nature portfolio

Peer Review File

Atmospheric river activity during the late Holocene exceeds modern range of variability in California

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

11th Jan 24

Dear Dr Knight,

Your manuscript titled "Atmospheric river activity during the late Holocene exceeds modern range of variability in California" has now been seen by 2 reviewers, and we include their comments at the end of this message. They find your work of interest, but some important points are raised. We are interested in the possibility of publishing your study in Communications Earth & Environment, but would like to consider your responses to these concerns 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. Should additional work allow you to address these criticisms (that is, either to incorporate the suggestions or provide a compelling argument why the point made by the reviewer is not valid, or relevant to the editorial threshold as outlined below)
AND meet our editorial thresholds as outlined below, then we would be happy to look at a substantially revised manuscript.

***** ***** ***** ***** *****

In the following, we list our main editorial concerns. Please, address these in your rebuttal letter

- Ensure that the spatial distribution of IVT data used for calibration is clearly defined. Discuss potential limitations of your approach if the original data set covers area larger than Lake Leonard watershed

- Compare your record visually with mentioned key hydroclimate reconstructions from the region and consider possible mechanisms driving observed trends

- Explain the apparent inconsistency between the magnitude of instrumental and reconstructed IVT data (Fig. 5, panels A and F) and the claim that past variability exceeded the modern one

***** ***** ***** ***** *****

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:
[link redacted]
** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

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,

Ola Kwiecien, PhD
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Communications Earth & Environment
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Alienor Lavergne, PhD
Associate editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMATTING

We ask that you ensure your manuscript complies with our editorial policies. Please ensure that the following formatting requirements are met, and any checklist relevant to your research is completed and uploaded as a Related Manuscript file type with the revised article.

Editorial Policy: Policy requirements (Download the link to your computer as a PDF.)

Furthermore, please align your manuscript with our format requirements, which are summarized on the following checklist:
Communications Earth & Environment formatting checklist

and also in our style and formatting guide Communications Earth & Environment formatting guide .

*** DATA: Communications Earth & Environment endorses the principles of the Enabling FAIR data project (http://www.copdess.org/enabling-fair-data-project/). We ask authors to make the data that support their conclusions available in permanent, publically accessible data repositories. (Please contact the editor if you are unable to make your data available).

All Communications Earth & Environment manuscripts must include a section titled "Data Availability" at the end of the Methods section or main text (if no Methods). More information on this policy, is available at http://www.nature.com/authors/policies/data/data-availability-statements-data-citations.pdf.

In particular, the Data availability statement should include:
- Unique identifiers (such as DOIs and hyperlinks for datasets in public repositories)
- Accession codes where appropriate

- If applicable, a statement regarding data available with restrictions
- If a dataset has a Digital Object Identifier (DOI) as its unique identifier, we strongly encourage including this in the Reference list and citing the dataset in the Data Availability Statement.

DATA SOURCES: All new data associated with the paper should be placed in a persistent repository where they can be freely and enduringly accessed. We recommend submitting the data to discipline-specific, community-recognized repositories, where possible and a list of recommended repositories is provided at http://www.nature.com/sdata/policies/repositories.

If a community resource is unavailable, data can be submitted to generalist repositories such as figshare or Dryad Digital Repository. Please provide a unique identifier for the data (for example a DOI or a permanent URL) in the data availability statement, if possible. If the repository does not provide identifiers, we encourage authors to supply the search terms that will return the data. For data that have been obtained from publically available sources, please provide a URL and the specific data product name in the data availability statement. Data with a DOI should be further cited in the methods reference section.

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

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of Knight et al. “Atmospheric river activity during the late Holocene exceeds modern range of variability in California”

This manuscript presents a proxy reconstruction of Atmospheric river (AR) activity in Northern California based on sediment geochemical data spanning the past 3,200 years. This is a creative idea to reconstruct this specific hydroclimate phenomenon, which is relevant for modern extreme precipitation event hazards. Certain aspects of the paper are deserving of praise. The statistical approach used is well-suited for the research question and in-line with modern paleoclimate methodological best practices. The results have important implications for the understanding of hydroclimate hazards in California and the Western US. However, several points need to be clarified before I can recommend publication in Communications Earth & Environment.

General comments
1) The definition of IVT data used for the calibration needs to be clarified. Is it the IVT associated with all AR events in the Gershunov et al. 2017 dataset? Is there any spatial limitation? If it is as, I understand it, the total IVT for all ARs in western N America during a year, that is an enormous region. There is no reason to expect the Leonard Lake sediment record to be sensitive to AR activity in British Columbia. I would be interested in seeing the analysis done only including ARs that likely impacted the Leonard Lake watershed.

2) The discussion of regional paleoclimate (lines 140-157) would be greatly enhanced by a figure showing some of the other key hydroclimate reconstructions from the region that are discussed in the text.

3) I would encourage the authors to mention possible mechanisms driving trends in AR activity during the late Holocene. Are changes in IVT likely driven by thermodynamic considerations (how much moisture can the atmosphere hold), or do latitudinal temperature gradients play a role (i.e. (Routson et al., 2019)?

4) On line 112 and elsewhere it sounds like you searched for correlations amongst other proxies (certainly clay). It would be worth showing those results, possibly in the supplement. It is also worth considering the risk of a false discovering from multiple hypothesis testing if testing correlations of many proxies.

5) It is worth considering the scale of variability of the reconstruction, and the potential for "regression dilution" or slope attenuation. See (Esper et al., 2005). The highest values of the reconstruction are much less than the highest values of the instrumental period, but this result might be due to regression dilution. Scaling might give a better idea of the magnitude of variability. This is at least worth discussing, if not implementing.

Specific Comments
Line 19: Saying clay layers feels almost misleading here. The correlation you report is with Si/Al, which is associated with clay, but not 1:1 the same.

Line 99 and 279: I am wondering why it appears you used a time uncertain approach to correlating geochemical data from a core record. If the data can be assigned to the same depth scale, then it does not make sense to use an ensemble of possible ages to correlate the datasets.

Line 99: is the p value corrected for autocorrelation?

Introduction: Worth citing Von Gunten 2012 in addition to Grosjean, 2009.

Line 135: Is this claim supported by the data? The highest values of the reconstruction are much less than the highest values of the instrumental period. Unless I'm misunderstanding something when comparing panels A and F in Fig. 5 (see comment 5 above)

To me, it is more logical to discuss the mechanism linking ARs and clay deposition before discussing the implications of the reconstruction. Therefore, I would move lines 158-185 to before lines 129-157.

Line 170-178: How are you able to rule out an alternative interpretation of instantaneous flood deposition of homogenous layers?

Line 250: This is phrased in a way that seems not quite accurate to me. I don't think the 1000 age model iterations are the most probable ones, they are simply drawn from the posterior distribution randomly.

Line 254: I don't think you need to cite this methodological choice. Acknowledgements feels appropriate.

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was not measured, were diatoms observed in smear-slide analysis of sediments?

Line 279: Between which datasets?

Line 282: The correlation of IVT or IWV and percent clay is never reported in any way. If you mention it in methods, it should be mentioned somewhere.

Line 289-291: Need more details here, see comment 1 above.

Fig. 2: There is a common convention in stratigraphic logs to draw profiles such that a greater extent on the x-axis corresponds with a higher grain size. Your profile happens to have the reverse pattern. It would be more intuitive for readers who often read stratigraphic logs if the sand layers were widest, and clay layers most narrow in the x-dimension.

Fig 3c and 5b: The units appear to match Si/Al, not IVT?

Supplement line 14: What caused the missing values? Are these from outliers, gaps in the sediment core? Datasets on different resolutions?

Fig. S4, it would certainly be interesting to place some AR data here. Are ARs associated with the biggest local precip and lake level changes?

References

Esper, J., Frank, D. C., Wilson, R. J. S., and Briffa, K. R.: Effect of scaling and regression on reconstructed temperature amplitude for the past millennium, https://doi.org/10.1029/2004GL021236, 2005.

von Gunten, L., Grosjean, M., Kamenik, C., Fujak, M., and Urrutia, R.: Calibrating biogeochemical and physical climate proxies from non-varved lake sediments with meteorological data: Methods and case studies, Journal of Paleolimnology, 47, 583–600, https://doi.org/10.1007/s10933-012-9582-9, 2012.

Routson, C. C., McKay, N. P., Kaufman, D. S., Erb, M. P., Goosse, H., Shuman, B. N., Rodysill, J. R., and Ault, T.: Mid-latitude net precipitation decreased with Arctic warming during the Holocene, Nature, 568, 83–87, https://doi.org/10.1038/s41586-019-1060-3, 2019.

Reviewer #2 (Remarks to the Author):

Review of Knight et al. "Atmospheric river activity during the late Holocene exceeds modern range of variability in California" for Comms E&E - Dec 2023

Summary

Knight and coauthors present a unique and valuable record of calibrated integrated vapor transport, a metric of AR intensity, using lake sediments in California. The authors present a well-written, thoroughly analyzed dataset that ultimately relies primarily on Si/Al and it's modern correlation to IVT to derive an IVT reconstruction for the last 3,200 years. This work is important and impactful, my comments below are minor but restructuring the discussion might require some work.

Major Comments

1) I would suggest some minor restructuring of the results/discussion. Specifically the sedimentological observations and geomorphic explanation (beginning line 158) should really go before any atmospheric interpretation. Then it was odd to discuss the proxy uncertainty (paragraph starting line 186) after having already used the proxy earlier in the discussion.

1a) Related to above, is it possible to further annotate or include on Figure 1 the geomorphic features described in the section beginning in line 158? Perhaps extending the C panel outward? It would also be helpful to have the surface streams on panel B.

2) Figure S5. a) I did not see mention of the outlier age at 236 cm, I noticed it was a (pine) needle? Please comment on this in methods section on age model. b) what do the authors make of the change in sedimentation rate at ~200 cm?

3) Is there a way to more impactfully illustrate the key result in Figure 5 suggesting that that the largest AR episodes occurred 2-3 millennia ago?

4) I was expecting a stack plot of the Si/Al record and IVT reconstruction with other proxy records (e.g. those discussed in section beginning line 140). Maybe that's too traditional, but I think it would be interesting to see how the tree ring records and other records discussed in that section visually align with this new record.

Line-by-line comments

19: I appreciate the thoroughness but the r and p values are likely not needed in an abstract unless required by the journal.

62-64: Not entirely true, see Kirby et al. (2021; 10.1130/2018.2536(09)), but agree limited.

85-87: I would say that for Holocene work it's a given at this point, not sure this sentence is really needed but I appreciate you might need to cite briefly what method(s) you will use to quantify uncertainty in the time domain.

137-139: This is vague, I don't disagree but we (paleoclimate scientists) often write such statements. Is there a way to be more specific than "underestimation of potential risks."

267-270: There is no petrogenic organic carbon from bedrock sources? It would (possibly) require LOI >550 to quantify.

285-291: It would have been nice to have more detail here. The IVT and IWV, what area/lat long box was this calculated over?

Figure 2: Is there a slight misalignment between the strat column (panel B) and the res of the panels?

Figure S4. First, this is an incredible dataset. It took me a while, but it looks like the x-axis is non-linear? Also, why is there a break in the line for precipitation? Please tidy this figure up.

Figure S7. Just to check - the two 'uncertainty' dashed colors represent the same CI range as outputted by the two software programs? (I do not have any experience with Plum).

Point-by-point responses to reviewers' comments
Comments from the reviewers are in bold; our replies are not bold.

Reviewer #1 (Remarks to the Author):

Review of Knight et al. "Atmospheric river activity during the late Holocene exceeds modern range of variability in California"

This manuscript presents a proxy reconstruction of Atmospheric river (AR) activity in Northern California based on sediment geochemical data spanning the past 3,200 years. This is a creative idea to reconstruct this specific hydroclimate phenomenon, which is relevant for modern extreme precipitation event hazards. Certain aspects of the paper are deserving of praise. The statistical approach used is well-suited for the research question and in-line with modern paleoclimate methodological best practices. The results have important implications for the understanding of hydroclimate hazards in California and the Western US. However, several points need to be clarified before I can recommend publication in Communications Earth & Environment.

We appreciate the Reviewer's comments about the strengths of our work. Their suggestions greatly improved our submission. We hope they will agree that we have addressed their comments thoroughly and appropriately.

General comments
1) The definition of IVT data used for the calibration needs to be clarified. Is it the IVT associated with all AR events in the Gershunov et al. 2017 dataset? Is there any spatial limitation? If it is as, I understand it, the total IVT for all ARs in western N America during a year, that is an enormous region. There is no reason to expect the Leonard Lake sediment record to be sensitive to AR activity in British Columbia. I would be interested in seeing the analysis done only including ARs that likely impacted the Leonard Lake watershed.

We regret the confusion from our original description on the Gershunov dataset. While the entire Gershunov dataset has AR events from across western North America, we had filtered their dataset in our original submission to the narrow latitudinal band around Leonard Lake: 40°N latitude at 2.5°resolution.

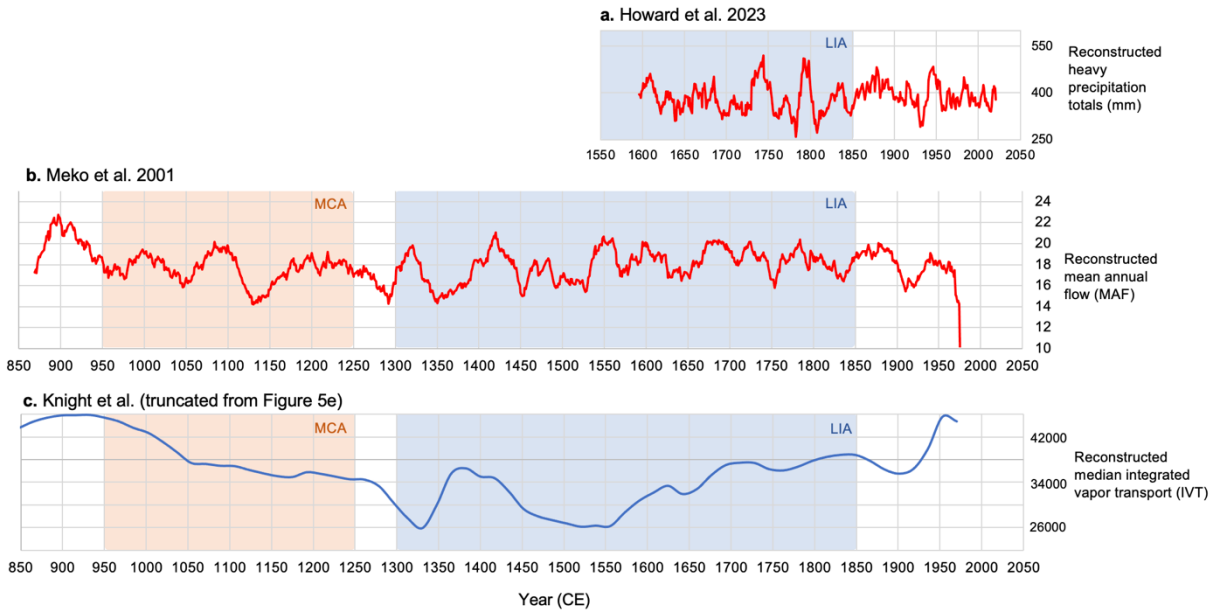
We clarify this in-text: "As such, we calculated an annual sum for both IVT and IWV for ARs that were specified at 40°N in the Gershunov et al. (2017) database. We then calibrated our proxy data to these sums." (Line 446). Hence, the analysis and results are constrained to only those ARs impacting our study site's watershed.

2) The discussion of regional paleoclimate (lines 140-157) would be greatly enhanced by a figure showing some of the other key hydroclimate reconstructions from the region that are discussed in the text.

We have added Figure S6a-c (see below or in-text page 35) to show the most relevant local/regional hydroclimate reconstructions. We plot Howard et al. and Meko et al.'s data on an x-axis that has been consistently scaled. We plotted the 30-year moving window (red lines) of their datasets and added the LIA and MCA as panels. Reviewer 2 also requested a similar contextual figure.

Howard's reconstructed heavy precipitation from tree rings is the better comparable to our dataset. Meko et al.'s data is annual flow for the Sacramento, which reflects all rainfall and snowmelt from southern Cascadia and northern Sierra Nevada. As tree-ring based records, they are inherently noisier and more temporally precise; our dataset integrates multiple years and has less precision. Our dataset also concerns a phenomenon (ARs) distinct from total precipitation. In short, a directly comparable dataset does not exist, making this research output particularly valuable to the community and difficult to compare directly with extant paleohydrologic reconstructions.

With these caveats in mind, the broad trend in hydroclimate points to fewer ARs in the MCA and more ARs in the LIA, suggesting a role for ARs in those climatic periods.



3) I would encourage the authors to mention possible mechanisms driving trends in AR activity during the late Holocene. Are changes in IVT likely driven by thermodynamic considerations (how much moisture can the atmosphere hold), or do latitudinal temperature gradients play a role (i.e. (Routson et al., 2019)?

We appreciate the encouragement to discuss mechanisms driving AR trends; we had previously shied away from speculating on this aspect without accompanying simulations (which would be a large separate effort). We now mention current

hypotheses; however, we cannot use this dataset to say which driver is more likely to influence the changes in IVT.

Our discussion now includes possible drivers, citing Routson et al. and others, which we believe provides more context to our findings:

“The high median IVT values reconstructed in the late Holocene (Fig. 5e) may have been driven by changes in latitudinal temperature gradients, consistent with increasing mid-latitude net precipitation since 8 ka inferred from proxy records (Routson et al. 2019) as well as midlatitude Northern hemisphere models showing increasing storm activity since 10 ka (Jackson and Broccoli 2003). Moreover, thermodynamic considerations, such as increased AR vapor content due to warmer air and sea surface temperature, also may have contributed to past changes in IVT seen here (Gao et al. 2015). These same atmospheric parameters are expected to predominantly drive increases in the number of ARs in the future (Gao et al. 2015). The mechanism(s) notwithstanding…” (Lines 231-243).

4) On line 112 and elsewhere it sounds like you searched for correlations amongst other proxies (certainly clay). It would be worth showing those results, possibly in the supplement. It is also worth considering the risk of a false discovering from multiple hypothesis testing if testing correlations of many proxies.

That’s correct, we looked at the correlations between clay and IVT/IWV, Si/Al and IVT/IWV (which was already presented in Figure 4a,b). We added the correlation result from clay and IVT/IWV to the supplement in a new figure, Fig. S4ab, page 33). In all four correlations, we used the Benjamini and Hochberg false discovery rate correction, as stated on line 155; we also note that we did not test “many proxies”, just clay and Si/Al because they were previously linked to erosion/floods. Hence, we reduced the risk of false discovery and over-interpretation by only looking at proxies with a physical explanation for correlations.

5) It is worth considering the scale of variability of the reconstruction, and the potential for “regression dilution” or slope attenuation. See (Esper et al., 2005). The highest values of the reconstruction are much less than the highest values of the instrumental period, but this result might be due to regression dilution. Scaling might give a better idea of the magnitude of variability. This is at least worth discussing, if not implementing.

We sincerely thank the Reviewer for calling this to our attention because the implementation of scaling showed there was, in fact, a regression dilution!

To take a step back about why regression dilution could be plausible in the first place, we plotted our proxy Si/Al with age uncertainty. The highest median Si/Al values in the record occur at -800 CE and -50 CE, followed by the Si/Al value measured in the calibration period at 1950 CE (See Figure 1 below). However, as the Reviewer notes,

this is not translated into the regression; that is, you would expect the highest IVT at the highest Si/Al values in deep time, but the original regression does not predict that. Upon implementation of the scaling, following Esper et al. 2005 provided by Reviewer 1, we can see that this is corrected and the highest IVT is indeed at the highest Si/Al values from the deep past, as opposed to the modern period (Figure 2).

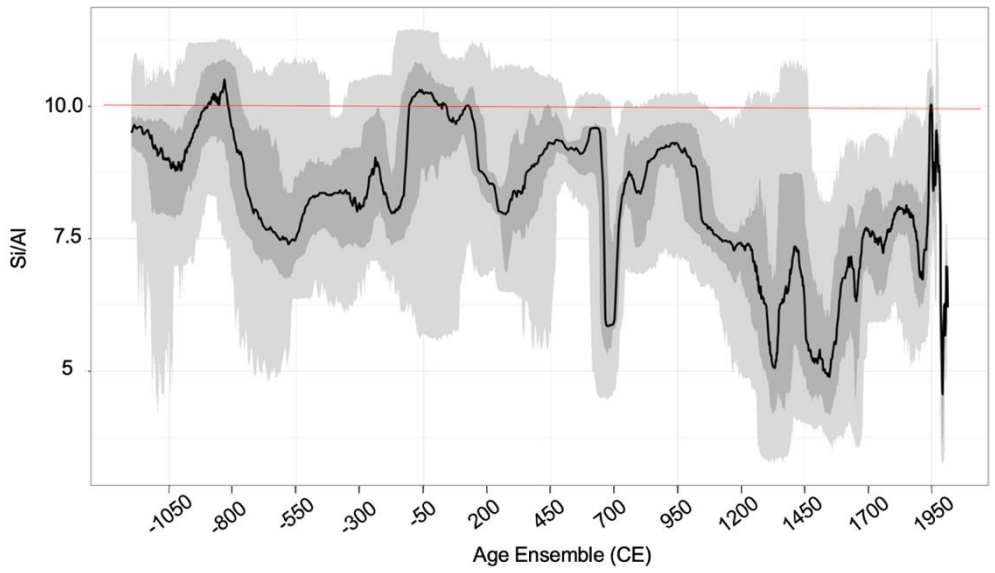


Figure 1 for comment 5. Evidence of regression dilution is made clear by the highest median Si/Al values (black line) in the record arising at approximately -50 CE and -750 CE. The red line is a visual aid at the highest detected values of Si/Al (over 10).

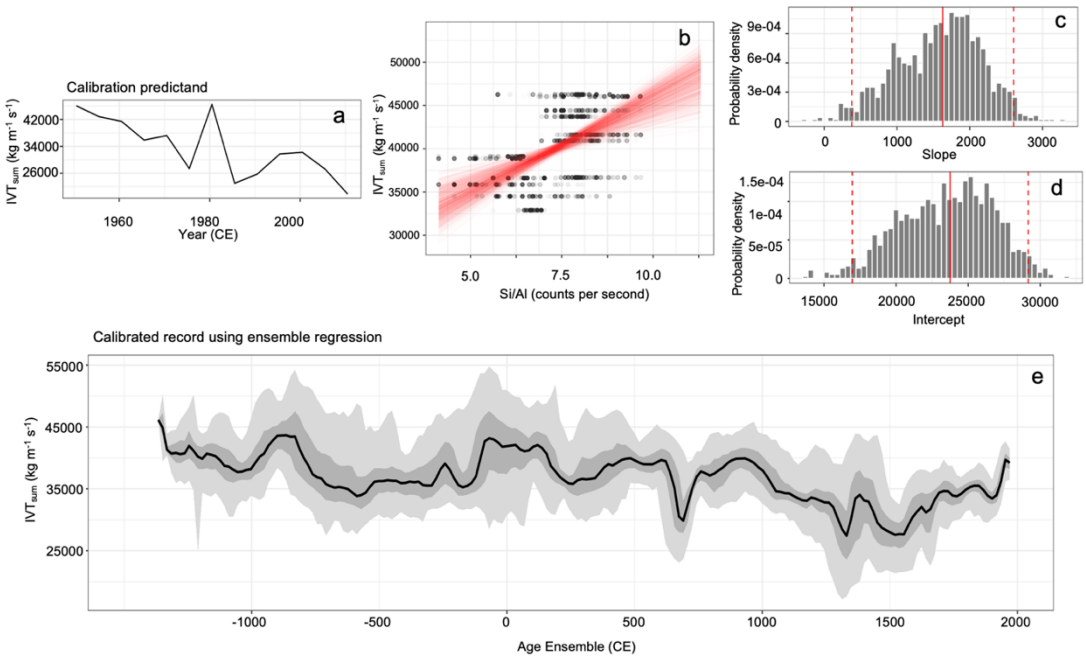


Figure 2 for comment 5. In panel **a)** the summed annual IVT instrumental data from Gershunov et al. 2017, where the largest values from 1950 and 1981 are shown. Panel **e)** shows the results from the IVT reconstruction where scaling was applied to the data (regression dilution controlled), where the largest predicted IVT values are found

approximately -50 and -750 CE (about 2000 and 2700 years ago), aligning with the highest Si/Al values in the record (see above Figure 1).

We revised the methods section in line with these changes (Lines 391-392):
“Proxy data were scaled, following Esper et al. (2005), to guard against slope attenuation (“regression dilution”).”

Specific Comments

Line 19: Saying clay layers feels almost misleading here. The correlation you report is with Si/Al, which is associated with clay, but not 1:1 the same.

We have edited this line to say: “Si/Al enriched layers” instead of “clay layers.”

Line 99 and 279: I am wondering why it appears you used a time uncertain approach to correlating geochemical data from a core record. If the data can be assigned to the same depth scale, then it does not make sense to use an ensemble of possible ages to correlate the datasets.

For Lines 226-227, the Reviewer is correct, and we regret the error. We have now fixed the correlation between the core’s geochemical data. We note it changed only slightly.

For Line 435, we were referring to the correlation between the proxy and instrumental datasets, where it is appropriate to use age-ensemble correlations. To clarify in the text, we added this information explicitly: “Pearson’s correlation coefficients between proxy and instrumental data were calculated for each age ensemble.” (Line 435).

Line 99: is the p value corrected for autocorrelation?

This p-value (above, Line 99) is from a correlation, not a linear regression.

Introduction: Worth citing Von Gunten 2012 in addition to Grosjean, 2009.

We have now cited Von Gunten 2012 with Grosjean 2009.

Line 135: Is this claim supported by the data? The highest values of the reconstruction are much less than the highest values of the instrumental period. Unless I’m misunderstanding something when comparing panels A and F in Fig. 5 (see comment 5 above).

We again thank the Reviewer for pointing this out and providing a method (Esper et al. 2005) to correct it. The claim holds. The highest values of the reconstruction are indeed the highest in the record. See details above in comment 5.

To me, it is more logical to discuss the mechanism linking ARs and clay

deposition before discussing the implications of the reconstruction. Therefore, I would move lines 158-185 to before lines 129-157.

We have moved the paragraph in question to the suggestion location. That is, it is now the first paragraph under the header "*Extreme precipitation over the late Holocene in California."*

Line 170-178: How are you able to rule out an alternative interpretation of instantaneous flood deposition of homogenous layers?

Our understanding of modern erosional processes into this lake strongly suggests the homogenous layers are the result of oxygenated bottom waters and bioturbation, not instantaneous floods. Flood deposits should preserve grading with fining upwards from the high energy to lower energy input. If these were instantaneous deposits, we would see that in our grain size data given the scale of the homogenous deposits.

Line 250: This is phrased in a way that seems not quite accurate to me. I don't think the 1000 age model iterations are the most probable ones, they are simply drawn from the posterior distribution randomly.

We modified the sentence to say: "a composite of 1,000 highly probable age models" (Line 401). We based our phrasing on this quote from McKay et al. 2021, which describes how GeoChronR leverages Bacon: "Bacon will optionally return a subset of the MCMC accumulation rate ensemble members with **high *a posteriori* probabilities**, which GeoChronR uses to form age ensemble members for subsequent analysis."

Line 254: I don't think you need to cite this methodological choice. Acknowledgements feels appropriate.

We moved this citation to the acknowledgments, as suggested.

Line 269-270: The residual could include biogenic silica. Was this assessed? This also has important implications for the Si/Al proxy, which has often been used as a proxy for diatom (bio-silica). If BSi was not measured, were diatoms observed in smear-slide analysis of sediments?

Yes, we assessed the biogenic silica (opal) and found minor levels. Diatoms were not observed in clay layers.

Line 279: Between which datasets?

We clarified: "Pearson's correlation coefficients between proxy and instrumental data…" (Lines 435).

Line 282: The correlation of IVT or IWV and percent clay is never reported in any way. If you mention it in methods, it should be mentioned somewhere.

We added Figure S4a,b to show these correlations in the supplementary materials (see page 33).

Line 289-291: Need more details here, see comment 1 above.

We added the latitudinal band used to filter the data, which was done in the original analysis.

Fig. 2: There is a common convention in stratigraphic logs to draw profiles such that a greater extent on the x-axis corresponds with a higher grain size. Your profile happens to have the reverse pattern. It would be more intuitive for readers who often read stratigraphic logs if the sand layers were widest, and clay layers most narrow in the x-dimension.

We weren't aware of that convention, but it makes sense. We have revised accordingly. See revised Figure 2 panel b, so that the clay layers are the narrowest and the sand layers are the widest (below).

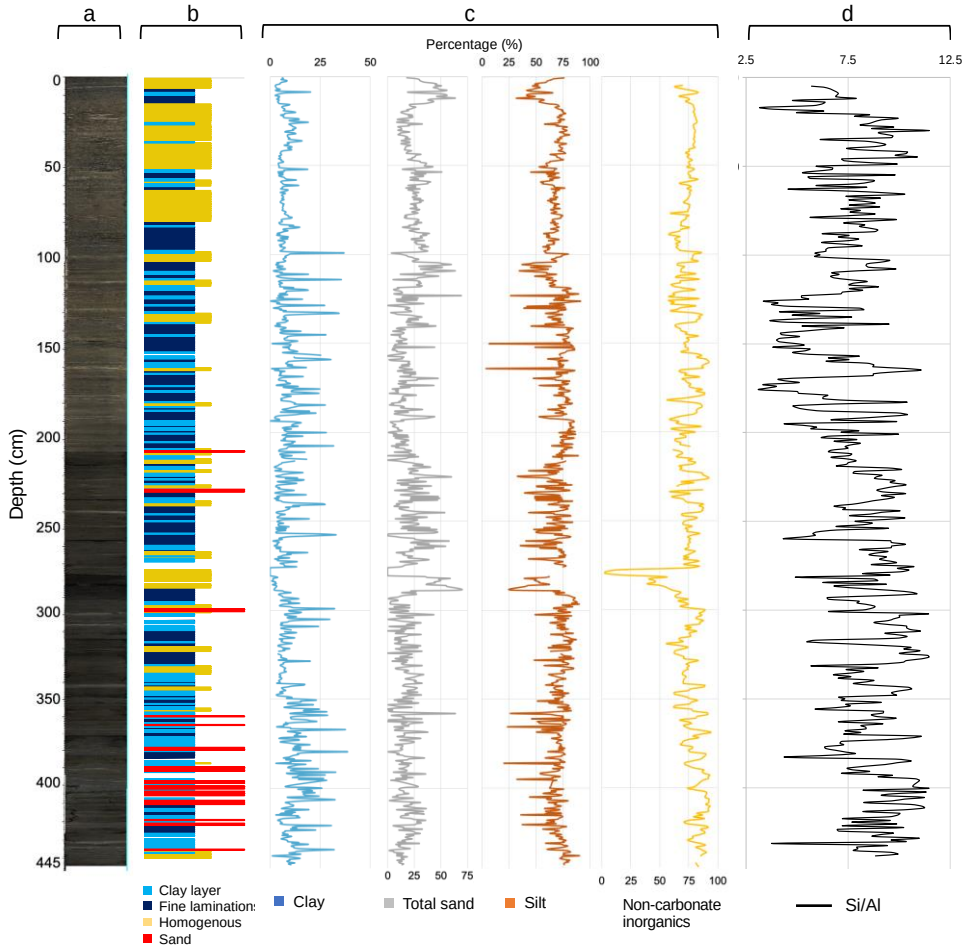


Fig 3c and 5b: The units appear to match Si/Al, not IVT?

For context, these figures were created using geoChronR and the labeling is automatic within the software. We checked our code and the McKay et al. 2021 paper (which debuted geoChronR) and there appears to be a mistake in the preloaded label for this figure output, both within McKay et al. 2021 and our original figures. That is, an incorrect label is automatically produced for Figure 3c/5b. Given that Figure 5a correctly shows the calibration predictand, we simply use that for clarity. Note there is no longer a "Fig. 3c."

Supplement line 14: What caused the missing values? Are these from outliers, gaps in the sediment core? Datasets on different resolutions?

The handheld XRF can return missing values (or values under the detection limit) when water attenuates the signal. This is a known consideration with handheld XRF. We also had an area of the core comprised of decomposed wood, which contributed to the data gap.

We added this line to clarify for the reader:
"Missing values (or values below the detection limit) can occur in handheld XRF measurements due to water attenuation." (Supplementary materials, line 24-25, page 30).

Fig. S4, it would certainly be interesting to place some AR data here. Are ARs associated with the biggest local precip and lake level changes?

We revised Figure S5 (page 34 and below) to include suggestions from both reviewers. The new figure and caption (below) show the large AR storms associated with increasing local precipitation and subsequent increases in lake level.

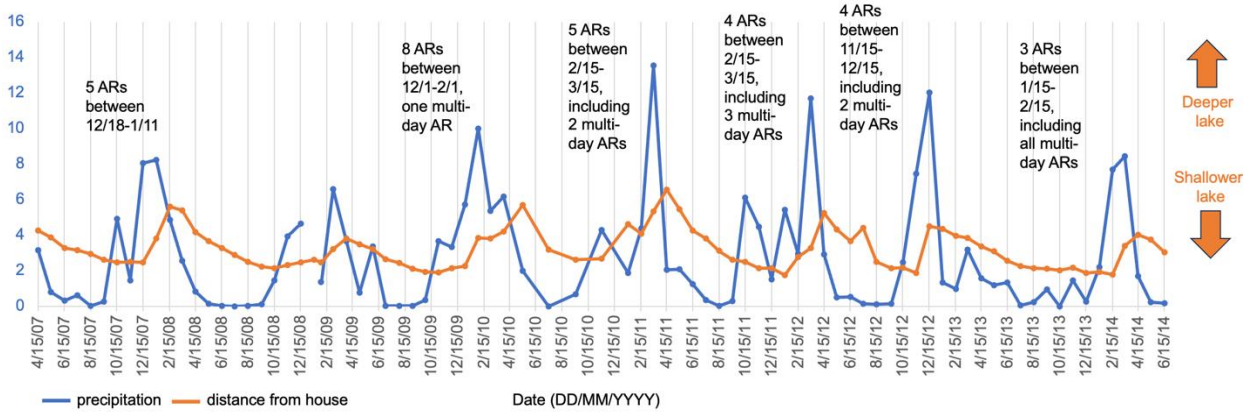


Fig. S5. Precipitation and lake level. Local precipitation (cm, blue) and the distance from lake shore (m, orange, a proxy for lake level) at Leonard Lake. AR storms preceding the largest peaks in local precipitation are noted. The y-axis for the orange

line is reversed to allow a more intuitive interpretation of lake level. Courtesy of Sandra Marshall (personal communication 2023).

References

Esper, J., Frank, D. C., Wilson, R. J. S., and Briffa, K. R.: Effect of scaling and regression on reconstructed temperature amplitude for the past millennium, https://doi.org/10.1029/2004GL021236, 2005.

von Gunten, L., Grosjean, M., Kamenik, C., Fujak, M., and Urrutia, R.: Calibrating biogeochemical and physical climate proxies from non-varved lake sediments with meteorological data: Methods and case studies, Journal of Paleolimnology, 47, 583–600, https://doi.org/10.1007/s10933-012-9582-9, 2012.

Routson, C. C., McKay, N. P., Kaufman, D. S., Erb, M. P., Goosse, H., Shuman, B. N., Rodysill, J. R., and Ault, T.: Mid-latitude net precipitation decreased with Arctic warming during the Holocene, Nature, 568, 83–87, https://doi.org/10.1038/s41586-019-1060-3, 2019.

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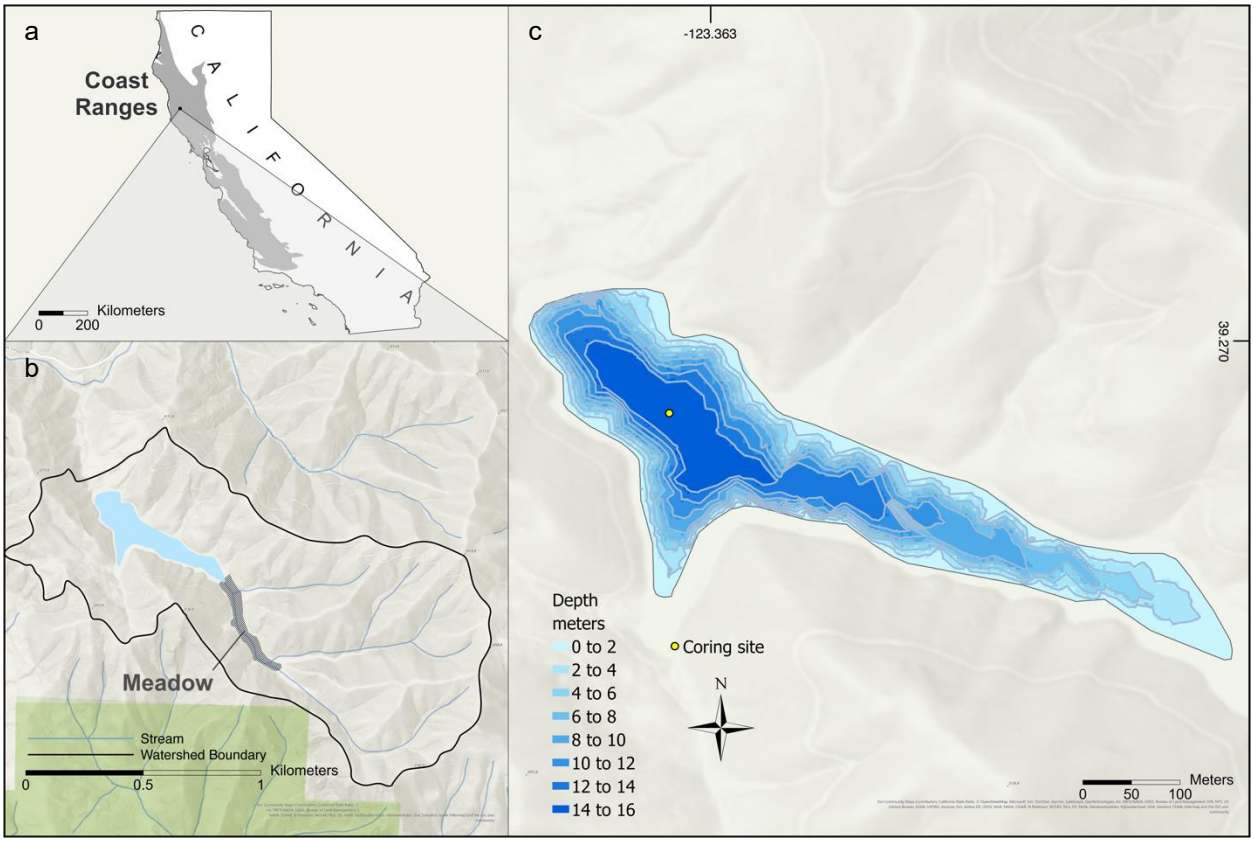
We appreciate the Reviewer's constructive assessment of our work. We outline below how our revised manuscript aligns with their comments.

Major Comments
1) I would suggest some minor restructuring of the results/discussion. Specifically the sedimentological observations and geomorphic explanation (beginning line 158) should really go before any atmospheric interpretation. Then it was odd to discuss the proxy uncertainty (paragraph starting line 186) after having already used the proxy earlier in the discussion.

Reviewer 1 also recommended this switch in the discussion to start with the sedimentological observations and then go into the atmospheric interpretation. The flow is now: sedimentological → reconstruction → reconstruction uncertainty, as suggested. See lines 187-222 (stratigraphic/sedimentological data) where text was moved up.

1a) Related to above, is it possible to further annotate or include on Figure 1 the geomorphic features described in the section beginning in line 158? Perhaps extending the C panel outward? It would also be helpful to have the surface streams on panel B.

We have revised Figure 1 (below). We include surface streams and add hatching to indicate the meadow in panel b This more clearly connects to the geomorphic features described in our discussion section. We note that panel c is not a good candidate for expansion because zooming out would hide the bathymetric details, which is why we modified panel b instead.

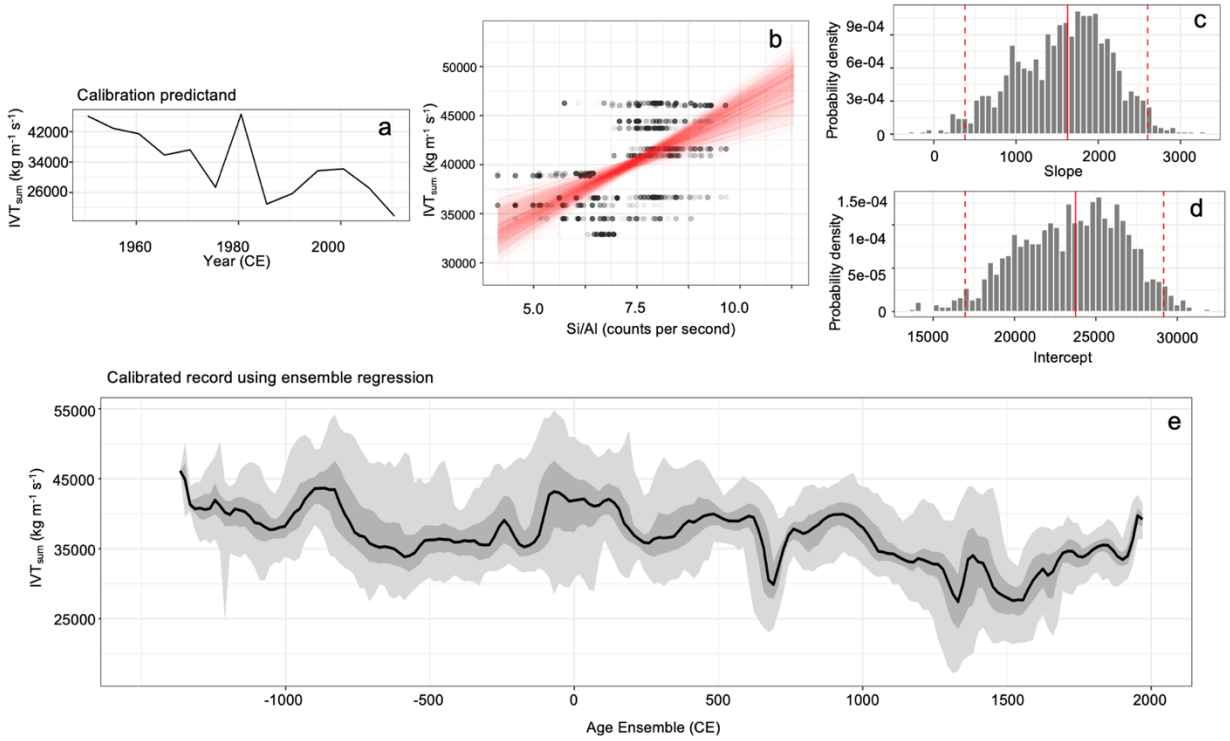


2) Figure S5. a) I did not see mention of the outlier age at 236 cm, I noticed it was a (pine) needle? Please comment on this in methods section on age model. b) what do the authors make of the change in sedimentation rate at ~200 cm?

A) Yes, this was the needle at 236cm. We now state in Lines 372-374 of the methods: “We note the date at 236 cm represents an age reversal and was treated as an outlier by the model. Given that the two proximal dates at 237.5 cm and 244.5 cm overlap at an uncertainty of two sigma, we deem the exclusion of the date at 236 cm as justifiable.”
B) That the change appears to be reasonably gradual.

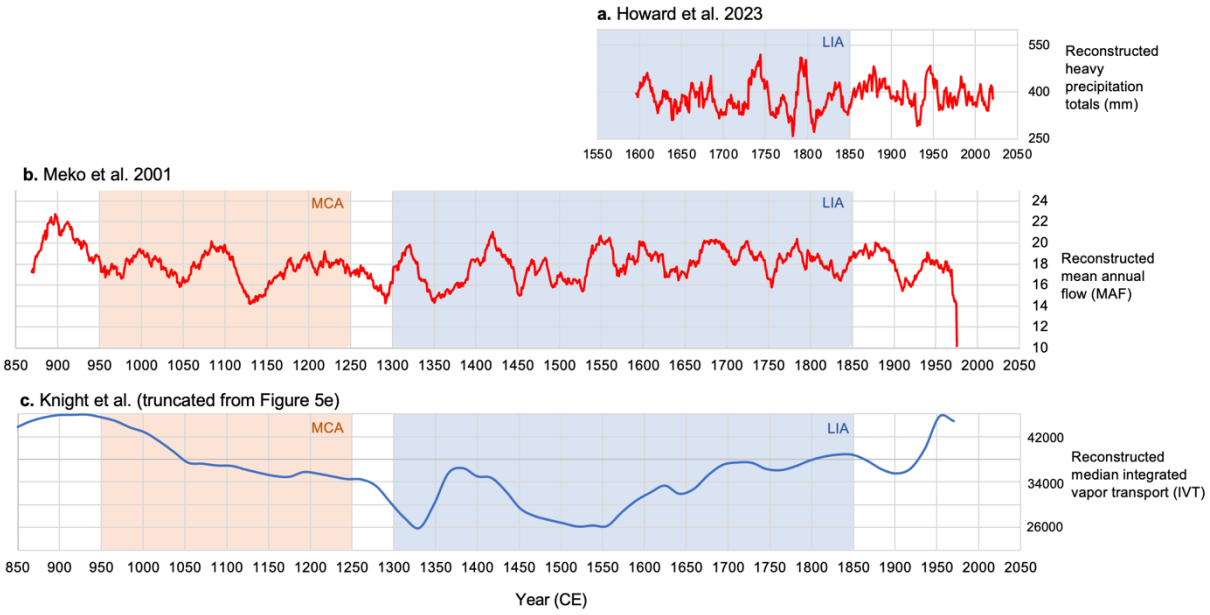
3) Is there a way to more impactfully illustrate the key result in Figure 5 suggesting that that the largest AR episodes occurred 2-3 millennia ago?

Yes, we have fully revised Figure 5, in line with Reviewer 1’s comments about the apparent inconsistency in the reconstructed IVT and instrumental data. Hence, the largest episodes from 2-3 millennia are much clearer. The process for this change is described above. The new Figure 5 is on page 26 and below.



4) I was expecting a stack plot of the Si/Al record and IVT reconstruction with other proxy records (e.g. those discussed in section beginning line 140). Maybe that's too traditional, but I think it would be interesting to see how the tree ring records and other records discussed in that section visually align with this new record.

We are currently at the limit of 5 figures for this style of paper. However, we added a supplemental figure (below, Figure S6a-c) that shows the regional paleoclimate reconstructions that we discuss (Howard and Meko) in a stack plot, with a 30-year moving window smoothing (red line) the original data. We added important information like the LIA and MCA. However, we refrain from trying to “wiggle match” these records with ours because our record is 1) much longer, 2) not annual resolution, and 3) recording a different aspect of the climate (as a first of its kind IVT record). For example, a high IVT year (or 5 years) is possible without necessarily having higher than average precipitation or river flow.



Line-by-line comments

19: I appreciate the thoroughness but the r and p values are likely not needed in an abstract unless required by the journal.

We have removed this information from the abstract.

62-64: Not entirely true, see Kirby et al. (2021; 10.1130/2018.2536(09)), but agree limited.

We appreciate learning about this study. (We note the DOI provided goes to a paper from 2018 not 2021, we assume the 2018 paper is the one the reviewer intended to suggest.)

We rephrased our statement to be:
“With few exceptions (Kirby et al. 2018), lacustrine data in California chronicle centennial to millennial-scale pluvial events from the Sierra Nevada (e.g., Street et al. 2012) and southern California (Kirby et al. 2012, Blunt and Negrini 2015) but lack the resolution to capture shorter-term events” (Lines 84-85).

85-87: I would say that for Holocene work it's a given at this point, not sure this sentence is really needed but I appreciate you might need to cite briefly what method(s) you will use to quantify uncertainty in the time domain.

We would like to err on the side of over-explanation, particularly for methodological aspects, and have kept this sentence.

137-139: This is vague, I don't disagree but we (paleoclimate scientists) often write such statements. Is there a way to be more specific than "underestimation of potential risks."

We added two examples about the knock-on risks from extreme precipitation; however, a full discussion of risks is not in the scope of a paleoclimate letter-style journal article.

Our revised statement (lines 243-246): “Mechanism(s) notwithstanding, these data provide a critical perspective to improve the precision and accuracy of risk assessments for precipitation related threats, such as enhanced flash flooding or debris flow (Huang and Swain 2022).”

267-270: There is no petrogenic organic carbon from bedrock sources? It would (possibly) require LOI >550 to quantify.

This is a good point. There may indeed be petrogenic organic carbon present in the sedimentary matrix; we note that its presence would affect the OM and carbonate percentages and not the non-carbonate inorganic plot we show in Figure 2. The Reviewer does not suggest a revision in-text.

285-291: It would have been nice to have more detail here. The IVT and IWV, what area/lat long box was this calculated over?

We regret the oversight in detail with the Gershunov data. We added the spatial distribution of the dataset. Reviewer 1 also requested this information.

Revised statement: “As such, we calculated an annual sum for both IVT and IWV for ARs that were specified at 40°N in the Gershunov et al. 2017 database. We then calibrated our proxy data to these sums.” (Line 435).

Figure 2: Is there a slight misalignment between the strat column (panel B) and the res of the panels?

Panel B is a record of the particle size and we did not measure the last few centimeters of the core, so it is slightly shorter than the other panels.

Figure S4. First, this is an incredible dataset. It took me a while, but it looks like the x-axis is non-linear? Also, why is there a break in the line for precipitation? Please tidy this figure up.

This dataset was generated by a community scientist and is admittedly unusual. The community scientist monitored the distance from a locally set survey benchmark from the lake shore. Data were recorded approximately every 60 days over seven years. The

local precipitation has a gap because one reading is missing. Nevertheless, the main point is clear: heavy rainfall at Leonard Lake translated into lake level rise and drought resulted in a drop of lake level.

We overlaid AR information to this figure, as suggested by Reviewer 1, which we hope adds context. We also combined the local precipitation data and lake level proxy data onto the same plot to help with clarity and tidying up this graphic.

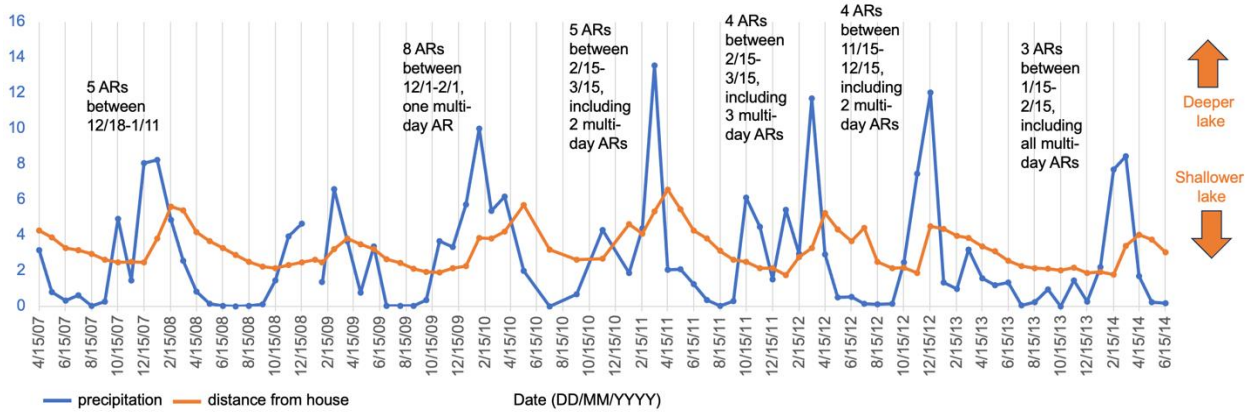


Figure S7. Just to check - the two 'uncertainty' dashed colors represent the same CI range as outputted by the two software programs? (I do not have any experience with Plum).

Correct. We added verbiage to the caption to clarify: “The median calibrated years from the age-depth models produced from *Bacon* (blue) and *Plum* (orange) with associated uncertainty (dashed lines, matching colors) **from the software programs**.”

Reconciliation of the USGS internal review

Mensing review of Knight et al. A 3,200-year sedimentary record of extreme precipitation in Northern California: links to atmospheric rivers

I found this paper very interesting and well-constructed. The language is clear and the results very important given the short history of ARs and the recognition of their importance to California water totals.

We are grateful for the thorough and constructive review of our manuscript.

Below are some thoughts about specific sections:

The age model, Fig 2, is given in cal yr BP, the sediment and XRF data (Fig. 3) are shown by depth, and the ensemble regression (Fig 4) is shown in YR AD. This makes it very hard to go back and forth between the figures to try and see the relationship between the raw data and the ensemble figure that is the proxy for Ars. I recommend using either CE age dates or cal yr BP consistently and putting the both the age and depth on the axes for figs. 3 and 4 – or at the very least add depth in addition to age to Fig. 4 so that one can try to see the patterns of the sedimentary data in relation to the AR inference.

Our figures have changed significantly since this review. The ensemble regression is now shown in year CE. We also added a new figure (Fig. 3a,b) to show the calibration Si/Al data and the instrumental IVT data. We changed all references to age to the year CE and BCE for consistency.

For example: “The last time median IVT was as high as the modern period (~40,000 kg m^{-1} s^{-1}) occurred before the onset of the Medieval warming in 950 CE, after which it appears to have declined for several centuries to the record’s nadir of ~27,500 kg m-1 s^{-1} in the 1500s (Fig. 5e). Median IVT rose with some fluctuation during the Little Ice Age (1300 to 1850 CE). The largest median peaks in IVT occur ca. -70 CE and -860 CE and were approximately 44,000 kg m^{-1} s^{-1} (Fig. 5e).”

The illustration of the original sedimentary data to the water vapor data is also a bit cryptic. It is mentioned in the text that 1970 had high Si/Al values, but in Fig 4 which as I understand it is the only representation of the modern calibration between sediments and water vapor, there is nothing unusual about 1970 to suggest that the clay layer is more associated with ARs than the other sediments.

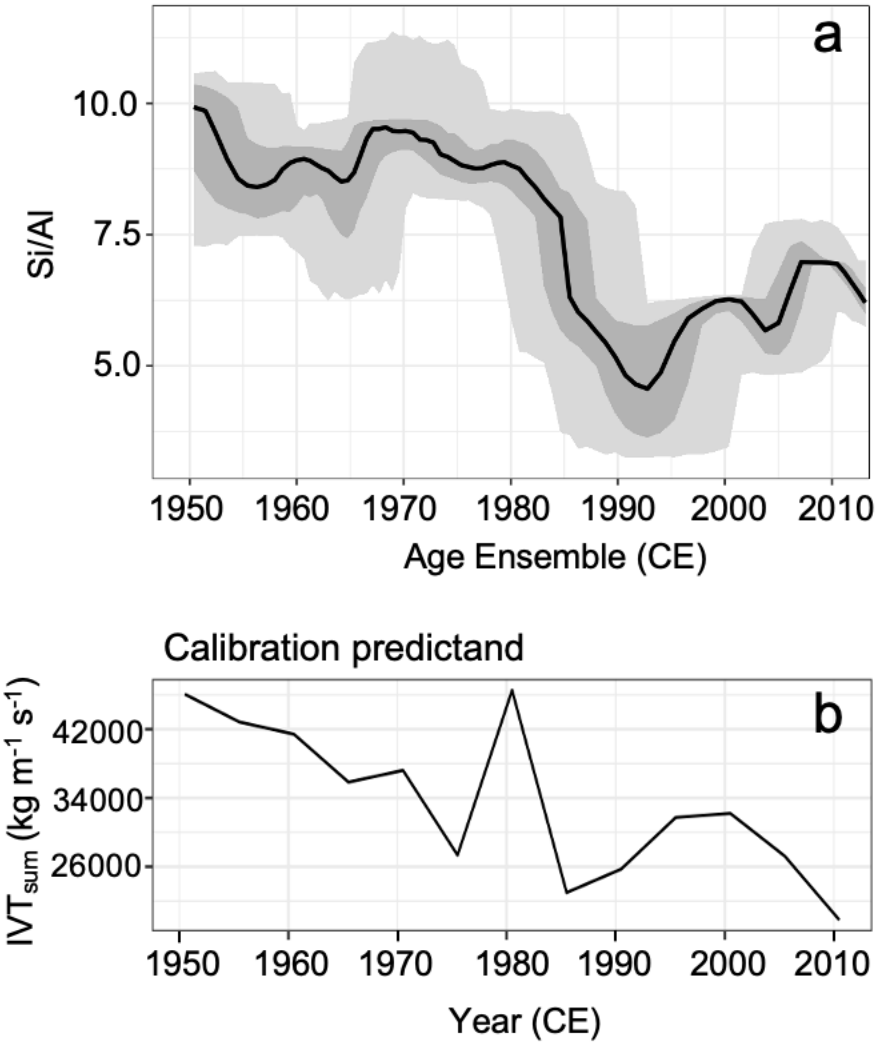
We regret the error in the text. The highest Si/Al value actually reflects 1950 CE, not 1970 CE, which is where the confusion lies in the connection to clay.

I do not fully follow the statistics in the ensemble regression and perhaps if I did this would be clearer. But to me there is no one figure that visually helps me see that the sediments in the calibration period are closely associated with AR

activity during that period, especially since AR activity during that period is not shown.

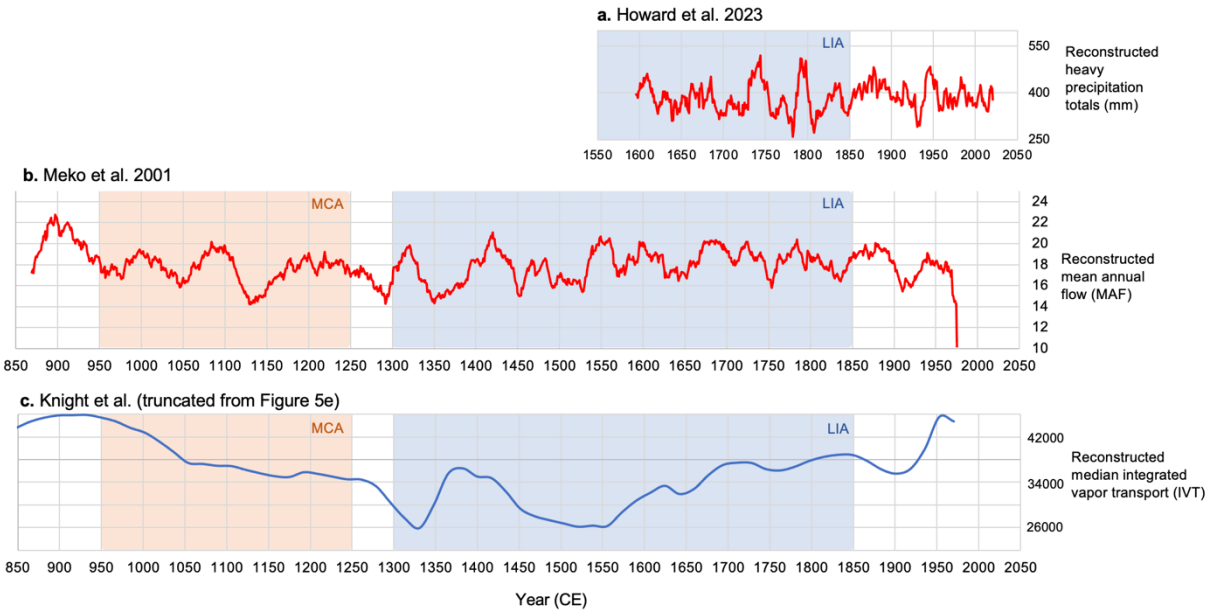
We agree that there was not one figure that shows the connection between Si/Al and AR activity. We have added Fig.3a,b which combines the Si/Al curve (with age model uncertainty) and AR activity in two forms during the calibration period. This visual allows an easy comparison between our proxy (Si/Al) and the instrumental record and demonstrates their strong correspondence, which underlies the calibration and regression model.

Newly added Fig. 3:



Lines 231 – 235: Several periods of low vapor transit are noted in the reconstruction, and then one of these is favorably compared with the 16th century mega-drought, which is generally strongest in the southwest such as Arizona. I looked at Dave Meko's 2001 reconstruction of Sacramento River flow from tree rings (Journal of the American Water Resources Assoc. v37, #4 pp. 1029 – 1039. Some of the driest individual years are indeed in the early 1500s, but the driest 50 year period is between about 1350 and 1400 CE. The driest 20 year period is in the early 1300s. The record does not go back far enough to capture the 600-725 period. I suggest taking a closer look at this paper and potentially there are others that have been reconstructed more recently as the comparison with Leonard Lake, since the Sacramento River system seems geographically more relevant than reconstructions from the southwest.

We appreciate the Reviewer bringing this information to our attention with regard to the drought interpretation. We have added additional information about the Meko 2001 tree ring reconstruction because as the Reviewer notes it is geographically relevant and in alignment with our findings. We now discuss three regional paleoclimate proxies: Howard et al. (2023), Trouet et al. (2013), and Meko et al. (2001). We also added a supplementary figure (Fig. S6a-c) to help visualize the Howard and Meko datasets, where a 30-year moving average of the original data is plotted in red.



New language is bolded and italicized below (Lines 250-277):

Heavy precipitation, as reconstructed from blue oak tree rings in northern California, also suggest an increase in the frequency or intensity of ARs in northern California since

the 1700s (Howard et al. 2023). The reconstruction also indicates decreasing IVT during the Medieval Climate Anomaly, a period characterized by centennial-scale aridity in the North American West (Trouet et al. 2013). Our reconstruction shows several eras of low IVT occurring in 600-725 and 1250-1350 CE, as well as the 1500s – the latter of which is concurrent with the widespread 16th century mega-drought (Woodhouse and Overpeck 1998, Trouet et al. 2013). ***A reconstruction of Sacramento River flows found the driest 20-year period was in the early 1300s (Meko et al. 2001), aligning with the low IVT predicted between 1250-1350 CE, and this record also shows some of the*** **driest individual years occurred in the 1500s.** ***The consensus across multiple records lends credence to our reconstruction****.* Although our record cannot reconstruct specific storm events, the alignment with these broader climate trends highlights the importance of extreme precipitation in driving pluvial/drought cycles in California going back millennia."

We note the 16th century mega-drought is described by Trouet et al. (2013) as "a **remarkably widespread** and persistent period of drought in the late 16th century (Woodhouse and Overpeck 1998)," so we kept the discussion of this event in our work.

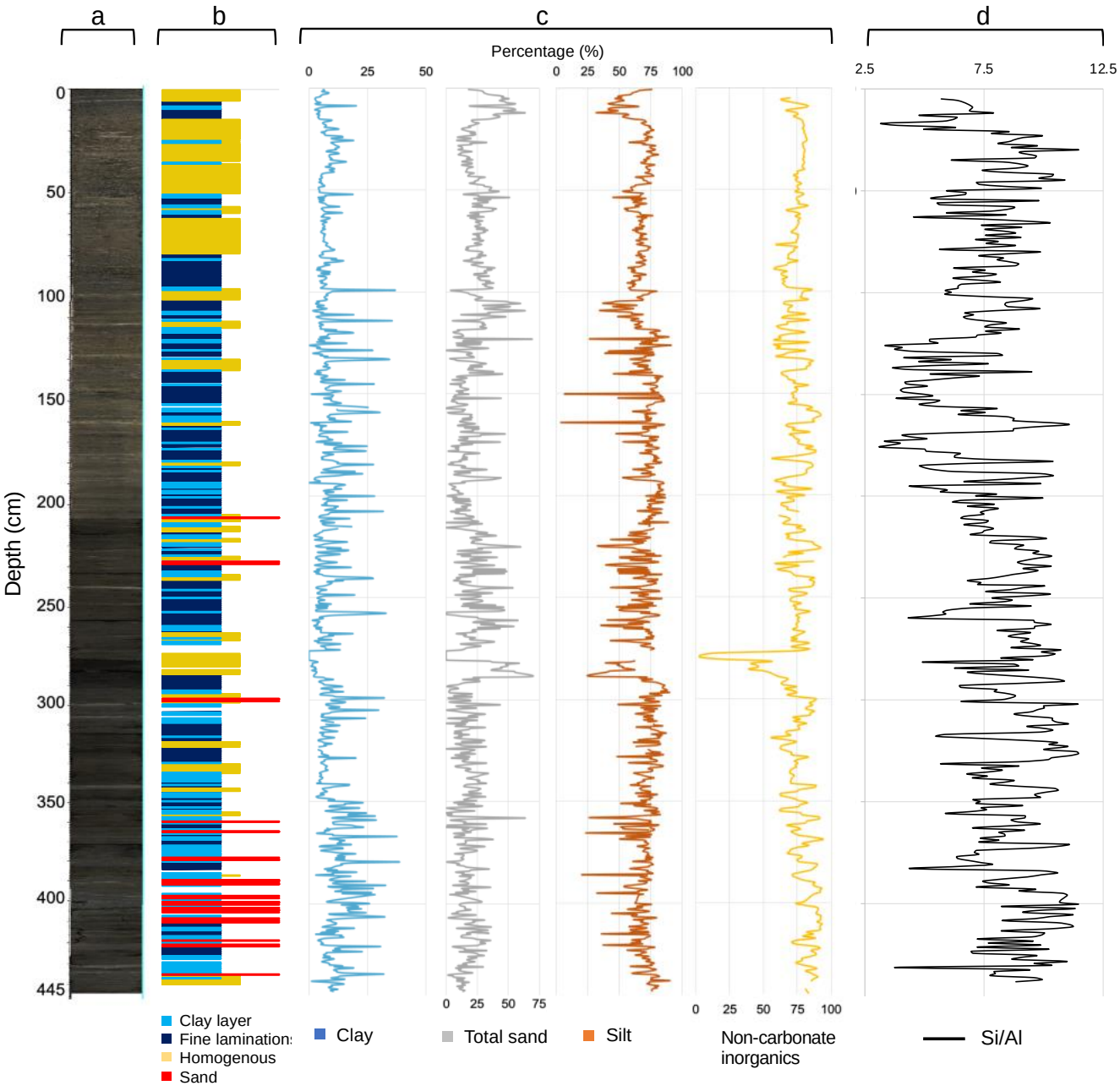
As another point of comparison, the patterns of the Mono Lake high and low stands during the MCA (Stine 1990 PPP) are not apparent in this record. The comparison of this record with other recognized paleorecords of streamflow and lake levels is not straightforward, however the conclusion suggest that it accords well with these records. The challenge is that the MCA and LIA are broad terms with a fair bit of climate variability within them and it would make a stronger conclusion to point to similarity in other recognized reconstructions rather than the climate periods by name only.

We have now pointed to four reconstructions and discussed the similarities with our work: Howard et al. 2023, Meko et al. 2001, Trouet et al. 2013, Woodhouse and Overpeck 1998. We agree that MCA/LIA are broad, but our record also aligns with the general climate expectation from these periods. That is, decreased IVT during the MCA and increased IVT with the LIA. We think this is important to note along with the specific periods already discussed.

Line 236 – These patterns are not easily visible in the figure as plotted. If there is some way to see a zoomed in image of the pattern I think it would help support this statement of the repeating patterns – perhaps in supplemental figures. Perhaps the image of the core in association with the measurements that support

this statement. I am thinking about this in relation to your sedimentation rates. In the calibration period, one year is about 0.5 cm.

We have updated the figure of core lithology so that the color-coded layers are thicker and the repeating patterns are clearer, see below.



If you have distinct clay layers in this section of the core that you think are related to AR activity – is there a way to show this? Downcore your average sedimentation is just over 1mm per year. How think are the clay layers? If you think the clay layers represent events somehow, is it possible to show these? I recognize that on the whole you cannot identify storm events because of the chronology etc., but it does sound from the text as if you think there is an event signature. Is there a way to show the closeup of this, even diagrammatically, and its relationship with the XRF data at the same depths to further strengthen the claim that you have AR events.

We want to be clear that there is not an "event signature" because we do not know how long it takes the clay to settle out. Instead, we think we have a signature of higher energy winter precipitation.

It is tricky to visually display the relationship between all the proxies and the core stratigraphy given the age uncertainty embedded. This is why we used *geoChronR* to quantify the relationships and not rely on "eye balling" the relationships. However, we agree that a close up of the Si/Al proxy and AR would helpful. We added Figure 3 (see above) which demonstrates that link with the uncertainty also plotted.

Fig 4B caption – you state dark and like gray but I think you mean dark and light.

We have fixed this typo.

Not much is made of the observation that the AR reconstruction of the calibration period is higher than anything in the last 1000 years – including any time during the Little Ice Age. What might be driving this recent pattern? Perhaps any discussion of the long term patterns is too speculative, however any reader is going to wonder about what is driving this since there have been much higher periods in the past. It would be useful to think about this and add something if it seems appropriate.

We note that Prof Mensing's comment that "the AR reconstruction of the calibration period is higher than anything in the last 1000 years" is not correct now that we've adjusted our analysis. In fact, the highest IVT values are in the deep past. We think this confusion stems from the regression dilution problem noted by journal Reviewer 1, which we have fixed (see pages 3-4).

We have added to the discussion about the driving mechanisms (Lines 231-246):

"The large median IVT values predicted by this reconstruction in the late Holocene (Fig. 5e) may have been driven by changes in latitudinal temperature gradients, consistent with increasing mid-latitude net precipitation since 8 ka inferred from proxy records (Routson et al. 2019) as well as modeled increasing storm activity since 10 ka (Jackson and Broccoli 2003). Moreover, thermodynamic considerations, such as increased AR vapor content due to warmer air and sea surface temperature, also may have

contributed to past changes in IVT seen here. These considerations are expected to predominantly drive increases in the number of ARs in the future (Gao et al. 2015). The mechanism(s) notwithstanding…”

Thanks for the opportunity to read this as part of the USGS review process.

Scott Mensing
Professor Emeritus
Geography, University of Nevada, Reno

29th Feb 24

Dear Dr Knight,

Your manuscript titled "Atmospheric river activity during the late Holocene exceeds modern range of variability in California" 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 under the open access CC BY license (Creative Commons Attribution v4.0 International License).

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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Best regards,

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REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Round 2 Review Knight et al.

We commend the authors for their thorough responses to the comments. I have a few more suggestions to improve the manuscript.

The discussion of possible drivers of AR activity should be clarified and requires more specificity. You write, “The high median IVT values reconstructed in the late Holocene (Fig. 5e) may have been driven by changes in latitudinal temperature gradients, consistent with increasing mid-latitude net precipitation since 8 ka inferred from proxy records (Routson et al. 2019) as well as midlatitude Northern hemisphere models showing increasing storm activity since 10 ka (Jackson and Broccoli 2003)“

However, your record extends only to ca 3 ka BP, so there is no way to know if late Holocene IVT is “high” relative to early or mid-Holocene. In fact, your record shows general decreasing trend in IVT over the past 3000 years while the latitudinal temperature gradient has increased, which is opposite of the relationship described in the quoted text. That trend is not strong or consistent, so I don’t necessarily say this is strong evidence, but I think its important to be clear because depending on the direction of the relationship, we would have different expectations for AR activity in the future with

a weakened latitudinal temperature gradient.

Another mechanism that could be explored is comparisons with Pacific SST trends over the past 3000 years, but I understand that mechanistic determinations are not the focus of this work.

Line 96 (clean revised version): Thanks for correcting that its not needed to use an ensemble approach to correlation between these proxies. My other question was whether the p-value was adjusted for autocorrelation, i.e., using the effective-n method, or another method. That question still stands.

Fig 3: please include information about the binning applied in this plot in the caption. It appears panel A is at a higher resolution than panel b? I think it would be preferable to compare the two datasets at the same resolution.

Regarding scaling, I think the sentence on scaling is misplaced in the Geochronology section, and fits better in Analyses and instrumental data. Additionally, please add text to say at what step in the data analysis workflow the amplitude scaling was applied. As it is written now, it reads like scaling was applied first and then a linear regression was applied, but I think this would result in the same slope attenuation as before. The results in your plots seem to reflect scaling done after or in place of the linear regression (which is correct, as described in Esper et al., 2005). It could also be mentioned if the scaling was done on the binned data, and if it was done with variable scaling coefficients for each ensemble iteration.

-Paul Zander

Reviewer #2 (Remarks to the Author):

Re-review of Knight et al. "Atmospheric river activity during the late Holocene exceeds modern range of variability in California"

I have no further comments, Knight and colleagues did an excellent job with both my comments and those from reviewer 1, as well as the USGS review. I look forward to seeing this work in the literature and hope that it can be a model for high-resolution work of this nature in the future related to phenomenon like atmospheric rivers.

Reviewer #1 (Remarks to the Author):

Round 2 Review Knight et al.

We commend the authors for their thorough responses to the comments. I have a few more suggestions to improve the manuscript.

We thank Dr. Zander for two helpful reviews. We address each remaining point below.

The discussion of possible drivers of AR activity should be clarified and requires more specificity. You write, "The high median IVT values reconstructed in the late Holocene (Fig. 5e) may have been driven by changes in latitudinal temperature gradients, consistent with increasing mid-latitude net precipitation since 8 ka inferred from proxy records (Routson et al. 2019) as well as midlatitude Northern hemisphere models showing increasing storm activity since 10 ka (Jackson and Broccoli 2003)" However, your record extends only to ca 3 ka BP, so there is no way to know if late Holocene IVT is "high" relative to early or mid-Holocene. In fact, your record shows general decreasing trend in IVT over the past 3000 years while the latitudinal temperature gradient has increased, which is opposite of the relationship described in the quoted text. That trend is not strong or consistent, so I don't necessarily say this is strong evidence, but I think its important to be clear because depending on the direction of the relationship, we would have different expectations for AR activity in the future with a weakened latitudinal temperature gradient. Another mechanism that could be explored is comparisons with Pacific SST trends over the past 3000 years, but I understand that mechanistic determinations are not the focus of this work.

We have added more nuance to our discussion on possible mechanistic causes, pointing out the trends in our data and how they compare to Routson et al. (32), Jackson and Broccoli (33), Shin et al. (34), and Skinner et al. (35), with additional nuance from and Gao et al. (36).

Our revised section is as follows:

"Based on inferences from proxy records, Northern Hemisphere midlatitude net precipitation has increased since 8 ka[32] and midlatitude Northern hemisphere models show storm activity has increased since 10 ka[33]. The early to middle Holocene was drier and a potential driver of these conditions was a weaker latitudinal temperature gradient. A weaker latitudinal gradient has again developed under anthropogenic warming; it has been argued that this could be consistent with future aridity and a reduction in mid-latitude water resources[32]. Our reconstruction appears to show that AR activity may not be linked to the northern hemisphere temperature gradient. In contrast, we find a slight decreasing trend in IVT values between -1300 and 1500 CE (Fig. 5e) while the latitudinal temperature gradient was strong, and an increasing trend in IVT values between 1500 CE and the present day (Fig. 5e) as the latitudinal temperature gradient weakened. The difference between our record's trends and others'[32] may simply reflect

the fact that ARs are distinct features of the hydroclimate that are not interchangeable with net precipitation. Differences could also be related to sub-regional variability detected by our site and/or changes in temperature and insolation gradients, such as the role of cooler tropical Pacific sea surface temperatures and North American aridity[34]. Nevertheless, our work is consistent with recent model output comparing midlatitude landfalling ARs in western North America between the mid-Holocene and the preindustrial period[35]. This research shows a drying trend in annual AR precipitation between the two periods, which is supported by the decreasing IVT trend in our record before the preindustrial era, and is attributed to decreased atmospheric vapor content related to decreasing summer insolation[35]. Thermodynamic considerations, such as increased AR vapor content due to warmer air and sea surface temperature, are expected to predominantly drive increases in the number of ARs in the future[36]. Mechanism(s) notwithstanding, these data provide a critical perspective to improve the precision and accuracy of risk assessments for precipitation related threats, such as enhanced flash flooding or debris flow[13]."

Line 96 (clean revised version): Thanks for correcting that its not needed to use an ensemble approach to correlation between these proxies. My other question was whether the p-value was adjusted for autocorrelation, i.e., using the effective-n method, or another method. That question still stands.

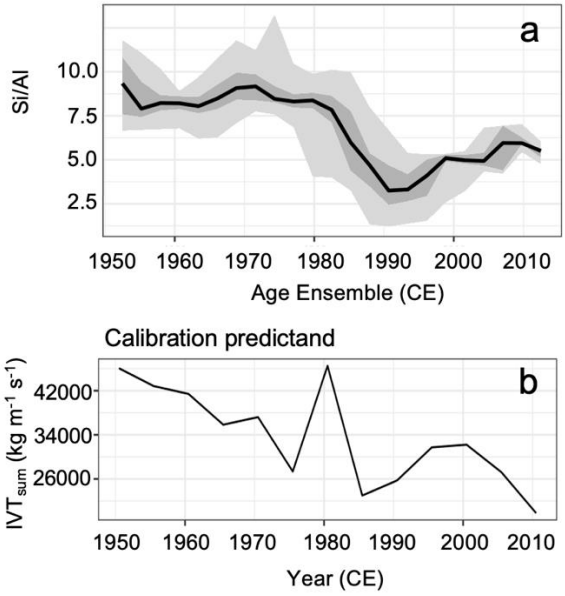
Our apologies for missing the second part of your original question. We went back and checked, and the p-value was adjusted for autocorrelation using the isospectral approach, which is the default in geoChronR.

We now state: "Clay values above ~9% tend to produce a clay layer detectable to the naked eye and the correlation between Si/Al and percent clay is positive and significant (r = 0.30, p = 0.02, where autocorrelation was accounted for with the isospectral approach)."

Fig 3: please include information about the binning applied in this plot in the caption. It appears panel A is at a higher resolution than panel b? I think it would be preferable to compare the two datasets at the same resolution.

We added the binning information to the caption: "The graph shows the calibration-in-time predictand where the y-axis is annually summed IVT (kg m-1 s-1) and binned at a 5-year interval."

Yes, panel A was at slightly higher resolution to reflect the Si/Al record as it naturally occurs in the upper sediment (about ~1.5 to 3 yr/cm) with age uncertainty bounds. However, we adjusted panel A to match the resolution as panel B. See below:



Regarding scaling, I think the sentence on scaling is misplaced in the Geochronology section, and fits better in Analyses and instrumental data. Additionally, please add text to say at what step in the data analysis workflow the amplitude scaling was applied. As it is written now, it reads like scaling was applied first and then a linear regression was applied, but I think this would result in the same slope attenuation as before. The results in your plots seem to reflect scaling done after or in place of the linear regression (which is correct, as described in Esper et al., 2005). It *could* also be mentioned if the scaling was done on the binned data, and if it was done with variable scaling coefficients for each ensemble iteration. -Paul Zander

We have moved the description of scaling to the analyses section and instrumental data. We edited the text so that it is clear scaling occurred after the linear regression on binned data:

“For each iteration in the analysis, an age ensemble member was selected and used to bin the Si/Al (or clay data) onto a 5-year interval where data were averaged. The instrumental AR data were also binned at the same timescale….Within *geoChronR*, a linear regression model was derived, and then proxy data were scaled, following Esper et al. (2005), to guard against slope attenuation (“regression dilution”).”

Reviewer #2 (Remarks to the Author):

Re-review of Knight et al. "Atmospheric river activity during the late Holocene exceeds modern range of variability in California"

I have no further comments, Knight and colleagues did an excellent job with both

my comments and those from reviewer 1, as well as the USGS review. I look forward to seeing this work in the literature and hope that it can be a model for high-resolution work of this nature in the future related to phenomenon like atmospheric rivers.

Wonderful, thank you for the nice comments and help to improve our work.