Climate of the Past Discussions

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Comment on cp-2022-72

Anonymous Referee #2

Referee comment on "On the importance of moisture conveyor belts from the tropical eastern Pacific for wetter conditions in the Atacama Desert during the mid-Pliocene" by Mark Reyers et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-72-RC2, 2022

Title: On the importance of moisture conveyor belts from the tropical East Pacific for wetter conditions in the Atacama Desert during the Mid-Pliocene

Overall comment: I found this work very novel and interesting to read. The focus on moisture conveyer belts in the Atacama during the mid-Pliocene is an important contribution to understand the mechanisms of past and present rainfall events in the region. The experimental design is well accomplished, and I liked very much the use of SOM and clustering techniques for MCBs detection and pattern analysis.

I have some general comments for different sections of the manuscript:

Introduction

Authors mention that the increased rainfall in the Southern Atacama Desert is mostly duo to a northward displacement of mid-latitudinal westerlies and extra-tropical winter cyclones. In my opinion they cite literature that does not support this statement. For example, they cite Jordan et al., 2019 as evidence of southwestern moisture source but Jordan et al., 2019 identifies the tropical Pacific as the main moisture source of the March 2015 extreme rainfall event. Can please the authors clarify this inconsistency. Also, I noticed that Bartz et al., 2019 do not actually state a southwestern moisture source in their study, the same with Stuut and Lamy, 2017.

In line #80 authors state the hypothesis of the tropical Southeast Pacific as a moisture source for the Atacama but this was demonstrated in Bozkurt et al., 2016. It is possible to clarify how their hypothesis differs from the mechanism that triggered the events of March 2015? In its present writing form, it is not obvious the connection with Bozkurt et al.,

2016's findings.

Data and Methods

Can the authors please explain why using orbital parameters from the pre-industrial period and not the orbital parameters of the mid-Pliocene. Orbital forcing of later periods has proved to be useful in reproducing past climates. For example, Engelbrecht, F. A., and Coauthors, 2019: Downscaling Last Glacial Maximum climate over southern Africa. Quat. Sci. Rev., 226, https://doi.org/10.1016/j.quascirev.2019.105879. I understand that PlioMIP simulations use orbital parameters for 1850 but it would be very useful for the non-specialized community to understand why we are modelling the climate of mid-Pliocene using orbital parameters for present day. This forcing is not negligible as discussed by Willet et al., 2013 (Willeit, M., A. Ganopolski, and G. Feulner, 2013: On the effect of orbital forcing on mid-Pliocene climate, vegetation. Clim. Past, 9, 1749–1759, https://doi.org/10.5194/cp-9-1749-2013). This is important for ice sheets extension and therefore albedo and the global energy balance.

What is the actual bias of WRF historical run? As precipitation is very reduced in the hyper-arid core of the Atacama, simulated vs observed precipitation can have many orders of magnitude of difference. This is not a problem and is common in modelling studies, but I missed a more robust measure of uncertainty of modelling experiments using WRF_{hist}.

Results

It is not clear to me which proxy data was used to validate model projections. Maybe these is all due to the lack of proxy records for such a long period of time. I think this is important since the authors assure that CESM2 agrees with reconstructions, but they don´t provide any evidence of to which extent the model agrees with proxy data. The only reconstructions available are those provided by Dowsett et al., 2013? Still, if possible, authors can provide a measure of uncertainty in their modelling design. In modelling experiments for future projections, as an example, is very important to measure the level of uncertainty and therefore the model ensemble is used, and a range of possible climates is provided. I can guess authors did not use the ensemble because the mean precipitation tended to be lower than current climate (?). Still, the question is, if only one model is used, how can we be sure that CESM2 model results are not due to chance? At least authors should mention the limitations of using only 1 model.