Dear Reviewer,

We thank you for your additional feedback and constructive comments on our study.
Below, you will find our responses to each of your questions and remarks.
Thank you again for your time and consideration.
Best regards,

Mathilde Banjan and co-authors

I would like to thank Mathilde Banjan and co-authors for their detailed replies to my earlier comments on their manuscript. I went through them, and in most cases agree that simply some further clarification in the main text of the paper would suffice to address my concerns. However, there are a few points where I would like to come back to which I feel haven't been fully addressed, as indicated below. I've used the same numbering as earlier on, so that the discussion can be easily followed.

1. I agree that log-ratios are indeed sufficient to simply check for downcore variability, but as currently presented in the manuscript (L251), you mention 'single XRF element (Ca and K) variations are used to confirm first correlations'. As counts are always relative to a unit sum, the increase of one element will result in the decrease of another element, and therefore, interpretations between cores cannot be confidently made. So either you will indeed have to use ratios here (and then also show them in the manuscript, as you did for ln(Ca/Ti) in figure 4), or alternatively, a clr transformation is required.

We agree that XRF data are relative and should be normalized to avoid misinterpretation. While the statement in the manuscript (L251) might imply that raw single-element counts were used for correlation, our actual methodology involved normalized log-ratios, such as ln(Ca/Ti), which are appropriate for assessing downcore variability and inter-core comparisons. To address this, we will:

- Revise the text in Section 3.3 to explicitly state that log-ratios were employed for core-to-core comparisons.
- Add a supplementary figure illustrating the log-ratio variations (e.g., ln(Ca/Ti), ln(K/Al)) for the relevant cores.

2. Indeed, various confidence intervals for earthquake triggering have been considered in the study using multiple proxies. However, as the authors mention, a summary is given in figure 9 and this shows that the thickness seems to be the main factor to add high confidence (as is also mentioned in its caption). If this is not the case, the figure is probably misleading considering that indeed, not all >2 cm deposits result from earthquakes as shown by the historical record. This should be further clarified in the text.

While deposit thickness provides a practical threshold for higher-confidence interpretations, it is only one component of our multi-proxy approach. Figure 9 summarizes confidence levels derived from multiple criteria, including AMS foliation, grain-size patterns, geochemical ratios, and IRM amplitudes. We acknowledge that the figure caption and text might emphasize thickness as a contributing factor.

To address this, we will:

- Revise the caption of Figure 9 to highlight that thickness is one of several factors contributing to confidence levels.
- Update Section 5.1 to explicitly state that confidence levels are determined through a combination of proxies, with thickness serving as a practical threshold for reliable AMS measurements and other analyses.

3. I fully agree that shaking is dependent on frequency, as already hypothesized by many other studies as the authors are also aware of. But, considering this dependency is irrelevant when not considering the proper site effects, which includes vs30 values. I understand that a full site effect study of the lake is beyond the scope of this paper and probably only possible in the future, the authors seem to have tested several possibilities, and this should be mentioned in the manuscript (at least the vs30 values that were used for the plots).

We agree with the reviewer that the shaking frequency is influenced by site effects, and that Vs30 is an important parameter. While a detailed site effect study is beyond the scope of this paper, it is part of planned future work, as noted in lines 502–505 of the manuscript: “However, it should be noted that the approach developed in this paper is relatively preliminary as it does not, for example, consider local conditions that could lead to changes in seismic motion for certain frequency ranges (Ergin et al., 2004; Maufroy et al., 2015, Courboulex et al., 2020) and thus could alter the proposed interpretations.”
For the GMPE-based PSA analysis presented in this study, we used a Vs30 value of 800 m/s, which corresponds to standard reference rock conditions and is consistent with expected velocities for molasse formations in the region. In the case of Lake Aiguebelette, the Quaternary cover on the slopes consists only of till and is limited to 0-5 m in thickness (according to field observation and geotechnical investigations), which supports the use of this higher Vs30 value (personal communication with Aurore Laurendeau and Ludmila Provost). We also conducted tests with lower Vs30 values (400 m/s) to assess sensitivity, and the results did not show substantial differences in the PSA range relevant to this study. Since the sensitivity to Vs30 within this range (400–800 m/s) remains limited for our conclusions, we chose to keep the representation simple. However, we will add a sentence in the manuscript to specify the Vs30 value used and clearly acknowledge this assumption.

To get reasonable outcomes, I would expect values similar to those measured by Shynkarenko et al. to be considered, but at this point, this cannot be evaluated. However, the outcomes here are rather surprising: “high-frequency shaking, particularly at 5 Hz is more relevant for triggering event deposits in Lake Aiguebelette”, and have currently not been addressed by the authors. Soft sediments are known to amplify low-frequency accelerations (see e.g. in Van Daele et al.

2019, or also the Shynkarenko paper the authors already referred to), as their spectral peak is typically located below 5 Hz. So this result definitely requires further discussion and/or revision. This discussion should also be considered for points 5 and 6 in my earlier review.

This result arises from an empirical approach. At this stage, we do not attempt to quantitatively model site effects or ground motion amplification in the basin, as the necessary site-specific parameters are currently not available.

The aim was to test in a relative way, whether the recent event record appears more consistent with seismic signals rich in high-frequency content (>2–3 Hz) or with lower-frequency energy (<1–2 Hz), which typically characterizes large distant earthquakes. The good match between observed event deposits and ground motion proxies in the 5 Hz range suggests that the lake's sedimentary slopes are more responsive to higher-frequency shaking.

This may imply that the lacustrine environment of Lake Aiguebelette is not particularly susceptible to low-frequencies, which is consistent with the absence of distal, high-magnitude earthquakes in the record. Had there been strong amplification at lower frequencies due to site effects, we would expect more frequent recording of large distal events. The fact that this is not the case suggests that either site amplification in the low-frequency range is limited, or that slope instability thresholds are reached under high-frequency loading.

We agree that a more detailed site response analysis would be a valuable next step, and we now explicitly state in the revised manuscript that this question remains open. However, as a first-order interpretation, our empirical observation supports the idea that the lake's subaqueous slopes are more sensitive to near-field events with richer high-frequency content.

Regarding the unexpected result of 5 Hz relevance, with more detail:

1. Sediment Response: Lake basin morphology and sediment characteristics may amplify higher-frequency shaking in certain conditions. For instance, thin homogenites may form preferentially under high-frequency shaking that remobilizes finer-grained surficial slope sediments (Sabatier et al, 2022).
2. Comparative Data: Previous studies (Kremer et al., 2017) present variability in frequency sensitivity across alpine lake settings and display frequency-selective sensitivities to seismically-induced event layers. We suggest that Lake Aiguebelette is favoring the record of close and moderate earthquakes rather than distant and strong seismic events. We acknowledge that this frequency bias could be further investigated through a quantitative analysis of slope susceptibility to seismic motion, this point will be clearly stated in the revised manuscript.

We will expand Section 5.2 to:

- Include a discussion of frequency-specific sediment responses, referencing relevant literature (e.g., Kremer et al., 2017; Shynkarenko et al., 2023).
- Acknowledge the need for future site-specific studies to refine these interpretations.

4. I agree that adding surficial remobilization to the discussion will increase the value of the paper and its outcomes. I do want to emphasize that whether or not a turbidite results from surficial remobilization or a slope failure, the depositional process is still the same (i.e. from a turbidity current), so I would not expect this to affect the Passega diagram.

We agree that surficial remobilization is a key process, particularly during seismically quiet periods. While the depositional mechanism (turbidity currents) remains consistent, triggers may vary, including surficial sediment reworking. In the Passega diagram, the alignment of data for deposits >0.5 cm supports a shared depositional process.

We will:

- Expand the discussion in Section 5.3 to include the role of surficial remobilization in forming thinner turbidites during quiescent periods.
- Make it clear that while the depositional processes are similar, the interpretation of the triggering origin (seismic or not) relies on a combination of multiple proxies.

5. I fully agree that it is important to consider multiple cores for meaningful recurrence statistics. However, it is important that the recording threshold stays the same. For example, in Moernaut et al. (2014) as mentioned by the authors, it is clear that not every site has the same shaking sensitivity. This complicates interpretations when combining them, potentially even making them invalid. As some of the events are not recorded in your long core but are in the pilot core, I suspect a similar thing happening here – even though sedimentation rate seems to have a stronger impact on earthquake sensitivity here (which should not be the case when surficial remobilization is considered, see point 8). I strongly recommend to make separate recurrence analyses for different timeframes, mentioning the recording thresholds of each to allow the readers to evaluate record sensitivity, or use a (thickness?) threshold before applying statistical analysis on the correlated long and pilot cores.

We acknowledge the concern regarding potential differences in recording sensitivity between the long core (AIG17III) and the pilot core (AIG16-05). In this study, these two cores were selected for their complementary characteristics: AIG17III covers the full Holocene sedimentation, while AIG16-05 provides coverage of the most recent ~1.2 meters, including the historical period.

Due to coring limitations and sediment disturbance in the upper part of AIG17III, several historical event layers are only recorded in AIG16-05. Importantly, below 1.2 meters all event layers identified in AIG16-05 are also present in AIG17III. This confirms a consistent recording threshold between both cores for the mid-to-late Holocene section.

To ensure consistency in our analysis:

- We apply a threshold of ≥0.5 cm deposit thickness across all cores, which serves as a proxy for minimum sensitivity.
- We will acknowledge in the revised Section 5.3 that this threshold may lead to slightly lower detection sensitivity in the older parts of the record, and potentially higher

sensitivity in the last few centuries due to improved preservation and higher sedimentation rates in the upper 1.2 m of AIG16-05.

Combining the two records with the same threshold (≥0.5 cm) allows us to maximize temporal coverage and maintain coherence in the interpretations of the results. Separate analyses would reduce statistical significance, particularly for recent centuries, given the overlap and continuity observed between the two cores. Because of the region's moderate seismicity and the limited number of events in the recent centuries, performing a statistical analysis separately on the upper 1.2 m of AIG16-05 would not provide sufficient resolution for a representative recurrence assessment. The combination of both cores increases coverage of the event layers, while being cautious in the interpretation (revised section 5.3., in the new version of the manuscript). The integration of these cores will be discussed in the revised manuscript as well as the implication of sensitivity variation on the most recent part of the record.

Thank you again for providing me with the opportunity to review this manuscript.

Reference:

Distinguishing intraplate from megathrust earthquakes using lacustrine turbidites

Van Daele, C. Araya-Cornejo, T. Pille, K. Vanneste, J. Moernaut, S. Schmidt, et al.

Geology 2019 Vol. 47 Issue 2 Pages 127-130

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Thank you again for this constructive review. Please find below the references cited in our responses.

<u>References</u>:

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