Review of Zheng et al.

Zheng et al. examined the contribution of sea ice albedo effects and insulation effects on the Arctic amplification using EC-Earth climate model simulations of preindustrial and Pliocene forcing. The attribution of different effects is assessed by the climate feedback and response analysis method (CFRAM) and the equilibrium feedback assessment (EFA)-like approach. Their analysis showed that the ice albedo effects were related to the summer SST warming and the insulation effects were related to the surface air temperature warming in winter. The results of this study are important to the understanding of the Pliocence warming as well as to improve the projection for the present warming trend. The organization of this manuscript is clear and easy to follow. The clarity of presentation is one of my main concerns. Some confusion or ambiguity are likely due to language issues. I suggest the authors either carefully examine the manuscript for grammar issues sentence by sentence or consult a native speaker to improve the writing of the manuscript. The assumption of linearity is very important for the attribution assessment. The authors have not provided physical reasons to support this assumption. It would be useful to provide some uncertainty estimates due to the inherent limitation of the analysis methods. Therefore, I recommend a major revision to the manuscript before it can be accepted for publication.

Other main concern:

The experiment setup. (1) If the objective of this study is to understand the contribution of ice albedo effects and insulation effects on the Arctic warming, why is it necessary to use the Pliocene simulation in the current setup? In this case, there are lots of publicly available model output from CMIP3 and CMIP5 to provide more robust results. (2) As is stressed by the authors, the justification of performing the Pliocene warming simulation is the availability of the proxy data constraint from paleo-records. If the objective is to understand the Pliocene warming, however, the orbital forcing adopts preindustrial conditions. Does it make the justification of this Pliocene simulation irrelevant? The Pliocene is a long time period and authors have not stated clearly what period are simulated. Are the fixed CO_2 with PRISM surface conditions the default setting for the PlioMIP2 simulations? If so, please give a brief description of it and the justification for this choice. It will be very helpful for readers who are not familiar with the PlioMIP2.

Specific comments:

Line 14: Define PRISM.

Line 24: “CO2” to “CO_2”.

Line 24-25: This sentence seems to suggest the CO_2 increase is due to the monitoring. Please rephrase.

Line 25-28: Break up this sentence to two sentences and rewrite.

Line 26: “increased 1.1°C to” change to “increased 1.1°C compared to”.

Line 31: “its reflectivity”. Do you mean the sea ice reflectivity changes is responsible to changes in surface energy budget, etc.? Please rephrase to clarify.

Line 47-54: Some justification for Pliocene simulation is provided here. But the simulation in this study use mixed Pliocene and preindustrial conditions. Can you still use proxy data to verify the simulation? Some comparison with proxy data are shown in Fig. 1 but performance constraint by proxy data is still qualitative.

Line 146-147: It is not clear what “an approach” is referring to.

Line 150: “linear”: do you have any reason to believe it should be linear. Please provide some argument to support this assumption or reference previous studies to support your statement.

Line 162: The choice of student-t test assumes Gaussian distribution. However, it is usually not the case for geophysical parameters. The spatial and temporal autocorrelation are not taken into account. The bootstrap method adopted by Liu et al. (2012) is more relevant, especially when “moving-block” is used to represent the length scale of the spatial or temporal features (van de Poll et al., 2006; Z. Liu et al., 2010).

Line 174-179: It is not clear what message these sentences are trying to convey. Is the Arctic amplification stronger in Pliocene simulation or not?

Line 180: Not exactly the SST and SAT in “Pliocene epoch”. They are from the Pliocene simulations using some of the Pliocene conditions.

Fig. 1: The overlay of proxy data over the filled contour maps does not show the difference well. An additional map showing the differences might be more informative. Maybe add it to supplement?

Line 187: “off America”. Do you mean USA or the America continents? The coastal warming is not very obvious or extensive in either. The warming off the west coast of Africa is more obvious. By “permanent El Nino-like feature”, I expect to see extensive warming in equatorial eastern Pacific, which is not present in Fig. 1. Am I missing anything?

Line 197: “very sensitive” to what? “preindustrial” is used noun here and in several other occasions. Please add proper noun after it so that it is clear what it is referring to.

Line 200-201: Please rephrase this sentence.

Line 201: The definition of heat loss or heat gain is not given here. Consider move up the definition in line 204-208 before they are discussed.

Fig. 5: What are these dots? Are they differences between corresponding years in Pliocene and preindustrial runs? Does it matter how you pair up the years in Pliocene and preindustrial runs? Would it change the response coefficients? How much uncertainty would it introduce?

Line 242: How do you define "anomalies"? Are they with respect to the 100-year Arctic averages, Arctic monthly mean, or something else? Please describe clearly. Otherwise, it is impossible to interpret Table 1 to Table 4.

Line 252-253: This sentence is confusing. Please rewrite.

Line 263: Again, do you have any reason to believe they should be linear?

Line 274: What do you mean by "significant interaction"?

Line 276-284: The partition of ice-free and ice-covered conditions is confusing. How do you have SIC changes when it is ice-free?

Fig. 9: Is the regression line with respect to the ice-covered only? Would the regression line be different between ice-covered and ice-free? You would expect so because it is why you separate them in the first place.

Line 289-293: I am confused with the definition of ice-free and ice-covered changes relative to SIC. So I cannot follow the argument here. I will have to revisit after the authors' clarification.

Line 297-299: Does the melting of sea ice refer to the sea ice melt in spring or due to warming by high CO_2? Are you sure it is conduction not convection?

References:

Liu, Y., Key, J. R., Liu, Z., Wang, X., & Vavrus, S. J. (2012). A cloudier Arctic expected with diminishing sea ice. *Geophysical Research Letters*, *39*(5). https://doi.org/10.1029/2012GL051251

Liu, Z., Marchand, R., & Ackerman, T. (2010). A comparison of observations in the tropical western Pacific from ground-based and satellite millimeter-wavelength cloud radars. *Journal of Geophysical Research: Atmospheres*, *115*(D24), D24206. https://doi.org/10.1029/2009JD013575

van de Poll, H. M., Grubb, H., & Astin, I. (2006). Sampling uncertainty properties of cloud fraction estimates from random transect observations. *Journal of Geophysical Research: Atmospheres*, *111*(D22), D22218. https://doi.org/10.1029/2006JD007189