

Comment on cp-2022-72

Teresa Jordan (Referee)

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-RC1>, 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

Author(s): Mark Reyers et al.

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Overall, this is an excellent contribution to paleoclimate studies of the Atacama desert of the west coast of South America, as well as a being a thought-provoking treatment of what future extreme events may arise in the Atacama Desert during a warmer climate future.

The largest excursion from hyperaridity in the Atacama Desert more recently than about 10 Ma was during the Pliocene, and this is the focus of Reyers et al. atmospheric modeling study. The proxy record has not firmly established most of the key parameters of the Pliocene wetter climate – was it only slightly wetter but over a very long period of time (e.g., 500,000 years) allow accumulated impacts on the landscape, or was it intensely wetter for some shorter period of time (e.g., 1000 years) which caused rapid changes of landscape features? Nor is it firmly established the timing of the wettest interval. Some data point to the wettest interval in the 20°S Atacama at the very beginning of the Pliocene (e.g., Jordan et al., 2014; Evenstar et al. 2017), thus not coinciding with the mid-Pliocene boundary conditions used by Reyers et al. (their conditions are appropriate to 3.2 Ma. Nevertheless, given the dearth of rigorous atmospheric studies that attempt to evaluate the warmer conditions of the Pliocene, I find Reyers et al's analyses to be both novel and important. In light of the lack of detailed proxy understanding of the Pliocene wetter climate extracted thus far from the geological record, perhaps the authors should state in their conclusions that the applicability to interpreting paleoclimate history also requires improved chronological resolution and more advanced proxy data.

Read as a person who is not an atmospheric scientist, I find the premises and methods to be clearly stated, and the nature of the experiments to be very good choices for both Pliocene conditions and with reference to the March 2015 extreme rain event case study. With only minor exceptions, the results presented support well the interpretations and conclusions.

The authors' choices of features to illustrate in figures and the simple clarity of the illustrations are very good. The connection between topics in the text and corresponding figure was easy to match.

The choices of materials to reference is suitable. In a few cases in which I know well the paper that is cited I recommend that the authors re-examine the paper cited and more clearly (or accurately) represent its conclusions.

Abstract: Easy to understand

Data and Methods, section 2:

The authors describe very clearly their analysis and machine learning methods. These topics are entirely outside of my expertise, yet I could follow the description easily.

Introduction: This frames the problem well, from a geological perspective.

A sentence spanning lines 54- 57 describes a prior interpretation that atmospheric circulation over South America east of the Andes controlled increased precipitation in the Atacama in the past, ie, paleoclimate. The citation of references is ambiguous. On first reading, it appeared that Jordan et al. (2019) and Amidon et al. (2017) both advocated this interpretation. However, Jordan et al *described* the previous literature on this topic but concluded that it is incorrect for the situations in the Atacama Desert of interest in their paper. (Amidon et al's paper focus on the eastern flank of the Andes, where perhaps the interpretation is correct.) While the authors of that paper are pleased that their description could be understood and contributed to thought, the citation should be changed to make clear that they conclude it to be incorrect.

A sentence spanning lines 66 to 68 treats the March 2015 extreme rain event and cites isotopic Jordan et al (2019) for relating that tropical Pacific-sourced event to a paleoclimate hypothesis. Because Jordan et al (2019) did not present isotopic data for paleoclimate proxies, the citation is somewhat distorted. Were a citation to be added to either Herrera and Custodio (2014) or Herrera et al. (2018), then the paleoclimate proxy isotope data would be encompassed and Reyers et al. intent for the sentence could be better fulfilled.

Herrera, C., and Custodio, E., 2014, Origin of waters from small springs located at the northern coast of Chile, in the vicinity of Antofagasta: *Andean Geol*, v. 41, p. 314-341.

Herrera, C., Gamboa, C., Custodio, E., Jordan, T., Godfrey, L., Jódar, J., Luque, J.A., Vargas, J., and Sáez, A., 2018, Groundwater origin and recharge in the hyperarid Cordillera de la Costa, Atacama Desert, northern Chile: *Science of The Total Environment*, v. 624, p. 114–132.

Line 79: Please remove the citation of Jordan et al. (2019). That paper advocates that the tropical Pacific is a key contributor to moisture in the Atacama Desert, and only describes the sources promoted by the other authors for completeness. Jordan et al's analysis aligns better with Böhm et al 2021 than with the other papers listed, and could be cited at the end of the sentence which terminates in the middle of Line 80.

Line 95: One of the two words with the same root, "description" and "described" should be removed, and the sentence slightly rewritten.

Line 121. I believe this is the first call for Fig. 1. The caption for Fig. 1 should specify what "surface temperature" refers to. Sure, among atmospheric scientists perhaps it is understood that this means "air temperature 1 m above the surface, irrespective of whether the surface is water or soil". But the paleoclimate-geologist does not know (and may have guessed incorrectly.)

Results

Section 3.1: Mid-Pliocene against present-day climate

Line 177. It seems appropriate to also refer to empirical data for the magnitude of temperature anomaly during the mid-Pliocene.

At the end of this section (Lines 209-210) the Atacama is split into two sectors, N and S, for further analysis. The choice of boundary between the two sections and the E-W dimensions of the sectors corresponds well to both modern and paleo-climate subdivisions.

Line 210-211. A phrase should be added to remind the reader what are the "things" of which there are 15 in both the mid Pliocene and historical simulations (this is the number of symbols). It is clearly stated that each "thing" is a spatial mean. However, it is not stated whether those 15 "things" are computational repetitions of the same model (like a Monte Carlo simulation) or something else (is 15 the maximum number of days it rained?). Perhaps this was explained in the Data and Methods sections, but the reader needs clarification at this point in the text.

Section 3.2; Potential drivers for stronger rainfall events in the mid-Pliocene

Figure 4: It would be slightly easier for the reader if, within the boxes that contain (a) and (b), the words "north" and "south" occur. Yes, that information is in the caption. But for the person glancing repeatedly at the figure while reading the text, this added label would be advantageous.

Line 242: The text should refer specifically to the inset within Fig. 7. The feature emphasized in this and the next sentences cannot be seen in the large-area plot.

Line 250 and caption for Figure 8: The first appearance of the word "cluster" in the sentence (line 250) and caption should be plural (i.e., "clusters").

Figure 8. It seems peculiar that the most common cluster, with 79 occurrences per winter (cluster 8), has 0 kg/m-s of water vapor. I deduce this from the lack of any color on the yellow-green scale. Has color been accidentally omitted? Perhaps the color bar needs a different stretch so that a non-zero value is visible? If 0 is the correct interpretation of Fig. 8 cluster 8, then the caption and text ought to mention this. At present, Line 252 discusses this cluster with reference to easterly IWVF, which I do not see.

Line 254. Clusters 1, 2, 5, 7, 9 for the mid-Pliocene are described as similar. To my eye, 7 does not belong in this group. Its IWVF looks more like 3 or 4. This leaves the impression that the designation of "zonal" is quite arbitrary. It would be appropriate to describe the differences in the systems responsible for clusters 3, 4, and 7 (e.g, was it SST that differences? Or wind strength?).

Line 258-260. Clusters 1, 4, 6, 7, 8 for Historical are described as similar. The text speaks of "landfall south of 25°S". Was that also the criteria used when comparing the Mid-Pliocene clusters, but was not stated?

Line 263. The text should note that the "magnitude" considered is integrated water vapor.

Line 263. The text states that the Historical MCBs that provide most of the rain (Fig. 9, clusters 3 and 9) are shifted south relative to those of the mid-Pliocene (Fig. 8, clusters 4 and 6). I do not see a significant latitudinal difference. The long axes of the mid-Pliocene clusters cross the 80°W meridian at about 18 and 19°S; the long axes of Historical clusters cross 80°W at 21°S and 17°S. If this is significantly different, the authors need to demonstrate the significance statistically.

Lines 264-265. It is stated that the MCBs of the mid-Pliocene are of clearly different origins than present-day. Clarification is needed of this statement. It seems to me that a few degrees of latitude would not constitute a "different origin" unless those few degrees place the air mass transport path over significantly different parts of the ocean, for instance, markedly different SST. If there is an important difference of this type, the authors should state this as justification of the statement that the origins are "clearly different."

Line 268. The phrasing is unclear. I believe it would be correct if written "Examples of the wind fields at 4000 m asl. are shown in Fig. 8 and 9."

Lines 286-288. This sentence is correct in detail (Bozkurt et al. did evaluate the impact on rainfall had the SST been lower in late March 2015), but contorts the logic relative to the purpose to Reyers et al. The reader must know independently that the SST was anomalously high during late March 2015 in order to understand that Bozkurt et al. study a real versus hypothetical system opposite to the current study. I think this can be rewritten for greater clarity.

Line 286. Bozkurt et al. specified that there was uncertainty about how SST impacted the March 2015 event, whether through activity in the marine boundary layer or through convection. This new paper specifies that the MCB is not expressed in the marine boundary layer. Is there a value to the authors reflecting back on this aspect of the Bozkurt et al. paper?

Line 295. A verb and adverb are missing. Insert "are" in the phrase "occur in the mid-Pliocene but ARE not PRESENT during present-day conditions".

Lines 300-304. It should be noted, however, that the isotopic composition of the March 2015 rain at coastal sites clearly indicates that the rain in that event had a tropical Pacific water source. This has not been made compatible with the B2021 atmospheric models. (Data presented in Jordan et al., 2019).

Conclusions and Outlook

Line 308. As a generality, in the Conclusions the authors should speak of the full names of features rather than using acronyms. Some readers will only look at the Conclusions, not all the preliminary material, and they will not gather anything useful by learning that MCBs did something. They will learn more if the statement is that Moisture Conveyor Belts did something.

Lines 391-395. The Bozkurt et al. references is duplicated.