Reviewer # 2

The manuscript entitled: "Seasonal variations in flooding inferred from lake sediments in Western Norway", submitted by Nielsen et al. provides a detailed and interesting paleoflood reconstruction from lake sediments in Western Norway. Overall, the manuscript is very well written in a simple, fluent and clear way that makes it easy to follow the authors' logic, and to clearly distinguish measurements from interpretation, as would be expected from papers submitted to CP. Nevertheless, although I think the presented work is novel, interesting and solid enough for publication in CP, the manuscript could benefit from some small improvements that would increase its impact. I would recommend that the authors address the comments below prior to accepting the manuscript for publication.

Thank you for your thorough and constructive evaluations of our manuscript. We greatly appreciate the positive feedback and the helpful suggestions that will improve the clarity and increase the impact of the paper.

Comments:

1. Your introduction feels somewhat too local. I think you should provide a couple of sentences that also highlight the role of paleoflood interpretation from a global perspective. Plenty of works have been published on that topic worldwide, so you can give the readers some broader, more global context (see works by V. Baker, for example).

We agree that the article has a local focus and is related to catchment specific processes along the western coast of Scandinavia, but we cite European studies in the introduction to broaden the topic. However, we will add “Globally, palaeoflood investigations have played a central role in extending the flood record far beyond the instrumental period, offering critical insights into the magnitude, frequency, and climatic sensitivity of extreme hydrological events (Baker, 2006).” In the introduction.

2. A crucial aspect to clarify is the possible role of lake level in sedimentation patterns at your coring site. Some of the observations you use as cardinal aspect of your discussion and interpretation rely, to a large extent, on lake level that would shift the coastline and alter sedimentation patterns. Thus, you need to provide some statement based on some previous studies in order to clarify to which extent would changing lake levels over the Holocene changed, and if so, to which extent would that affect sediment sorting and deposition at the coring site. Even if you consider this effect to be of negligible impact, you should state that as a premise of the presented work.

We thank the reviewer for raising this point. In western Norway, lake-level evolution since deglaciation is fundamentally controlled by rapid early-Holocene isostatic rebound followed by stabilization once the marine limit was passed. After the local lake basins became isolated from the sea (ca. 11000–9000 cal yr BP), lake levels in inland, non-regulated catchments such as Vangsvatnet have remained effectively stable, with only decimetre-scale natural fluctuations around a fixed outlet threshold. Previous geomorphological work in the region consistently shows that Holocene lake-level change is negligible in such systems because outlet thresholds are bedrock-controlled and hydrologically constrained. Consequently, long-term shoreline migration capable of influencing sediment sorting or the sedimentary environment at our deep-basin coring site is not expected.

3. Another crucial aspect of your claims relies on the interpretation of the four sediment types you mention in the text (B, O, OM, and M in L402-408) - could you improve your statements about their interpretation? Since no thinsections were done, it is hard to judge how robust the claims made about their mode of formation are. Is it in agreement with previous studies that you can cite?

We appreciate the reviewer's attention to the sedimentological interpretations. Although thin sections were not produced for this study, the classification and interpretation of the four sediment types (B, M, O, MO) are supported by a multi-proxy dataset (CT density, XRF, MS, grain-size distributions, LOI), all of which show internally consistent and diagnostic signatures. This integrated approach has been successfully applied in numerous palaeoflood studies, and our interpretations follow the same methodological framework

4. You mention using Munsell color, but no data is presented - is it worth correlating this with your RGB values? chemical composition? for the various types of layers?

The reference to Munsell color observations was originally included as part of the standard visual core logging procedure, but these qualitative color descriptions were not used in any analytical step nor in the interpretation of sedimentary processes. Because the Munsell observations do not contribute to the interpretation and are not required for reproducibility, we have removed this statement from the Methods to avoid confusion.

5. L 184: You mention switching the technical parameters of the XRF - would this affect your measurement in terms of saturation or limit of detection? I noticed you only present key elements such as Fe, Ti and Ca, so maybe this is not an issue. Either way you should clarify if such a change would alter the measurement and how you address that if so. Please also state the list of elements you measured.

We appreciate this comment and agree that a clarification is in place. The two core sections (VV-17 and VATG-316) were scanned at slightly different XRF settings (29 kV / 28 mA vs. 33 kV / 50 mA). However, the elements used in our interpretation (Ca, Ti, Fe, and Fe/Ti) are all high-energy, high-abundance elements that are well above the detection limit under both settings and are not prone to saturation at these voltage/current combinations.
Elements measured by the ITRAX scanner include Si, K, Ca, Ti, Mn, Fe, Rb, Sr, and Zr, though only the most robust and commonly used flood proxies (Ca, Ti, Fe, Fe/Ti) were retained for interpretation.

6. Table 2 - what is "sorting"? how was this calculated? presenting the mean of grain size is often misleading, and presenting the peak of the distribution would make more sense. If the distribution is bimodal please present both peaks in the table.

We agree that grain-size statistics must be clearly defined, and that mean grain size can be misleading for skewed or bimodal distributions. In this study, grain-size parameters were calculated using the GRADISTAT software (Blott & Pye, 2001), applying the Method of Moments in geometric (φ/µm) space. The "sorting" values reported in Table 2 refer to moment-based sorting (standard deviation), which is the conventional output of GRADISTAT and is widely used in sedimentological studies. Only a small number of samples showed bimodality (3 M-layers and 1 MO-layer), as stated in Figure 6. Because bimodality is rare and not central to the interpretation, we chose to retain the summary statistics in Table 2 for simplicity.

7. You present only Fe, Ti, and Ca - is that all you could measure? commonly Al and K are discussed as well. What about Si?
Yes, that is true. The ITRAX XRF scanner measures a broader suite of elements than those presented in the text. We focus on Ca, Ti, Fe, and the Fe/Ti ratio, as these high-energy peaks are stable across both scanning parameter sets and have been demonstrated to be the most reliable flood proxies in similar sediment records. Because our sediment-type classification is strongly supported by CT density, grain size, MS, LOI, and the high-signal XRF elements, we restricted the presented XRF data to those elements that most consistently tracked minerogenic influx and event-layer formation. See also comment in question 5. We will state this explicitly in the Method and Result section.

8. Fig. 5 presents some interesting potential. I would recommend to plot some correlation plot between few selected elements, e.g., Ca. vs. Fe/Ti (Al, Si, K?) and color\shape the layers according to their types in order to highlight the major differences between the identified layers and the background (B, O, M, etc.)
Producing element–element correlation plots and coloring/shape the event layers (230 in total) is indeed a useful way to visualise differences. However, since the figure is already packed with data, we chose not to add additional plots/coloring. The high variability of the XRF-data throughout the record – due to high minerogenic content in the 'background sediments' – will result in low correlation between elements.

9. Fig. 5 - the image of the core is really not visible. You can create another figure to highlight the key types of layers you identify. Ideally this would include some high resolution imaging as well, such as thinsections.
We will add an RGB image to figure 7 to highlight the key types of layers presented there.

10. Fig. 6 presents some bimodal GS distributions - I think you should address that. Is this expected? what do you think this means? Is it correlated with a specific period or type of events? Please mention and address that in the text.
We agree and will do so. The bimodality in mixed layers seems to overlap with that of the organic layers, which may suggest a source. More on this in the revised version. Please also see comment to question 6.

11. Fig. 7 - I recommend adding the RGB image aligned with the presented CT
Yes, will do in the revised manuscript

12. Fig. 8 - how does this interpretation fit against your LOI values? can you correlate the CT of the event layers with the LOI?
The CT-derived identification of minerogenic versus organic components is indeed expected to correlate with LOI values, a relationship reflected in our dataset. LOI values are systematically higher in O-layers and vice versa in M- and MO-layers. This is consistent with the low-density (organic) versus high-density (minerogenic) patterns captured in CT data. We do not have a continuous, high-resolution LOI dataset from the core, only single measurements from different depth intervals (see fig 5). Hence, we cannot do a proper correlation between single LOI-values with the high-resolution CT-data.

13. Discussion - I think the the concept of nonstationrity is very important, and serves a critical insight from the presented study. I therefore think you could improve the impact of the discussion by mentioning additional examples from places where it was described worldwide (even if not coeval), rather than limiting your discussion to Sweden. I wonder if you should

clearly state that in your title, something like: Lake sediment record reveals nonstationary flood occurrence over the last 8 ka in Western Norway
We agree that non-stationarity is of central importance and is in fact global. We will consider how we best can highlight this in the revised ms.

Minor comments:

a. Fig. 1 has no legend for altitude. depicting landcover somehow could also be useful for the reader (maybe in another panel)
We will add a legend for altitude. We're not entirely sure what is meant by "depicting landcover"? Could it be different sorts of vegetation, or also human versus natural surfaces? We will evaluate if it is possible to include more information in the current map. Figure 2 already show more details around landcover.

b. L115: This is a strange sentence: "The annual precipitation at Voss is currently 1369 mm" - are you referring to mean annual precipitation? a specific year?
Yes, the sentence will be rephrased to: "The annual precipitation at Voss for the 1991-2020-normal is 1370 mm."

c. L117 - is the station code really 62.5?
Yes, the station code is 62.5.0 (https://sildre.nve.no/station/62.5.0?62.5.0_tab=2)

d. L194 - you are citing a paper in review. I am not sure what the journal policy is. perhaps better refer to a preprint.
Yes, we will follow that suggestion.

e. L243-244 "The glacier component..." - is this your result? or this some interpretation?
We appreciate the reviewer's comment and can clarify. This is contextual information on regional glacier extent rather than an inferred signal from our data. The purpose is to provide context for potential glacial-flour supply to the catchment.

"Small cirque glaciers (<0.25 km^2) along the highest peaks in Raundalen produce glacial flour that can be transported downstream. Regional glacier cover was more extensive during the Little Ice Age (Vasskog et al., 2012), which likely increased the supply of glacial flour to the catchment".

f. L580 - the mean of what? please explain what this number is referring to.
It's unclear to us which "mean" this refers to as, L580 is in the reference list. If it is L280 you refer to, it is a mean sorting of 2.59.

g. L307 - you mention OM layer instead of MO
This have been changed accordingly