SEE RESPONSES TO BOTH REVIEWER COMMENTS BELOW:

Maarten Van Daele comments to manuscript (bold), with author responses in italics. **AUTHOR-ADDRESSED REVISIONS IN THE VERSION SUBMITTED TO COPERNICUS ON APRIL 17, 2024 ARE IN RED.**

It is clear that a lot of work and methods have gone into this impressive dataset and it contains a promising record. However, I do have concerns with some of the interpretations in the discussion, which can be summarized as: the authors invoke many poorly constrained mechanisms (e.g., lake level lowering, fine particles leaking out of a delta) and events (e.g. additional earthquakes in the historical part of the earthquake) too explain observations that can be more easily explained by widely recognized processes such as a delta failure. Also, some 137Cs dates would be really helpful to pinpoint the 1963/64 depth. My main comments are below and small comments are added to an annotated pdf that is attached.

Thank you for your helpful comments. There are no major disagreements with respect to the minor comments in the annotated pdf. The authors of this paper will correct the word "historic" to "historical." Author responses to the major comments can be found below. ***DONE; Also changed is the name of the lake (an official name change).***

There are (too) many figures in the manuscript. Please consider to move some to supplement.

Agreed. Some figures will be moved to the supplement. ***Some figures have been removed, not moved to the supplement.***

Methods. Ideally you present the XRF data as centered-log-ratio (CLR) transformed to reduce the closed sum effect. See Weltje et al. (2015), or application in Schwestermann et al. (2020). This is nowadays routinely done for XRF scanning data.

The authors understand and provide a lengthy description as to why the data are presented in raw form and show a comparison to the log-ratio method. Although we agree that to accurately represent the true geochemical composition of the data one needs to present the data as suggested (log-normalized) to reduce the closed sum effect, the objective here is to show the observed patterns in the raw data, demonstrate their relationship to other core data (density and magnetic susceptibility), and suggest an explanation as to why these patterns exist. Most important uses of the raw data were to 1) identify exactly where the deposit began and ended in the core, and 2) infer that deposit grading is a function of both elemental composition and variables associated with sediment density based on a comparison of normalized xy plots as a result of a comparison between normalized (scaled by CT radiodensity) and raw data.

Using methods to transform the data (such as the centered log ratio method of Weltje et al. (2015)) introduces noise and obscures the observed patterns (which we are trying to explain). This is clearly described in the manuscript. Furthermore, smoothing the high frequency noise (as in Schwestermann et al., 2020) is an additional transformation that adds to the uncertainty, *obscuring the patterns. The goal is to be able to identify where the disturbance deposits begin and end, and how they evolve in elemental XRF space. Because the patterns in the raw XRF data correlate to other geophysical (e.g., density) and other compositional (e.g., % organic content as inferred by the CT radiodensity and measured Loss-on-ignition) data, it is considered a valid representation of changes in the sediment and a useful tool by which disturbance deposits can be differentiated from background sediment and mechanisms inferred in this study. That said, an approach for future research is to calibrate the XRF data using measured compositional data to get at the actual compositional changes through the disturbance deposits. This data could then be scaled by CT radiodensity data to more accurately reflect how the disturbance deposits evolve in composition as they settle.*

THE AUTHORS STAND BY THEIR DECISION TO USE THIS APPROACH TO EVALUATE THE DATA EVEN IF IT DOES NOT STRICTLY REPRESENT SEDIMENT GEOCHEMISTRY. THE REASONING IS DESCRIBED IN DETAIL THE TEXT; A REFERENCE HAS BEEN INCLUDED TO SUPPORT THAT THIS IS AN APPROPRIATE WAY TO REPRESENT THE DATA.

Discussion.

Deposit J. The tail related to deposit J was initially not included to the event deposit, event though from Fig. 14 it is pretty clear that there is a tail (Bouma Te division) that is indeed not included in the deposit. This tail should, however, be included already in the results, so that it can also be taken into account for the age model. Furthermore, I am far from convinced that the silt deposit below J is part of the same event. We know from comparison with well-described events (e.g., Van Daele et al., 2017; Wils et al., 2021) that a long muddy tail means a significant time lag of at least days to weeks.

Original author response:

Identifying the tail as the Bouma Te division implies that it is a turbidite, the result of a turbidity current. This is not the case as explained in the manuscript: the silt and the tail are part of the same event because the XRF data shows that sediment composition does not return to background until after the tail. Because the tail is part of the deposit that formed over a short time period (likely minutes to hours, depending on how quickly the flocs settle – which is less time compared to normal fine-grained sediment), they should not be included in the age model. The age model should only include normal background sedimentation, which is why it is so very important to know when a deposit starts and ends (see discussion about XRF data above).

Author corrected response:

The tail of the deposit was NOT included in the age-depth model, even though Maarten seems to have inferred otherwise. It would be good to know why he made this interpretation.

Other comments: Identifying the tail as the Bouma Te division implies that it is a turbidite, the result of a turbidity current. This is not the case as explained in the manuscript: the silt and the *tail are part of the same event because the XRF data shows that sediment composition does not return to background until after the tail. Because the tail is part of the deposit that formed over a short time period (likely minutes to hours, depending on how quickly the flocs settle – which is less time compared to normal fine-grained sediment), they should not be included in the age model. The age model should only include normal background sedimentation, which is why it is so very important to know when a deposit starts and ends (see discussion about XRF data above).*

SINCE THE TAIL WAS NOT INCLUDED IN THE AGE-DEPTH MODEL, THERE ARE NO CHANGES THAT NEED TO BE MADE. Every turbidite identified as a possible Cascadia earthquake deposit in the downcore companion paper has the same thin silt layer just before the primary deposit, which supports the interpretation that it is part of the same deposit. It is now considered that the deposit J IS the Te or Te,Td portions of a turbidite, with some other process operating (for example an Internal wave) because of the complex grading through the deposit.

What about the deposit around 40 cm in SQB5 (Fig. 14), this also seems like an event deposit with a tail reaching until the base of event I.

This was not described because it was not originally visually identified in the record (deposits A-J) during the initial core description. This should be included, however, in the discussion as suggested. This deposit is a very thin silt deposit with a long tail that has some of the characteristics of a subduction earthquake deposit and therefore warrants a substantial discussion. THIS MAY BE A CASCADIA EARTHQUAKE DEPOSIT IN PART BECAUSE OF THE LONG TAIL AND IN PART BECAUSE OF THE XRF DATA.

THIS WAS INVESTIGATED AND DETERMINED NOT TO BE, USING THE XRF METHOD OF DEPOSIT EVOLUTION, A CASCADIA EARTHQUAKE, BUT RATHER THE RESULT OF A STORM, PROBABLY THE ATMOSPHERIC RIVER EVENT OF 1861/62 CE. The "tail" is not a tail at all, but reflects the land-use changes that began at around 1850 CE. This is described in a separate section near the end.

Deposits G, H, I. I have sincere problems with the proposed interpretations. A lot of new mechanisms are invented (e.g. line 550-555 and section 4.3.3), while there are plenty known mechanisms (rock avalanche, delta failure...) that can much easier explain the observed deposits.

The authors will revise lines 550-555 and section 4.3.3 appropriately to avoid "inventing mechanisms."

THIS HAS BEEN CORRECTED.

- I: do the authors interpret this as sourced from terrestrial or subaquatic slopes? If subaquatic, why did the 1700 CE earthquake not trigger any (!!!) failures on these slopes? Hence, I suggest the authors clarify in the text that this must be the terrestrial slopes.

We will clarify the interpretation of deposit I as sourced from *<u>subaquatic</u>* *slopes; we are not sure from the comment above why Dr. Van Daele believes that deposit I must be sourced from the terrestrial slopes.*

Regarding deposit J (inferred to represent the 1700 CE Cascadia earthquake): Recent evaluation of deposit J has slightly modified the interpretation to include a small slope failure deposit preceding the base of the silt unit and this will be included in the revised manuscript. This evidence includes a few grains of mica at the contact between the sediment below deposit J and the basal silt of deposit J. The authors interpret this to reflect a bypass flow from the shallow water (where the mica is virtually absent) to the deep water (where this mica silt unit is more obvious as a thin turbidite). The platy mica has a large surface area and would settle less quickly, staying in suspension and resulting in the water/mica mixture to be denser than water that becomes a gravity flow.

Note that there is also evidence not previously reported to support the interpretation that the silt units from deposits J and H are sourced from the delta and will be included in the revision. Watershed-sourced silt that is exposed to oxygenated water would have an orange color (likely iron oxide – rust). The silt that is watershed-sourced in these deposits is not orange in color and therefore has not recently been exposed to oxygenated water. This is evidence that the silt is sourced from within the delta where any minerals coating the grains would have removed by the delta's groundwater.

IT HAS BEEN CLARIFIED THAT DEPOSIT I IS A SUBAERIAL DEPOSIT AND THAT DEPOSIT J IS THE RESULT OF A SUBAQUEOUS DELTA SLOPE FAILURE.

The summary further includes a dam collapse and lake lowering for which no further evidence is provided. I have the feeling that a lot of additional mechanisms are invoked for which there is no evidence. In my opinion the authors make the story more complicated then it needs to be.

"Dam collapse and lake lowering": this will be reduced in importance in the discussion given there is no evidence. It will be presented as a potential mechanism that could explain the deposit, but the simplest explanation is the more likely (slope failure) for deposit I.

THIS HAS BEEN CORRECTED; the reference to a dam collapse has been removed.

- H: in my opinion the authors give too much credit to core SBQ9. This is the only core where multiple pulses are observed. Why is the option that it is in fact an amalgamated turbidite considered unlikely? This core is in the depocenter (this is indeed the location where this could be expected), and apart form this core, only in SQB13 and SQB10 there is perhaps some evidence of such amalgamation, which is indeed also in the depocenter and away from the main (deltaic) sources, where also flow partitioning could get more influence. Furthermore, also event deposit J seems to have 2 pulses in exactly these cores, (and event G!) indicating

that the presence of multiple "pulses" seems to be related to these locations, rather then to the specific event(s). Also, how do the authors explain these additional earthquakes, while they have not been historically reported?

The authors agree that an amalgamated turbidite is likely the simplest explanation, rather than a stacked turbidite given the historical reports of shaking do not support the hypothesis of multiple earthquakes. The presence of multiple pulses does indeed likely reflect the location of the cores. This will be fixed in the text.

A REVISED ARGUMENT HAS BEEN MADE THAT EXPLAINS DEPOSIT H AS THE RESULT OF AN AMALGAMATED TURBIDITE. PLEASE SEE HIGHLIGHTED SECTION IN THE TEXT.

- G: as reverse grading is observed, could this be a catchment response ("flood") related to events H and I? The authors link it to a documented dam failure, in that case the deposit should be coarser and thicker towards the dam (e.g. 1929 dam collapse in Eklutna Lake; Boes et al., 2018), is this the case?

REVISED RESPONSE: There is not the distribution of cores that would allow for the evaluation of particle size with distance from the dam (the deep-shallow signal dominates over the distance from the dam). Note that there is a time-gap (sediment accumulation) between H and G, which would not be expected if it were a post-earthquake flood removal of watershed sediment. Post-earthquake watershed removal of sediment would more likely be an immediate response post-earthquake (unless there was a period of less precipitation). It is possible that this is the result of a flood that removes post-seismic sediment. ***YES, Catchment response to earthquake – postseismic removal of sediment. DONE.***

An alternative interpretation of this sequence would be something similar to what's discussed in Van Daele et al. (2019) (this is anyway a pretty important reference in this paper, as it also deals with the sedimentary imprint of megathrust and intraslab earthquakes and how to distinguish them). As the 1873 earthquake was an intraslab earthquake, the high-frequency content of the shaking could have cause onshore landslides (in contrast to 1700, which would've caused more voluminous deltaic failures due to the longer duration of low frequency shaking). Hence, initially onshore landslides in the schist along the lake could have traveled directly into the lake (event deposit I).

Onshore landslides are considered unlikely because deposit I does not look the same brown color as the undercut flood deposits A and B (which are brown because they contain detrital organics at the base). Earthquake-triggered landslides are likely localized and not lake-wide failures, therefore it is considered that deposit I is a subaqueous lake-wide failure (because deposit I is found in all the cores).

SUBSEQUENT EVALUATION HAS SUGGESTED THAT DEPOSIT I IS LIKELY A SUBAERIAL SLOPE DEPOSIT AS STATED BY M. VAN DAELE. DONE. VAN DAELE et al. (2019) is included now.

The shaking would've also cause delta failures (albeit small ones), which arrive slightly later to the core locations (event deposit H).

Both deposits J and H are single (not amalgamated) deposits in all the cores, even in the depocenter, suggesting they are not flow deposits but rather settled directly out of the water column.

WE AGREE WITH THE REVIEWER. OUR PREVIOUS COMMENT IS INACCURATE AND HAS BEEN SCRAPPED.

Finally, onshore landslide in the catchment would've been transported to the lake in the years following the earthquake (event deposit G). UNLESS there is actually background sediment between event deposits I and H...?

Event deposit G does not have a watershed-sourced composition and therefore is not considered to be a flood deposit, which is why it was interpreted to be the result of the dam failure.

Our interpretation has changed. Event deposit G is now considered to be a flood deposit. Flood deposits are sourced from both watersheds and would have a mixed composition. We agree that onshore landslide material would have been transported to the lake (event deposit G).

There are leaves between deposits I and H, but no intervent sedimentation. Still unsure as to one or two earthquakes, but now suspect two. If two events, then the inference is that the 1873 CE Earthquake is the result of an intraslab earthquake followed immediately by a Cascadia earthquake (deposit H). This is supported by the presence of a small tsunami in coastal southern Oregon (Crescent City Courier, 29 November 1873).

DEPOSITS H AND I ARE NOW INTERPRETED TO BE THE RESULT OF THE SAME EARTHQUAKE. THE LEAVES BETWEEN THE TWO DEPOSITS DOES NOT NECESSARILY REFLECT THE PASSAGE OF TIME. This set of deposits has been completely re-evaluated as described in the text.

Events C-A: Some 137Cs dates seem to be indispensable to locate the 1963/64 atomic bomb peak and thus confidently attribute the corect deposit to the 1964 floods, and probably also to the 1955 floods.

Future work will look for the position of the atomic bomb peak using radiocarbon (and accurately date sediment deposited since ~1955) but this will not be done for this study (given the stage of this manuscript).

WE AGREE, BUT ARE UNABLE TO ACQUIRE 137Cs or bomb carbon DATES FOR THIS MANUSCRIPT.

Fig. 23: the ratio is probably organic/inorganic, unlike what is mentioned in the caption. This data should be plotted in the same style as all other figures, and both with the same software.

The ratio IS organic/inorganic and is labeled as such (caption is in error and will be fixed). Figure 23 could be included in the supplement.

THIS FIGURE HAS BEEN REMOVED FROM THE MANUSCRIPT.

References:

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Van Daele, M., Meyer, I., Moernaut, J., De Decker, S., Verschuren, D. and De Batist, M. (2017). "A revised classification and terminology for stacked and amalgamated turbidites in environments dominated by (hemi) pelagic sedimentation." Sedimentary Geology 357: 72-82.

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Weltje, G.J., Bloemsma, M.R., Tjallingii, R., Heslop, D., Röhl, U., Croudace, Ian W., 2015. Prediction of Geochemical Composition from XRF Core Scanner Data: A New Multivariate Approach Including Automatic Selection of Calibration Samples and Quantification of Uncertainties, in: Croudace, I.W., Rothwell, R.G. (Eds.), Micro-XRF Studies of Sediment Cores. Springer Dordrecht, pp. 507–534. https://doi.org/10.1007/978-94-017-9849-5_21

Wils, K., Deprez, M., Kissel, C., Vervoort, M., Van Daele, M., Daryono, M. R., Cnudde, V., Natawidjaja, D. H. and De Batist, M. (2021). "Earthquake doublet revealed by multiple pulses in lacustrine seismo-turbidites." Geology 49(11): 1301-1306.

ALMOST ALL MINOR HAND-WRITTEN COMMENTS ON THE DRAFT WERE ADDRESSED.

Shmuel Marco comments to manuscript (bold), with author responses in italics.
AUTHOR-ADDRESSED REVISIONS IN THE VERSION SUBMITTED TO COPERNICUS ON APRIL 17, 2024 ARE IN RED.

Review by Shmuel Marco, 27 Sep 2023

Sorting out the various causes for changes in the sediment character is challenging in light of the multiple possible triggering processes, both natural and man-made.

The authors use numerous analytical methods for characterizing the layers suspected as seismites generated by a particular earthquake. The discussion on the depositional processes and how they were resolved is somewhat tedious and hard to follow, but I cannot suggest an improvement. The information is indeed relevant to the conclusions, so I suppose it better remain as is. The elaborate discussion is crucial because the radiocarbon dates cannot bracket the events tightly enough. Therefore, I value the submission as an example for applying multiple considerations in order to reach the best fit solution to a complex geological situation.

My main criticism is that the authors consider only mass transport deposits and do not ignore the option of sediment-water interaction during seismic shaking. This was shown to be significant in many previous works, in particular the ones related to the paleo Dead Sea seismites (e.g., Wetzler et al., 2010). Previous research also addressed the difference between earthquake-triggered in-situ deformation of lake bottom sediments and slope-originated mass transport deposits. However, this extensive body of works (e.g., Lu et al., 2017, 2020) is unfortunately ignored here.

RESPONSE: Thank you for bringing to our attention the articles on Dead Sea seismites. Although there are seismites present in the downcore record, the upper portion of the record does not have any obvious seismites. There are load structures that are the result of rapid settling of silt through the water column, but these are likely not the result of in-situ deformation of lake bottom sediments. We should make our reasoning clear in the manuscript. Also, the deposits that produce the load structures are interpreted to be the result of injection, then settling, of silt into the water column . . . and therefore are not mass transport deposits.

THE AUTHORS HAVE NOT OBSERVED ANY EVIDENCE OF SEDIMENT-WATER INTERACTIONS (SUCH AS IN WETZLER ET AL., 2010), OR IN-SITU DEFORMATION OF LAKE BOTTOM SEDIMENTS (LU ET AL. PAPERS) IN THE HISTORICAL PORTIONS OF THE CORES. It is difficult to reference these papers for this manuscript, but very appropriate to include in the companion manuscript #1638.

Minor comments:

The authors conclude that their results "suggest that inland lakes can be sensitive recorders of earthquakes". This is old news since evidence that inland lakes can be sensitive recorders of earthquakes has been around for over three decades. It is not a new revelation of this research.

<u>RESPONSE</u>: This comment refers to inland lakes in Cascadia. This will be rephrased. ***FIXED***

Line 27: Earlier, much longer earthquake records (220 ka) have been reported from the Dead Sea Basin, where seismites are directly linked to synsedimentary faults and historical accounts of earthquakes (Lu et al., 2020 and references therein).

ADDED

L. 406: Flood deposits usually sink to the bottom within hours, even in saline lakes, where the debris/brine density contrast is smaller than in fresh water.

THANK YOU

L 585: "steep" is too vague, please provide a measure of the slope. Slope failures can occur on less the 1°.

DONE

<u>RESPONSE</u>: Thank you for these comments!

The bottom line: Accept with minor revision.

References

Lu, Y., Moernaut, J., Bookman, R., Waldmann, N., Wetzler, N., Agnon, A., Marco, S., Alsop, G.I., Strasser, M., and Hubert-Ferrari, A., 2021, A New Approach to Constrain the Seismic Origin for Prehistoric Turbidites as Applied to the Dead Sea Basin: Geophysical Research Letters, v. 48, doi:10.1029/2020GL090947.

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Wetzler, N., Marco, S., and Heifetz, E., 2010, Quantitative analysis of seismogenic shear-induced turbulence in lake sediments: Geology, v. 38, p. 303–306, doi:10.1130/G30685.1.