

Interactive comment on “A comparative study of large scale atmospheric circulation in the context of future scenario (RCP4.5) and past warmth (Mid Pliocene)” by Y. Sun et al.

Anonymous Referee #2

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I recommend publication after the comments below are addressed in the manuscript.

General comments:

The authors do a nice comparison of the large-scale atmospheric circulation features – the Hadley and Walker circulations - and related precipitation patterns in simulations with the IPSL model. But fail in proving their premise (stated several times throughout the manuscript) that the mid-Pliocene (MP) might be a good analogue for future climates because for the MP, unlike the future, data is available to test the validity of the simulations. No data is shown! Although there is, of course, no data for the MP atmospheric circulation features, and as yet no reconstruction of precipitation, Salzmann

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et al. [Global Ecology and Biogeography, 17, 432–447, 2008] have published a biome reconstruction for the MP. The consistency of the simulated precipitation changes with those MP biome changes primarily influenced by precipitation should be discussed. This assessment will provide some confidence (or not) on the reliability of the IPSL simulated RCP4.5 precipitation and atmospheric circulation changes.

Additionally, Table 1 has a multi-model comparative analysis of the Hadley circulation properties, with discussion in the text of modeling consistency between the IPSL model and other models. Haywood et al., CP, 2012 show large spread of the MP precipitation changes, especially in the tropics, where changes in the Hadley and Walker circulations are important. Modeling consistency in terms of precipitation changes between IPSL and other models needs to be discussed. Ultimately, we want to understand, evaluate, and project the precipitation changes.

Specific comments:

P1450, L13-14: It is confusing having the “Hadley Cell (HC)” changing intensity at different latitudes. It is a “cell”. Preferable would be to indicate changes in the vertical motion – rising and subsiding branches of the HC.

P1451, L11: Suggest removing “at least” since you are giving a range of sea level high stand.

P1451, L26-28: Participating modeling groups were not “required” to complete both experiments.

P1452, L12: wording is awkward here. Not sure what is meant by “the multi-coupled models and corresponding atmospheric components proposed by PlioMIP”

P1452, L26-29: You should note that the Kamae et al., 2011 study presents an AGCM simulation with specified PRISM SST.

P1454, L3-26: Only include the resolutions and features of the IPSL-CM5A model that are being used in this study.

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P1455, L14: Explain “fixed inherent ocean boundary”.

P1456, L7-12: The details of the other RCPs are not needed here.

P1456, L16-20; Table 1: Clarify if for all six models compared, you use the last 50 years of 300-year RCP4.5 simulations. If not, you need to justify why this is still a valid comparison or delete.

P1457, L7-13: Before discussing the changes in the MSF in the MP and RCP4.5 simulations, you need to include a discussion on how well the IPSL PI simulation simulates the intensity and extent of the HCs as compared to observations.

P1457, L16-24; P1458, L1-10: See comment P1450, L13-14 above.

P1458, L17-21: Use southward and northward for shifts rather than S and N.

Section 3: You need to include whether the MP and RCP4.5 changes in intensity and latitudinal extent of the Hadley circulations are significant statistically as compared to the PI control or with each other.

P1458, L23+: Just because IPSL agrees in the location of maximum MSF with three other models does not make it right. First, the IPSL PI results need to be compared to observations (see comment above). This paragraph should instead discuss changes in the maximum MSFs and poleward extents of the northern and southern Hadley cells in the MP and RCP4.5 simulations for the models analyzed in Table 1.

P1459, L14: Explain that “omega” is the pressure vertical velocity so negative values represent ascending motion.

P1459, L19: The descending motion in the eastern Pacific seems to be concentrated along and just west of the Andes. Is this realistic?

P1459, L24: Better wording would be “large reduction” rather than “drastic decay”. Is the reduction in the upward motion in the tropical western Pacific warm pool statistically significant?

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P1461, L1-2: A sample size of 3 models agreeing is small. Too small to conclude that the “behavior of the Walker cell in future warm climate is robust”.

P1465,L9-10: Are the changes in precipitation in the subtropics consistent with the MP biomes in Salzmänn et al.?

P1465,L26-27: See comment P1450, L13-14 above.

Figure 2: panels c and d not needed. This information is already deducible from Panel b.

Figure 3: This figure is only useable if it can reproduced at much larger size in final paper. I found it difficult to read even after enlarging it to over 300% on my screen.

Technical comments:

I recommend that the Authors have a scientist who is a native English speaker copy-edit the manuscript.

Interactive comment on Clim. Past Discuss., 9, 1449, 2013.

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