RESPONSE TO REVIEWS (minor revision)

Responses are in blue

Editor

The three reviewers are very satisfied with the revision of your initial manuscript and thank you for this considerable effort, with a special mention for the improvement of figures 2 and 3.

Thank you

However, they do highlight certain points that I would ask you to correct or improve. I reproduce here the various points I ask you to address.

We have addressed each point in our response.

In particular, I share reviewer 3's concerns about the use of luminescence dates in the authors' reasoning, but these dates do not appear to be available in the publication, which is a serious problem that I sincerely ask you to tackle.

Those dates are now available in a completed ms thesis (https://doi.org/10.26076/fa75-82e1) that we cite in the revised ms. We did not see the reviewer's concern in the review available to us online.

Reviewer #1:

In their rebuttal, the authors write that they are now citing Larson et al. (2016) and Menzies and Reitner (2016), but it does not appear in the revised manuscript.

These are now cited. Our omission.

The authors were also keen to keep their interpretation of "slump" for Unit 3, although the three reviewers were questioning this interpretation. If one looks at the Glossary of Geology definition of "slump", one can read :"Slump: a landslide characterized by a shearing and rotary movement of a generally independent mass of rock or earth along a curved slip surface and about an axis parallel to the slope from which it descends, and by backward tilting of the mass with respect to that slope so that the slump surface often exhibits a reversed slope facing uphill." OR "The sliding-down of a mass of sediment shortly after its deposition on an underwater slope." The first definition

implies that you need to know the geometry of the movement, which is not the case here. The second definition implies the presence of a water body, such as a lake, but this seems very unlikely in this context, as the authors are describing subaerial processes (line 595). I guess the misunderstanding arises from the confusion between the process that is taking place and the sedimentary facies that is the result of that process. I suggest the authors should use a more generic term such as “mass wasting” that can form the sediment structure that is described in the paper.

We agree with the term mass movement and have made the change in the ms. Mass movement is more clear and less specific than the word “slump”

Also, the authors use “flowtill”, but its definition is “A superglacial till that is modified and transported by plastic mass flow.” I’m not sure this is the correct use of that term in this paper. Maybe the authors mean “a till affected by solifluction”?

We agree with the reviewer. Other datasets (some yet to be published) suggest strongly that the material in unit 3 is derived from unit 1. Because we cannot be sure that solifluction is the processed we have reworded from *flowtill* to *diamicton (likely till) transported by a mass movement.*

Minor comments:
In the very nice new figure 2, sample 1061-D5 seems to belong to both unit 1 and unit 2 in the a. part of the figure, but it looks like it only belongs to unit 2 in part b. of the figure. The text seems to infer that sample 1061-D5 belongs to unit 1 and unit 2. Maybe should you change the lower limit unit 2 upward in panel b. of figure 2?

This is correct. Thank you for catching. We revised the figure.

L363: invert the depths here and below, the lowest first and then the highest, i.e., 327-223 cm

Change made

L371: there are only 6 samples in unit 2.

We make the change to reflect the dual nature of the top sample so that it now reads: “The next *six and a half* samples (1061-D2 to *the lowest portion of* 1060-C4) comprise Unit 2”…

L530-532: “As depth is relevant in stratigraphy and interpretations of stratigraphic deposits, we elected to retain depth as a variable to include as much information as possible in the algorithm.” I’m not sure about this. If authors want to determine stratigraphic units, depth is an important variable, but if the authors want to identify different groups of sediments based on their properties, then including the depth indeed forces samples to be grouped in the same group if they are close to each other in the stratigraphy (spatial autocorrelation, as mentioned by a reviewer).

Thanks for this comment which indicates that we need to better explain the integration of depth in our clustering approach.

It is true that the integration of depth in our approach introduces a bias toward sample positioning in relation to each other. However, in stratigraphy, depth information is an explicit observation and typically integrated into clustering. This approach, sometimes called “depth-constrained cluster analysis” is used routinely in the context of well log zonation to improve grouping with an adjacency constraint (Gill et al., 1993; Yabe et al., 2022)

Within the context of parametric independence, we therefore choose to integrate stratigraphic interdependency as a deterministic factor. The central tenet of this approach is that relative depth is an inherent control on stratigraphic properties. In other words, it embraces the reality that, at the scale of the core, a given (artificially defined) sample is part of a stratigraphic continuum. Removing this parameter or rendering it independent would effectively negate the underlaying principles of stratigraphic continuity and superposition. By integrating depth as a parameter in the clustering we integrate the fact that samples are stratigraphically interconnected.

To that effect, we have now added a better justification to the use of depth dependency in the clustering method description so that the text reads (L324):

“By integrating depth as a factor in the clustering, we follow traditional clustering stratigraphic models acknowledging depth-dependency as a deterministic factor (Gill et al., 1993; Yabe et al., 2022)” The corresponding references are also added to the ms.

Reviewer 2:

Line 304: 4*Area_/ (π*major) -> Replace asterisk with multiplication symbol

Replace hyphens with en-dash for ranges throughout manuscript (eg Line 305-306: 0-1should be 0–1)

We are hesitant to make these changes prior to copy-edit but have no objection for these to be changed to journal style at copy edit.

Line 309: 1000 um2 à μ instead of u

Change made

Line 312: ImageJ, and a Tukey-Kramer honestly significant difference

Change made

Line 320: “performed a PCA analysis” is redundant (i.e. Principial Component Analysis analysis”)

Change made

Line 326 a k-means clustering à the article “a” is unnecessary

Change made

Line 327: capitalize “z score” since starting sentence

Change made

Describe scans as either Micro-Ct or μCT rather than switching between abbreviations. Is the distinction between CT and μCT (lines 360 to 370 for example) denoting an actual difference in resolution or is it referring to the same μCT scan?

CT scan in our usage is different than μCT. We have adopted μCT throughout.

Figure 8 caption line 1: K-means clustering à should be “clustering”
circularity z score=0.9”, “depth z score =1.2 à ensure correct spacing around equal signs eg Lines 546, 549, 551 etc

Change made

"subglacial" is standard phrasing in the literature rather than "sub-glacial" (note that both are used in line 646)

We have standardized to subglacial.

Review 3

Reviewer 3 has some concern about dates used but not suitable for the publication.

We as authors do not see that concern in material made available to us but those dates are now available in a completed ms thesis that we cite in the revised ms.