISM4 orography includes vegetation, soil, and lakes.”

**P5L10** Changed “giving” to “providing”, I added “the” before “PlioMIP2”, and then changed “experiment design” to “simulation ensemble”

**P5L11** I changed “ .. a sensitivity outside the ...” to “..a sensitivity study that is beyond the...”

**P5L12** I removed “the” and added “ the subscript” to “..by the subscript orb, such..”

**P5L13** I added “sensitivity studies” to make “..and total solar irradiance sensitivity studies”

**P5L15** Correct – I added the “vs.” to make “(Eoi<sup>280</sup> vs. E<sup>280</sup>)”

**P5L16** I agree, I changed the heading from “Pre-industrial experiment description ..” to “Pre-industrial and CO<sub>2</sub> sensitivity experiments description...”

**P5L17** “year” was changed to “years”

**P5L18** I have changed the sentence from “..Levitus observed ocean state (Levitus and Boyer 1994)” to “...the observed ocean state of Levitus and Boyer 1994”.

**P5L20** Good point. I have added a “runoff” before “basin” to avoid confusion with ocean basin.

**P5L23** the “ .. is 280, 400 and 560 ppmv” was changed to “ ..are 280, 400 and 560 ppmv.”

**P5L25** Another good point. I have changed “..total solar irradiance..” to “..TSI ..”

**P5L25** I have clarified the use of a hyphen within the Experimental design subsection with the sentence “Here we use a comma separated list in the superscript to indicate 2 or more experiments or a hyphen to indicate all inclusive experiments (e.g. Eoi<sup>280,350,400,450</sup> is equivalent to Eoi<sup>280-450</sup>).”

**P5L30** I have capitalised “Pliocene”

**P6L5** I have inserted “regions of the “ before “ Eurasian Arctic.”

**P6L6/7** I have changed “..the MOHC developed pre-industrial boundary conditions we ..” to “..the pre-industrial boundary conditions developed by MOHC we..”. I have also changed “..omit..” to “..removed..”, changed “subaerial extensions” to “subaerial exposure”, and added “Pliocene ” within “.. the Strait..” to clarify that I was referring to the Pliocene. I have also replaced “..the same as ..” to “..identical to the..”.

**P6L9** I agree, I added the hyphen to make “..model-resolution ..”

**P6L10** I agree – the “..was interpolated using similar methodology.” was a vague and lazy phrase. I have replaced this with “..generated using area-weighted regridding.”

**P6L13** I have changed “..manual correction in corrected regions in circumstances when..” to “..manual correction in regions when..”

**P6L14** I have changed “..new..” to “..the regridded Pliocene ..”

**P6L15** I have added a hyphen to make “..model-resolution..”

**P6L17** I have added an apostrophe to make “model’s”

**P6L21** I have removed capitalisation of “island”

**P6L24** I have added “(Section 3.2.1)” after “aforementioned ”

**P6L27** “represent fully” changed to “fully represented”

**P6L30** Good point. I have changed this the two sentences “Figure 1 compares the pre-industrial and PRISM4 HadCM3 island specification. Within PRISM4, 8 islands have been specified.” To “Figure 1 compares the pre-industrial and PRISM4 Pliocene HadCM3 island specification in which the latter has 8 islands specified.”

**P6L31** To clarify this sentence I have changed “..within pre-industrial HadCM3 experiment the Bering Strait” to “..within the pre-industrial HadCM3 model setup the Bering Strait”.

**P6L32** This was a Latex error. I have corrected this reference to “(Section 2.2)”

**P6L34** To avoid plural “conditions” I have changed “... the PRISM4 boundary conditions specifies these throughflow regions as closed” to “... the PRISM4 Pliocene geography has these throughflow regions closed”. I agree that “..we will not see..” is informal so have changed this to “..our simulations do not resolve..”

**P7L1** I agree that this model limitation will be apparent within absolute quantities too, so I have removed the “,when we look at climatological anomalies” as it is now necessary.

**P7L3** I have added “from” after “as well as”

**P7L4** I have corrected the spelling of “rigid lid”. I am incredibly sorry to RC1 for the time she/he has had to spend on spelling and grammatical errors.

**P7L5** I have changed “channels” to “gateways” as it is more terminology

**P7L8** I have removed capitalisation on “atmosphere”

**P7L9** I changed the sentence “..sea mask, pre-industrial CO<sub>2</sub>..” to “..sea mask, as well as pre-industrial CO<sub>2</sub>..”

**P7L10** I added “distribution” to make “..sea ice distribution..”

**P7L12** I removed “the” and changed “PRISM4” to “Pliocene” to make “100 year AOGCM run with Pliocene bathymetry and river scheme”. In the next sentence I replaced “Here ..” with “So far ..”

**P7L16** I removed the erroneous “(1” and surplus “then”

**P7L17** I have split this sentence up so that I now have “At stage five we have an AOGCM incorporating full barotropic physics. CO<sub>2</sub> is then ramped up at 1% per year to 400 ppm and then held fixed.”

**P7L18** I changed “vegetation boundary conditions” to “vegetation boundary condition”.

**P7L20** I corrected the phrasing and expanded on the description of the artefact such that the sentence went from “*..Peninsula following a persistent and unsatisfactory model artefact in this region*” to “*..Peninsula to resolve a persistent numerical mode within the barotropic solver in this region*”

**P7L21** I have left in the repeated mention of CO<sub>2</sub> being held fixed at 400 ppm as it useful for the reader when then read the next sentence regarding forking off experiments at the other CO<sub>2</sub> values.

**P7L22** I have since changed the breakdown of the modelling stages to a numbered list. I have kept the bracketed statement to remind the reader of the CO<sub>2</sub> value.

**P7L23** I understand the confusion. I have clarified the sentence by adding a “configured” so that the sentence now reads “*At the final stage, stage eight, the models are run for the final 100 years configured with full climatological output.*” This is because the model when it is spinning-up doesn’t generate full climatological output.

**P7L24** I have changed the sentence from “The final 50 years is used for climatological averages” to “*The final 50 years of output is used for climatological averaging*”

**P7L25** The number of Pliocene experiments within the paper is nine, this has been corrected within the text.

**P7L26/27** This paragraph has been clarified in relation to the total combined 7500 model years (recalculated to reflect the correct number of experiments). I have also moved the plural of “achieve” to before “*with full physics..*”

**P7L32** I agree with the reviewer and so have changed the sentence “*..and the upper 200 m and globally integrated ocean potential temperature trends are -0.026 °C century<sup>-1</sup> and +0.041 °C century<sup>-1</sup> respectively.*” To “*..and ocean potential temperature trends within the upper 200m and globally integrated are -0.026 °C century<sup>-1</sup> and +0.041 °C century<sup>-1</sup>*”

**P8L1** The superfluous sentence has been removed

**P8L2-3** The sentence has been clarified to “*Positive TOA imbalance is indicative of a warming of the earth system, the small heat capacity of the atmosphere and land means that residual energy is predominantly taken up by the ocean, which is reflected in the volume averaged ocean temperature evolution.*”

**P8L4** I have changed the term “*..volume averaged..*” to “*..volume integrated..*”, and have added a space after these sentence closure.

**P8L6** I agree, I have changed the “*..>2000 m..*” to “*..deeper than 2000m..*” and the occurrence of “*..greater..*” to “*..deeper..*” to clarify.

**P8L7** I have changed the sentence from “All experiments are satisfactory, although E<sup>560</sup> has above average warming within the deep ocean” to “*All experiments are deemed to be in a satisfactory state of equilibrium, although the outlier high TOA simulations Eoi<sup>450</sup> and E<sup>560</sup> present above average warming within the deep ocean.*”

**P8L8** I agree, I have changed “*inconsistent*” to “*not meaningful*”

**P8L13** I have changed the sub heading from “*Atmospheric and surface climatology*” to “*State of the atmosphere and earth surface climatology*”

**P8L15** Within the file naming of Figure 3 and Figure 4 were incorrect. This has been corrected and the sentence now correctly refers to the correct Figure 3. Figure 4 now correctly refers to the Seasonal plots.

**P8L17** I agree that the term “*..regions of Pliocene ice sheet retreat (and topographical reduction)*” is incorrect and confusing, I have replaced this with your suggestion “*..regions where Pliocene ice sheets and the respective elevation are smaller than pre-industrial.*”

**P8L19** “.. is in a similar distribution to HadCM3..” to “..is similar to results derived with HadCM3..”

**P8L24** I have replaced “UK” with “Ireland and Scotland” .

**P8L25** This has been corrected with the correction of Figure 3 and Figure 4 pdf filenames.

**P8L26** I have replaced “..in the present)..” with “..during the pre-industrial)..”

**P8L27** “..he..” corrected to “..the..”

**P9L1** “..pre-industrial E<sup>280</sup>” has become “pre-industrial (E<sup>280</sup>)”

**P9L7** “(Table 2; Haywood et al., 2013)” replaced with “(Table 2 of Haywood et al., 2013)”

**P9L19** hyphen replaced with “e.g.”

**P9L20** “models” changed to “model’s”

**P9L23** have added “(not shown)”

**P9L25** “is” changed to “are” to make “..although changes in seasonal latitudinal distribution are not evident.”

**P9L26** I have inserted a reference to Figure 6 (c-f)

**P9L27** Comma replaced by a period.

**P9L28** I have removed the end of sentence from “,for example ..” as this was surplus.

**P9L30** superfluous “the” removed

**P9L29-P10L4** I have removed these two paragraphs as this level of specificity is inappropriate to the scope of the manuscript

**P10L28** changed “stable latitudinally” to “latitudinally stable”

**P10L29,31,34** spaces removed from “ -E<sup>280</sup> ”

**P10L32** “summer” changed to “southern”

**P11L1** All instances of “equaterward” replaced with “equatorward”

**P11L2** I agree, I have changed the reference to “Figure 6e and f”

**P11L3** The subheading has been changed from “Ocean state: Description of the gross hydrographic, circulation features, Overturning and ocean heat transports.” to “State of the Ocean climatology” to remain consistent with subsection heading (see **P8L13**)

**P11L8** hyphen added to make “CO<sub>2</sub>-induced”

**P11L9/10** These incomplete sentence has been corrected to make “The greatest warming occurs within the North Atlantic subpolar gyre where Eoi<sup>400</sup> – E<sup>280</sup> reaches 9.3 °C”

**P11L25** “A complex picture emerges in the geographic and CO<sub>2</sub> sensitivity of seasonal sea ice distributions as..” changed to “ A complex picture emerges in the sensitivity of seasonal sea ice distribution to geographic and CO<sub>2</sub> as .. “

“winters” changed to “winter”. The term “the paleogeographic and vegetation changes” has been changed to “the paleogeography changes”. I have removed “extent” and “suppresses” corrected to “suppress”.

**P11L30** 2 commas were added.

**P11L31** “ice” removed before “..concentration.”

**P12L6** “the” added before “HadCM3L”, “s” added to make “occurs”. “Greenland Seas” capitalised. “off of the Antarctic Peninsula” changed to “near the Antarctic Peninsula”

**P12L14** “AMOC of” added to make “consistency between E<sup>280</sup> and E<sup>400</sup> and the observed AMOC of 17.2 +/- 4.6 Sv”

**P12L15** The Sv unit was corrected to “10<sup>6</sup> m<sup>3</sup> s<sup>-1</sup>”, “..differs to..” replaced with “..differs from..”. I have added a comma to make “..(RAPID array 26.5° N, Apr 2004 - Oct 2012 ; McCarthy et al., 2015)..”. The citation has also been corrected

**P12L30** I have changed the sentence to “..level (for simulation Eoi<sup>400</sup> the circulation pattern is 22% and 6 % stronger than E<sup>280</sup>).”

**P13L3** The transect definition has been fixed so that it now reads “..across a 64.375 - 56.875°S, 65°W transect..”

**P13L4** After consideration I left it as “..the positive aspect of the U component..”

**P13L7** I changed “..had an ACC..” to “..simulated an ACC..”

**P13L9** I removed the “s” to make “gradient”

**P13L10** I replaced “..on the equator side..” with “.. towards low latitudes..”

**P13L14** Corrected “interpreting”

**P13L16** After consideration, I left the sentence as “..so is dominantly barotropic in nature.” I switched “island Peninsula” to “Peninsula island”

**P13L18/19** Comma added after “line-integral configuration”. The sentence was reorganised and also split into two such that the two sentences read “Also, the model's barotropic solver, given a more complex line-integral configuration, may not be converging to a solution. This requires further investigation.”

**P13L21** Comma changed to “to” and corrected “latitudinal”

**P13L21** Reordered so that it now reads “..and an equatorward shift of its centroid.”

**P13L29** Changed to “..a more continuous counter current in the Pliocene..”

**P13L33** Provided more consistent capitalisation such that it now reads “The Weddell Sea sub-polar gyre is weakened and restructured whilst the Ross Sea gyre is less intense and extends more equatorward”

**P13L29** The repeated block of text has been removed.

**P14L9** Space added after “modern”

**P14L10** “Tab. 6 SST” replaced by “Tab. 6 MASST”

**P14L11** I have changed “warm pool dynamics” with “warm pool areal extent”. “difference” added after “statistical”. I have corrected “AMOC<sub>max</sub>”

**P14L12** Corrected “26.5°N”

**P14L18** “(TSI)” removed from subsection title

**P14L23** Text changed to “(Pliocene minus pre-industrial) for simulations based upon 1365 to 1361 W m<sup>2</sup> for ..”

**P15L5** I agree, I changed it to “..atmosphere and ocean state of these simulations.”

**P15L10** I added “results of” to make “..are similar to results of PlioMIP..”

**P15L12** Significant digits of numerical results corrected.

**P15L13** Changed to “..demonstrates an insensitivity of these quantities to the degree..”

**P15L18** Again my apologies. I added an “in”, a comma and “the variation” to make the sentence “We find an AMOC which is more intense in the Pliocene than in the pre-industrial, the variation driven principally by the change in geography”

**P15L19** I corrected the sentence by adding E280 so that it is now “We determine this by comparing AMOC strength of E<sup>280</sup> against Eoi<sup>400</sup> and Eoi<sup>280</sup>”

**P15L34** Comma added after “KM5c”

**P16L10** Comma added after “grid type”

**P16L16** “subariel” changed to “subaerial”

**P16L23** I have added a “palaeogeographical changes to” before “..the Maritime continent”

**P16L30** italic font removed from Web address

**P17L9** Full stop added to end of line

**P18L5** I cannot find the error with author Peterchmitt, J.-Y., the bibtex bibliography text was copied from Climates of the Past. My apologies if I have missed something.

**P18L16** Cox(1984) bibliography information corrected (changed to book)

**P18L18** Removed duplicate doi information

**P18L25** spaces around page number hyphen removed

**P18L31** Bibliography information corrected

**P18L34** Dowsett reference corrected

**P18L35** Flato et al., 2013 reference removed

**P18L21, P19L5, P19L18, P19L28**

No page numbers available for these papers as they are from GRL:Oceans. < Note to editor>  
The correct bibliography information is present within the provided paper.bib file but it seems the Copernicus.bst bibliography style file doesn't incorporate the "number" field for references.

**P19L10/11** Bibliography information corrected

**P19L16** spaces around page number hyphen removed

**P19L20** Download link added to Li and Shine (1995)

**P19L34-35** Roether et al., 1994 changed to inbook

**P19L36** Semtner (1974) entry was not required for the ocean model description – so this was removed. A more suitable Semtner (1976) reference was used for sea ice model.

**P20L1** reference to IPCC AR4 was removed from the manuscript and so the bibliography information was now not required.