

Response on RC2

Dear Climate of the Past editorial board, dear authors,

As requested, I read and reviewed the manuscript titled “Cooling and Rainfall Isotope Composition During the Last Glacial Maximum in the Low-Latitude Eastern Canary Islands: Insights from Carbonate Clumped Isotopes in Land Snails” by Haylay Bricker and co-authors. In their work, the authors describe clumped isotope analyses on land snails from the last glacial maximum (LGM) found on Lanzarote (Canary Islands) in the northern low-latitudes of the Atlantic Ocean. They use these data to infer differences in surface air temperature between the LGM and the present-day and compare these anomalies to those simulated by the HadCM3 climate model.

Overall, I think this is a well written manuscript about a good quality dataset. The discussion does a good job of placing the results in a wider (paleo)climatic context, even though the data itself has limitations in terms of space and time. I believe this manuscript would be a good fit for Climate of the Past, and most of my feedback in relatively minor and will not impact the general discussion and conclusions of the study. I organize my feedback below, starting with some general points followed by line-by-line comments that occurred to me while reading through the study.

General remarks

It surprised me a bit to find that there were no local co-authors, or at least authors associated with institutes on the Canary Islands, involved in the study. There are also no local scientists named in the Acknowledgements as far as I can see. The study includes a lot of local context to the sampling and the location which is very helpful for the discussion, and this seems to be the type of information that would be hard to obtain without local collaborators. I do not want to interfere with the authors’ choices with whom should or should not deserve co-authorship on a manuscript like this one, but it does make me curious about whether all due credit is given for the study. There probably is a good explanation for this, so perhaps the authors could comment on the way the samples and associated information were collected.

Thank you for acknowledging that place-based studies should include local researchers. The second author of this manuscript, Dr. Yurena Yanes, is from the Canary Islands, collected the samples herself (and this collection is noted in the manuscript as well as cites the 2013 paper she wrote with the original data set), and moreover has a body of work devoted to characterizing endemic land snail populations across the islands. We believe we have given credit appropriately. The scope of the present manuscript (clumped isotope analyses and analysis of the results) was completed at UCLA among the coauthors otherwise credited.

I found that including results from the isotope-enabled model was very useful for the discussion and to place the isotope data in a wider climatic context. I also really enjoyed the discussion on what plausibly drives the (dis)equilibrium precipitation of shell carbonate

using the snail body water model with different values for relative humidity. This also leads to a nice discussion of local effects and the sampling of different water sources by the snails.

While these discussion points are thoughtful, I do think that overall the authors should probably consider slightly reordering the discussion section to explain earlier on how they see a potential seasonal bias on isotope measurements play a role in their discussion. I include some more detailed instances of where I thought this discussion becomes relevant during my reading of the manuscript in the line by line comments below.

Line-by-line comments

Line 42-43: I do not contest this statement on the spatial bias of climate reconstructions, but it would be useful if the authors could support it with a reference to a source where reconstruction locations are mapped to show it.

Thank you, we have added a citation to support this framing.

Line 73: Figure 1 is very clear and helpful!

Thank you!

Line 115-116: Very useful references that should be included here (or later in the clumped isotope section) are the recent studies by Guo et al. (2024) and Zong et al. (2026) who also used clumped isotope measurements in land snail shells to reconstruct LGM conditions, albeit in China not in the Atlantic region.

Guo, J., Ziegler, M., Wanders, N., Vreeken, M., Yin, Q., Lu, H., Fuchs, L., Dong, J., Sun, Y., and Peterse, F.: Robust land surface temperature record for north China over the past 21,000 years, *Science Advances*, 10, eadj4800, <https://doi.org/10.1126/sciadv.adj4800>, 2024.

Zong, X., Guo, J., Yang, W., Dong, J., de Winter, N. J., Yan, H., Song, Y., Sun, Y., Peterse, F., and Ziegler, M.: Unlocking sub-annual hydroclimate and temperature variability through land snail shell records, *Geochimica et Cosmochimica Acta*, 416, 168–182, <https://doi.org/10.1016/j.gca.2026.01.018>, 2026.

We appreciate these suggestions, and have added them to the bibliography.

Line 132-138: I wonder if all the references are needed here. I can think of more examples, but perhaps the authors can pick a few (max 5?) studies to cite here which are more or less representative of the field.

We have reduced the references to a maximum of 5.

Line 168: “and therefore dominant” I am not sure why this is added. The signal is indeed averaged over the seasons, except when careful subsampling of snails is done (as in Zong et al. cited above).

We have edited to remove the redundancy.

Line 227-228: “individuals are to be active year-round” This statement makes it seem like the authors infer this conclusion from the climatology of Lanzarote and the knowledge that this species is generally active within the temperature and humidity conditions on the island. Are there actual observations of the species being active year-round? If so, the authors should provide a reference for this here, as this factoid is important for the interpretation of bulk shell-based temperature reconstructions.

Yes, we have grounded this assumption in the work of second author Yurena Yanes, who observed individuals alive before personally collecting them from the site herself.

Line 235-236: I do not understand why this additional samples were only referenced in the appendix. I assume no isotope measurements were done on these? If there is isotope data for these specimens, that would be a valuable addition to this manuscript. But of course, it is up to the authors to decided whether that is within the scope of this study

We have removed them – initially, we sought to include the mid-Holocene sample set to simulated mid-Holocene conditions, but because the temporal offset between the age of the samples and the time range the model simulated was substantial, we chose not to. Further, incorporating another set of proxy-model comparisons proved cumbersome to the readability of the manuscript. The samples were included at the request of author Aradhna Tripathi but placed in the supplement. According to the guidelines set forth by the journal, supplemental material can only be included if it is mentioned in the main manuscript.

Line 262-268: I believe this description of (especially) the clumped isotope analysis and data processing is very concise and does not facilitate easy replication. For example, it is common practice in the carbonate clumped isotope community to report D47 values relative to the I-CDES reference frame using ETH standards (Bernasconi et al., 2021). It is not clear if the D47 values reported here are in that same reference frame, which is a prerequisite for inter-study comparison and for application of the Anderson temperature calibration used by the authors. I appreciate that the methodology is explained in more detail in a previous study, but I would still strongly recommend the authors to provide this information here in the Methods section so it is clear how the values the authors report can be compared.

Bernasconi, S. M., Daëron, M., Bergmann, K. D., Bonifacie, M., Meckler, A. N., Affek, H. P., Anderson, N., Bajnai, D., Barkan, E., Beverly, E., Blamart, D., Burgener, L., Calmels, D., Chaduteau, C., Clog, M., Davidheiser-Kroll, B., Davies, A., Dux, F., Eiler, J., Elliott, B., Fetrow, A. C., Fiebig, J., Goldberg, S., Hermoso, M., Huntington, K. W., Hyland, E., Ingalls, M., Jaggi, M., John, C. M., Jost, A. B., Katz, S., Kelson, J., Kluge, T., Kocken, I. J., Laskar, A., Leutert, T. J., Liang, D., Lucarelli, J., Mackey, T. J., Mangenot, X., Meinicke, N., Modestou, S. E., Müller, I. A., Murray, S., Neary, A., Packard, N., Passey, B. H., Pelletier, E., Petersen, S., Piasecki, A., Schauer, A., Snell, K. E., Swart, P. K., Tripathi, A., Upadhyay, D., Vennemann, T., Winkelstern, I., Yarian, D., Yoshida, N., Zhang, N., and Ziegler, M.: InterCarb: A Community Effort to Improve Interlaboratory Standardization

of the Carbonate Clumped Isotope Thermometer Using Carbonate Standards, Geochemistry, Geophysics, Geosystems, 22, e2020GC009588, <https://doi.org/10.1029/2020GC009588>, 2021.

We have added the Bernasconi citation and clarified the reference frame that samples are reported in.

Line 280-281: I believe I have read this statement about the maximum relative humidity being ~91% three times in the manuscript by now, so I believe there is some redundancy. Perhaps the authors can double-check whether statements like these are repeated more than needed throughout the manuscript and remove some iterations.

Thank you for pointing out the redundancy, we have removed the repeated mention.

Line 283-284: Redundant sentence, the sensitivity analysis for varying humidity has just been introduced at the beginning of this paragraph.

Thank you, we have removed this redundancy.

Line 304: Please explain in a bit more detail what is meant by the “z-score method” here. How many standard deviations were used as a cutoff for considering outliers?

We have elaborated on the z-score method in the text. Three standard deviations were used as the cutoff.

Line 311-319: I believe this paragraph is better placed in the Introduction or Background section than here in the Methods.

We have moved it to the Background section.

Line 334: It is not clear to me which “reference” the parameter “n” represents. Perhaps this requires a bit more explanation.

An explanation has been added to the text.

Line 335-336: As far as I can judge, the function does not have a division, so the reference to the “numerator” and “denominator” is ambiguous here. I suspect this is a formatting issue that happened during processing by the submission system, please double-check.

Thank you for catching this formatting error – it has been fixed.

Line 350-365: I wonder if this paragraph is not better placed in the Discussion, as it does not in itself present new results.

We have moved this to the discussion section.

Line 367-368: It would be helpful to cite the temperature reconstructed from the outlier here in the text alongside the temperature range so the reader can judge this statement.

We have included the outlier temperatures, and also amend that two outliers were calculated from the distribution.

Line 410-414: I think care should be taken with this line of discussion, as it seems the authors use the proxy-model comparison to say something about growing season. Since the latter underlies the proxy interpretation, this line of reasoning risks circularity. I think the authors have put forward enough evidence to make year-round snail shell growth plausible, so perhaps this line of evidence is not necessary.

We agree with your assessment that it undermines our previous line of reasoning, so have removed this paragraph.

Line 443-462: I quite like this part of the discussion and the way the authors model the d18O of precipitation here, even though the approach has its assumptions. The uncertainty about equilibrium precipitation of the snail shells is handled and discussed well by this RH-modelling approach, but there remains some uncertainty regarding the potential seasonal bias of bulk shell clumped isotope measurements. I do not think this issue is likely to change the overall conclusions about the difference in d18O of precipitation between LGM and present-day, but do think the possibility that D47 measurements in the different time periods may not sample the same seasonally weighted average should be briefly acknowledged here.

We have added this caveat to our conclusions.

Line 464-467: The uncertainty on d18O of precipitation should be included in this figure for completeness' sake. It is mentioned in the text, and could be easily added here. In addition, please clarify in the text whether this uncertainty propagates uncertainties from both the d18O measurements and the D47 measurements. This aspect is often forgotten in studies reporting D47-based d18O_{water} reconstructions.

We have amended the figure to include uncertainty. Please note that both outliers have been included, as in a previous figure only the outlier at T = 8 was included.

Line 471-473: I fully agree with this statement, but as mentioned above it is worth adding here that this uncertainty on the seasonal scale also factors into the comparison of annual averages due to potential seasonal bias. This effect is probably not very impactful on the general conclusion of the study, but it cannot be ignored.

To resolve this comment, we reordered the discussion as suggested below.

Line 484-501: There is some discussion here of the seasonality effect I flagged in my previous comments, but it comes a bit late. Perhaps moving this discussion up before the proxy-model comparison gives the discussion a bit better structure and does not leave the

reader (such as myself) with this question about potential seasonal bias while reading the discussion above.

We have reordered the discussion points.

Line 567-570: The paper by Zong et al. (2026) I refer to above is a good example (in another region) of this proposal for further research using high-resolution snail carbonate clumped isotope measurements and could be referred to here as an example.

Thank you, we have added this citation.