Maarten Van Daele comments to manuscript (bold), with author responses in italics

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.

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

Agreed. Some figures will be 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.*

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.

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).

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.*

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."

- 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 subaquatic 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.

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.

- 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.

- 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?

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. Also, watershed-sourced post-earthquake removal of sediment would begin thicker, then become thinner units (representing a higher, then lower, amount of sediment being transported from the watershed) post-

earthquake as supply is being depleted. This supports the idea that deposit G is either the response to the dam failure or a flood.

NOTE: A comparison between the upper and lower lake records demonstrates the above (thicker to thinner post-earthquake watershed removal of sediment, and that these units are different from deposit G) and this information will be included in the supplement.

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).

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.

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.

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).*

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). 137Cs dates could also be helpful, but not as useful as radiocarbon.

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.

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Maarten Van Daele