

Clim. Past Discuss., referee comment RC1
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Comment on cp-2022-63

Anonymous Referee #1

Referee comment on "No changes in overall AMOC strength in interglacial PMIP4 time slices" by Zhiyi Jiang et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2022-63-RC1>, 2022

Review of

No changes in overall AMOC strength in interglacial PMIP4 timeslices

by Z. Jiang et al.

Here the authors discuss a very important topic in paleoclimatology. In the last decades, the role of AMOC and its variability in the past has been researched on with emphasis on glacial and deglacial climates. Now there is a shift in the focus towards climate conditions more similar to pre-industrial. Hence, the topic is very timely and the study is very welcome.

The manuscript makes use of new available simulations by direct comparing the simulated AMOC regimes for the Holocene and the last interglacial. The study may be of interest beyond the modelling community and should thus provide more information and justifications for the approach taken.

The manuscript is well written starting with a helpful introduction. Overall I recommend publication with CoP.

Details:

Line 7: better introduce the examined time periods in the abstract instead of using experiment abbreviations such as lig127k.

Line 33: its not so clear to me how past changes in the AMOC can "... explain the recent global temperature changes..."

Line 38: during tWo interglacials

Line 38-39: midHolocene is the simulation's abbreviation, not an interglacial. The abbreviation lig127k should be defined (last interglacial is not mentioned before). Further, 6 and 127 ka are time slices no period. What is the rationale for choosing this time slices. With >10 kyrs of duration for the Holocene, what makes 6 ka so special? Why not e.g. 4.2 ka representing the onset of the last Holocene subdivision? Same for the Eemian.

Line 45: GHG is not introduced.

Line 46: "...interglacial period with orbital forcing being the main difference" to what? "..., while other forcing" which? "...remains very similar to thepiControl..." add space.

Line 61: what would be inappropriate methods?

Line 76: what is meant by "data availability". All available models have been used? Are there others? Why are these not available?

Line 77: 'msftmz' and 'msftmyz' are cryptic and non-introduced abbreviations not understandable by non-specialists.

Line 79: "The data from FGOALS-f3-L ..." this is a very detailed information about a single model output and should be moved from the main text.

Line 87: "... is computed as zonal average..." what does that exactly mean. From the very eastern to western grids coast to coast? Is there a water depth threshold?

Tab.1 : what is the significance of the Length parameter? It differs up to ~25 times

between the simulations. Already add references (from A1) here. I'm concerned about the huge range of resulting AMOCs for PI (12 to 33 Sv). Instead of showing the individual simulation compared to themselves in Fig. 1, it would be more helpful to show an inter-model comparison better reflecting the absolute uncertainties on the resulting AMOCs, as well as highlighting unrealistic outputs. What are the (likely) reasons for some models clearly under/over- estimating PI AMOC? I think this an important lesson we can learn from this study. In particular, when (from line 96 on) secondary fingerprints of AMOC on the wider climate patterns are examined. The relevance of modelled T and precipitation is questionable, when the underlying AMOCs are already inconsistent (e.g. in line 216 the authors claim that there is a strong relationship between AMOC changes and T).

Line 120: I can't see the feature described in front of "Fig.1" in th figure.

Line 130: "a changes"

Line 141: check grammar

Fig.1: the legend and caption of Fig.1 is very confusing. Actually whole Fig. 1 is confusing. E.g. what is the orange triangle? What are the Brierley et al 2020 simulations, and why are they shown at all? The numbers of symbols is higher than the simulations listed in Tab.1. Why are there no error ranges shown for individual simulations? Maybe indicate OSNAP and RAPID by arrow and name. Sometimes 30° and 50° is indicated sometimes not. Please revise the concept of the figure.

Line 156: what is meant by shallow and deep branches. Its not clear to me what the paragraph is telling us.

Figure 3: its hard to see what we can learn from this figure with the x-axes varying for each simulation. Please keep the axis constant to allow for inter-model comparison.

Line 181: grammar

Line 204: I don't understand this sentence.

Line 208: show(s)

Line 235: its not clear to me why the similarity to hosing experiments demonstrates any appropriateness.

Section 4.2: this part seizes a considerable part of the manuscript. In my opinion, this section should be shortened down and the study should keep the focus on AMOC itself, as the reader would expect from the title.

Figs. 4+5: what are the criteria for showing "selected" model outputs.

Fig.5 caption: what is "low contribution"?

Fig. 6: due to the hatched areas there is little to see, please revise concept of figure.

Section 5: compared to section 4 the discussion is too short and only comprises the comparison to observational data. Better insert here a detailed discussion on why the models partly differ so strongly from OSNAP, RAPID and from each other. Besides, it is interesting to see that paleoceanographic reconstructions by PA/Th indicate similarly constant interglacial AMOCs.

Line 276: Bradtmiller et al. 2014 focuses on Heinrich AMOC. Better cite the ground-breaking McManus et al. 2004 along with Yu et al. 1996 when it comes to introducing the proxy.

Line 279: There are a number of (low resolution) Pa/Th time series available, which, however, comprise more data points for the Holocene than the ones mentioned here: Negre, C., et al. (2010). *Nature*, 468; Lippold, J., et al. (2016). *Earth and Planetary Science Letters*, 445, showing indeed a faint AMOC weakening during the course of the Holocene. This is, however, NOT supported by both high-resolution studies, which indicate no observable change but stay fairly constant.

Line 282: Guihou et al. 2010 is not sufficient for referring to the existing data base. There are Böhm, E. et al. (2015). *Nature*, 517, Guihou, A., et al. (2011). *Quaternary Science Reviews*, 30, and Jonkers, L., et al. (2015). *Earth and Planetary Science Letters*, 419 covering this time period in higher temporal resolution, all indeed showing values in support of the statement made at line 283.

Line 285: I don't understand this sentence.

Lien 293: In the meantime there are a number of more sophisticated recent proxy-enabled models available giving more information on proxy related response uncertainties: Missiaen, L., et al. (2020). *Climate of the Past*, 16; Rempfer, J., et al. (2017). *Earth and Planetary Science Letters*, 468; Sasaki, Y., et al. (2022). *Geosci. Model Dev.*, 15; van Hulten, M., et al. (2018). *Geoscientific Model Development*, 11.

Line 314: This statement is very true and I expected more detailed explanations for this inconsistency.