

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

Anonymous Referee #2

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Review of manuscript “Evaluation of Arctic warming in mid-Pliocene climate simulations” - de Nooijer et al.

In the present manuscript, de Nooijert 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 been 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 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

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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, the authors could deepen their analysis to compare the different models together to provide partial answers to some of the questions posed

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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.

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...”.

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.

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

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.

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.

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.

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.

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g. In Fig.1)?

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 resolution-dependent? Same for Alaska?

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?

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.

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

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.

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

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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.

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.

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

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.

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

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. . .

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2020-64>, 2020.