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Interactive comment on "Evaluation of Arctic warming in mid-Pliocene climate simulations" *by* Wesley de Nooijer et al.

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Thank you for your review. We have addressed each of your comments one-by-one and we feel that there were some substantial improvements following your comments. We also attached this author response in the supplement PDF file for you to refer.

Review comments: In the present manuscript, de Nooijer et al. present an analysis of Arctic climate as simulated by the coupled models ensemble from the PLIOMIP2 initiative. PLIOMIP2 focuses on the specific KM5c interval, peak of the mPWP. Notable improvements have also being done for the boundary conditions (e.g. closed Arctic gateways during this period). Models generally simulate an Arctic amplification larger than 2.5, increase in SAT and SST. Comparison with the few existing proxies suggest

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that only few models of the ensemble are able to fit the warm climatic conditions of the particular KM5c interval. However, the lack of proxies prevent a more detailed comparison. An attempt is made to compare those new results to projections. Conclusion of the authors is that using the simulated mPWP KM5c is not yet informative for the future, given the current state of models and limitations of the design of the experiments and lack of proxies to validate the paleo-simulations. In general, what this phase 2 of the PLIOMIP initiative shows is that boundary conditions improvements and focus on a specific interval of the mPWP generally increase the agreement with the few existing proxies. However, the paper remains rather very elusive and not detailed too much about the causes of the simulated anomalies. In addition, there is a distinct dichotomy within the models with only few models increasing the MMM. An aspect that is really unclear throughout this manuscript is the impact of the models that do not used closed gateway in their simulations and how much this impact on the interpretation of the entire metrics presented here. In addition to closed gateways, individual model resolution might also have an impact on the representation of those gateways and this is not discuss here. The attempt made to compare with CMIP5 projections is to my opinion unsuccessful given the striking difference in gateways between the modern geography and that of the mPWP. In addition, the authors attempt to compare the mode of variability which is a non-sens here since the paleoclimatic simulations are equilibrium simulations while projections are transient short-term simulations. Authors warn about the lack of “slow-feedbacks” in the projections, but the contrary is also true, the short-term variability present some limitation in the paleoclimate runs. I do not advise to remove it. However, some improvements are needed to strengthen those parts and to make them meaningful in a way or in the other. The manuscript is written quite well (though in some places that I have indicated in my comments below, some improvements in the writing is needed to clarify). My impression is that this paper remains superficial and does not provide a real analysis of the Arctic warming. There is no real analysis of the causes/consequences of this warming (i.e. albedo, seasonal cycle in temperature, snow cover, westerlies etc.). . . Even if the number of proxies is limited,

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the authors could deepen their analysis to compare the different models together to provide partial answers to some of the questions posed paper by the authors themselves within the different sub-section of the manuscript. They should also explore the dichotomy amongst the models visible in almost all the figures of this manuscript and the impact this dichotomy has on the MMM and thus the overall interpretation of the MMM. I therefore recommend moderate revisions.

Reply: A small response on the main summary of the reviewer, with regards to the paper remaining superficial: This paper mainly describes results and highlights differences between models. To investigate causes of the differences, e.g. because of albedo/seasonal cycle, we would need sensitivity experiments. This is not plausible for a multi-model analysis. We agree that the paper remained somewhat superficial, but we do not think it is possible to do deep analyses without sensitivity experiments.

Review comments: Line 68: I would remove “future” and just write “as warming in the Arctic directly affects. . .”. This is because this is always true, not only for future. Or perhaps just reformulate in “as it is shown that projected Arctic warming affects. . .”.

Reply: Good spot. The sentence has been adjusted to the suggested reformulation.

Review comments: Line 84: Would it be worth mentioning that the interest of the KM5c interval is because orbitals are similar to present? I think this is important and relevant to the comparison with projections.

Reply: Indeed, it is an important feature of the KM5c time slice that it has a similar-to-modern orbital forcing and we have added emphasis on this.

“Additionally, the KM5c time slice is characterized by a similar-to-modern orbital forcing (Haywood et al., 2013b; Prescott et al., 2014). These factors give lessons learned from the mPWP, and the KM5c time slice in particular, potential relevance for future climate change (Burke et al., 2018; Tierney et al., 2019), and this is one of the guiding principles of PlioMIP (Haywood et al., 2016).”

Review comment: Line 141: correct “model resultsaere calculated” in “model results are calculated”

Reply: Good spot, fixed.

Review comments: Line 196-203: I find interesting to note that most of the models simulate air and sea temperature values below the mean and that only a couple of models exhibit values much higher than the mean. It could also be worth mentioning this somewhere (though it is not a paper about individual model performances) because it also impacts on the interpretation that one does about the ensemble mean.

Reply: Indeed, good observation, a subset of the ensemble simulates much larger temperature anomalies than the rest of the ensemble. To note readers on the potential impacts this may have on the multi-model mean results we added the following:

“There is a large variation in the magnitude of the simulated Arctic SAT anomalies, with five out of sixteen models, namely CCSM4-Utrecht, CCSM4-UoT, CESM1.2, CESM2, and EC-Earth 3.3 all simulating much stronger anomalies than the rest of the ensemble. This subset of the ensemble raises the MMM substantially and this has to be taken into account when interpreting the MMM results. The MMM SAT anomaly for the PlioMIP2 ensemble excluding this subset of five models is 5.8 °C.”

Additionally, we added a sentence about SST, as the same five models are seen here to raise the MMM. “Furthermore, the five models that simulated the largest Arctic SAT anomalies also simulate the largest Arctic SST anomalies.”

In section 5.1 we discuss that this subset of the ensemble generally matches the SAT proxies best. No change was made here.

Review comments: Line 209: but did not you write that also the Bering Strait is closed in some of the models? We don’t see a particularly large anomaly around this area.

Reply: The Bering Strait is closed in the PlioMIP2 simulations (mentioned in line 122) as a part of both the standard and enhanced boundary conditions. Indeed, the closure

of the Bering Strait did not lead to a large SAT anomaly. Upon closer inspection, the largest SAT anomalies are mostly above the Baffin Bay, rather than over the Canadian Archipelago. The first part of the paragraph has been adjusted accordingly. “The greatest MMM SAT anomalies in the Arctic are found in the regions with reduced ice sheet extent on Greenland (Haywood et al., 2016), which generally show warming of over 10 °C and even up to 20°C. Additionally, temperature anomalies of over 10 °C are simulated around the Baffin Bay”

Review comments: Line 212: and thus? What causes such an increase in the Baffin Bay? The lack of sea ice due to no arctic waters flowing through the CA? If yes, it would be good to mention.

Reply: This line was meant as a description of the results and of the figure. While it would be interesting to know the underlying mechanisms for the warming in this location, and while we do discuss a potential mechanism later in the paper (AMOC), we did not mean to describe the causes of the temperature increase in the Baffin Bay here. No changes were made.

Review comments: Line 196 - 215: How does the discrepancy in land sea mask, especially in the Bering Strait, affect the interpretation of the MMM in Figure 2? I would find very informative to indicate which models closed the Bering Strait and or the Canadian archipelago in Table 1. It seems from Figure 2b that only a few models keep the Bering Strait open. Are the models with open Bering Strait the ones with highest SST and SAT values (e.g. In Fig.1)?

Reply: Sorry for the confusion, all models have a closed Bering Strait and Canadian Archipelago as part of the PlioMIP2 boundary conditions. We added “in the mPWP simulation” to line 122 to emphasize this and to avoid future confusions for other readers. The stippling in Figure 2b has been removed as it was found to be incorrect after comments from reviewer 1 and they became redundant in the updated version. Description of stippling in Figure 2 caption has been removed.

Review comments: Lines 272-289: How much is the MMM-proxy comparison valid in the Canadian archipelago? I mean, in Figure 7 the proxies there are very closed to each others (while already slightly shifted for better understanding) and, how many grid points are there in in the simulations this area? Is the comparison here valid? Or not resolutiondependent? Same for Alaska?

Reply: Valid point. Given the coarse resolution of global climate models it could be impossible for simulations of SAT anomalies to match all five reconstructions in the Canadian Archipelago. We added the following: "It has to be noted, however, that SAT anomalies are underestimated at three other sites within the Canadian Archipelago. Given the resolution of global climate models and the close proximity of the sites, it may be impossible for simulations to match all five of these SAT estimates."

Review comments: Figure 8: Since the beginning, there are two distinct groups amongst the models and the MMM is shifted to higher value because of 7 models. This discrepancy between the two groups is very neat. Thus I really wonder what are the causes of such dichotomy and what is the impact on the interpretation of the MMM in the paper in general?

Reply: Indeed, good spot, there are two distinct groups amongst the models. We would argue, however, that the first group consists of the five previously discussed models (CCSM4-Utrecht, CCSM4-UoT, CESM1.2, CESM2, and EC-Earth 3.3) when looking at the median bias (rather than the extent of the box-whiskers). We already mention here that these are the models with the highest Arctic SAT anomalies. We added some emphasis on these five models and that it may be interesting to uncover why they are simulating distinctly larger anomalies than the other simulations.

"Future research into the underlying mechanisms for the increased Arctic warming in these five simulations, compared to the remaining eleven simulations in the ensemble, may form a way to uncover factors that contribute to improved data-model agreement."

Added the following to the eleventh line of the abstract; "although the degree of under-

estimation varies strongly between the simulations”

Added the following at the second line of the conclusion: “although large differences in the degree of underestimation exist between the simulations. The models that simulate the largest Arctic SAT anomalies tend to match the reconstructions better, and investigation into the mechanisms underlying the increased Arctic warming in these simulations may help uncover factors that could contribute to improved data-model agreement.”

Review comments: Lines 320-321: but also models should also all use the same boundary conditions. Because if some fo the models do not close some fo the straits, or if they have no sufficient resolution to capture the width of some passages etc. . .how can we interpret the misfit between data and models correctly? I mean, as it is now, it is impossible to determine wether or not in some models the different boundary conditions or different physics affect the misfit and in which proportion. I know it is very difficult to modify the land-sea mask in coupled models and in some cases it will also require more computational resources to increase spatial resolution enough to capture the different gateways properly. However, at some points, we will need to do it to further advance those types of data-model exercises.

Reply: Sorry for the confusion. All models used the same boundary conditions, quoted from Haywood et al. (2020): “All model groups participating in PlioMIP2 were required to use standardised boundary condition data sets for the core midPliocene-eoi400 experiment”. We added a sentence in the methods section to emphasize this. “All model groups incorporated the standardised set of boundary conditions from the PlioMIP2 experimental design in their simulations (Haywood et al., 2016).”

Review comments: Figure 10: yellow and white squares are reconstructions from proxies? I guess yes. . . but this is not mentioned in the caption.

Reply: This information has been added to the figure caption. “Depicted squares represent the locations of the reconstructions and their respective colour the inferred mPWP

sea ice conditions at that location."

Review comments: Figure 11: is the vertical Y scale in frame b) the same as in frame a)? In any case, please add the ticks for dSAT values on the graph for projections.

Reply: Indeed, the Y scales are the same. It is a good idea to add the ticks in b) also, for added clarity. This has been done and the figure has been updated.

Review comments: Lines 377- 381: When reading those lines, it seems that only CO2 forcing matters here. But in many of your models, some gateways are closed, and as you cite Otto-Bliesner et al. (2017), this matters. . . Thus I disagree with the formulation of those sentences. Please also discuss the difference in Arctic geography and how this impact ton the comparison with the projections.

Reply: Indeed, CO2 is not the only forcing that matters. The dominant mechanism of warming in both ensembles is CO2 (for PlioMIP2 this can be found in papers from Tan et al. (2020), Chendan and Peltier (2018), and Stepanek et al. (2020)). We simply state here that this is the dominant mechanism of warming, but that there are additional mechanisms for warming in PlioMIP2. We discuss that this may be due to changes in Arctic ocean gateways or other changes in orography in the following sentence. No changes were made.

Review comments: Lines 396-400: Given the different boundary conditions, I find very difficult to make a direct comparison here. In most of PLIOMIP2 models, the Arctic gateways are closed and this generates a strengthening of the AMOC. While under modern geography, the Arctic gateways are open and a weakling of the AMOC is projected. You cannot compare those two situations here directly. In general, this short paragraph is not very clear. If you state more clearly at the beginning and in Table 1 that not all models prescribed closed gateway, this would definitely improve the reader understanding of the paper.

Reply: The main point of this paragraph is to show that there are differences between

the two ensembles, regardless of their cause, in AMOC strength and thus one of the mechanisms underlying Arctic warming. We mention in the previous paragraph that strengthening of the AMOC in PlioMIP2 is likely due to the closure of the Arctic ocean gateways. As the purpose of this paragraph is to highlight the difference, rather than investigate it, we did not make any change. Sorry again for the confusion that not all models have the closed gateway, they all do, and previous comments of the reviewer led us to put more emphasis on this to avoid confusion for future readers.

Review comments: Line 397: “This is consistent” To what does “this” refer to?

Reply: Indeed, we could make this more clear. We changed “this is consistent” to “The strengthening of the AMOC in the PlioMIP2 ensemble is consistent” and added a space to make it a separate paragraph. We also added “compared to the future climate ensembles” in the previous sentence for improved clarity.

Review comments: Subsection 7.3: To my opinion, it is very difficult to compare transient short-term projections variability with equilibrium climate variability of a few centuries (as just say line 440). Thus I find not very much straight forward and informative the conclusions from this comparison here.

Reply: Upon further inspection and thorough discussion, we decide to remove the section about the NAO/NAM. Based on comments of both Reviewer 1 and Reviewer 2. With the following reasons: - The results for both the PlioMIP2 and the RCP4.5 simulations are not very robust. There is a low signal-to-noise ratio. - The comparison of the PlioMIP2 and RCP4.5 simulations is significantly hindered by the different nature of the simulations: Equilibrium versus transient. As pointed out by reviewer 2. - The comparison is further hindered by the potential strong effect orography has on Arctic variability in the mPWP simulation. Hill et al. (2011) ascribed most of the change in the NAM they observed in the mid-Pliocene simulation to changes in orography. Since the changes in orography in PlioMIP2 are non-analogous with future climate change we do not feel that this comparison is useful.

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We therefore remove Section 7.3, and make appropriate changes in the abstract, introduction, the start of Section 7, and the conclusions to represent this.

Review comments: Lines 427-429: This sentence is very unclear, please reformulate.

Reply: Thank you for the comment. The section in which this sentence was stated has been removed based on earlier comments.

Review comments: Lines 455 - 458: You state about the discrepancies between mPWP and projections simulations: “firstly the incomplete manifestation of slow responses in transient simulations”. But not only, I would say also vice-versa: “the lack of transient variability in equilibrium climate”. Then you state “secondly the observed differences in Arctic climate features between the ensembles”: which ensembles are you referring too here? PLIOMIP1 versus PLIOMIP2 or PLIOMIP2 versus projections? If this is the second option, then I would say the entire sentence does not make sense because of course they are different, besides equilibrium versus transient, boundary conditions also differ. . .

Reply: Good point, there is a difference between comparing simulations of different climates, and different climates themselves. Indeed, both the nature (transient versus equilibrium) and boundary conditions of the ensembles differ. We focus on the differences in Arctic climate features we observe between the ensembles, and their implication for attempting to use mPWP simulations to learn about future climate change.

Changed the sentence to: “Lastly, we find differences in Arctic climate features between the PlioMIP2 ensemble and future climate ensembles, including the magnitude of Arctic amplification, changes in AMOC strength, and northern modes, which highlight that caution has to be taken when attempting to use simulations of the mPWP to learn about future climate change.”

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2020-64/cp-2020-64-AC2-supplement.pdf

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