General comments

It is good to see development of this novel technique is continuing with the addition of the $IRSL_{50}$ and post-IR $IRSL_{225}$ signals from feldspar to the OSL_{125} signal from quartz. It is encouraging that the three signals combined show similar trends.

The use of local calibration sites to address known variations in the optical electron detrapping parameters due to geographical and mineralogical variations is a good solution to a challenging problem.

Testing the effect of different sample aspect is a very valuable contribution. As is the comparison of the results to the world-wide compilation of glacial and nonglacial erosion rates which supports the idea that nonglacial erosion rates are not significantly different to glacial erosion rates.

Clarification is needed on some aspects of the manuscript.

We thank the reviewer for his positive feedback and for recognising the potential of our findings. We have addressed his comments, and this is detailed in the following section.

Specific comments

The anti-correlation between erosion rate and elevation is intriguing and much less strong than in the Mont Blanc study. Please provide a reasoned explanation for the difference.

While a definitive answer for this remains speculative, we agree that it is an interesting observation. Therefore, we have provided three possible explanations for this difference: (1) lithology, (2) elevation and (3) a potential relationship between exposure time and erosion rate (lines 479-490).

Although the ^{10}Be data for the lower three samples are compromised by inheritance, could the authors deliberate on the inverted erosion rates for GG02 and GG03. The ^{10}Be derived steady state erosion rates for these two samples are about $4.8E^{-2}$ mm a^{-1} and $5.8E^{-2}$ mm a^{-1}. These are roughly half of the rates predicted by the inversion method. The ^{10}Be derived steady-state erosion rates are directly related to the measured ^{10}Be concentration in the samples and represent maximum steady-state erosion rates.

The higher erosion rates calculated using the inversion method used in this study are not compatible with the measured ^{10}Be concentrations. It is not possible to get the measured ^{10}Be concentrations with the calculated erosion rates. It is important that the authors state very clearly if the ^{10}Be data was in fact used in the inversion, or did they derive the erosion rates simply from Eq. 1, which does not incorporate the ^{10}Be data.

If the ^{10}Be data was used, please explain how the erosion rates from the inversion method are reconciled with the measured ^{10}Be concentrations. What was the exposure/erosion history of GG02 and GG03, especially given Figure 3b suggests that the inversion modelled erosion rate is invariant for erosion onset times ts >102 a. Is it the case that the OSL signal only records the

last few hundred years at the inversion method erosion rate, and prior to that time the samples were eroding at half the rate to accumulate the measured ^{10}Be concentrations? If that is the explanation, what caused the acceleration in the erosion rate?

We apologise that our methods sections were unclear. While we are not entirely sure where the values stated above for ^{10}Be derived steady state erosion rates have come from, we assume they are from the standard ^{10}Be erosion rate calculation method. This integrates over the entire exposure history of a sample, whereas here the inversion method only applies an erosion rate following an erosion onset time (which is not equal to the exposure time). Since the ^{10}Be steady state erosion calculation is integrated over longer periods, this explains the lower erosion rates produced compared to the inversion erosion rates here which integrates over a shorter period.

If the ^{10}Be data was not used, explain why, and revise the title of the paper to reflect that ^{10}Be data was not used to quantify the post-glacier erosion rates discussed in the manuscript.

It was used and we hope we have clarified this with our explanation above.

Specific comments by line number:

104 "…since TCN are formed ~50-60 cm (Lal, 1991) below the rock surface…" is incorrect. TCN are formed at the surface and down to several metres. The ~50-60 cm is the e-folding depth for common rock densities.

Thank you for bringing this to our attention. We have amended the sentence accordingly.

117 "…due its…" should be 'due to its'

Thank you for your detailed reading of the manuscript. The typo has been corrected.

303 Table S1 does not show summary for each sample. It shows data for Sample 5 (which I assume is GG05). Table S1 is not referred to in the main text. It is referred to in the Supplement. Table S1 in the main text should be Table S2, or change the labels in the Supp.

It was indeed an oversight on our part to have this mismatch between the table numbers and the main text. We have now changed the labels in the Supplementary, and furthermore changed "sample 5" to "GG05" for consistency purposes.

306 1.13 x 10^{-6} is 1.8E^{-6} in Table S2. Check the data.

Thank you for pointing this out, it has been corrected.

307 7.34 x 10^{-7} is 7.3E^{-6} in Table S2. Check the data.

This has now been done.

337 1.12 x 10^{-2} is 7.22E^{-2} in Table 4.

Unfortunately, we do not follow the reviewer's point here.

338 Add reference to Table 4 so the sentence ends… 0.16 mm a^{-1} (Table 4).

We have now done this.

484 "…local differences…" This is vague. Please elaborate.

Thank you for your suggestion. As mentioned in the "Specific comments" above, following the reviewer's comments, we have added a few sentences to elaborate that this observation could be a reflection of local differences such as lithology and/or elevation influencing the erosion mechanisms, or also due to the presence of a potential relationship between erosion rate and exposure time.