Subject: Response to Reviewers' Comments and Revised Manuscript Submission

Dear Editor-in-Chief,

We sincerely thank you and the reviewers for the thoughtful and constructive feedback. We carefully addressed all comments provided by the reviewers and made substantial revisions to improve the clarity, structure, and technical content of the manuscript.

A detailed Response to Reviewers document is included, outlining how each comment was addressed. In brief:

- All reviewer comments were addressed individually, with clarifications provided where direct changes were not made.

We appreciate the opportunity to revise and resubmit our work and hope the revised version meets the expectations of the editorial committee and reviewers.

Thank you for your time and consideration.

Mahya Roustaei

Comment (Editor)	Action taken/clarification
ORIGINALITY / NOVELTY (1-4): 2 *The use of oc CT scans is not new in investigations of permafrost. However, a detailed revision of extensive number of samples opens new perspectives on this topic and may be relevant for TC*	No action needed.
SCIENTIFIC QUALITY / RIGOR (1-4): 2 *I am not sure about the method used to evaluate the ice content in the samples. It seems the authors dried and saturated/froze their samples to obtain an independent measure of ice content in the samples and evaluate the estimates obtained by means of CT. In such case I am not sure whether we compare ice content in a sample with potential ice-content. Moreover, no information is given on how the authors kept frozen samples during the scan. In my opinion the scan takes time in order to increase the resolution. However, no information is given about the time for the ct scan, the logistics to keep the sample frozen and other settings for the ct scan. However, these issues may be addressed by the authors during the revision and may be that other experts agree on the methodology used.*	Thanks for this good comment. The samples were not dried and re-saturated/frozen to estimate ice content. Instead, all measurements were performed on **natural, undisturbed permafrost cores**, preserving their in-situ ice content. Volumetric ice content was determined using a **standard gravimetric/volumetric approach**, based on measurements of sample mass and volume in the frozen and oven-dried states. This method does not involve re-saturation and therefore provides a direct measurement of the original ice content, rather than a potential or reconstructed value. Samples were extracted in the field and maintained in a frozen state throughout handling. After drilling, the outer 2-3 mm of each core which could be covered by refrozen drilling mud or minor thaw, was removed prior to analysis. This ensured that laboratory measurements and CT analyses were performed on intact core material. Regarding thermal conditions during scanning, samples were consistently maintained below freezing. After collection, the samples were kept frozen in a chest freezer until scanning. During handling and CT scanning, they were wrapped in insulating materials, including plastic and foam layers, to limit heat exchange. Exposure to ambient conditions during handling and scanning was brief (on the order of minutes), and no phase change was observed. Medical CT scans were relatively rapid in comparison to high-resolution industrial CT systems that require longer acquisition times. So, the duration of scan did not significantly affect the frozen state of the samples. These procedures ensured that both CT imaging and laboratory measurements were conducted on samples that remained representative of their frozen state. Manuscript was revised to clarify these aspects of the methodology, including (i) the use of natural undisturbed samples, (ii) the gravimetric determination of ice content, and (iii) the handling and thermal control procedures during CT scanning.
SIGNIFICANCE / IMPACT (1-4): 2 *The large scale of samples could provide new perspective on the CT-scan as a methodology for ice investigations. The discussion about the role of organic*	No action needed

Comment (Editor)	**Action taken/clarification**
matter into the reliability of ice estimates from CT-scans may also provide extra relevance to the study.	
PRESENTATION QUALITY (1-4): 1 *I marked a few issues above regarding the lack of detailed information on the settings of the CT-scan (those could be added as an Appendix if needed to keep the length of the paper within the accepted range. I think the introduction also requires some further edits, considering that at the end it reads rather as a conclusion (due to the presentation of results) than a revision of the literature, presentation of objectives, research questions and hypothesis.* *Summary statement and decision.* *I am adding here an edited PDF that may be helpful to the authors.*	Thanks for this helpful comment, the introduction has been edited to improve its structure and focus, and it now clearly presents the relevant literature, as well as the objectives and scope of the study, without including result-oriented statements. Regarding the CT scan settings, the scans were performed using a Siemens SOMATOM Sensation 64 medical CT system, and we have clarified the image resolution (voxel size and matrix) in the revised manuscript. Detailed acquisition parameters such as tube voltage, tube current, and exposure time were not available for all scans, as the measurements were conducted using standard clinical protocols at the facility.
ice content?	Addressed. (Text has been edited)
how much is low?	To clarify, the term "low" has been replaced with a quantitative range (10–20%).
explain?	The term "unresolved pore ice" was replaced with " ice occurring at scales smaller than the CT voxel resolution"
formulation?	Thanks for your input, sentence has been rephrased.
maybe introduce this?	Pore ice definition has been added: ice occupying the natural pore spaces within the soil matrix)
is this a conclusion of this study? as suggested by the abstract? or is a conclusion form the revision of literature? Would be recommended to add citations.	Citation were added.
improve formulations	Addressed. (Text has been edited)
conclusions? references?	Citation were added.
what are the alternative methods to evaluate the ice content estimated?	Alternative methods have been introduced at the beginning of the Introduction.
how were the cores stored and taken to avid change sin the ice content? prior to the analysis?	Please check the response to your previous comment on PRESENTATION QUALITY.
information on the settings for the CT scanning are missing	Please check the response to your previous comment on SCIENTIFIC QUALITY
is this way suitable to measure the ice content in the sample or the potential ice content?	Yes, this method provides a direct measurement of the in-situ ice content and not the potential ice content.
are those two quantities comparable? the CT scan was conducted just after drilling ?	Thank you for this comment. To address points raised by other reviewers, additional explanation has been added to clarify the rationale for comparing

Comment (Editor)	Action taken/clarification
	laboratory-measured and CT-derived ice content. The Methodology section has also been expanded to describe sample collection, handling, and the timing of CT scanning, providing clearer context on the measures taken to minimize sample disturbance and support the comparability of the two measurements.
why not add a percentage deviations to avoid the qualitative assessment?	Table 1 has been updated to report volumetric ice content rather than ice volumes. In addition, a Residuals column has been added for each method to provide a more quantitative basis for comparison. The residual bounds are now explicitly described in the text.
check	Typo has been edited.
would be good to have a clear percentage of misfit instead of a qualitative assessment	Thank you for this suggestion. The sentence has been rephrased for clarity.

Comment (R1) Benoit Faucher	Action taken/clarification
The manuscript addresses an important methodological question regarding the use of medical CT scans for quantifying volumetric ice content in frozen sediments, which is highly relevant for permafrost research. The dataset is substantial, and the comparison with laboratory measurements is valuable. However, I believe that minor revisions are needed to clarify several aspects of the interpretation before the manuscript is ready for publication. Below, I outline comments and questions aimed at clarifying the interpretation of CT - derived versus laboratory - measured volumetric ice content, with particular attention to the direction and magnitude of bias, the proposed influence of organic matter, and the consistency between the figures and the accompanying discussion.	We thank the reviewer for the positive feedback and for carefully identifying inconsistencies between Figure 1a and the accompanying results and discussion. We apologize for the confusion caused. Upon review, we identified an error related to a mismatch between the axis labels and the plotted values in Figure 1a. This error has now been corrected, and the revised figure is consistent with the results and discussion. Addressing this issue also resolved the related comments raised by the reviewer.
According to Figure 1, it appears that CT overestimates VIC in ice -poor sediments and underestimates it in ice-rich sediments. The text seems to describe the opposite trend and should be revised for consistency.	Thank you for bringing this to our attention. The apparent inconsistency arose from an error in Fig. 1a (a mismatch between the plotted values and axis labels), and the figure has been corrected. After correcting Fig. 1a, the figures and text are now consistent.
This sentence is unclear and would benefit from rephrasing.	Thanks for your input, sentence has been rephrased.
Consider adding a typical HU range for organic matter to provide context for the classification.	Thanks for this suggestion. However, a single representative HU range for organic matter cannot be defined, as its density spans a wide range depending on the degree of decomposition and composition. This variability often results in overlap with both ice and sediment HU ranges, making reliable threshold-based classification difficult.
Lapalme et al. (2017) suggested that CT imagery may be better suited to estimating EIC, particularly when pore-space diameters are within pixel resolution. Why was CT segmentation not compared with EIC in this study? Clarification would be helpful.	Thank you for this comment. We focused on volumetric ice content (VIC) because it is the only laboratory-measured ice parameter available for this dataset. Estimating laboratory-measured excess ice content (EIC) was not feasible. A sentence has been added to the opening paragraph of Section 3.1 to clarify this limitation, and EIC comparison is noted as a potential direction for future work.
Please provide justification for the chosen organic matter classification thresholds (<10%, 10 –20%, >20%).	Justification have been added to the text. Results were also checked using alternative cut points (e.g., quantile-based bins), and the observed trends were not materially changed.
The statement "Underestimation exceeded 60% at the low end of the $VLab$ ice range, while overestimation reached about 30% in ice -rich, organic -rich	Thank you for this comment. The text has been revised to better reflect the trends shown in Figures 1a and 1b.

Comment (R1) Benoit Faucher	Action taken/clarification
samples" appears inconsistent with Figure 1b, which suggests the opposite pattern. This should be reconciled.	
The text states systematic underestimation in ice -poor samples and overestimation in ice -rich samples. Based on Figure 1, the trend appears reversed.	Figure 1a has been revised to correct an error caused by a mismatch between the axis titles and the plotted values. The updated figure is now consistent with the text and interpretations presented in the manuscript.
The phrase "…indicating systematic underestimation" appears inconsistent with the plotted data, which suggest overestimation.	No additional revisions were required, as the figure and accompanying text are consistent after the update to Figure 1a (see response to previous comments).
The statement that low -OM samples were underestimated and high -OM samples overestimated does not appear to align with Figure 1. Please clarify.	Thanks for pointing this out. Following the update to Figure 1a, the text is now fully consistent with the figure.
The general statement regarding systematic misestimation should reflect the direction of bias observed in Figure 1.	A sentence has been added to clarify the direction of the bias
The explanation invoking mixed voxels would typically result in an underestimation of ice content. However, Figure 1 appears to show overestimation in ice -poor samples. Please clarify how this mechanism explains the observed bias.	Figure 1 a has been updated and now shows underestimation of ice content for ice-poor sample, in agreement with the discussion.
The manuscript states that organic matter misclassification produces overestimation. However, Figure 1 does not clearly demonstrate that ≥20% OM samples exhibit the strongest overestimation; many appear underestimated. Quantitative support or clarification would strengthen this interpretation.	Please refer to the updated Figure 1a. This discrepancy arose from an error in the previous version of Figure 1a, which has now been corrected. The revised figure clearly shows overestimation of ice content for samples with >20% organic matter.
The summary statement describing underestimation in ice -poor samples and overestimation in organic - rich samples appears inconsistent with Figure 1 and should be revised.	Please see the updated version of Figure 1a.

Comment (R2)	**Action taken/clarification**
This is a well-written manuscript that addresses the limitations of medical-CT in terms of routine application and quantitative analysis within permafrost research. However, please find below a few minor revisions that require clarification, particularly regarding the interpretation of the results.	We thank the reviewer for the positive and constructive feedback, all the comments has been addressed as follows:
L58: Please write out the full term before using "ADAPT" as abbreviation. Additionally, provide more details on the sample dimensions, such as the core diameter range and length range of the collected specimens.	The permafrost samples were collected using a portable permafrost core drill (10 cm diameter core barrel). The upper 2 m of permafrost was drilled, excluding the thawed active layer. The cores were then subsampled into 5–20 cm sections for CT scanning. Full term of "ADAPT" has been added to the revised version.
L61: Was the sample temperature maintained at 0 °C during scanning? If not, could melting of ice and associated phase transitions have affected the results?	Samples were consistently maintained below freezing during scanning. After collection, the samples were kept frozen in a chest freezer until scanning. During handling and CT scanning, they were wrapped in insulating materials, including plastic and foam layers, to limit heat exchange. Exposure to ambient conditions during handling and scanning was brief (on the order of minutes), and no phase change was observed. Medical CT scans were relatively rapid in comparison to high-resolution industrial CT systems that require longer acquisition times. So, the duration of scan did not significantly affect the thermal state of the samples. These procedures ensured that both CT imaging and laboratory measurements were conducted on samples that remained representative of their frozen state. Manuscript was revised to clarify this in the methodology.
L64: Please provide more details on the scanning parameters, such as tube voltage, tube current, and exposure time, as these directly influence the reconstructed Hounsfield Unit (HU) values.	The scans were performed using a Siemens SOMATOM Sensation 64 medical CT system, and we have now clarified the image resolution (voxel size and matrix) in the manuscript. Detailed acquisition parameters such as tube voltage, tube current, and exposure time were not available for all scans, as the measurements were conducted using standard clinical protocols at the facility. This has now been explicitly stated in the manuscript. While such parameters can influence absolute Hounsfield Unit (HU) values, our analysis focuses on phase segmentation within individual scans, where relative contrasts are preserved.
L65: Please also provide more details on the image processing. Were any noise filtering steps applied to the data prior to segmentation? Furthermore, were any binary or morphological operations performed after segmentation before measuring the ice, sediments, and gas phase?	No explicit noise-filtering step was applied prior to segmentation. Instead, segmentation was based on Hounsfield Unit thresholds and visually verified to ensure consistency in phase identification. The only morphological

Comment (R2)	Action taken/clarification
	operation applied was the reduction of the sample ROI by a few millimetres to remove outer regions potentially affected by drilling disturbance.
L66: Please provide more detailed explanation of the Regions of Interest (ROIs) definition process. The term "ROI" is commonly used to denote a specific area of the sample selected for further analysis, while a "trained classifier" in image processing usually refers to an AI-assisted algorithm trained to segment a specific phase.	We agree that the description of the ROI definition could be clarified. In this study, the trained classifier was not used for phase segmentation but to distinguish the sample from the surrounding background (e.g., air and external materials) and define the region of interest. The resulting ROIs therefore represent the sample volume within which phase segmentation (ice, sediment, gas) was subsequently performed using HU thresholds. We have revised the manuscript to clarify this distinction and to state that the ROIs were visually inspected and corrected where necessary.
L67: There appears to be an overlap of 1 HU between the gas-ice and ice-sediment ranges. This implies that some voxels may be counted twice, either as gas and ice or as ice and sediment. Could you clarify this HU overlap? Would it potentially lead to an overestimation of the phase volumes?	The HU ranges are defined as contiguous intervals, with each boundary value assigned to a single phase (Gas: **–1024 to –321,** Ice: **–320 to 560,** Sediment: **561 to 3071) so t**here is **no overlap**
L75: Could you justify the selection of half of the total samples (81 out of 261) for the determination of the organic matter (OM)?	81 samples were analyzed for organic matter content. The remaining samples couldn't be analyzed for organic matter content. Indeed, these remaining samples were used in a previous study where only volumetric ice content was measured. The samples were discarded after analysis, and we couldn't therefore measure their organic matter content. They were used in this study to increase the number of samples with measured ice content.
L87-88: The underestimation of 60% is stated here; however Figure 1(b) suggest a larger overestimation of the CT-derived volumetric ice content (). Could you please check whether this is correct, or maybe this is due to the typo on the x-axis label that differs from the definition in the text? Additionally, please clarify the 60% and 30% values-are they percentages out of the 261 samples?	Thank you for the comment. We clarify that the x-axis in Figure 1b shows residuals, defined as CT-derived volumetric ice content minus laboratory-derived volumetric ice content. Therefore, negative residuals indicate underestimation by the CT method, while positive residuals indicate overestimation. As shown in Figure 1b, negative residuals exceed -60%, whereas positive residuals reach approximately +30%. The reported 60% and 30% values therefore refer to the magnitude of the residuals, not to percentages of the 261 samples. To address the reviewer's concern, the text in Lines 87–88 has been revised to improve clarity and ensure consistency with Figure 1b. We also confirm that there was a typo in Figure 1a, where the axis labels did not match the plotted values. Figure 1a has been corrected, and the figure and text are now consistent.

Comment (R2)	Action taken/clarification
L90: Similar to Lines 87-88, please clarify this point. The figure caption states an underestimation of CT-derived volumetric ice content () for ice-poor samples measured from laboratory. However, figure 1(a) appears to indicate an overestimation of the .	This inconsistency was due to the same issue identified in Fig. 1a, which has now been corrected, and the figure and caption are consistent with the interpretation presented in the text.
L90: To enhanced visualization, please maintain the same legend style for presenting the organic matter content (%) between figure 1(a, text) and (b, colormap)?	The figure has been revised to ensure a consistent legend style for organic matter content (%) across Fig. 1a and Fig. 1b.
L102: Could the "widely varying thresholds" originate from differences in scanning parameters across different studies, as well as variations in ice type (e.g., pure ice vs. Sea ice), and sample composition? It may also be helpful to include this information in Table 1, as these parameters directly influence the reconstructed HU values.	We agree that differences in scanning parameters, ice type, and sample composition can contribute to variability in reported HU thresholds. However, detailed scan settings are not available in all referenced studies and therefore could not be included in Table 1. To address this point, Table 1 has been revised to indicate the type of materials investigated (e.g., sea ice versus permafrost cores). We also clarify in the revised manuscript that sea ice generally exhibits less variability in HU values due to its relative homogeneity, whereas permafrost shows greater variability because of its heterogeneous composition (e.g., sediment, organic matter, and mixed phases).
L103: Could you please elaborate on how the two representative samples were selected? Were they chosen because their CT-derived volumetric ice content () is similar to the laboratory measured ice content()? Or were they selected simply to represent two different localities (Bylot Island and Beaver Creek)?	The two representative samples were selected to reflect different locations (Bylot Island and Beaver Creek) and, as noted in the text, Sample 1 exhibited a broad density range, whereas Sample 2 from Beaver Creek showed a narrower range.
L134: Could you elaborate further on the calibration against reference data?	In this study, no explicit calibration against external reference materials was performed. The reference to calibration in the discussion was intended in a general sense, highlighting approaches reported in the literature where CT-derived densities are adjusted using materials with known properties.

Comment (R3)	Action taken/clarification
The abstract does not fully show what has been done. It gives the impression that high-resolution micro-CT experiments have been done as well, and that there has been a comparison with the medical CT. However, this is not the case. Furthermore, high-resolution micro-CT is not discussed in the document at any moment. Therefore, I suggest making the abstract clearer and adding a paragraph in the discussion section about high-resolution micro-CT on ice cores. Does it really quantify the ice fraction better compared to medical CT? Has this been tested? If not, you can add it in future research, as this is missing as well.	We thank the reviewer for the positive and constructive feedback, all the comments has been addressed as follows: We agree that the original abstract could give the impression that high-resolution micro-CT experiments were conducted and compared with medical CT in this study. This was not the case, as our work is exclusively based on medical CT scans. The abstract has been revised to clarify that this study is based solely on medical CT and does not include high-resolution CT experiments or comparisons. Following the reviewer's suggestion, we have also added a sentence in the Discussion addressing the potential of higher-resolution CT (e.g., micro-CT) and clearly stating that its ability to reliably quantify ice content in permafrost samples has not yet been systematically evaluated and remains a topic for future research. These revisions clarify the scope of the study and better position high-resolution CT within the context of future work.
Another sentence of the abstract is, in my opinion, not discussed later as well: "we conclude that medical CT is better suited for visualizing cryostructures and heterogeneity than for routine quantification of ice content". Can you elaborate more on this in the discussion or introduction? For which structures has it been successfully used?	We agree that this statement require further elaboration. To address this, we have edited the introduction and expanded the Discussion to explicitly highlight the types of cryostructures and heterogeneities that can be reliably identified using medical CT. These additions clarify that, despite its limitations for quantitative analysis, medical CT remains a powerful tool for qualitative characterization of permafrost cores.
Introduction and also in other sections: make a distinction between medical-CT and high-resolution micro-CT, as it is sometimes confusing (don't use just CT). Furthermore, it is important to note in line 41 that for high-resolution micro-CT or micro-CT, the voxels have the same size in all dimensions (in contrast to medical CT)	Clear distinction has been made between medical-CT and high-resolution micro-CT in introduction.
Line 64-65: The voxel size was not constant for each sample? Did this affect the results of the ice quantification?	Thanks for this useful observation, the voxel size was not constant across samples because scanning parameters were adjusted to accommodate differences in core diameter and ensure that the entire sample was captured within the field of view.

Comment (R3)	Action taken/clarification
	While variations in voxel size can influence the resolution of fine-scale features, all scans were performed within a relatively narrow resolution range typical of medical CT. Therefore, the main sources of error identified in this study (e.g., partial-volume effects and density overlap) are primarily controlled by the intrinsic resolution limits of medical CT rather than small variations between scans. We have clarified this point in the revised manuscript.
Line 67-68: Did you visually check if the chosen HU ranges work well for your samples? Do they visually capture the phases of interest? How do you know that your chosen HU units are better or worse than the ones in Table 1? Otherwise, you can assume that it was just misclassified.	The HU ranges used for phase segmentation were adopted from published studies; however, the segmentation was not applied blindly. For each sample, the segmentation results were visually inspected to ensure that the selected HU ranges appropriately captured the spatial distribution of ice, sediment, and gas, and minor adjustments were made where necessary to maintain consistency in phase identification. Despite this, we acknowledge that due to the overlap in density between phases, particularly organic matter and ice, some degree of misclassification is unavoidable. This limitation is discussed in detail in the manuscript and represents one of the key sources of uncertainty in CT-derived ice content estimates.
Table 1: The variation seems so large that it suggests to me that medical CT is unable to quantify the ice content at all (and not just limited). How can the correct HU units be chosen? For me, this is a bigger issue than the misestimation of the ice phase. I think this should be addressed in more detail, and/or the discussion should be more critical (e.g., the final statement in line 140 should be stronger, including the title).	We agree that the large variability observed in Table 1 highlights a more fundamental limitation than simple misestimation. The results demonstrate that the choice of HU thresholds is not transferable and cannot be uniquely defined for permafrost samples with varying composition, particularly due to partial-volume effects and density overlap between ice, sediment, and organic matter. Rather than indicating that a "correct" HU range exists, our results show that different published thresholds can lead to drastically different estimates for the same sample, ranging from severe underestimation to substantial overestimation. This supports the conclusion that medical CT, under typical resolution conditions, cannot reliably quantify volumetric ice content using threshold-based approaches alone. Following this comment, we have strengthened the Discussion to more explicitly reflect this limitation and clarified that the issue lies in the non-uniqueness of HU-based segmentation, rather than in calibration alone.