Comment on egusphere-2022-43
Derek Fabel (Referee)

Referee comment on "Quantification of post-glacier bedrock surface erosion in the European Alps using $^{10}$Be and optically stimulated luminescence exposure dating" by Joanne Elkadi et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-43-RC1, 2022

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.

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.

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 $t_0 > 10^2$ 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 10Be concentrations? If that is the explanation, what caused the acceleration in the erosion rate?

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.

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.

117 “…due its…” should be ‘due to its’
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.

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

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

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

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

“...local differences...” This is vague. Please elaborate.