General comment

The study addresses an important and timely problem: how sediment and debris are produced, transported, stored, and re-entrained within glacierised catchments. The integration of luminescence rock-surface dating, cosmogenic nuclide measurements, and glacier-flow/sediment-transport modelling is a strong aspect of the manuscript. In particular, the attempt to link headwall erosion, englacial particle transport, moraine storage, and downstream sediment export using a process-based model is valuable. The use of a general-order kinetic model for luminescence-depth profiles is also appreciated, as this is a meaningful step beyond simple first-order treatments of rock-surface luminescence bleaching. Overall, the study has the potential to provide an important contribution to understanding sediment residence times and transport pathways in alpine glacier systems.

However, I have several concerns regarding the exposure-age and erosion-rate estimation component of the study. These points should be addressed before the numerical results are used for broader interpretation.

Sincerely,

Arbaz Pathan

Specific comments

1. Topographic shielding values in Table 1 require clarification and rechecking

In Table 1, samples MDG-HF-01, MDG-HF-02, and MDG-HF-03 have very similar coordinates and the same altitude of 3219 m a.s.l. However, their shielding corrections differ substantially: 0.773169, 0.722932, and 0.611187, respectively. Since these samples appear to have been collected very close to each other, such a large difference in shielding correction is unexpected unless the correction includes strongly varying local surface geometry, strike–dip effects, or local skyline obstruction.

I request the authors to clarify exactly how the shielding corrections were calculated. If the shielding factors include local rock-surface orientation, this should be explicitly stated. If they represent only topographic shielding, the values should be rechecked. Cross-verification using an independent tool, such as an ArcGIS-based topographic shielding calculation or equivalent skyline-based method, would strengthen confidence in the reported values.

For MDG-HF-04, the higher shielding correction may be reasonable because the sample is located approximately 200 m higher than the other three samples. However, the large variability among MDG-HF-01, MDG-HF-02, and MDG-HF-03 requires further explanation.

2. Typographical correction in Table 1

In Table 1, the signal name for one row of MDG-HF-01 appears as “RSL50”. This should likely be corrected to “IRSL50”.

3. The SP term should be corrected to $\overline{\sigma\varphi_0}$ throughout the manuscript and supplementary dataset. $\sigma\varphi_0$ or SP is misleading.

4. Figure 2: Concern regarding calibration of $(\overline{\sigma\varphi_0}), (\mu), and (b)$ using only one known-age calibration duration.

The general-order kinetic model involves three coupled parameters: $(\overline{\sigma\varphi_0}), (\mu), and (b)$. These parameters are not independent; they can co-vary strongly during fitting. In the present study, the calibration appears to rely on approximately one-year known-age samples collected from the same sampling locations. Even if multiple cores or profiles are measured, they represent the same exposure duration. Therefore, it is not clear whether one known-age calibration duration can uniquely constrain all three parameters.

In a three-parameter calibration, the authors should demonstrate that the solution is not non-unique or biased by parameter covariance. Ideally, independent calibration samples with at least two different known exposure durations should be used as shown in Pathan et al. (2024) and Freiesleben et al. (2023) (*Novel luminescence kinetic models for rock surface exposure datingm, Freiesleben, T.H., Thomsen, K.J. & Jain, M., Radiation Measurements, 2023*). If this is not possible, the authors should provide additional diagnostic plots showing the covariance and solution space among the fitted parameters.

Specifically, I request the authors to show parameter-covariance or misfit-surface plots for:

1. $(\overline{\sigma\varphi_0}) versus (\mu)$
2. $(\overline{\sigma\varphi_0}) versus (b)$
3. $(\mu) versus (b)$

These should preferably be shown in log scale where appropriate, especially for $(\overline{\sigma\varphi_0})$. If the solution is well constrained and not biased along parameter-covariance valleys, then the derived parameters will be more convincing.

5. Table S4Large relative uncertainty in fitted kinetic parameters

The uncertainty reported for the fitted kinetic parameters is large, particularly for IRSL50. For example, Table S4 reports $(\overline{\sigma\varphi_0})$for IRSL50 as $(8.464 \times 10^{-4} \pm 1.214 \times 10^{-3}\ \mathrm{s}^{-1})$, where the uncertainty is larger than the mean value. This suggests that the parameter is poorly constrained. Similar concerns apply, although to different degrees, to the other signals.

This large uncertainty may directly affect the calculated luminescence exposure ages and erosion-rate inversions. The authors should discuss how parameter uncertainty propagates into the final RSED ages and erosion rates. At present, it is not fully clear whether the final erosion-rate estimates are dominated by the measured luminescence-depth profiles or by poorly constrained kinetic parameters.

6. Calibration and fitting results should be presented in a way that highlights uncertainty

The calibration results should be presented in a form that allows readers to assess the quality of the solution. In addition to the fitted depth profiles, I suggest that the authors show the parameter distributions or likelihood/misfit surfaces, preferably using log scaling for $(\overline{\sigma\varphi_0})$ (refer Figure 2 of Pathan et al. (2026)). This would make the uncertainty and covariance structure more transparent.

The profiles may visually appear well fitted, but a visually good fit does not necessarily imply that $(\overline{\sigma\varphi_0}), (\mu), and (b)$ are uniquely constrained. This is especially important because the fitted parameters are subsequently used for erosion-rate estimation.

7. Normalisation and saturation-plateau definition require further justification

Some luminescence-depth profiles appear shallow and may not contain a sufficient number of points in the saturated plateau region. For example, profiles such as MDG-HF-01 IRSL50 appear to have limited plateau representation. This is problematic because profile normalisation depends strongly on the accurate definition of the saturation level. If the plateau is poorly constrained, the normalised profile shape and therefore the fitted exposure age can become unstable.

I request the authors to clarify how the plateau/saturation level was defined for each profile. If manual or subjective plateau selection was used, this should be stated. A more objective plateau-detection and normalisation method would improve reproducibility. The authors may also consider re-evaluating the profiles using an objective normalisation workflow, such as the approach implemented in CoRSEER (Pathan et al. (2026).

8. Table 1: Consistency between ^{10}Be apparent ages and luminescence apparent ages

The ^{10}Be apparent ages for MDG-HF-01, MDG-HF-02, and MDG-HF-03 are relatively young, approximately 80 ± 10 a, 70 ± 10 a, and 50 ± 20 a. However, the luminescence apparent ages show large variability between cores and signals. For example, MDG-HF-01 IRSL50 gives 1.66 ± 0.13 a for one core and 191.10 ± 26.76 a for another. Similarly, MDG-HF-04 shows a large contrast between IRSL50 cores.

Some divergence between ^{10}Be and luminescence ages is expected because the two methods have different depth sensitivities and respond differently to erosion and exposure history. However, the magnitude of intra-sample variability should be more clearly explained. The authors should discuss whether this variability reflects true surface erosion, local microtopographic effects, incomplete normalisation, parameter uncertainty, or signal-specific behaviour.

9. Re-analysis using objective modelling workflow could strengthen the study

The exposure and erosion component of the study is based on a valuable dataset. The profiles appear potentially informative, and the low analytical uncertainties in several measurements suggest that the dataset could yield robust results if the mathematical treatment is fully constrained. I suggest that the authors consider testing the RSED component using an independent or objective modelling framework, such as CoRSEER, to evaluate whether the derived ages and erosion rates are stable under different normalisation choices, kinetic-parameter bounds, and parameter-covariance assumptions.

This would be especially useful for testing whether the IRSL50 profiles can yield exposure estimates that better agree with the ^{10}Be constraints, unless substantial recent erosion has genuinely modified the luminescence-depth profiles.

References:

Pathan, A., Biswas, R.H., Kumar, D., CoRSEER-Calculator of Rock Surface Exposure Age and Erosion Rates: The MATLAB App for Rock Surface Luminescence Dating.

Freiesleben, T.H., Thomsen, K.J., Jain, M., 2023. Novel luminescence kinetic models for rock surface exposure dating. Radiation Measurements 160, 106877.