nature portfolio

Peer Review File

Onset of summer aridification and the decline of *Homo floresiensis* at Liang Bua 61,000 years ago

Corresponding Author: Professor Michael Gagan

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Professor Gagan,

Your manuscript titled "Disappearance of *Homo floresiensis* from Liang Bua alongside seasonal aridification of Flores 61,000-47,000 years ago" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

The key editorial thresholds for revision include:

1. Strengthen the link between the speleothem-based rainfall reconstruction and in-situ paleoenvironmental data, from the hominin site. In specific discuss your findings in relation to previously published faunal records. Referee 1 has raised multiple significant concerns, including potential misrepresentation and incorrect citation of previous research. A thorough response and/or detailed rebuttal addressing these issues is required.

2. Validate the speleothem-based rainfall reconstruction by comparing it with independent hydroclimate proxies from the region and/or climate models.

3. Provide a more structured explanation of the binary geochemical mixing model to enhance clarity and accessibility.

We hope you will find these comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Jiaoyang Ruan
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Carolina Ortiz Guerrero, PhD
Associate Editor
Communications Earth & Environment

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• Life sciences
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(https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This manuscript is essentially about attempts to reconstruct past climate events (in this case, rainfall) during the late Pleistocene using d18O and Mg/Ca values as measured from a speleothem sampled from the Indonesian island of Flores. However, the authors try to go considerably further by using their reconstructions to make a number of claims about the disappearance of Homo floresiensis, an extinct hominin species that tends to attract a lot of attention whenever anyone tries to say something new about it. I want to be clear that I am not a climate scientist but I have expertise in human evolution and how various lines of evidence (including those from paleoclimatic and paleoenvironmental data) are used correctly or incorrectly to interpret the hominin fossil record.

From my reading of this manuscript, the authors suggest that measuring both "... Mg/Ca and d18O in speleothems potentially enables the reconstruction of summer and winter rainfall patterns through time" because speleothem d18O by itself provides insights into broad-scale monsoon circulation rather than local rainfall amounts. The key words here are 'potentially enables'. I was expecting to read some kind of validation of this idea with a well-known or perhaps established speleothem record elsewhere that has additional supporting lines of evidence that are independent of any speleothem sequence itself. However, that does not appear to be the case here. Instead the authors suggest they are validating this idea using the main dataset of their study. This doesn't sound like validation of their idea to me but perhaps this reflects my lack of expertise in climate science. So I was rather curious to see how their results match up with what is known from the faunal record at Liang Bua, which is the site that the authors want their data and results to speak to.

In their Discussion, the authors claim that their results are consistent with stratigraphic indicators and the murine rodent fossil assemblage at Liang Bua, citing Westaway et al. (2009) (citation #85) and Veatch et al. (2019) (citation #86). I went through these cited articles carefully and failed to find the corroborative support the authors have suggested exists. First, the stratigraphic interpretation of Westaway et al. (2009) was entirely revised in 2016 by Sutikna et al. (citation #4) so it is completely unclear what evidence the authors may be referring to here. Second, Veatch et al. (2019) showed that prior to ~60 thousand years ago, the murine rodent assemblage at Liang Bua was dominated by medium-sized rats and Komodomys cf. rintjanus, specifically, which is a species known to prefer drier, more open habitats. This is completely opposite to what the authors state on lines 353-357: “During Phase 1 (92–76 ky BP), mean annual rainfall was higher than at present and seasonality was low. Using the current climate as a guide, the favourable MIS5 rainfall regime would have sustained perennial base-flow in the Wae Racang river. This is consistent with stratigraphic indicators (85) and the murine rodent fossil assemblage (86) at Liang Bua, both of which reflect relatively wet, forested conditions at this time, thus freshwater availability was likely unconstrained.” There doesn’t seem to be any indication of significantly wet, forested conditions around Liang Bua based on the faunal record from the site in the indicated temporal range of 92–76 ky BP.

For Phase 2 (76–61 ky BP), the authors suggest this temporal period was “marked by falling sea-levels and drier conditions” and “punctuated by abrupt increases in summer rainfall at 74, 71 and ~68–62 ky BP.” They further state that “The fossil record at Liang Bua supports this; the rodent assemblage shows a significant increase in open-habitat adapted species around 69–62 ka (86), and the concentration of H. floresiensis and Stegodon remains peaks at ~74–61 ka (4–6,85).” However, there is no significant increase in open-habitat adapted species around 69–62 ka (unless of course time runs backwards and that doesn’t happen in reality!), as all deposits prior to ~60 thousand years ago at Liang Bua were dominated by Komodomys cf. rintjanus as noted above. I was also unable to find any evidence indicating that Homo floresiensis remains are concentrated at ~74–61 ka; the primary research articles on this topic all state this is ~100–60 ka.

Although the authors’ Phase 2 reasonably matches the reported faunal sequence at Liang Bua, their Phase 3 (like their Phase 1) again does not, despite what the authors state: “Phase 3 (61–47 ky BP) encompasses the relatively dry MIS3 onset and lowest mean annual and summer rainfall on record (58–55 ky BP). An abrupt change in the rodent size distribution at Liang Bua around 60 ka indicates a return to more closed habitats (86), consistent with a return to lower rainfall seasonality.” Closed habitats are ones that are densely forested, and dense forests are not consistent with a return to lower rainfall seasonality. I fail to see the authors argument here. Again, the rodent record from this temporal period at Liang Bua suggests that local conditions around the site were opposite to what the authors interpret for their Phase 3 (61–47 ky BP). Why would the most arid-adapted rat species known on Flores disappear almost entirely from the faunal record during the time when the conditions were supposedly the most favorable? Why would forest-adapted rats return in greater numbers instead?

Clearly there are some major discrepancies between these two very different lines of evidence (faunal records on one hand, speleothems on the other) about the local conditions of an archaeological site on Flores many tens of thousands of years ago. I think most scholars would agree that the fauna (and the rats in particular) provide the better local signal in this case. They are animals that lived and died around the site and they were buried in the same deposits as the Homo floresiensis and pygmy elephants. The paleoclimate reconstructions from this speleothem may well be correct but these are likely more appropriately interpreted in a wider, regional context rather than such a specific, local one. This comes back to my earlier comment about validating the model used here for interpretation. If one uses the faunal record a potential validation tool, then this particular model of using Mg/Ca to reconstruct local seasonal rainfall fails demonstrably. Paleoclimate data such as provided by the core elements of this study are extremely important and valuable for our broad understanding of past events, but in this case I think these data are being stretched too thinly to apply to a specific problem that they are unable to reliably speak to.

Reviewer #2 (Remarks to the Author):

Gagan et al. presents a high quality study that provides a well-structured and insightful reconstruction of past rainfall amount and seasonality changes based on speleothem d18O and Mg/Ca from Flores, and further links the climate change with during H. floresiensis extinction. The study is quite novel as it is probably (as far as I know) the first to systematically utilize Mg/Ca and d18O to isolate summer and winter rainfall signal, and quantitatively estimate rainfall amount and its seasonality based on binary isotope mixing model.

The authors have done rigid step-by-step argumentation on the application of the methodology, including 1) careful evaluation of the prerequisites to apply the method; 2) drip water-rainfall-speleothem d18O agreement and reproducibility test; 3) validation of PCP from perspectives including Mg-Sr covariation, correlation, and a Rayleigh Fractionation model, 4) careful application of the isotope mixing model and calibration, as well as cautious consideration of potential uncertainty sources, and 5) comparison with model outputs and cross-validation between Fossil Stegodon and speleothem $\delta^{18}O$ on the range of value and aridity trends. The manuscript also shows the best efforts made to connect the climate with the extinction based on correlation as well as more mechanical causal linkage. I therefore suggest ‘accept with minor revisions regarding below comments and suggestions’ for the manuscript.

Comments and suggestions:
The paper relies heavily on multi proxies of one single stalagmite, although they agree well among themselves, it is still necessary to compare the original/reconstructed hydroclimate changes with other regional hydroclimate records to validate the trends and identify any potential biases. Borneo speleothem records (both $\delta^{18}O$ and δ13O) and some marine/lake sediment (SST, $\delta^{18}O_{sw}$, trace metal, and compound-specific d13C/dD) should cover this period.

I have spent quite some time trying to understand the binary mixing model. I suggest either break this section into smaller parts or providing a step-by-step guide to improve the readability. A clearer justification on the corrections for periods when divergence occurs and Equation 13 does not apply would be helpful. The overall procedure should be briefly in the method section of the main text and slightly more detailed in the supplemental file, potentially by using a flow chart/summary diagram that summarizes the sequence of the steps or conceptualize the framework.

Could other proxy like Sr/Ca and Ba/Ca be potentially helpful in validating the estimates and enhance their robustness? The effect of fractionation can be added in the captions to describe

A sensitivity analysis, by systematically varying key input parameters like the non-convective d18O and endmembers, can be applied to assess their impact on reconstructed rainfall estimates.

The study can benefit from a brief discussion on the potential/effort of applying the method using modern/younger stalagmites (with decadal resolution) with rainfall amount observation directly comparable and whether such application would be comparable to or helpful for the construction of the older portion.

Reviewer #3 (Remarks to the Author):

Gagan et al. present a new speleothem multiproxy record of δ18O and Mg/Ca spanning 91.4~46.8 ka to determine both summer and winter rainfall. One of the most significant highlights of this study lies in the quantitative reconstruction of summer and winter precipitation in Flores through the Geochemical modelling of the Mg-18O system. The attempt to quantify paleo-precipitation using speleothem proxy indicators proves both challenging and methodologically essential. Furthermore, the authors have combined the reconstructed precipitation records with additional stratigraphic evidence to investigate the climatic drivers behind the disappearance of the H. floresiensis. Overall, I think this manuscript should be published published.

However, this manuscript has some issues worth further attention by the authors. Some suggestions are listed below:

1. Lack of stalagmite δ13C record. The δ13C record can provide a wealth of additional information at the local scale, such as vegetation, water-rock interactions, and PCP, thereby supporting the variations in Mg/Ca ratios.

2. When verifying the rainfall reconstruction results, the comparison is mainly made with the simulation results of climate models. There is a lack of comparison with other independent geological records, such as pollen and lake sediment records. The mutual corroboration of multiple geological records can more strongly support the reliability of the research conclusions.

3. In the author's affiliation section, the 9th institution "Xi'an Jiaotong University" is misspelled.

4. Line22-25
Homo sapiens arrived on Flores Island later than the time when Homo floresiensis went extinct. However, the time gap between these two events is not significant. Moreover, these two time points are merely the results of dating the geological carriers that have been found. Considering the errors in dating, it is not entirely possible to rule out the temporal and spatial overlap between these two hominid species.

5. As shown in Supplementary Figure 3, the variation amplitudes of Mg/Ca and Sr/Ca exhibit significant differences during the period of 54.4~48.6 ky BP, whereas they were consistent prior to this interval. The reason for this discrepancy is not explicitly addressed in the text.

6. Line 185 mentions that the precessional changes are evident in the K2 δ18O record, and there is no comparison graph of δ18O with insolation, which could be added to Figure 4b.

7. Line 271-282.
When assessing the accuracy of reconstructed precipitation, is it appropriate for the authors to draw a simple analogy between the changes in reconstructed precipitation and those in model simulations for different periods? For example, comparing MIS4 with LGM, and H1 with GIS 20 and GIS 19.2.

8. Line392-398
Here it is mentioned that the time of the volcanic ash layer (T3) is very close to the time of the Homo floresiensis extinction, but only briefly refers to the impact of volcanic activity. Such a thickness of volcanic ash may represent a very intense volcanic eruption. Compared with climate reasons, is it more likely to be the direct cause of the extinction of Homo floresiensis? Is the short - term interference of volcanic ash generated by volcanic activity on the chemistry of cave dripping water and the climate response reflected in the oxygen isotopes and trace elements of stalagmites?
It is possible that the species migrated to other water sources instead of becoming extinct. It would be more convincing if there is evidence to support the long-term evolution of the local ecosystem (other animal groups and vegetation cover types) during that period. Can the carbon isotopes of stalagmites provide such evidence?

9. Line448-452
The resolution (43 years) of the oxygen isotope record in stalagmites may be insufficient to capture short - term climate events (such as droughts on a decadal scale), and the extinction of H. floresiensis may have been driven by extreme events.

Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Professor Gagan,

Thank you very much for submitting the revised version of your manuscript.

Your version has now been seen by two reviewers, and we include their comments at the end of this message. Based on these assessments and our own reading, we recognize that you have addressed the referees' previous concerns and have significantly improved the manuscript. We appreciate your efforts in this regard. Nevertheless, one key issue remains: Referee #2 emphasizes the importance of comparing your new rainfall records with existing regional proxy data. We agree that such a comparison is essential to place your findings in a broader context and to better understand the complex spatiotemporal climatic variability of the region.

We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your response to this remaining concern and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response that takes into account the points raised. Please highlight all changes in the manuscript text file.

Please submit your point-by-point responses as a separate file, distinct from your cover letter where you can add responses to the Editors’ comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees’ comments (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and the completed checklist:
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We hope to receive your revised paper within six weeks; please let us know if you aren’t able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

Please do not hesitate to contact us if you have any questions or would like to discuss these revisions further. We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Jiaoyang Ruan
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Mengjie Wang
Associate Editor, Communications Earth & Environment
Consulting Editor, Communications Sustainability

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Please refer to our data policies at http://www.nature.com/authors/policies/availability.html.

REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

The revised manuscript overall presents a better shape regarding the clearness of the isotope mixing model and the quantification of seasonal rainfall amount, but still lacks a critical contextual element: direct comparison with independent regional paleoclimate records.

While the authors argue that regional hydroclimates in the region are complex and that direct comparisons may be difficult, I believe this makes it even more essential to explicitly compare with the few available archives that do cover the same period and could provide insight into broader-scale hydroclimate trends (e.g., Borneo speleothems, Sulawesi lake sediment records, marine $\delta^{13}C$ or leaf-wax records from Java/Sunda margin). The fact that these records are "not directly comparable" does not diminish their value; on the contrary, presenting agreements and discrepancies transparently would highlight the local–regional dynamics and help assess the broader relevance of the K2 record.

Therefore, I strongly recommend that the authors include one or more figures comparing the K2 record with other regional archives, accompanied by a concise but critical discussion of the similarities and differences. These figures can be placed in the Supplementary Information if needed, but they must be clearly referenced in the main text.

Without this comparative framework, the manuscript risks remaining too internally self-referential. Given the bold implications for hominin extinction mechanisms, this additional effort is warranted to evaluate how the Liang Luar rainfall history fits, or diverges, from broader Indo-Pacific climate signals.

Some other comments:

Panels e–g of Figure 4 are useful schematic illustrations of hypothesized proxy responses to seasonal rainfall changes. However, they are not essential for interpreting the main data and could be moved to the Supplementary Information to streamline Figure 4. Doing so would better emphasize the empirical results shown in panels a–d and improve overall figure clarity.

In lines 217-223, the effect of sea level and precession are discussed without supporting comparison in the figure, please add accordingly, can try separate panels in in Figure 5.

The inclusion of the >1 Ma $\delta^{18}O$ data from Tangi Talo and Mata Menge can be confusing and even distracting regarding the main story on K2 dataset , which centers on the 91–47 ka rainfall reconstruction and extinction timing at Liang Bua. Given the large temporal, ecological, and depositional gaps, I suggest minimizing the description (to 1-2 sentences) and relocating the relevant plots to the Supplementary Information to maintain clarity and focus on the key findings supported by high-resolution, well-dated records, and avoid potential issues associated with the discussion on these data.

I suggest that the authors more directly address the proposed concerns raised by the reviewers this time, particularly those related to the validation of the climate interpretations and the broader relevance of the isotope-based rainfall reconstruction. As several important critiques were acknowledged in the rebuttal but not fully resolved. A more transparent and focused response to these points would strengthen the manuscript's scientific rigor and credibility.

Reviewer #3 (Remarks to the Author):

I think the authors have addressed all my concerns. This paper could be accepted now.

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Version 2:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Professor Gagan,

Your manuscript titled "Onset of summer aridification and the decline of *Homo floresiensis* at Liang Bua 61,000 years ago" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Mengjie Wang
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REVIEWERS' COMMENTS:

Reviewer #2 (Remarks to the Author):

The authors have addressed my main concerns. The revision now looks good to me. I recommend to accept it for publication.

The authors wish to thank the reviewers for their many helpful suggestions. In light of these reviews, we have revised the manuscript substantially. Our detailed responses to the reviewers' comments and our rationale for the revisions are set out below.

Reviewer #1 (Remarks to the Author):

This manuscript is essentially about attempts to reconstruct past climate events (in this case, rainfall) during the late Pleistocene using d18O and Mg/Ca values as measured from a speleothem sampled from the Indonesian island of Flores. However, the authors try to go considerably further by using their reconstructions to make a number of claims about the disappearance of Homo floresiensis, an extinct hominin species that tends to attract a lot of attention whenever anyone tries to say something new about it. I want to be clear that I am not a climate scientist but I have expertise in human evolution and how various lines of evidence (including those from paleoclimatic and paleoenvironmental data) are used correctly or incorrectly to interpret the hominin fossil record.

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In their Discussion, the authors claim that their results are consistent with stratigraphic indicators and the murine rodent fossil assemblage at Liang Bua, citing Westaway et al. (2009) (citation #85) and Veatch et al. (2019) (citation #86). I went through these cited articles carefully and failed to find the corroborative support the authors have suggested exists. First, the stratigraphic interpretation of Westaway et al. (2009) was entirely revised in 2016 by Sutikna et al. (citation #4) so it is completely unclear what evidence the authors may be referring to here. Second, Veatch et al. (2019) showed that prior to ~60 thousand years ago, the murine rodent assemblage at Liang Bua was dominated by medium-sized rats and Komodomys cf. rintjanus, specifically, which is a species known to prefer drier, more open habitats. This is completely opposite to what the authors state on lines 353-357: "During Phase 1 (92–76 ky BP), mean annual rainfall was higher than at present and seasonality was low. Using the current climate as a guide, the favourable MIS5 rainfall regime would have sustained perennial base-flow in the Wae Racang river. This is consistent with stratigraphic indicators (85) and the murine rodent fossil assemblage (86) at Liang Bua, both of which reflect relatively wet, forested conditions at this time, thus freshwater availability was likely unconstrained." There doesn't seem to be any indication of significantly wet, forested conditions around Liang Bua based on the faunal record from the site in the indicated temporal range of 92–76 ky BP.

For Phase 2 (76–61 ky BP), the authors suggest this temporal period was "marked by falling sea-levels and drier conditions" and "punctuated by abrupt increases in summer rainfall at 74, 71 and ~68–62 ky BP." They further state that "The fossil record at Liang Bua supports this; the rodent assemblage shows a significant increase in open-habitat adapted species around 69–62 ka (86), and the concentration of H. floresiensis and Stegodon remains peaks at ~74–61 ka (4–6,85)." However, there is no significant increase in open-habitat adapted species around 69–62 ka (unless of course time runs backwards and that doesn't happen in reality!), as all deposits prior to ~60 thousand years ago at Liang Bua were dominated by Komodomys cf. rintjanus as noted above. I was also unable to find any evidence indicating that Homo floresiensis remains are concentrated at ~74–61 ka; the primary research articles on this topic all state this is ~100–60 ka.
Although the authors' Phase 2 reasonably matches the reported faunal sequence at Liang Bua, their Phase 3 (like their Phase 1) again does not, despite what the authors state: "Phase 3 (61–47 ky BP)

encompasses the relatively dry MIS3 onset and lowest mean annual and summer rainfall on record (58–55 ky BP). An abrupt change in the rodent size distribution at Liang Bua around 60 ka indicates a return to more closed habitats (86), consistent with a return to lower rainfall seasonality." Closed habitats are ones that are densely forested, and dense forests are not consistent with a return to lower rainfall seasonality. I fail to see the authors argument here. Again, the rodent record from this temporal period at Liang Bua suggests that local conditions around the site were opposite to what the authors interpret for their Phase 3 (61–47 ky BP). Why would the most arid-adapted rat species known on Flores disappear almost entirely from the faunal record during the time when the conditions were supposedly the most favorable? Why would forest-adapted rats return in greater numbers instead?

Clearly there are some major discrepancies between these two very different lines of evidence (faunal records on one hand, speleothems on the other) about the local conditions of an archaeological site on Flores many tens of thousands of years ago. I think most scholars would agree that the fauna (and the rats in particular) provide the better local signal in this case. They are animals that lived and died around the site and they were buried in the same deposits as the Homo floresiensis and pygmy elephants. The paleoclimate reconstructions from this speleothem may well be correct but these are likely more appropriately interpreted in a wider, regional context rather than such a specific, local one. This comes back to my earlier comment about validating the model used here for interpretation. If one uses the faunal record a potential validation tool, then this particular model of using Mg/Ca to reconstruct local seasonal rainfall fails demonstrably. Paleoclimate data such as provided by the core elements of this study are extremely important and valuable for our broad understanding of past events, but in this case I think these data are being stretched too thinly to apply to a specific problem that they are unable to reliably speak to.

The authors thank the reviewer for raising these thought-provoking points, which have led to new results that strengthen the link between the stalagmite K2 rainfall reconstruction and the Pleistocene faunal history at Liang Bua (see brief summary below). As a result, the comparison of the rainfall reconstruction with the murid rodent fossil assemblage at Liang Bua has been superseded. We appreciate the reviewer's pointing-out that the two records are in fact very different, and we have removed the three sentences attempting to relate them from the Discussion.

The revised manuscript presents more direct, detailed and relevant results on the connection between rainfall and the *Stegodon florensis insularis* fossil assemblage. Briefly, in the revised Discussion (and Fig. 6) we describe the correlation between the precisely dated stalagmite K2 ^{18}O time series and *Stegodon* tooth enamel ^{18}O (as a function of excavation depth) in the Pleistocene deposits at Liang Bua. Importantly, the synchronisation of the two ^{18}O records (based on U-Th-dated tie-points with the K2 ^{18}O record) clarifies the age range of *Stegodon* (and associated *Homo floresiensis*) in the stratigraphic sequence.

As a result, the relationship between aridification and the histories of the two species at Liang Bua has been clarified substantially (see revised Fig. 7). The key findings are that 92% of *S. florensis insularis* remains occur within a narrow stratigraphic interval equating to 76–62 ka. The last occurrence, located at 3.25–3.15 m in Sector XI, dates to 57 ka, when rainfall was at a record low. The last appearance of associated *H. floresiensis* remains dates to 61 ka.

Our rationale for the revisions is described below along two fronts. First, we summarise the scientific grounds for interpreting the stalagmite K2 record in terms of local rainfall, which the reviewer has questioned. We then summarise the revisions related directly to the refinement of the age-depth relationship for the Pleistocene fossil-bearing deposits at Liang Bua.

(1) Stalagmite K2 as a recorder of local rainfall and aridification at Liang Bua

It is important to note that the perception of speleothems as recorders of regional (not local) climate is largely due to the field's traditional reliance on measurements of speleothem ^{18}O. Fortunately, the science is advancing rapidly, with the recognition of elemental ratios in speleothems as archives of local rainfall (the ‘multi-proxy’ approach described in the text).

Five lines of evidence supporting the interpretation of the stalagmite K2 record in terms of local rainfall are presented in the manuscript (including new detailed information on *Stegodon* ^{18}O). Briefly, they are:

Stalagmite Mg/Ca and Sr/Ca. The presence of prior calcite precipitation (PCP) in speleothem records is widely regarded as a proxy for local rainfall, and aridification in particular [(see review by Fairchild and Treble 2009 (ref. 99)]. The diagnostic ln(Sr/Ca) vs. ln(Mg/Ca) slope relationship observed in the K2 stalagmite record is based on well-documented principles of carbonate geochemistry. This record alone shows that the driest interval on record at Liang Luar cave (only 1.3 km from Liang Bua) occurred at 61–55 ky BP.

Stalagmite 18O. The increase in ^{18}O to record highs at 61–55 ky BP in lock step with Mg/Ca provides a cross-check on local aridification. When measured with Mg/Ca, ^{18}O (and positive covariation with Mg/Ca) provides information on local rainfall seasonality that allows the reduction in rainfall to be attributed to summer.

Stalagmite $[^{234}\mathrm{U}/^{238}\mathrm{U}]_i$. The increase in $[^{234}\mathrm{U}/^{238}\mathrm{U}]_i$ in stalagmite K2 to record highs starting at ~55 ky BP provides another cross-check for the local impact of aridification, due to its effect on groundwater residence times in the surrounding karst during hydroclimate Phase 3 (61–47 ky BP).

Local palaeoclimate model and data agreement. Our calculated rainfall amounts for summer and winter are the first of their kind, and thus cannot be validated with other palaeoclimate records, Instead, we must compare the results with model simulations of past rainfall available for the 2° model grid encompassing Flores (see details in response to Reviewer 3). The updated text describes the good agreement of the model and proxy results for mean annual rainfall and rainfall seasonality (on both glacial and millennial scales) in more than one respect (see updated lines 273–287 in text).

***S. florensis insularis* tooth enamel ^{18}O.** In the revised Figure 6, we describe the marked similarity of the precisely dated stalagmite K2 ^{18}O time series and the *S. florensis insularis* tooth enamel ^{18}O record (as a function of excavation depth in the well-documented Pleistocene fossil-bearing deposits of Sectors XI, XV and XVI). The agreement provides an independent cross-check on both ^{18}O records, and shows that the isotopic composition of *Stegodon’s* drinking water was concordant with the infiltrating waters feeding-in to stalagmite K2. This connection could only occur if the *Stegodon* individuals represented at Liang Bua (the majority of which were subadults) relied on the Wae Racang river as the primary source of isotopically homogenous drinking water (sustained by groundwater-driven base flow).

(2) Revisions related to the refined age-depth relationship for the Pleistocene fossil-bearing deposits at Liang Bua

The primary revisions made in this regard are:

- In light of the new results, the title of the manuscript has been revised to: ‘*Onset of summer aridification and the decline of* Homo floresiensis *at Liang Bua 61,000 years ago*’;
- The Abstract, Introduction, Results and Discussion, and Methods have been revised (highlighted in green), and the references have been updated;
- Figures 6 and 7 have been revised;
- Supplementary Figures 11 (stratigraphic context of the *Stegodon* fossil tooth samples in excavation Sectors XI, XV and XVI) and 12 (stratigraphic context and age of the fossil-bearing deposits in excavation Sector IV have been added); and
- Supplementary Tables 2 (^{18}O and ^{13}C datasets for *Stegodon*) and 3 (chronology of *Stegodon* and *H. floresiensis* in Sectors XI and IV) have been added.

It is important to note that our brief interpretation of the implications of the research results in the last paragraph of the Discussion is actually quite conservative. We simply point out that aridification and the accompanying decline in *Stegodon* and *H. floresiensis* after 61,000 years ago may reflect progressive abandonment of the site, perhaps with implications for the extinction of the two species elsewhere on Flores. The final sentence makes it clear that our goal (from the outset) was to provide a well-dated, reliable rainfall record as a basis for future research to examine these extinctions in the context of climate change.

Reviewer #2 (Remarks to the Author):

Gagan et al. presents a high quality study that provides a well-structured and insightful reconstruction of past rainfall amount and seasonality changes based on speleothem d18O and Mg/Ca from Flores, and further links the climate change with during H. floresiensis extinction. The study is quite novel as it is probably (as far as I know) the first to systematically utilize Mg/Ca and d18O to isolate summer and winter rainfall signal, and quantitatively estimate rainfall amount and its seasonality based on binary isotope mixing model.

The authors have done rigid step-by-step argumentation on the application of the methodology, including 1) careful evaluation of the prerequisites to apply the method; 2) drip water-rainfall-speleothem d18O agreement and reproducibility test; 3) validation of PCP from perspectives including Mg-Sr covariation, correlation, and a Rayleigh Fractionation model, 4) careful application of the isotope mixing model and calibration, as well as cautious consideration of potential uncertainty sources, and 5) comparison with model outputs and cross-validation between Fossil Stegodon and speleothem δ^{18}O on the range of value and aridity trends. The manuscript also shows the best efforts made to connect the climate with the extinction based on correlation as well as more mechanical causal linkage. I therefore suggest 'accept with minor revisions regarding below comments and suggestions' for the manuscript.

We thank the reviewer for supporting the publication of our manuscript following minor revisions. Their constructive feedback and suggestions helped us to clarify key aspects of the manuscript, particularly the oxygen-isotope mixing model.

Comments and suggestions:

The paper relies heavily on multi proxies of one single stalagmite, although they agree well among themselves, it is still necessary to compare the original/reconstructed hydroclimate changes with other regional hydroclimate records to validate the trends and identify any potential biases. Borneo speleothem records (both δ^{18}O and δ13O) and some marine/lake sediment (SST, δ^{18}Osw, trace metal, and compound-specific d13C/dD) should cover this period.

We very much agree that validation of the new rainfall reconstruction for Flores with independent hydroclimate proxies would be ideal. In practice, however, it is challenging to do (skillfully) for geographically complex Australasia, where differences among local hydroclimates are the norm.

To briefly illustrate the difficulty, consider the case of two high-quality records that span the last full glacial–interglacial cycle in Australasia; the speleothem ^{18}O hydroclimate record for north-western Borneo [Carolin et al. 2013 (ref. 12)] and the marine core leaf wax ^{13}C record from near south-western Java [as an indicator of rainfall seasonality, Windler et al. 2019 (ref. 14)]. These two locations are on the margin of the Sunda Shelf at lower sea levels, and the affinity of their isotope (hydroclimate) proxies makes the records natural choices for comparison at orbital (sea level) and precessional scales. Carolin et al. (2013) and Windler et al. (2019) report the following:

Carolin et al. (2013) Abstract: '*The records suggest that northern Borneo's hydroclimate shifted in phase with precessional forcing but was only weakly affected by glacial-interglacial changes in global climate boundary conditions*'. (mkg: i.e. ice volume, sea level, shelf exposure).

Windler et al. (2019) Discussion: '*The lack of a clear, substantial precession cycle signal in our spectral analysis demonstrates that the shelf effect is dominant over seasonally-driven changes in the hydrological cycle and supports sea level as a substantial driver of Indian Ocean Walker circulation strength. In this respect, our record differs from other hydroclimate data from the IPWP, such as the hydroclimate record from a cave in Borneo, which has strong precessional variability (Carolin et al. 2013, 2016). The difference can be attributed in part to the fact that, according to our model simulations, the east-central area of the Sunda shelf is less sensitive to sea level change*'.

The key point here is that in-depth modelling was essential to show that both records are 'correct' in their own contexts, despite clear differences that otherwise could be misinterpreted as 'faults'.

As expected, the new record for Flores shares attributes with both records, but also has points of difference. The addition of sophisticated palaeoclimate modelling, new figures and new text to the manuscript would be required to assess the emerging pattern of regional hydroclimate change. Please be assured, however, that we have revised the text to make it clear that the stalagmite K2 geochemical records are definitely compatible with the available records of past climate change in Australasia. As our reconstructed rainfall amounts (the first of their kind) cannot be compared with proxy records, we instead validated them with published palaeoclimate model simulations.

Here is a brief summary of what we have done to let readers know that the stalagmite K2 record is accurate at glacial-interglacial, precessional and millennial scales.

Glacial-interglacial. The prominent reduction in mean annual rainfall (and increase in rainfall seasonality) in the K2 record during the MIS 4 glacial is well supported. The text and citations in lines 218–220 have been enhanced for nearby hydroclimate records that are sensitive to the drying effects of Sunda/Sahul shelf exposure at times of lowered glacial sea levels [Windler et al. 2019 (ref. 14), Kimbrough et al. 2023 (ref. 29)]. Lines 273–280 have been revised to show that the calculated mean annual rainfall and rainfall seasonality during MIS 4 are well validated by version 1.2 of the coupled Community Earth System Model [CESM1, DiNezio et al. 2016 (ref. 67)].

Precessional. Lines 180–216 are devoted to the decomposition of the precessional signal in the K2 record. Lines 184–186 have been revised to note the widespread recognition of precessional pacing of speleothem ^{18}O in the Australasian monsoon domain, and elsewhere in the tropics [Carolin et al. 2013 (ref. 12), Scroxton et al. 2022 (ref. 18), Wang et al. 2001 (ref. 36), Cheng et al. 2012 (ref. 59), 2016 (ref. 58)]. The highlight here, however, is that the new K2 Mg-^{18}O record shows that the ^{18}O precession signal in Flores is more pronounced in winter than in summer.

Millennial. New text in lines 280–287 points out that the observed antiphasing of rainfall in Flores with the northern hemisphere East Asian summer monsoon during cold Greenland stadials is well supported by the new K2 record [with citation of Griffiths et al. 2009 (ref. 15), Ayliffe et al. 2013 (ref. 17), Scroxton et al. 2022 (ref. 18), Wang et al. 2001 (ref. 36), Cheng et al. 2016 (ref. 58)]. The Mg/Ca and ^{18}O records both indicate that conditions were wetter (in summer) during the early MIS 4 stadials and Heinrich Event 5. The restriction of the signal to summer, and the amplitude of the change in rainfall, are in remarkably good agreement with the CESM1 simulation available for Heinrich Stadial 1 [Buckingham et al. 2022 (ref. 69)].

I have spent quite some time trying to understand the binary mixing model. I suggest either break this section into smaller parts or providing a step-by-step guide to improve the readability. A clearer justification on the corrections for periods when divergence occurs and Equation 13 does not apply would be helpful. The overall procedure should be briefly in the method section of the main text and slightly more detailed in the supplemental file, potentially by using a flow chart/summary diagram that summarizes the sequence of the steps or conceptualize the framework.

We thank the reviewer for recommending good ways to clarify our description of the binary mixing model. We have made substantial revisions to Supplementary Note 3 based on their advice. To improve clarity, the description is now provided under four subheadings: (1) overview and boundary conditions for the mixing model; (2) calculation of rainfall amounts; (3) summary of rainfall results; and (4) an excerpt titled “Supplementary Note 3 Box 1”.

Importantly, section (2) on the calculation of rainfall has been shortened (from ~4.5 pages to ~2.5 pages) and simplified (from 15 to 9 equation steps). The ‘flow’ of the original Note 3 was interrupted by four equations (and associated text) that are not required for the rainfall calculation. This full page of ancillary information on the relationship between the ^{18}O of summer rainfall and the summer rainfall amount has been moved to ‘Box 1’ at the end of Note 3. Additionally, a non-essential ‘example’ figure (the previous Supp. Fig. 9) has been removed to reduce the length of Note 3 by another full page.

These two amendments result in a smooth, step-by-step presentation of the key rainfall calculation steps (Eqs. 3–11). The methodology is still illustrated graphically in Supplementary Figures 8 and 9 for readers who prefer a visualisation of the key concepts.

In addition, a new ‘Supplementary Data 2’ Excel file further summarises the essential steps required for the calculation of rainfall amounts. The step-by-step spreadsheet defines the mixing model solutions for each Mg/Ca–^{18}O data pair in the K2 record.

The 3,000-word maximum for the Methods could not accommodate a detailed description of the mixing model components due to the addition of critical information on the new age-depth model for *Stegodon* and *H. floresiensis* (presented in revised Figs. 6 and 7 and the Discussion). Nevertheless, we hope that our consolidation of Note 3 and addition of the Data 2 file will provide readers with a more intuitive and navigable presentation.

Could other proxy like Sr/Ca and Ba/Ca be potentially helpful in validating the estimates and enhance their robustness? The effect of fractionation can be added in the captions to describe.

We haven’t yet measured other elemental ratios (e.g. Ba) in stalagmite K2 because we are fortunate that the PCP signature is clearly evident in the geochemically diagnostic slope relationship for ln(Sr/Ca) vs. ln(Mg/Ca) [Sinclair et al. 2012 (ref. 33)]. And the initial ^{234}U/^{238}U activity ratios in stalagmite K2 provide a cross-check as indicators of the groundwater residence time and karst dryness at Liang Luar (Fig. 4).

Of course, the reviewer is absolutely correct that measurements of Ba (as another divalent cation that replaces Ca in the carbonate crystal lattice with a partition coefficient < 1) could provide another effective, complementary palaeohydrological proxy.

A sensitivity analysis, by systematically varying key input parameters like the non-convective d18O and endmembers, can be applied to assess their impact on reconstructed rainfall estimates.

We appreciate the reviewer’s raising of this important point, and have added sensitivity test results for the reconstructed rainfall amounts described in Supplementary Note 3. The key input parameter in the mixing model is the winter rainfall end-member ^{18}O value. This value influences the slope of the summer in-mixing line (Eq. 2) and affects the resulting range of modelled summer rainfall (the key parameter for the identification of landscape aridification at Liang Bua from ~61 ky BP onward).

It is important to note, however, that the observed positive covariation between Mg/Ca and ^{18}O in stalagmite K2 across a broad range of climates could not occur if the ^{18}O of winter rainfall varied significantly over time. Nevertheless, we evaluated the effect of winter end-member ^{18}O values ranging from –1‰ to –3‰ (vs. –2‰ in the model) in the event that such modes were to occur in the past at timescales exceeding the 50-year resolution of the K2 record.

A new table in Note 3 (with accompanying new text highlighted in green) summarises the results for the MIS 5b summer rainfall maximum and the MIS 3 minimum in the stalagmite K2 record. Briefly, the overall pattern of rainfall change over time remains robust, with the lowest modelled summer rainfall occurring during MIS 3. With the winter value at –1‰, the MIS 3 summer rainfall increases from 410 mm to 474 mm (+16%). Conversely, winter at –3‰ decreases MIS 3 summer rainfall to 322 mm (–21‰). The relatively wet summers during MIS 5b remain within 5–8% of their modelled values with the winter value at –2‰.

The study can benefit from a brief discussion on the potential/effort of applying the method using modern/younger stalagmites (with decadal resolution) with rainfall amount observation directly comparable and whether such application would be comparable to or helpful for the construction of the older portion.

The reviewer's suggestion is well taken. Indeed, we have an ultra-high-resolution (~4 years on average) Mg-^{18}O record for Liang Luar based on two coeval stalagmites spanning the last 2,000 years (Griffiths et al., *Nat. Comm.* 7:11719, 2016). Griffiths et al. (2016) note century-scale differences between the Mg/Ca and ^{18}O records, and base their palaeoclimate interpretation largely on the elemental record of local rainfall.

The differences in stalagmite Mg/Ca and ^{18}O over the last two millennia are relatively small, but generally well replicated. Thus, they likely represent meaningful changes in rainfall seasonality amenable to the mixing model approach. However, the challenge would be to establish the relationship between Mg/Ca and ^{18}O specific to these particular drip sites (using Common Era Mg–^{18}O signals that are much smaller than those in the stalagmite K2 record). The same ^{18}O mixing model parameters would apply, but the Mg–^{18}O relationship would be different due to the inevitable spatial differences in bedrock Mg/Ca (and Sr/Ca) and flow paths with unique dripwater residence times, CO_2 degassing histories and solute compositions that would influence the absolute values of stalagmite Mg/Ca.

Also, even at 4-year sampling resolution, the relatively short instrumental records of rainfall available for Flores probably would be insufficient for the calibration of reconstructed rainfall amounts at decadal resolution.

Reviewer #3 (Remarks to the Author):

Gagan et al. present a new speleothem multiproxy record of δ18O and Mg/Ca spanning 91.4~46.8 ka to determine both summer and winter rainfall. One of the most significant highlights of this study lies in the quantitative reconstruction of summer and winter precipitation in Flores through the Geochemical modelling of the Mg-18O system. The attempt to quantify paleo-precipitation using speleothem proxy indicators proves both challenging and methodologically essential. Furthermore, the authors have combined the reconstructed precipitation records with additional stratigraphic evidence to investigate the climatic drivers behind the disappearance of the H. floresiensis. Overall, I think this manuscript should be published.

We thank the reviewer for recommending the publication of our manuscript, and for the valuable comments and suggestions, some which were also made by Reviewer 2. The resulting improvements to the manuscript, and our rationale for making them, are described below.

However, this manuscript has some issues worth further attention by the authors. Some suggestions are listed below:

1. Lack of stalagmite δ13C record. The δ13C record can provide a wealth of additional information at the local scale, such as vegetation, water-rock interactions, and PCP, thereby supporting the variations in Mg/Ca ratios.

We certainly agree with the reviewer that a stalagmite ^{13}C record would broaden the information on local environmental change at Liang Bua. However, ^{13}C analysis is complex, and the identification of PCP and/or changes in vegetation productivity with raw data is difficult. Skillful assessment of the record would require data processing, carbon system modelling and further analytical work involving at least four more steps:

(1) Correction of the record for the effect of atmospheric CO_2 concentration on carbon isotope fractionation in C_3 plants. Breecker (*EPSL* 458: 542–549, 2017) recommends correction for a C_3 plant ^{13}C sensitivity of –1.6 ± 0.3‰ per 100 ppmV change in CO_2, but the magnitude of the effect is still a matter of debate.

(2) As the reviewer points out, positive covariation between stalagmite ^{13}C and Mg/Ca can aid the further identification of PCP [beyond the diagnostic ln(Sr/Ca) vs. ln(Mg/Ca) signature]. However, advanced geochemical modelling of ^{13}C–PCP systematics in dripwater is required to definitively identify (and quantify) the effect of PCP on stalagmite ^{13}C [e.g. Johnson et al. 2006 (ref. 32)].

(3) To dig deeper into the ^{13}C record, the effect of PCP must be removed. Thereafter, under ideal conditions the remaining periods of difference between Mg/Ca and ^{13}C are likely attributable to changes in vegetation productivity (and soil pCO_2), but they also could be due to changes in the proportion of C_3 versus C_4 vegetation cover. Either interpretation requires the assumption of no change in the contribution of heavy bedrock carbon to the dissolved inorganic carbon content of dripwaters.

(4) To 'tease apart' the two contrasting interpretations, measurements of transition metals, phosphorous and sulphur (as biomass proxies) might help to distinguish a change in C_3/C_4 plant abundance from a change in vegetation productivity and soil CO_2 production [see Fairchild and Treble 2009 (ref. 99)].

We intend to investigate stalagmite K2 along these lines, but this work will take time. Meanwhile, we have done our best to demonstrate that the Mg, Sr, U and ^{18}O signals in the K2 record are robust for the purposes of hydroclimate reconstruction.

2. When verifying the rainfall reconstruction results, the comparison is mainly made with the simulation results of climate models. There is a lack of comparison with other independent geological records, such as pollen and lake sediment records. The mutual corroboration of multiple geological records can more strongly support the reliability of the research conclusions.

We very much agree that the ideal situation would be to validate the new rainfall reconstruction for Flores with independent hydroclimate proxies. At present, only two hydroclimate reconstructions covering 92–47 ky BP [Carolin et al. 2013 (ref. 12), Windler et al. 2019 (ref. 14)] could be genuinely compared with the stalagmite K2 record. However, in-depth palaeoclimate modelling would be essential to explain the similarities and differences among the three records (as interesting as this endeavour would be, see our response to Reviewer 2).

Regarding pollen and lake sediment records, the sediment Ti record available as an indicator of runoff into Lake Towuti in central Sulawesi, about 350 km north of Flores with a relatively wet climate (~2,700 mm per year), begins around 60 ka (Russell et al., *PNAS* 111: 5100–5105, 2014). Uncertainties in the dating and provenance of pollen in marine core G6-4 from the Lombok Ridge, ~400 km southwest of Flores (Wang et al., *PPP* 147: 241–256, 1999), and in core SHI-9014 from the Banda Sea, ~900 km to the northeast (van der Kaars et al. *PPP* 155: 135–153, 2000), preclude meaningful comparison of the pollen assemblages with the stalagmite K2 record.

3. In the author's affiliation section, the 9th institution "Xi'an Jiaotong University" is misspelled.

Thank you so much for catching this!

4. Line22-25
Homo sapiens arrived on Flores Island later than the time when Homo floresiensis went extinct. However, the time gap between these two events is not significant. Moreover, these two time points

are merely the results of dating the geological carriers that have been found. Considering the errors in dating, it is not entirely possible to rule out the temporal and spatial overlap between these two hominid species.

We fully agree with the reviewer that the available evidence does not preclude a role for *Homo sapiens* in the extinction of *H. floresiensis*, and we have revised the last paragraph of the Discussion to bolster this point. However, the revised Figure 7 indicates that the time gap between *H. floresiensis* (based on the reduction in their numbers) and *H. sapiens* at Liang Bua is significant (see further notes below). Thus, instead of making a case for modern human agency at Liang Bua specifically, we propose the next most likely scenario, that of interaction between *H. floresiensis* and *H. sapiens* elsewhere on Flores due to the abandonment of Liang Bua alongside climate change. The final Discussion sentences read:

'Whether H. floresiensis and S. florensis insularis interacted with modern humans (H. sapiens) elsewhere on Flores remains uncertain. Fossil evidence places modern humans on an eastward dispersal trajectory from continental Asia (Sunda) by ~75–63 ka[83] with initial arrival on Sahul by ~70–60 ka[84]. The southern dispersal route is aligned with the Lesser Sunda island chain, including Flores[85,86], thus the evidence for abandonment of Liang Bua at the onset of hydroclimate Phase 3 allows for overlap with migrating modern humans. Our precisely dated rainfall record establishes a firm chronological framework for future research to examine the extinctions of these iconic species in the context of major climate change.'

Support for the scenario is provided by the refined age estimates for *Stegodon* and *H. floresiensis* at Liang Bua (see revised Fig. 7), which suggest that both species were in decline by 61 ka, thousands of years before the first evidence for *H. sapiens* at the site. The significant stratigraphic shift to chert as the preferred material for stone tool making (attributed to the arrival at *H. sapiens*) dates to ~46 ka [Sutikna et al. 2016, 2018 (refs. 4 and 6)]. And precise U-Th dating of a flowstone underlying volcanic tephra T3 (LB07-SXII-F1), below the shift to chert, yielded an age of 49.6 ± 0.5 ka [Sutikna et al. 2016 (ref. 4)].

5. As shown in Supplementary Figure 3, the variation amplitudes of Mg/Ca and Sr/Ca exhibit significant differences during the period of 54.4~48.6 ky BP, whereas they were consistent prior to this interval. The reason for this discrepancy is not explicitly addressed in the text.

The cause of the baseline shift to lower Sr/Ca during the period of 54.4–48.6 ky BP is unknown, but the answer may lie in a careful analysis of ^{13}C, Sr/Ca and Ba/Ca in stalagmite K2. All three proxies are sensitive to changes in vegetation productivity and soil biological activity [see the example in Hellstrom and McCulloch 2000 (ref. 60) and the review by Fairchild and Treble 2009 (ref. 99)]. The important point for our study is that the PCP signal and relationship between Mg/Ca and ^{18}O continue uninterrupted across this interval (see Supplementary Figs. 3 and 4).

In the Methods (lines 534–541), we describe the adjustment in Sr/Ca required to account for its consistent offset relative to Mg/Ca, and note the need for further research to evaluate the possibility of a biological cause. We also note that the shift is unlikely to be due to a switch to prior aragonite precipitation [e.g. Wassenburg et al. 2012 (ref. 97)] or a change in the stalagmite growth rate and its potential effect on the Sr-Ca distribution coefficient for calcite [e.g. Lorens 1981 (ref. 98)].

We thank the reviewer for raising this point. Because the exact cause of the shift in Sr/Ca is not yet known, the Mg/Sr record has been removed from Figure 7 along with two sentences relating to it in the Discussion (with no implication for our conclusions).

6. Line 185 mentions that the precessional changes are evident in the K2 δ18O record, and there is no comparison graph of δ18O with insolation, which could be added to Figure 4b.

Following the reviewer's valid advice, we attempted to superimpose a local southern summer insolation curve on the stalagmite K2 ^{18}O record in Figure 4, but the addition of a third curve (and a third *y* axis) to panel 4b created a visual challenge. However, the 23-kyr pacing of insolation forcing is shown in panel 4d. Hopefully, this insolation curve (superimposed on the winter rainfall component of the K2 ^{18}O record) will allow readers to identify the net effect of precession on the ^{18}O record as a whole (due to the change in rainfall seasonality). Lines 220–223 (relating to Fig. 4) have been revised to clarify that precession forcing at Liang Luar is more pronounced in winter than in summer, and that the winter and summer signals are antiphased.

7. Line 271-282.
When assessing the accuracy of reconstructed precipitation, is it appropriate for the authors to draw a simple analogy between the changes in reconstructed precipitation and those in model simulations for different periods? For example, comparing MIS4 with LGM, and H1 with GIS 20 and GIS 19.2.

We appreciate the reviewer's good point that our calculated rainfall amounts (as the first of their kind) would ideally be compared directly with model simulations of rainfall for the MIS 4 glacial and times of abrupt climate change during the MIS 5/4 transition (and Heinrich Event 5). However, the comparisons need to be made at seasonal resolution, and with reliable high-resolution model output including the island of Flores. To our knowledge, season-specific model output of this type for Flores is only available (at 2° grid resolution) for the Last Glacial Maximum, Heinrich Stadial 1 and the Younger Dryas.

The season-specific CESM1 simulations of DiNezio et al. [2016 (ref. 57)] available for the Last Glacial Maximum are state-of-the-art and well regarded, as are the CESM1 simulations for Heinrich Stadial 1 [Buckingham et al. 2022 (ref. 69)]. While not a perfect match, the model and proxy rainfall amounts are clearly similar at glacial–interglacial and millennial scales in more than one respect (see revised lines 273–287).

To contextualise the comparison, lines 273–275 have been revised to let readers know that high-resolution, season-specific model output is available only for more recent periods of lowered sea levels and abrupt climate change.

8. Line392-398
Here it is mentioned that the time of the volcanic ash layer (T3) is very close to the time of the Homo floresiensis extinction, but only briefly refers to the impact of volcanic activity. Such a thickness of volcanic ash may represent a very intense volcanic eruption. Compared with climate reasons, is it more likely to be the direct cause of the extinction of Homo floresiensis? Is the short - term interference of volcanic ash generated by volcanic activity on the chemistry of cave dripping water and the climate response reflected in the oxygen isotopes and trace elements of stalagmites?

We agree that the extent of the impact of local volcanic ashfall at Liang Bua on the demise of *S. florensis insularis* and *H. floresiensis* is a key question requiring further research. The new U-Th time scale for the two species in revised Figure 7 suggests that their decline, as recorded in the fossil-rich excavation Sectors XI and IV, began thousands of years before the deposition of T3 around 50–47 ka [Sutikna et al. 2016 (ref. 4)]. In these sectors, only 10 skeletal elements attributable to *S. florensis insularis* occur after 62 ka, and the youngest dates to 57 ka. And *H. floresiensis* skeletal remains, while much less common, end at 61 ka. The distribution of *S. florensis insularis* in Sectors XI and IV is consistent with the broader spatio-temporal distribution of the species in 12 excavation sectors at Liang Bua [Sutikna et al. 2018 (ref. 6)].

At 50-year resolution, the stalagmite K2 record is too coarse to register the impacts of even large local volcanic eruptions. Thus, although volcanism around 50–47 ka, and perhaps water supply contamination, may have contributed to final site abandonment, the adaptive challenge posed by persistent aridification after 61 ka, as indicated by the declining faunal assemblage at Liang Bua, may have taken precedence. Lines 398–401 of the Discussion section have been revised to cover these points.

It is possible that the species migrated to other water sources instead of becoming extinct. It would be more convincing if there is evidence to support the long-term evolution of the local ecosystem (other animal groups and vegetation cover types) during that period. Can the carbon isotopes of stalagmites provide such evidence?

Reviewer 2 also raised this good point. We agree that further analysis of the K2 ^{13}C record in the context of additional proxies would broaden the information on local environmental change at Liang Bua. As noted above, ^{13}C analysis is complex, and the identification of the effects of PCP, vegetation productivity and/or changes in C_3/C_4 plant abundance from the raw data would be difficult. Skillful assessment of these factors would require ^{13}C data processing, carbon system modelling and further analytical work.

However, the case for abandonment of Liang Bua is stronger now with the clarification of the temporal connection between aridification and the decline of *S. florensis insularis* and *H. floresiensis* (see new results presented in revised Figures 6 and 7 and the Discussion).

9. Line448-452
The resolution (43 years) of the oxygen isotope record in stalagmites may be insufficient to capture short - term climate events (such as droughts on a decadal scale), and the extinction of H. floresiensis may have been driven by extreme events.

The reviewer makes a very good point; considerable observational data confirm the vulnerability of proboscideans to periods of drought [e.g. Haynes 1985 (ref. 9), Dudley et al. 2001 (ref. 10)]. The role of extreme events is an important question that might be answered by an investigation of the transition to hydroclimate Phase 3 (before and after 61 ka) in the stalagmite K2 record at higher temporal resolution. One possible approach would be the *in-situ* measurement of trace element variability by laser ablation–inductively coupled plasma mass spectrometry. With an average stalagmite growth rate of about 0.035 mm per year, a laser spot size of 35 microns (certainly feasible) would yield 1-year sampling resolution, which might be sufficient to reveal changes in the amplitude of interannual climate variability. As rainfall in Flores is sensitive to El Niño–Southern Oscillation variability, a significant increase in interannual variability at the onset of hydroclimate Phase 3 would be a fascinating outcome with implications.

Reviewer #3 (Remarks to the Author):

I think the authors have addressed all my concerns. This paper could be accepted now.

We appreciate Reviewer 3's contributions to the peer-review of this work and their endorsement of publication.

Reviewer #2 (Remarks to the Author):

The revised manuscript overall presents a better shape regarding the clearness of the isotope mixing model and the quantification of seasonal rainfall amount, but still lacks a critical contextual element: direct comparison with independent regional paleoclimate records.

While the authors argue that regional hydroclimates in the region are complex and that direct comparisons may be difficult, I believe this makes it even more essential to explicitly compare with the few available archives that *do* cover the same period and could provide insight into broader-scale hydroclimate trends (e.g., Borneo speleothems, Sulawesi lake sediment records, marine $\delta^{13}C$ or leaf-wax records from Java/Sunda margin). The fact that these records are "not directly comparable" does not diminish their value; on the contrary, presenting agreements *and* discrepancies transparently would highlight the local–regional dynamics and help assess the broader relevance of the K2 record.

Therefore, I strongly recommend that the authors include one or more figures comparing the K2 record with other regional archives, accompanied by a concise but critical discussion of the similarities and differences. These figures can be placed in the Supplementary Information if needed, but they must be clearly referenced in the main text.

Without this comparative framework, the manuscript risks remaining too internally self-referential. Given the bold implications for hominin extinction mechanisms, this additional effort is warranted to evaluate how the Liang Luar rainfall history fits, or diverges, from broader Indo-Pacific climate signals.

We thank Reviewer 2 for providing further valuable contributions to the peer-review of this work. Following the Reviewer's advice, we have designed a new Figure 6 with a comparative framework that places the stalagmite K2 rainfall reconstructions within the regional context of the Australasian monsoon. The accompanying text concisely summarises the probable influences of well-established monsoon forcing mechanisms on three time scales: glacial–interglacial (sea level, SST), orbital precession (insolation) and millennial (interhemispheric temperature gradients).

To standardise the mechanistic analysis on all three scales, Figure 6 is necessarily based on the available high-resolution (50–100 y), U-Th dated speleothem records for China (Wang et al. 2001, Wang et al. 2008, Cheng et al. 2016), northern Borneo (Carolin et al. 2013; Carolin et al. 2016) and Liang Luar that provide continuous coverage of 92–47 ky BP, and form a north–south transect across the Australasian monsoon domain. Ten references have been added to the main text in support of Figure 6 (highlighted in green in the reference list).

An important advantage of Figure 6 is that pre-existing text on sea level, precession, and the reconstructed monsoon seasonality index is now gathered together to good effect under one heading (see '*Season-specific monsoon forcing mechanisms*'). Briefly, Figure 6 describes how the season-specific rainfall records for Flores help clarify key attributes of the Australasian monsoon through time, as follows.

Panel A: Monsoon seasonality on Flores (lines 270–279). Panel A compares the reconstructed monsoon seasonality index and the stalagmite K2 $\delta^{18}O_{iv}$ record. The point here is that the $\delta^{18}O_{iv}$ record can be viewed as an approximate measure of the *relative intensity* of local summer monsoon rainfall through time (a finding that wasn't noted in the original manuscript).

Panel B: Glacial–interglacial sea level, SST and the reconstructed mean annual rainfall for Flores (lines 280–286). Panel B illustrates the approximately synchronous reduction in all three parameters across the MIS 5/4 transition (consistent with previous studies on the effects of shelf exposure and SST cited in the original manuscript).

Panel C

Precession modulation of the reconstructed winter rainfall for Flores (lines 303–314): Panel C shows that precession-scale modulation of rainfall seasonality in Flores is primarily driven by changes in winter rainfall (as noted in the original manuscript).

Northward displacement of the Australasian monsoon rain belt and summer aridification on Flores during MIS 3 (lines 315–325): The Greenland ice-core $\delta^{18}O$ and speleothem records in panel C illustrate a critical point for the paper: a coordinated northward displacement of the Australasian monsoon rain belt when the Northern Hemisphere warmed abruptly at the onset of MIS 3, resulting in summer aridification in Flores.

Season-specific millennial phasing of summer monsoon rainfall across the Australasian monsoon domain (lines 326–338): The available records support the idea that strengthening of the Asian winter monsoon during Greenland stadials acts as a season-specific 'pressure-push' mechanism to drive southward expansions of the Australasian monsoon and wetter summers in Flores (see new main text and citations for An 2005, Denton et al. 2005, Denton et al. 2010 and Liu et al. 2015).

To comply with the journal's word limit, we are pleased to report that the new text required for Figure 6 (+440 words) is offset somewhat by deletions related to the So'a Basin (–200 words), based on the Reviewer's recommendation below. As a result, the main text has only increased to 5,620 words (~600 beyond the recommended limit).

We hope Figure 6 will serve to stimulate the integration of the new records for Flores into palaeoclimate syntheses and model experiments with the aim of understanding the season-specific mechanisms driving climate change in Australasia.

Some other comments:

Panels e–g of Figure 4 are useful schematic illustrations of hypothesized proxy responses to seasonal rainfall changes. However, they are not essential for interpreting the main data and could be moved to the Supplementary Information to streamline Figure 4. Doing so would better emphasize the empirical results shown in panels a–d and improve overall figure clarity.

We appreciate the Reviewer's suggestion for Figure 4. This figure is unusually important because it needs to illustrate (for the first time) the role of rainfall seasonality in driving divergence between stalagmite Mg/Ca and $\delta^{18}O_{iv}$. In this respect, it represents the methodological centerpiece of the paper. In our experience, the schematic examples in panels e–g have acted as a useful conceptual 'signpost' for readers who vetted the manuscript prior to submission, so we have chosen to retain them in support of the main text.

In lines 217-223, the effect of sea level and precession are discussed without supporting comparison in the figure, please add accordingly, can try separate panels in in Figure 5.

This oversight has been resolved via the new Figure 6 with its comparison of sea level and the reconstructed mean annual rainfall for Liang Luar, and precession modulation of winter rainfall.

The inclusion of the >1 Ma $\delta^{18}O$ data from Tangi Talo and Mata Menge can be confusing and even distracting regarding the main story on K2 dataset , which centers on the 91–47 ka rainfall reconstruction and extinction timing at Liang Bua. Given the large temporal, ecological, and depositional gaps, I suggest minimizing the description (to 1-2 sentences) and relocating the relevant plots to the Supplementary Information to maintain clarity and focus on the key findings supported by high-resolution, well-dated records, and avoid potential issues associated with the discussion on these data.

We thank the Reviewer for suggesting we revisit the discussion material on the early coexistence of *Homo floresiensis* and *Stegodon* on Flores. We agree that it warrants trimming, and have minimised the description to the first 2 sentences in the original introductory paragraph, as recommended. This has allowed us to consolidate two paragraphs of text into one (see lines 339–354). The merged paragraph that remains summarises the basic timeline of the two species on Flores, and notes previous work on *Stegodon* tooth enamel $\delta^{18}O$ that identified a pattern of drier climate conditions leading-in to its last appearances at key archaeological sites in the So'a Basin.

Figure 7 is designed to communicate the similarity of the scale of change in *Stegodon* $\delta^{18}O$ between the older and younger stratigraphic sequences in the So'a Basin, and at Liang Bua. We hope that the inclusion of all three records will help to stimulate further research on the potential role of recurrent migration as a strategy for adaptation to climate change on Flores.

I suggest that the authors more directly address the proposed concerns raised by the reviewers this time, particularly those related to the validation of the climate interpretations and the broader relevance of the isotope-based rainfall reconstruction. As several important critiques were acknowledged in the rebuttal but not fully resolved. A more transparent and focused response to these points would strengthen the manuscript's scientific rigor and credibility.

Gagan et al. presents a high quality study that provides a well-structured and insightful reconstruction of past rainfall amount and seasonality changes based on speleothem d18O and Mg/Ca from Flores, and further links the climate change with during *H. floresiensis* extinction. The study is quite novel as it is probably (as far as I know) the first to systematically utilize Mg/Ca and d18O to isolate summer and winter rainfall signal, and quantitatively estimate rainfall amount and its seasonality based on binary isotope mixing model.

The authors have done rigid step-by-step argumentation on the application of the methodology, including 1) careful evaluation of the prerequisites to apply the method; 2) drip water-rainfall-speleothem d18O agreement and reproducibility test; 3) validation of PCP from perspectives including Mg-Sr covariation, correlation, and a Rayleigh Fractionation model, 4) careful application of the isotope mixing model and calibration, as well as cautious consideration of potential uncertainty sources, and 5) comparison with model outputs and cross-validation between Fossil Stegodon and speleothem $\delta^{18}O$ on the range of value and aridity trends. The manuscript also shows the best efforts made to connect the climate with the extinction based on correlation as well as more mechanical causal linkage. I therefore suggest 'accept with minor revisions regarding below comments and suggestions' for the manuscript.

Comments and suggestions:

The paper relies heavily on multi proxies of one single stalagmite, although they agree well among themselves, it is still necessary to compare the original/reconstructed hydroclimate changes with other regional hydroclimate records to validate the trends and identify any potential biases. Borneo speleothem records (both $\delta^{18}O$ and $\delta^{13}O$) and some marine/lake sediment (SST, $\delta^{18}O_{sw}$, trace metal, and compound-specific d13C/dD) should cover this period.

I have spent quite some time trying to understand the binary mixing model. I suggest either break this section into smaller parts or providing a step-by-step guide to improve the readability. A clearer justification on the corrections for periods when divergence occurs and Equation 13 does not apply would be helpful. The overall procedure should be briefly in the method section of the main text and slightly more detailed in the supplemental file, potentially by using a flow chart/summary diagram that summarizes the sequence of the steps or conceptualize the framework.

Could other proxy like Sr/Ca and Ba/Ca be potentially helpful in validating the estimates and enhance their robustness? The effect of fractionation can be added in the captions to describe

A sensitivity analysis, by systematically varying key input parameters like the non-convective d18O and endmembers, can be applied to assess their impact on reconstructed rainfall estimates.

The study can benefit from a brief discussion on the potential/effort of applying the method using modern/younger stalagmites (with decadal resolution) with rainfall amount observation directly comparable and whether such application would be comparable to or helpful for the construction of the older portion.

The revised manuscript overall presents a better shape regarding the clearness of the isotope mixing model and the quantification of seasonal rainfall amount, but still lacks a critical contextual element: direct comparison with independent regional paleoclimate records.

While the authors argue that regional hydroclimates in the region are complex and that direct comparisons may be difficult, I believe this makes it even more essential to explicitly compare with the few available archives that *do* cover the same period and could provide insight into broader-scale hydroclimate trends (e.g., Borneo speleothems, Sulawesi lake sediment records, marine $\delta^{13}C$ or leaf-wax records from Java/Sunda margin). The fact that these records are "not directly comparable" does not diminish their value; on the contrary, presenting agreements *and* discrepancies transparently would highlight the local–regional dynamics and help assess the broader relevance of the K2 record.

Therefore, I strongly recommend that the authors include one or more figures comparing the K2 record with other regional archives, accompanied by a concise but critical discussion of the similarities and differences. These figures can be placed in the Supplementary Information if needed, but they must be clearly referenced in the main text.

Without this comparative framework, the manuscript risks remaining too internally self-referential. Given the bold implications for hominin extinction mechanisms, this additional effort is warranted to evaluate how the Liang Luar rainfall history fits, or diverges, from broader Indo-Pacific climate signals.

Some other comments:

Panels e–g of Figure 4 are useful schematic illustrations of hypothesized proxy responses to seasonal rainfall changes. However, they are not essential for interpreting the main data and could be moved to the Supplementary Information to streamline Figure 4. Doing so would better emphasize the empirical results shown in panels a–d and improve overall figure clarity.

In lines 217-223, the effect of sea level and precession are discussed without supporting comparison in the figure, please add accordingly, can try separate panels in in Figure 5.

The inclusion of the >1 Ma $\delta^{18}O$ data from Tangi Talo and Mata Menge can be confusing and even distracting regarding the main story on K2 dataset , which centers on the 91–47 ka rainfall reconstruction and extinction timing at Liang Bua. Given the large temporal, ecological, and depositional gaps, I suggest minimizing the description (to 1-2 sentences) and relocating the relevant plots to the Supplementary Information to maintain clarity and focus on the key findings supported by high-resolution, well-dated records, and avoid potential issues associated with the discussion on these data.

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