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## **Comment on egusphere-2022-43**

Pierre Valla (Referee)

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Referee comment on "Quantification of post-glacier bedrock surface erosion in the European Alps using  $^{10}\text{Be}$  and optically stimulated luminescence exposure dating" by Joanne Elkadi et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-43-RC2>, 2022

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Dear Authors, dear Editors,

Please find below my evaluation concerning the manuscript by Elkadi and co-authors entitled "Quantification of post-glacier erosion in the European Alps using  $^{10}\text{Be}$  and OSL exposure dating" (manuscript egusphere-2022-43).

This manuscript investigates bedrock surface erosion in an alpine environment, focusing on post-glacial surfaces and combining OSL and in situ  $^{10}\text{Be}$  surface exposure dating. The authors targeted different samples along a formerly-glaciated topographic profile, and used a multi-signal OSL investigation to constrain bedrock erosion rates and durations. Their results show variable erosion rates between signals and samples, with an elevation relationship that they relate to periglacial erosion mechanisms (e.g. frost-cracking). They compare their results to recent study in a similar environment, and finally propose a compilation of "non-glacial" vs. glacial surface erosion rates (bedrock and boulder) from the literature that they discuss in terms of rates variability and magnitude.

This is an interesting manuscript, well-written and referenced. It follows the recent developments of OSL surface exposure dating and the original approach combining OSL with  $^{10}\text{Be}$  data to retrieve local erosion histories. In the present study, the authors used a multi-signal approach for OSL exposure dating, which is a very good illustration of the potential of luminescence techniques for quantifying exposure/erosion histories of bedrock or boulder rock surfaces. They also investigated the usefulness of artificial calibration surfaces, with short exposure time (1 year), to efficiently constrain bleaching parameters, as well as the influence of surface orientation on these parameters. Finally, they placed their results within a large-scale compilation of surface erosion rates from the literature, discussing the relative overlap between non-glacial and glacial surface erosion rates. I thus think that the present manuscript would be a very interesting contribution for Earth Surface Dynamics, bringing new evidence and quantification of bedrock surface erosion rates in post-glacier settings, and nicely complementing recent studies in this topic while raising fruitful discussion in the geomorphology community.

I have outlined below my questions and suggestions in a set of general and specific comments below. Most of my suggestions are concerning the presentation of information related to the OSL exposure dating and calibration/multi-signal investigation. In addition, I would think that more discussion about the actual erosion/weathering processes (physical mechanisms) would help the readers to better appreciate the discussion about compiled glacial/non-glacial erosion rates.

### **General comments:**

1 – In the present study, the authors refer to “post-glacier erosion” for the surface erosion rates they aim to quantify. I agree with the used term, although this is maybe too vague and can be specified already at the beginning of the manuscript. This should go along with a description of the sampled landscape/morphological features and a clear statement of the adopted strategy: why targeting formerly-glaciated bedrock surfaces and not random surfaces in the catchment? how comparable would be then the output surface erosion rates between GG01 and other samples, given that GG01 has never been glaciated? What are the exact erosion mechanisms investigated there? Wind erosion, surface chemical weathering, frost cracking, a mixture of all?

This is not an easy question, but I feel that the readers will better appreciate the approach and outcomes if these are better clarified in the manuscript.

2 – OSL surface exposure dating. The multi-signal approach is really interesting and promising, however the comparison between signals could be extended and complemented in my opinion. First of all, this is not entirely clear to me why bleaching parameters would be similar between different signals, as we know from literature that bleaching of IR signals are more difficult/slower than OSL signal. I would encourage the authors to provide more discussion about this interesting result. Second, the output erosion rates differ between signals, can these be indicative of the uncertainty in erosion quantification? In their output results (Table 4), the authors provide estimated erosion rates but these are not associated to any uncertainty. Would it be possible to estimate some uncertainties from the likelihood results?

More importantly, how can we explain that some bleaching profiles are in steady state for a given signal and in transient state for another signal, within the same sample/core? This is really intriguing but would need I think more discussion.

3 – Compilation of non-glacial/glacial surface erosion rates. This is a nice compilation and this questions the relative idea of efficient subglacial processes in shaping mountainous landscapes. However, I think several clarifications/information are missing to fully appreciate this compilation. First, this is unclear to me what are “non-glacial” erosion rates, since I have the impression that fluvial or landslide rates have not been included. So this is more a comparison between periglacial/hillslope erosion vs. glacial erosion, for the later the geomorphic agent being easily identified (subglacial ice or water). Second, I

think that for surface erosion rates the setting/environment is also very important, i.e. one would expect different erosion rates for a bedrock/surface exposed since long time to atmospheric agents than a recently deglaciated surface, no?

Finally, I guess that the measurement time could be also important in the output erosion rate; have the authors tried to confront the compiled erosion rate to the measurement period?

### **Specific comments, by line number:**

- Line 1. "post-glacier erosion...". Maybe precise in title that this study investigates "bedrock surface" erosion, and is thus focusing rather on local/small-scale erosion and not large-scale landscape evolution (e.g. fluvial erosion...).

- Line 14. "glacial and non-glacial". Please be more specific there, what is considered as "non-glacial" in the present study. Are these post-glacial evolution of glacial surfaces (by atmospheric erosion/weathering), periglacial processes or more generally fluvial/hillslope erosion? See also my general comment about this.

- Line 19. "in Zermatt, Switzerland". Maybe precise that this is located in the (central) European Alps.

- Line 24. "...could be equally important." I would suggest to add a sentence there for the potential implications of such result, this appears not entirely clear as presently phrased.

- Line 37. "...global feedback loop that exists...". Some references there would be needed to introduce this feedback loop.

- Lines 42-43. "In contrast, studies exploring erosion during interglacial times have mainly investigated at catchment-wide erosion rates". I don't entirely agree with this statement, some studies have also investigated more local fluvial erosion (gorge incision etc., e.g. for the European Alps Korup and Schlunegger, 2007; Rolland et al., 2017; van den Berg et al., 2012; Valla et al., 2010) or the spatial distribution within a catchment (e.g. Fox et al., 2015 for the Alps). Maybe rephrase or add more information there.

- Line 42. "glacial erosion, bedrock surface erosion and rockfall". See my general comment about this, all terms refer to "bedrock surface erosion" but physical processes and scales differ. Please check and rephrase.

- Line 49. Again there, what is "post-glacier erosion". Hillslope, fluvial, or atmospheric weathering? This needs specification for your study.

- Line 49. "six samples". Please precise what kind of samples (i assume glacially-polished bedrock or glacial morphologies like roches moutonnées no?). This is important to understand what processes are targeted.

- Line 75. " $2 \times 10^{-1}$ ".

- Line 83. "post-glacier erosion rates...". There is a good reason why targeting formerly-glaciated bedrock surfaces in the present study, but this is not really explicit in the introduction. Please consider adding one or two sentences on the adopted strategy and why targeting post-glacier surfaces rather than other bedrock surfaces randomly in the landscape.

- Line 88. "converted into an exposure age". Add "apparent" there.

- Line 95. "surface traps". Unclear whether these relates to traps at the rock surface or energetically for luminescence. Please rephrase. Also, maybe already precise the depth range at which the sun's energy is sufficient to reset the OSL signal (lines 96-99).

- Line 101. Maybe add "apparent" there too for exposure age.

- Line 105. Maybe also include the recent work of Sellwood et al. 2019 and/or Sellwood and Jain 2022.

- Line 106. "influenced by exposure". Sunlight exposure? Exposure time? Please specify.

- Line 119. "in the local area". Not clear, please rephrase.

- Line 121. "six sampling sites down a vertical transect". Same comment as line 49. The reader is missing a morphological/geomorphological description of the targeted bedrock surfaces (glacially-polished or not, glacial or periglacial features, etc.) and explanations for the adopted sampling strategy. This is really difficult to have a good understanding based

on small insets in figure 1. Also, this is important I think to present the surface slopes for the different samples, etc.

- Line 123. "aside from the highest sample". So this is important to explain that this sample is not reflecting "post-glacier" erosion, but periglacial erosion as this was never ice-covered or at least no during the LGM). Also, then what is the bedrock surface morphology for this sample (see my previous comment)?

- Line 136, Figure 1. This is a nice figure, but not totally informative for the setting area. Is it possible to add the LGM ice contours on panel b? and to replicate the ice lines on panel c for clarity (for instance I cannot really tell if the three bottom samples have been lastly exposed in 1973 or 2009 based on panel b)? Pictures as inset in panel c are really small, and scale is missing? What is the source(s) of the photos showed in panel b and c? Another question, what is above the sample GG02, it looks like a small plateau or morainic ridge (Younger Dryas?)? Maybe consider adding also a topographic profile for the transect, on which you can locate the samples and ice thickness/extent from LGM to present-day.

One suggestion would be to add another figure (supp or main text) to show the sampled morphologies and potentially the different lithologies (rock-slice pictures?).

- Line 136. "Sample preparation". Please specify where sample preparation and chemical Be extraction have been performed.

- Line 165. There is a ) to be removed for the blank value.

- Line 189. "with a DASH head". I would suggest to describe the different filters listed in Table 2 for non-specialists.

- Lines 192-194. Are the different criteria arbitrary or common for rock-slice luminescence? Maybe refer to technical paper to support these, e.g. Elkadi et al., 2021?

- Line 205. This is unclear and not explained in the main text how equation 1 is treated with respect to the recombination distance  $r'$ . For non-specialist readers this will appear relatively obscure, given that athermal detrapping parameters are not presented for these measurements/samples. This is also similar for the dose rate parameters ( $D_0$  and  $D_{dot}$ ), no information about their values (and how  $D_0$  is obtained) is provided, only description in Table 3.

- Line 220. "Previous calibration sources". Unclear, please rephrase.
- Line 224. "unknown parameter values". Please specify this parameter for clarity ( $\sigma\phi$ ?).
- Line 225. "the influence of the surface orientation". Additionally, I think discussion about the outcome results would be interesting for readers if reported in main text, not in supp (at least briefly).
- Line 239. "... using the random parameter values and Equation 1".
- Lines 250-251. " $1.25 \times 10^8$  trials". For each individual sample or in total?

Also, I don't fully understand how the ranges for the inverted parameters have been defined, especially for exposure time  $t$  only between 1 and 200 years but setting information suggest much longer exposure times for high-elevation samples no?

Please clarify on which basis/information the parameter ranges have been defined.

- Lines 258-259. "...simple, step wise erosion history where, at a specific time in the past, the surface goes from experiencing no erosion to an instantaneous onset of fixed rate of erosion". I am wondering whether this is possible to also have a simpler scenario where you estimate erosion rate since the exposure of the bedrock surface (i.e.  $t_s = t$  from  $^{10}\text{Be}$  data). Have you tested this and if yes is there any compatible scenario(s) with OSL/ $^{10}\text{Be}$  data?

- Line 259. For the inversion of erosion history, what are the bleaching ( $\sigma\phi$  and  $\mu$ ) parameters and exposure times used? Best-fitting values for bleaching parameters (Table S1)? Please clarify.

- Line 265. Several questions for Table 1:

Can you add more information for surface orientation? Two values are given, but no unit nor details.

Please also provide  $^{10}\text{Be}/^{9}\text{Be}$  ratios in the table, so that  $^{10}\text{Be}$  concentrations can be recalculated in the future.

Are the uncertainties reported for exposure ages internal or external?

- Line 274. "3. Results and interpretation". The presented results are already quite interpreted in this section, so I would suggest to rephrase the section label.

- Line 276. "apparent exposure ages". I would also suggest to add a figure with  $^{10}\text{Be}$  apparent exposure ages and topography for illustration.

- Line 277. "The highest elevation sample (GG01) is younger than suggested from ice thickness reconstructions (Bini et al., 2009)". If this sample has been collected above the LGM ice surface, then it reflects periglacial exposure and its apparent exposure age is not related to LGM glaciation, see for instance results in Gallach et al. 2018; 2020. Please consider rephrasing or clarifying this sentence.

- Line 279. This is a very interesting result as you can reconstruct the YD ice thickness from your  $^{10}\text{Be}$  apparent exposure ages, which may be linked to this small plateau/surface just above sample GG02. Please consider expanding this result, this is relatively similar outcomes compared to Lehmann et al. 2020.

- Line 288. Maybe also consider citing the work of Goehring et al. 2011 on the Rhone glacier.

- Lines 297-299. This sentence may be moved to methods.

- Line 303. "results for each sample summarised in Table S1". I would strongly encourage the authors to present results as figures (like figure 2) for all samples, either in main text or in supplementary. This would be important for the readers to evaluate the noise in data and reproducibility between cores for each sample (old and calibration, and also for different orientations).

- Line 308. Is it possible to present there quickly the results about different orientations? I guess this would be interesting for some readers to have such information, not all in supplementary.

- Line 313. "...mineralogical variations". Is there a link between  $\mu$  values and lithology? Can the authors provide some pictures of the rock slices, especially for GG02 which seems different from others?

- Line 315, Figure 2. I would suggest to have at least one figure showing the bleaching profiles of the different signals, at present only IRSL50 signals are shown. Is it possible to provide such information?

On figure 2, inversion outcomes for  $t$ , the OSL apparent exposure time, is shown. However, this outcome is not presented in Table S2, nor discussed in the main text. I think this is important to show this, and to clearly present the differences in apparent exposure ages between OSL and  $^{10}\text{Be}$  data for all samples.

- Line 318. "inversion outcomes for  $e$  and  $t_s$ ". Please provide the range for these parameters.

- Line 320. "exposure age information from the historical maps and photos were employed". Where can the reader access the used exposure ages for these samples? Please specify in main text what exposure durations you used.

- Line 323. "transient state". This is not totally clear what is transient state from looking at figure 3d, please clarify for non-specialists that there is a wide range of  $e/t_s$  combinations, reflecting non-steady bleaching profile, or something similar.

- Line 324, Figure 3. Please indicate the used exposure time for model without erosion in panels a and c. Concerning panel d, since total exposure time used is historical data (so few tens of years), I don't understand how  $t_s$  range can be explored between 0.1 and 10000 years with an output likelihood. If I understand well,  $t_s \leq$  exposure time, so there should be a large white (non-possible) area in panel d no?

Please justify the adopted approach, this is not really clear at present.

What is the red line on panels a and c (model with erosion)? The best-fitting parameter combination (maybe indicate with a star in b and d panels) or the region of high likelihood? Please clarify (same question for figure 4).

- Line 338-339. "When looking at the signals individually, the OSL125 and post-IR IRSL225 results reveal an anti-correlation between post-glacier erosion rates and

elevation, whereas no trend is observed in the IRSL50 data (Fig. 5)". On Figure 5a one cannot differentiate the different signals (same symbols), can the authors change the symbols so that the reader can evaluate the differences?

- Line 341. "Based on this, an average of the three signals was calculated for each site to generate one post-glacier erosion rate value".

I think this would be first interesting to discuss the different  $e/t_s$  results between signals, before going to an average calculation. Is there some variability between signals in the output surface erosion rates? Why some signals appear in steady-state while other appear in transient state? I would think this is important for readers to have such information.

In addition, would it be possible to estimate some uncertainties (standard deviation? from likelihood?) and to show these on figure 5 for individual/averaged erosion rates?

- Line 346. "minimum  $t_s$ ". There is no presentation of these outcomes in the section, I would suggest to provide more details about these and to confront them to total exposure time. For low-elevation samples,  $t_s$  is close to exposure time, whereas it is really different (much lower) for high-elevation samples. I think this is important for the exposed results on lines 347-351, otherwise the readers could think longer exposure time = more eroded material...

- Lines 368-370. It reads a bit strange to have the presentation of the slope relationship there (discussion), and not in the previous section along with the elevation relationship. Please consider presenting these in results too.

- Line 375. "local variations influencing the dominant post-glacier erosional mechanisms". Really vague, please specify what are those variations and mechanisms.

Alternatively, have the authors thought about potential correlation between erosion rate and exposure time? For Lehmann et al. (2020), the exposure times vary between  $\sim 20$  ka and few years, while there the difference in exposure times is much lower. I agree that GG01 is not following this potential relationship, with a young exposure age, but given the different morphology/settings (cliff with periglacial erosion over 10s of ka), this may explain the low erosion rate.

- Lines 379-390. I agree that this is worth noting low bedrock surface erosion rates for such high-elevation environments, but these low erosion rates may also be the result of the sampling strategy, no? The sampling targets are specifically glacially-formed surfaces that are more or less preserved in the landscape, so they do reflect low surface erosion. I

think that some further clarification could be given there.

- Line 403. "...bedrock surface erosion rates from surfaces in glaciated environments, not currently subjected to glacial erosion,..." Reads a bit odd, please rephrase.

- Lines 411-415. Are the referenced studies targeting bedrock/boulder surfaces that have been previously glaciated or not? Maybe this is important to specify. Same question for line 421 (" In Europe, Andrée (2022b)...").

- Line 415. "bedrock erosion rates". I thought Sohbaty et al. (2018) only targeted boulders, please check.

- Line 427. "these orders of magnitude are comparable with estimations of sub-glacial erosion rates and a summary of glacial and non-glacial erosion rates worldwide is displayed in Fig. 6". Have the authors tried to perform a pdf of the glacial and non-glacial erosion estimates. From visual inspection, I have the impression that glacial erosion rates, although they do overlap with non-glacial ones, are higher (and the presented scale is a log one!).

I appreciate this comparison and think that the compilation is interesting to discuss, however, I have a doubt about the actual comparison: "non-glacial rates" are apparently referring to "atmospheric" erosion/weathering and fluvial or landslide/hillslope erosion rates are not included right?

Then, what is really compared between these rates and glacial rates which do involve geomorphic agent as subglacial water/ice? I think this is important to clarify this point and justify why fluvial or landslide erosion rates (which are non-glacial agents) are not considered.

- Line 444. "The large range is due to differences in sample locations...". How about differences in lithology (e.g. carbonate vs. crystalline bedrocks)?

- Line 462. "A full compilation of glacier erosion rates, calculations and methods can be found in Herman et al. (2021)". Maybe the authors can provide there the range in compiled glacial erosion rates?

- Line 484. "the dominant post-glacier erosion mechanisms". Please specify.

I hope these comments and suggestions may be useful for revising the manuscript, and I look forward to seeing it published.

Sincerely,

Pierre Valla

Grenoble, 2 May 2022