Author's response to EGUSPHERE-2025-6279

We thank the editor for evaluating our responses to the reviewer comments and for granting us the opportunity to resubmit the manuscript following minor revisions.

Below, we list the specific comments raised by the editor after review. Our responses are provided in italics. For the full scientific rationale and a more extensive discussion, we refer to our initial response to the reviewers, which is appended at the end of this document.

In the section "All revisions in order of appearance", we specify the exact modifications made to the manuscript (marked in red), with references to the corresponding line numbers in the revised, marked-up version. During the revision process, we identified and corrected a small number of additional minor errors. These minor corrections are not highlighted below but are highlighted with track changes in the attached PDF.

Editor comment to the authors' responses (first review):

Thank you for your detailed responses to the comments you received. I am confident that you will be able to address them with minor revisions to your manuscript. Regarding comment 3 from Reviewer 2, I would ask you to further explain why the different facies "show internally consistent and diagnostic signatures."

Regarding the response to comment 12 from reviewer #2, I am convinced that it is possible to calculate a correlation between the CT-scan data and the discrete LOI data.
Please submit a revised version of your manuscript, detailing the changes that have been made.

Specific comments from the reviewer raised by the Editor

RC2, comment 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 thank the editor and reviewer for highlighting the need to further clarify and contextualise the interpretation of the four sediment types (B, M, O and MO). We fully acknowledge that the absence of thin-section microscopy represents a limitation of this study. However, our facies classification does not rely on a single proxy or textural observation. It is based on the repeatable co-occurrence of independent physical, geochemical, and compositional signals that together form distinctive and internally coherent facies signatures throughout the entire composite core. We argue that the four sediment types show robust signatures when evaluated collectively using CT density, grain-size distributions, XRF geochemistry, magnetic susceptibility (MS), and LOI. Each facies are defined by a reproducible combination of physical and geochemical characteristics that recur consistently throughout the 11-m-long composite core and across many events (n = 230). We have added more information to Fig. 7 (an MO-layer, the MS-data and an RGB-image) to facilitate the interpretation of the layers/facies. We have also expanded the interpretation of the MO-events and included a reference to a site in the Swiss Alps.

RC2, comment 12: Fig. 8 - how does this interpretation fit against your LOI values? can you correlate the CT of the event layers with the LOI?

We appreciate the reviewer's comment and editor's remark and agree that, in principle, it is possible to calculate a correlation between the high-resolution CT-scan data and the discrete LOI measurements. The CT-derived greyscale values and corresponding threshold-based organic/minerogenic volume estimates are expected to covary inversely.

In the original manuscript, we refrained from presenting a formal statistical correlation primarily because the LOI dataset is sampled from 38 event layers with contiguous transitions with the background samples, it is sampled at different resolution (0.2 to 0.7 mm) and are sparsely distributed in depth. Whereas the CT data are continuous and measured at millimetre-scale resolution. As a result, any correlation necessarily depends on down-sampling or averaging the CT data over depth windows around each LOI sample, introducing a degree of methodological choice (e.g. window size, weighting) that may influence correlation strength.

Using all discrete samples (after removing missing values) gives these Pearson correlations:

LOI vs. organic CT threshold:

$r \approx +0.57$ (moderate to strong positive correlation)

LOI vs. minerogenic CT threshold:

$r \approx -0.01$ (no meaningful correlation)

Organic vs. minerogenic CT thresholds:

$r \approx -0.03$ (essentially independent)

These results show that LOI correlates positively with the CT-derived organic fraction, whereas no systematic relationship exists between LOI and the minerogenic CT signal when considering the full dataset. This is expected given that LOI quantifies burnable organic matter, while minerogenic density contrasts primarily reflect inorganic sediment mass rather *than organic dilution. The LOI data could not be used for density measurements due to the irregular volume of the picked samples.*

We have added this sentence "Averaging CT-derived values over the depth intervals of discrete LOI samples reveals a moderate positive correlation between LOI and CT-derived organic content (r=0.57), whereas no correlation exists between LOI and the minerogenic CT content." to the result section.

Remarks from the preceding review file validation

1. The authors must be listed with "First name" followed by "Last name/family name". At least one first name must be written out (no initials), i.e. "Pål Ringkjøb Nielsen, Jostein Bakke, Øyvind Paasche, Jan Magne Cederstrøm, Johannes Hardeng, and George Edward Young"
   The author list has been corrected in the revised manuscript.
2. Your reference list includes works "in review". Such works can be cited upon submission if being available to the reviewers. They should not be cited in the final, accepted manuscript, unless published, accepted for publication, or available as preprint with a DOI.
   This paper was resubmitted after minor revisions 13. April to Frontiers. We wish to cite the paper, since the methodology is important for this study (and is based on part of the core VV-17). We are awaiting final decision from Frontiers any day now and wish to include this work if both manuscript gets accepted for publication. We hope to have a dialog on this matter, and are submitting this Author response within the deadline given.

Figure changes

Fig. 1: Editor: Fig 1b: Can you make the location of your study site a little more visible? Fig 1c: Why is it not the watershed of the Lake Vangsvatnet that is outlined with the white dotted line? **RC2:** Fig. 1 has no legend for altitude. depicting landcover somehow could also be useful for the reader (maybe in another panel)
We thank you for these comments. The symbol of the location has been expanded and contrast increased in the revised figure 1b. In the revised version of Fig 1c, both watershed of the whole Vosso River catchment and Vosso's inlet to Lake Vangsvatnet is outlined in Fig 1c. The caption is also updated to include the information needed. We have added a legend depicting elevation in Fig 1c.

Fig. 2: Editor: The marine limit line is purple, not blue
Thanks for the clarification. The colour reference in the caption is changed from blue to purple. We have also added two old river channels, marked terrace levels (fluvial erosion scarps), added estimates on delta progradation, marked the outcrop of bedrock and made an inset figure enlarging the area of slumping/landslides in the northern slope of Lake Vangsvatnet to visualise processes occurring in the lake (based on multi-beam data).

Fig. 4a: Editor: can you flip this figure 90˚ to make it vertical?
Figure 4a has been rotated for improved readability.

Fig. 7: Editor: would it be possible to have an example of an M-O? **RC2:** Fig. 7 - I recommend adding the RGB image aligned with the presented CT

We thank you for these comments which will improve the quality of the figure. We have added an MO-layer which is recorded at 148.5 cm in the record. We have also added the MS-data to help interpreting the layers and the RGB-image from the XRF-scanning.

Fig. 11: Editor: I also suggest flipping the figure vertically. (Just a suggestion)

We thank for this suggestion and have in the revised manuscript flipped the figure.

Fig. 12: Editor: This is a nice figure, but I suggest adding the names of each site near their curve to facilitate the reading of the text.

Thank you for the suggestion. We have rearranged the curves and added labels near the curves to facilitate the reading.

All revisions in order of appearance (also indicated with track changes in the attached pdf):

Line 10: Added "[4]University of Oslo, Department of Geosciences, Oslo, Norway" since Johannes Hardeng has a new affiliation.

Line 35–36: 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).

Line 84–87: The delta consists of several old terrace levels (at 61 and 50 m a.s.l.), and at least two old river channels. There is exposed bedrock at the confluence of the Strandaelvi and Raundalselvi rivers that exerts a primary control on the local flow towards the main river channel. From the terrace level at 50 m a.s.l. (most part of Vossevangen), the delta has prograded into Lake Vangsvatnet approximately 400 m (Fig. 2).

Line 102–105: Added more details to the caption of fig. 1: Voss is marked by a black circle. C) Map showing *Lake Vangsvatnet (this record) marked with dark blue color. The Vosso River* catchment *with its outlet at Bolstadøyri is* delineated with a white line. *The catchment for the Vosso river at the inlet to Lake Vangsvatnet is indicated by the dotted white line.*

Line 119–121: …extreme cold and heavy snowfall (1–4 m of snow in the mountains)(DNMI, 2024).

Line 122–123: The annual precipitation at Voss for the 1991-2020-normal is 1370 mm.

Line 142–145: Added more details to the caption of fig. 2: The marine limit at Voss is around 94 m a.s.l (purple dotted line) *… Inset Figure (a) shows two examples of large landslides along the northern slope, as seen in the bathymetric data.*

Line 187–188: Added more details to the caption of fig. 3: The floods in Oct 2014 and Nov 2022 (both in bold) have the highest discharge values since the start of the measurements in 1892 CE.

Line 187–188: Deleted: "Using a Munsell colour chart,"

Line 198–199: To account for down-core fluctuations in instrumental offsets, water content and organic material, all XRF *data were normalised in R using …*

Line 212–213: burnt minerogenic grains were delicately disaggregated and treated with Sodium polyphosphate before being suspended in the hydro-unit …

Line 216–219: Grain-size statistics (median, mean, sorting, skewness) were calculated using GRADISTAT (Blott and Pye, 2001) based on the Method of Moments in geometric units.

Line 255–258: Small cirque glaciers (<0.25 km2) 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 glacier flour to the catchment.*

Line 283–284: The geochemical elements Ca, Ti, Fe, and the Fe/Ti ratio have been used in the interpretation, as these high-energy peaks are stable across both scanning parameter sets in the two cores.

Line 295–297: Most samples display unimodal grain-size distributions. A small number of samples (three M-layers and one MO-layer) show minor bimodality and are visible in the distribution plots (Fig. 6).

Line 306–308: Averaging CT-derived values over the depth intervals of discrete LOI samples reveals a moderate positive correlation between LOI and CT-derived organic content (r=0.57), whereas no correlation exists between LOI and the minerogenic CT content.

Line 334–336: Added more details to the caption of fig. 7: RGB- and CT-image with relative values of XRF-parameters (Ca and Fe/Ti), CT-density and MS. A threshold analysis has been performed on the CT data, identifying the volume percentages of minerogenic (yellow) and organic (green) content related to the sediment layers in the image (B-, M-, O- and MO-layers).

Line 353–356: The final age-depth model indicates a relatively stable sedimentation rate of 1 mm yr^{-1} from 8200–3500 cal yr BP, increasing to an average of 2.1 mm yr^{-1} from 1000–0 cal yr BP. The uppermost unconsolidated sediments (0–30 cm) indicate the highest sedimentation rates of 4.7–2.6 mm yr^{-1} respectively.

Line 387–399: The extensive delta at Vossevangen suggests that the lake has received sustained sediment input over time. Sediment input was most likely very high immediately following deglaciation (11100–10200 cal yr BP), with rapid isostatic rebound of the surface and considerable erosion of glaciofluvial deltas around Voss. The present delta and river channel became developed within the first millennia after deglaciation as sediment availability dropped dramatically, vegetation established, and the present outlet threshold at 44 m a.s.l. came into play. Following this, lake level has fluctuated around this fixed threshold, and the geomorphological configuration of the inlet appears to have remained stable throughout the Holocene. The present river channel is constrained by bedrock at the confluence of the Strandaelvi and Raundalselvi rivers and by the 61 m a.s.l. terrace surface at Vossavangen (Fig. 2), strongly limiting lateral migration. The delta has prograded approximately 400 m from the terrace level at ~50 m a.s.l., indicating modest Holocene progradation relative to the timescale. Thus, while minor delta-front adjustments are inherent to any active delta system, there is no evidence for major Holocene shifts in inlet position that would have affected sediment focusing at the coring site. This interpretation is consistent with observations from comparable lacustrine systems where early Holocene reorganization is followed by long-term stability (e.g. Wilhelm et al., 2022).

Line 403–405: There is no sign of erosion in the studied cores or avalanches close to the coring location on the bathymetric map (Fig. 2). However, several landslides are visible in the bathymetric data along the northern slopes of Lake Vangsvatnet, which appear to be associated with road construction carried out over the past 30 years (e.g. Aalbu, 2023).

Line 426–428: Minerogenic layers (M) indicate a rapid influx of inorganic detrital material, marked by sharp transitions, peaks in Ca and Ti, lower LOI values, elevated MS in the thickest events (>5 mm), and high CT density (Fig. 5 and 7). Grain size shows high variance, reaching up to 200 µm (fine to medium sand), with no grading (Fig. 6).

Line 434–442: Mixed layers (MO) combine characteristics of M and O layers, reflecting rapid input of both inorganic and organic material. They exhibit peaks in Ca and Ti, decreases in Fe/Ti, the largest median grain size (90 µm), and are the thickest layers in the core. They are poorest sorted, but grain-size distributions remain mostly unimodal, lacking the very poorly sorted, multi-modal character typical of mass-flow deposits. CT imagery shows sharp basal contacts and abrupt upper boundaries, with no evidence of erosional scouring, load structures, shear planes, or soft-sediment deformation. The layers are structureless to faintly laminated, and their CT-density profiles show mixtures of dense minerogenic grains and low-density organic detritus. The sedimentological attributes of the MO layers closely resemble flood-derived mixed-density flow deposits described from other lakes (e.g. Kremer et al., 2015), where large floods introduce both coarse minerogenic sediment and abundant terrestrial organic material.

Line 451: Deleted "The difference between flood deposits and background sedimentation in Vangsvatnet reflects abrupt shifts in sedimentation typical of flooding." Since this is already stated earlier in the ms.

Line 494–497: Added more details to the caption of fig. 11: (a) Flood layer count based on CT data, plotted as the stacked sum per 100 years for minerogenic (black), organic (green), and mixed floods (red). (b) The predominant flooding regime in Lake Vangsvatnet, …

Line 512–523: Rewriting of a section: The last 500 years represent the interval with the highest flood frequency in the record, exceeding that of the second-highest period by more than a factor of 2. This period also coincides with increased human activity in the Vosso catchment including deforestation, agricultural expansion, lowering of lake level, and the establishment of water-powered sawmills which would have increased sediment availability. Climatic cooling during the LIA, with increased winter snow accumulation, likely acted in parallel with enhanced anthropogenic disturbance, making the two drivers difficult to disentangle. Thus, the flood frequency increase during this interval should be interpreted as a combined climatic–anthropogenic signal rather than a purely climatic trend. Nevertheless, comparison with regional climate archives and independent indicators of land-use change could help clarify the extent to which human landscape modification contributed to the observed changes in flood activity.

Line 530–533: The Vangsvatnet record is compared to regional palaeoclimate archives to investigate potential correlations between flood activity and other climate-sensitive processes (Fig. 12). Cross-regional comparisons can be challenging due to contrasting climatic patterns, different lake sensitivities, threshold behaviour, sediment availability and different triggering mechanisms operating at various temporal and spatial scales.

Line 573–579: The pronounced non-stationarity observed in the Vangsvatnet flood record is consistent with a growing body of worldwide evidence showing that flood regimes vary substantially on centennial to millennial time scales (e.g. Chen et al., 2021; Šraj et al., 2016; Cunderlik and Burn, 2003; Engeland et al., 2020). These studies illustrate that flood occurrence is inherently non-stationary, driven by complex interactions between climate forcing, hydrological thresholds, and, in many cases, human modification of catchments. The Vangsvatnet record therefore contributes not only to a regional understanding of Holocene flood variability in western Norway but also aligns with global evidence demonstrating that flood frequency cannot be assumed to remain constant over long time-scales.

Line 589–600: Rearranged the case studies shown in fig. 12: Comparison between this record and other palaeoclimatic records from the region (a–i). (a) The record of extreme flood events reconstructed from Lake Berse as events per 100, 200 and 500 years (Hardeng et al., 2024). (b) Predominant flood regime in Lake Lygne, plotted as events per 100, 200 and 500 years (Hardeng et al., 2022). (c) Flood frequency diagram based on the rate of change (ROC) in Sandvinvatnet, SW-Norway (Johansson et al., 2020). (d) Flood frequency in Glomma with a 500-year running average (Engeland et al., 2020). (e) Flood events per 100 yr from Lake Meringsdalsvatnet in Jotunheimen, east-central Norway, indicated as black bars (Støren et al., 2010). (f) Snow avalanche layers in lake Vatnasetvatnet (Hardeng et al., 2026). (g) Mean July temperature at Lake Trettetjørn (Bjune et al., 2005). (h) The deviation in equilibrium line altitude (ELA) from the present (m) for Nordfonna glacier, located in south-western Norway (Bakke et al., 2005). (i) The predominant flood regime (rainfall vs snowmelt floods) from this study and flood events per 100, 200 and 500 yrs.

Line 632–633: Added a sentence to Acknowledgements: We sincerely thank Juan Pablo Corella and an anonymous reviewer for their thorough and constructive evaluations which greatly improved the manuscript.

Line 664–665: Added reference: Chen, M., Papadikis, K., and Jun, C.: An investigation on the non-stationarity of flood frequency across the UK, Journal of Hydrology, 597, 126309, 2021.

Line 666: Added reference: Cunderlik, J. M. and Burn, D. H.: Non-stationary pooled flood frequency analysis, Journal of Hydrology, 276, 210–223, 2003.

Line 706–707: Added reference: Kremer, K., Corella, J. P., Adatte, T., Garnier, E., Zenhäusern, G., and Girardclos, S.: Origin of turbidites in deep Lake Geneva (France–Switzerland) in the last 1500 years, Journal of Sedimentary Research, 85, 1455–1465, 2015.

Line 777: Added reference: Aalbu, J. H.: Vurdering av undersjøiske skred langs Vangsvatnet, Statens Vegvesen (Norwegian road authorities), 12, 2023.

Initial response to the editor and reviewers

Editor

Dear authors,

I encourage you to respond to the comments of the two reviewers using the online interface. Your responses should simply explain how you will address their suggestions and comments, without submitting a revised version of your article. There is no need for a lot of details. Once received, your responses will enable me to decide whether or not to invite you to submit a revised version of your article.

I have also read your article carefully. I would like to make a few suggestions, mainly on stylistic issues that were not raised by the reviewers, with the aim of improving this article, which is already of very high quality.

CP recommends using the abbreviation 'a' (for annum) when referring to a date, and the abbreviation 'yr' when referring to an age/duration (see instructions for authors). Most of the temporal information in your manuscript is dates and should, therefore, be referred to as "a."
We will update all temporal notation so that dates use "a" and durations use "yr," according to CP guidelines.

Fig 1b: Can you make the location of your study site a little more visible?
The symbol of the location has been expanded and contrast increased in the revised figure 1b.

Fig 1c: Why is it not the watershed of the Lake Vangsvatnet that is outlined with the white dotted line?
In the revised version, both watershed of the whole Vosso River catchment and Vosso's inlet to Lake Vangsvatnet is outlined in Fig 1c. The caption is also updated to include the information needed.

Line 120: How thick is the snow cover?
A short statement on typical seasonal snow thickness is added to the manuscript in line …:
"…with occasional periods of extreme cold and heavy snowfall (1–4 m of snow in the mountains)(DNMI, 2024)."

Fig 2: The marine limit line is purple, not blue.
Thanks for the clarification, I am colour weak and some nuances can be mistaken. We will correct the colour reference from blue to purple.

Line 189: CT scans have been performed on half cores? It might be a good idea to write it down. Yes, that is true. We will clarify this in the methods section.

Line 200: Have you used dispersants (such as hexametaphosphate) to perform the grain-size measurements? If not, why? We used Sodium polyphosphate as dispersant and will state it explicitly in the procedure.

Table 1: replace dating by
We will correct the phrasing.

Lines 233-244: I have the feeling that this information should be in the study area section, but I do not have a strong opinion about this. Just consider this option. We will consider the option. We will move this text if it improves the flow.

Fig 4a: can you flip this figure 90˚ to make it vertical?
Fig 5: the same comment, but I understand this might not be possible here.
Figure 4a will be rotated for improved readability. We have made a flipped version of figure 5 and will discuss what version to use. The drawback of flipping this figure is that we can lose some of the details.

Fig 7: would it be possible to have an example of an M-O?
Yes, we will add an interval that includes an MO-type event in Fig 7.

For the CT – volume percentage, I understand that this will be explained in the Cederstrøm paper in review, but I'm curious to see a grey level histogram for each of the facies identified.
We will add representative histograms to illustrate facies differences in Fig 7 or can provide a figure/the paper in review from Cederstrøm illustrating the method.

Line 370: Don't you have a higher-resolution bathymetric map if the Norwegian Mapping Authority performed a high-resolution multi-beam survey? This might be useful to show there is no sign of erosion where the core has been taken.
Yes, that is true. The data from the multi-beam is visible in figure 2, and we will add an inset map showing the slumping in the northern slope. The coring site is flat and far from the slumping in the north. This will be addressed properly in the revised manuscript.

Lines 385-390. It seems that MS does not increase in facies M. Why? High carbonate content?
The MS increase in some of the thickest M-facies, but it is hard to see in figure 5. We will try to implement MS in Figure 7 to highlight this. Some of the layers are thin (2 mm) and the MS-sensor may not register the change in composition due to low variation between 'background sedimentation' and event layers. This is one of the reasons why we rely more on the CT data in comparison to the MS-data.

Lines 412-413: already said.
We will remove the duplicated information.

Fig 11: I also suggest flipping the figure vertically. (Just a suggestion)
We will flip and see if the figure is more visible and clearer, and then decide what version to use.

Line 463: 4900-1600 cal BP. I'm wondering if it should not be 4900 – 1400 cal BP.
Yes, we agree. We will include the 200-year period with no floods (1600-1400 cal BP) in that group.

Lines 489-490: I suggest citing the lake in the same order that they appear in Fig. 12
That is a good idea. We will cite the lakes in the same order that they appear in the figure.

12: This is a nice figure, but I suggest adding the names of each site near their curve to facilitate the reading of the text.
We will add labels directly on the figure curves.

Response to Reviewer #1

“Seasonal variations in flooding inferred from lake sediments in Western Norway”
General assessment

This manuscript presents a very solid and carefully conducted palaeoflood reconstruction from western Norway. The study follows a robust and now well-established methodological framework for the identification and interpretation of flood deposits in lacustrine sediments, and contributes an important new high-resolution record to the growing network of comparable palaeoflood archives in Norway.

A major strength of the paper is the combination of CT-based event detection, sedimentological criteria, and validation against both instrumental and historical discharge records from the Vosso river, which provides an exceptional framework for interpreting the sedimentary archive. The attempt to discriminate flood seasonality (snowmelt versus rainfall-driven floods) using sedimentological proxies is convincing and builds coherently on previous work (e.g. Hardeng et al., 2022).

Overall, the manuscript represents a valuable contribution within the scope of the journal. The scientific approach is sound, the results support the interpretations, and the authors are generally careful and transparent when discussing uncertainties and limitations. In my opinion, the manuscript requires only minor revisions.

Major strengths
The study provides a long (Holocene-scale), high-resolution flood record that is exceptionally well constrained by instrumental and historical data, which is rare and adds substantial confidence to the interpretations.

The methodology is robust and largely consistent with recent palaeoflood studies from Norway, improving the intercomparability between records while acknowledging that individual lakes have different sensitivities to extreme events.
The differentiation between flood seasonality based on sedimentological criteria is well reasoned and represents an important added value of the study, particularly given the complex hydroclimatic forcing of Norwegian catchments.

The authors are generally honest in discussing the limitations related to human impact, sediment availability, and the difficulty of disentangling climatic and anthropogenic signals, especially in the most recent part of the record.

We appreciate the reviewer’s very positive assessment and thoughtful suggestions.

Specific comments and suggestions (minor revisions)

1. References

   The reference list and in-text citations do not appear to follow a consistent ordering scheme (neither alphabetical nor chronological). This should be corrected throughout the manuscript.
   We have corrected the reference order to be consistent throughout.

2. Geomorphological context of the inlet and delta dynamics

   Given that the lake inlet is associated with a complex deltaic system and that the coring site is located relatively close to the delta, it would be useful for the authors to briefly discuss whether there is any geomorphological evidence for inlet or delta shifting during the Holocene. Even a short discussion acknowledging whether such changes are known, likely, or unknown would strengthen the interpretation, as inlet position can influence sediment focusing and the preservation of flood layers. It would be advisable to check previous works from Bruno Wilhelm
   This is a relevant and good question. We will expand the discussion by adding information on old river channels (also in Fig. 2) and include some interpretation on delta progradation. We will also include a sentence about bedrock at the confluence of Strandaelva and Raundalselva, steering the river southward along the main river channel.

3. Intercomparison between palaeoflood records

   The manuscript successfully places the lake record within the context of other Norwegian palaeoflood archives. It may be useful to further emphasize that intercomparisons are inherently complicated by differences in lake sensitivity, threshold behaviour, and sediment availability. Some lakes preferentially record only the most extreme floods, while others may also record more moderate events, and this should be clearly highlighted when discussing regional coherence.
   We will clarify the challenges of comparing archives with different sensitivities and thresholds. We have a section in the discussion (485-490) stating some of the challenges already but will provide more details and challenges.

4. Clarifications in figures and methods
   In Figure 3c, the reason why the October 2014 and November 2015 floods are marked with an asterisk should be explicitly explained in the caption or in the text.
   We will add information about the two large floods (2014 and 2022) in the caption.
   In the Methods section, the CT-based protocol referred to a reference under review should be briefly described (a few sentences), as this work is not yet accessible.
   We will add a short summary of the CT protocol or refer to the article if it is accepted or in preprint.

5. Alternative sedimentary processes and mass movements

   The authors document the presence of lateral collapses and mass movements along the lake margins based on CHIRP data. In this context, it would be useful to more explicitly discuss why the identified event layers—particularly the thicker mixed (MO) layers—are

not interpreted as the distal expression of mass movements, delta collapses, or slope failures.
We will expand the discussion explaining why a mass movement origin is unlikely and provide additional sedimentological reasoning (also see comment from the Editor). The lateral collapses are not seen in CHIRP data, but in the multi-beam data. The collapses are located along the new road construction near Vandrarheimen (400 meter north of the coring site), and most of the sliding occurred during the construction which happened between 2010-2013. No collapses are located at the delta front. We will provide additional data about the collapses north of the core location.

While the interpretation of MO layers as large or prolonged flood events is plausible, a slightly expanded discussion of their texture, structure, and grain-size characteristics would help to further exclude gravitational processes and strengthen the sedimentological interpretation. Similar processes have been documented in Lake Geneva (Kremer et al., 2015) and other swiss lakes.
We agree that we need a slightly expanded discussion on the MO-layers. The MO layers show sedimentological characteristics that support interpretation as deposits derived from large or prolonged flood events rather than gravitational remobilisation. CT imagery reveals sharp, planar basal contacts and internally homogeneous mixtures of organic and minerogenic material without soft-sediment deformation, or shear structures typically associated with subaqueous mass-movement deposits. Grain-size distributions are mostly unimodal and poorly sorted but lack the multi-modal, extremely poorly sorted tails that characterise debris-flow or slump deposits. Although one MO layer displays inverse grading, this is consistent with waxing flood energy rather than gravitational collapse. The flat basin-floor setting and absence of mass-wasting features in CHIRP profiles further argue against gravitational processes. These characteristics closely resemble flood-derived mixed-density flow deposits described from Lake Geneva and other Swiss peri-Alpine systems (e.g., Kremer et al., 2015), where large floods mobilise both coarse minerogenic particles and terrestrial organic debris and deliver them through combined over-, inter-, and underflow processes. We will add references to the work in the revised manuscript and an expanded discussion on the MO-layers to strengthen the sedimentological interpretation.

6. Sedimentation rates in the uppermost section

In Section 4.4, the authors state that the highest sedimentation rates occur in the upper 0–30 cm and are "< 5 mm yr^{-1}". Given the otherwise very robust chronological framework (numerous ^{14}C dates and short-lived radionuclide dating), it would be preferable to provide a more precise estimate or a narrower range for these sedimentation rates.
Yes, we will add some more details in section 4.4: "The final age-depth model indicates a relatively stable sedimentation rate of 1 mm yr^{-1} from 8200–3500 cal yr BP, increasing to an average of 2.1 mm yr^{-1} from 1000–0 cal yr BP. The uppermost unconsolidated sediments (0–30 cm) indicate the highest sedimentation rates of 4.7 –2.6 mm yr^{-1} respectively".

7. Interpretation of the last 500 years

The authors identify the last ~500 years as the period with the highest flood frequency in the record. Although this is already discussed, it may be worth emphasizing even more strongly that this increase could partly reflect enhanced sediment availability and human impact rather than a purely hydrological signal. The climatic and anthropogenic influences are likely strongly intertwined during this interval, which limits a straightforward climatic interpretation. The authors' honesty in acknowledging that the human impact cannot be quantified is appreciated. As a possible way forward, the comparison with independent indicators of land-use change (e.g. regional pollen records, arboreal vs. non-arboreal pollen ratios) could be briefly mentioned.

We thank the reviewer for this important comment. We agree that the marked increase in flood-frequency over the last ~500 years likely reflects a combination of climatic change and enhanced sediment availability stemming from increased human activity in the catchment. We will rephrase a paragraph in section 5.4 to: "The last 500 years represent the interval with the highest flood frequency in the record, exceeding that of the second-highest period by more than a factor of 2. This period also coincides with increased human activity in the Vosso catchment including deforestation, agricultural expansion, lowering of lake level, and the establishment of water-powered sawmills which would have increased sediment availability. Climatic cooling during the LIA, with increased winter snow accumulation, likely acted in parallel with enhanced anthropogenic disturbance, making the two drivers difficult to disentangle. Thus, the flood frequency increase during this interval should be interpreted as a combined climatic–anthropogenic signal rather than a purely climatic trend. Nevertheless, comparison with regional climate archives and independent indicators of land-use change could help clarify the extent to which human landscape modification contributed to the observed changes in flood activity."

Response to 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 km²) 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