Anonymous Referee #2:

(Referee comments in regular font, author response in italic gray font)

I thoroughly enjoyed reading the manuscript entitled "*Decoupling climate and avalanche activity: Holocene insights from lacustrine sediments in western Norway*". The manuscript is well-written and presents multi-proxy reconstruction of Holocene snow avalanche activity using lacustrine sediments from Lake Vatnasetvatnet in western Norway. The study employs sedimentological, geochemical, and CT scanning techniques to reconstruct ~10,000 years of snow avalanche activity from lake sediment records, which seems methodologically sound. Avalanche event layers are identified using two semi-automated approaches: (1) rate of change in Ti concentrations and (2) thresholding of CT grayscale data. Based on these methods, the authors distinguish three main phases of Holocene avalanche activity: (1) low activity during Early Holocene (>6500 cal yr BP), (2) increased activity during the mid-Holocene (6500–4200 cal yr BP), and (3) the highest frequency during Late Holocene (~4200 cal yr BP to the present). These patterns are interpreted as being primarily influenced by large-scale atmospheric circulation (i.e., variability in North Atlantic sea surface temperatures and fluctuations in the North Atlantic Oscillation). This study represents a valuable contribution to palaeoclimatology and natural hazard research, particularly in a region where long-term avalanche dynamics are poorly constrained.

Strengths

The manuscript demonstrates several notable strengths: (1) the 10,000-year record are particularly impressive in avalanche research, (2) combined use of LOI, DBD, grain-size analysis with magnetic susceptibility, XRF, and CT scanning greatly enhances the robustness of event layer identification, (3) the application of dual detection methods (Ti-based rate of change and CT thresholding), helps to minimize subjectivity in distinguishing event layers, (4) the chronostratigraphy is supported by AMS dating and rBacon modeling, and (4) the comparison of avalanche frequency with regional climate systems provides valuable context and strengthens the broader climatic interpretation of the findings.

We sincerely thank reviewer 2 for the positive and encouraging assessment of our manuscript. We appreciate the recognition of the study's methodological approach, robustness, and contribution to improving the understanding of long-term avalanche dynamics in western Norway. We are particularly pleased that the reviewer highlights the value of the multi-proxy design, the detection methods for identifying event layers, and the integration of the avalanche record within a broader climatic framework.

Limitations/improvement

Despite these strengths, several areas of the manuscript could benefit from further clarification and refinement: (1) The use of the term decoupling in the title seems not fully explored. The manuscript primarily demonstrates co-variability between avalanche activity and climate proxies rather than a clear decoupling. (2) More explicit discussion of uncertainties, especially in threshold calibration and sediment source attribution would be nice. (3) The novelty as highlighted in the manuscript may seem overstated. Similar reconstructions studies exist, and this study can be discussed as more on refinement than on conceptual breakthroughs.

1) We appreciate this observation and understand the concern. In this context, decoupling refers to a weakening or alteration of an expected relationship between two variables that are normally linked, in this case, between climate variability and avalanche frequency. It highlights that avalanche activity does not always respond linearly or synchronously to temperature-driven climate forcing. While the manuscript documents several intervals of co-variability between avalanche frequency and large-scale climate modes (e.g., the NAO), it also identifies periods where this relationship appears weakened or non-linear, reflecting shifts in the relative influence of temperature and precipitation. We are open to considering alternative, more precise titles should the editor find this advisable. Other suggestions: "10,000 years of snow avalanche activity in Western Norway: A multi-proxy lake sediment record from lake Vatnasetevatn, Hardanger". Or alternatively: "North Atlantic climate variability and Holocene snow avalanche activity in Western Norway recorded in lake sediments".

2) We agree that a more explicit discussion of uncertainties would strengthen the manuscript. The CT- and Ti-based thresholds were calibrated empirically by comparing the automated detection outputs with visually identified layers, as described in lines 495–496 and 501–506. We can elaborate on the uncertainties in a revised version. Regarding sediment source attribution, we refer to our responses to Reviewer 1 for a detailed rationale; in short, the geomorphological setting strongly indicates snow avalanches as the dominant process, although minor contributions from fluvial reworking cannot be entirely excluded.

3) It is unclear which specific formulation(s) reviewer 2 is referring to here. We respectfully disagree that the manuscript overstates its novelty, we do not claim any conceptual breakthrough, nor do we highlight novelty beyond the context of applying established multi-proxy techniques to a previously unstudied catchment. The manuscript explicitly situates our work within the framework of earlier avalanche-related sediment studies in Norway and elsewhere.

For instance, lines 54–58 clearly acknowledge previous snow avalanche reconstructions from western Norway (e.g., Blikra & Nemec 1998; Nesje et al. 2007; Vasskog et al. 2011; Aa et al. 2022), and from the Alps (e.g., Fouinat et al. 2017). Likewise, our methodological discussion (489–503) directly references earlier studies that have employed RoC analysis (Støren et al., 2010; Røthe et al., 2019b; Johansson et al., 2020; Hardeng et al., 2022), and CT scanning (van der Bilt et al., 2021; Cederstrøm et al., 2021; Ballo et al., 2023; Støren et al., 2010; Hardeng et al., 2022; 2024; Fouinat et al., 2017; Røthe et al., 2019b) for detecting sedimentological structures and event deposits.

Other Comments

All event layers are interpreted as avalanche deposits, but what is the likelihood that some of these layers were instead formed by fluvial processes, erosion, or landslides coupled with fluvial activity? While the surrounding landscape may not support a large fluvial system, smaller streams, in combination with upstream erosion and landsliding during climatic extremes, could plausibly produce similar deposits. Would more detailed analyses of the sedimentary characteristics help to distinguish between these potential depositional processes? For example - It was discussed that >3m layers are related to the slumps - are those sudden deposits derived from slumps upstream? Clarification on why these layers are slump-related would be helpful. Given that, I felt some discussion on alternative sediment sources, such as debris flows, rockfalls, fluvial pulses as well as vegetation dynamics and anthropogenic influences (i.e. during the late Holocene) would be appreciated.

We thank the reviewer for this comment, which aligns with similar points raised by Reviewer 1. We acknowledge that our rationale for excluding fluvial or landslide-related processes could be presented more clearly, and we are happy to expand this discussion in the revised manuscript using the arguments outlined in our detailed response to Reviewer 1. Extensive geomorphological mapping shows no evidence of landslides, debris flows, or fluvial erosion within the catchment.

Reviewer 2 states that "It was discussed that >3 m layers are related to the slumps...", this is not correct. The manuscript reads (Line 364-366): "Layers >3 mm thick were marked as instantaneous deposits (slumps) in the rBacon age-depth model to improve chronological accuracy and better reflect true depositional ages (Fig. 8)." We are never discussing 3 m thick slumps; we are referring to the thin (3 mm-10 cm) event layers that we classified as "slumps" within the rBacon age-depth modelling software. We acknowledge that this terminology could cause confusion and acknowledge that this should be clarified.
We emphasise that the described event layers are not slump-related deposits. The word "slump" is only mentioned once in the manuscript, and that is in relation to settings within the rBacon age-depth modelling. Our interpretation is that the layers are deposited by snow avalanches triggered in the upper slopes (Avalanceh tracks 1 and 2, Fig. 2, 3, and 4). We describe the depositional process and supporting sedimentological evidence in depth in lines 466–479.

The role of solar forcing is discussed but not quantitively modeled. A more rigorous climate-forcing attribution or discussion would strengthen the manuscript.

We appreciate the reviewer's suggestion. We agree that a quantitative climate-forcing attribution would further strengthen the mechanistic understanding of the observed variability. However, such modelling is beyond the scope and aims of this study, which focuses on the sedimentary reconstruction and its interpretation in relation to established palaeoclimate records. Our discussion of solar forcing is therefore intentionally qualitative, based on well-documented orbital and insolation trends (e.g., Laskar et al., 2004) and their established influence on Holocene hydroclimate variability in the North Atlantic region. We believe this contextual approach is appropriate for the present dataset and ensures the manuscript remains focused on the reconstruction and interpretation of avalanche activity rather than extending into climate modelling or quantitative attribution analyses, which would require a different methodological framework and data resolution.

Ti as a proxy for minerogenic input is well-established, but its specificity to avalanche deposits may require further discussion. I assume, Ti can also reflect fluvial or colluvial processes too. Similarly, the selection of thresholds (i.e., 95th percentile for Ti RoC and the 40% CT thresholding) lacks sufficient justification. A sensitivity analysis or probabilistic modeling approach could help validate these thresholds and reduce subjectivity.

We agree that Ti is a general indicator of minerogenic input and not inherently specific to avalanche deposits. In our study, Ti was not used to distinguish between depositional processes per se but to objectively identify minerogenic layers within the sediment sequence. The process attribution, whether these layers represent snow avalanches or alternative mechanisms, is discussed separately, based on the geomorphological and sedimentological context of the catchment.

We do not agree that the threshold selection lacks justification. Both thresholds (the 95th percentile for Ti rate-of-change and the 40% CT threshold) were calibrated through iterative comparison with visually identified minerogenic layers and supporting sediment proxies (e.g., MS, DBD, LOI), yielding consistent results between two independent methods (r = 0.76). This strong agreement demonstrates the robustness of our approach and aligns with procedures applied in comparable studies. While threshold determination inevitably involves some subjectivity, a probabilistic or sensitivity-based calibration would require independent "ground truth" data unavailable for this type of archive. Our approach provides a transparent and reproducible framework for event-layer identification within these constraints.

Although the manuscript discusses different avalanche types (e.g., wet vs. dry), no attempt is made to differentiate them within the sedimentary record. If feasible, distinguishing between avalanche types based on sediment characteristics would add valuable depth to the interpretation.

We respectfully note that attempts to distinguish between different depositional processes were indeed made through the Principal Component Analysis (PCA), which is designed to detect compositional variability within the dataset. The PCA successfully identified three sediment types, glacially derived silt (Unit B), background sedimentation, and event layers (Unit A). The event layers share the same sedimentological characteristics, indicating deposition by a single dominant process. This uniformity, supported by both visual inspection and statistical analysis, suggests that further subdivision into separate avalanche types (e.g., wet vs. dry) is not warranted based on the available sedimentary evidence.

The manuscript should clarify how slope gradients were calculated - if derived from a digital elevation model (DEM), the type and horizontal resolution should be specified. The identification of two avalanche tracks is well-supported, but the possibility of lateral migration or undocumented tracks should be acknowledged.

The slope gradients were derived from the LiDAR-based digital elevation model (0.25 m resolution) provided by the Norwegian Mapping Authority, as stated in lines 113–115. We can clarify this further in a revised version by explicitly describing how slope gradients were calculated from the DEM.

Regarding the possibility of lateral migration or undocumented avalanche tracks, we consider this unlikely. The steep topography of the southern catchment strongly constrains avalanche paths, and the treeless corridors marking Tracks 1 and 2 correspond clearly with geomorphic and sedimentological evidence of recurrent activity. No additional tracks were observed in the high-resolution LiDAR data or during field mapping, although we acknowledge that minor variations in runout width and extent likely occur between individual events.

Fig. 1C - The horizontal blue lines likely stem from ESRI data artifacts. I would suggest regenerating the map using better DEM data. Also, the catchment boundary should be clearly labeled and described.

The horizontal blue lines in Fig. 1C are not ESRI data artifacts but represent mapped bogs and marshes derived from national land-cover data. We agree that this could be *misinterpreted in the absence of a legend and will clarify or remove these symbols in a revised figure, as they are not essential to the discussion.*

The DEM used for this map is a LiDAR-derived dataset with 0.25 m resolution from the Norwegian Mapping Authority (www.hoydedata.no), which represents the highest-quality topographic data available for this region. Regarding the catchment boundary, it is already shown as the black outline around Lake Vatnasetvatnet and described in the figure caption, but we can explicitly label it in the revised version if necessary to avoid confusion.

Fig. 2B (GPR Profiles): if possible, labeling the water-sediment and sediment-bedrock interfaces would help readers assess the effectiveness of the GPR method.

We thank the reviewer for this helpful suggestion. We agree that labeling the water-sediment and sediment-bedrock interfaces will improve the clarity of Fig. 2B.

Fig. 5 –26th layer appears missing.

We thank the reviewer for this careful observation. The reference to the 26th layer in the caption is a typographical error, it should read 25, consistent with the sediment log and core description provided in Section 4.2. This will be corrected in the revised version.

Fig. 7: More visual clarity would be helpful, for example through clear labeling of the cores .

We thank the reviewer for this comment. Figure 7 is a CT visualisation of the interval 90–110 cm in core VAPG, as stated in the caption. To improve clarity, we can add "VAPG" directly to the figure itself. It is somewhat unclear what other aspects of the figure the reviewer found visually unclear, but we will review the layout and contrast to ensure that key features are as legible as possible in the revised version.

To further enhance the manuscript, I recommend incorporating additional sedimentological and geochemical indicators to better distinguish avalanche deposits from other high-energy events. A probabilistic framework for threshold selection and age-depth modeling could improve the robustness of the reconstruction. Integrating climate modeling and linking avalanche frequency to regional climate simulations would provide valuable insights into future scenario analyses. Finally, incorporating land-use history and vegetation dynamics more explicitly would help contextualize the Late Holocene trends. In conclusion, this is a well-written, well-structured and timely study with significant potential to advance our understanding of long-term avalanche dynamics. However, I think the interpretation linking depositional layers exclusively to avalanche events requires more sedimentological justification and differentiation from other depositional processes. I believe addressing the points outlined above would greatly enhance the clarity, robustness, and impact of the manuscript.

We thank the reviewer for their constructive comments, and we appreciate the recognition of the study's overall quality and potential contribution. We acknowledge that additional sedimentological and geochemical analyses (e.g., mineralogical or isotopic indicators) could provide further insights into the depositional mechanisms. However, incorporating such datasets lies beyond the scope of the present study, which was designed to establish a long-term, first-order reconstruction of snow-avalanche activity. The integration of probabilistic or climate-modeling frameworks, as well as detailed land-use reconstruction, would certainly *add further depth, but would require data types and resolutions not available for this catchment. We regard these as valuable directions for future research.*

As detailed in our responses above, we are prepared to strengthen the discussion on alternative depositional processes and clarify the geomorphological and sedimentological reasoning behind our interpretation of the event layers as avalanche-derived. Together, these clarifications should improve both the transparency and robustness of the interpretation while keeping the study focused within its intended scope.