Summary

This paper uses innovative ^{10}Be-OSL measurements to derive erosion rates in the Swiss Alps during the Late Glacial and Holocene. It targets a vertical transect to assess the influence of elevation on erosion rates in this setting and shows that a negative correlation exists i.e. as the elevation decreases, the erosion rate increases. This is new and useful information as little is known about the factors that control erosion rates, especially in interglacial times. The erosion rates derived are similar in magnitude to existing studies, which gives confidence in the robustness of this new technique. Finally, the authors apply their data to address the long-standing uncertainties in our understanding of glacial vs non-glacial/interglacial erosion rates. Interestingly, their data suggests that interglacial erosion rates can be equally as important as glacial erosion rates in deglaciated environments, which is a key finding because this has important implications for understanding the drivers of rock erosion rates (e.g. climate) and thus, future rock erosion with anthropogenic climate change.

Overall, this is an excellent study, applying new techniques to a long-standing, challenging research question. The methods applied are robust, well justified and well performed. The study is generally well contextualised within the literature, but the understanding of the factors driving erosion rates could be better explained in the text in places (see specific comments) so it is easier for the reader to follow the authors interpretations. It was a very interesting read and I have some comments and questions below. It will be an excellent contribution to the literature in this area and I hope the comments are constructive.

We thank the reviewer for her positive feedback, helpful comments and appreciate her recognition of the relevance of our work. Her comments are addressed in detail in the sections below.

General comments:

1. *From my understanding, this is the first study to determine rock surface erosion rates using this technique using both K-feldspar and quartz, which is very important and interesting. The authors may wish emphasise this more in the intro/rationale/abstract, but I leave it to their discretion.*

   We thank the reviewer for her interest in our results, however since we don't have pure quartz/feldspar signals, we prefer not to expand on the matter as it would mostly be speculative.

2. *One of the advantages of the Lehmann et al. (2018) approach is that transient erosion rates can be derived, in addition to steady-state erosion rates. Given that this paper is focussed on interpreting the character and drivers of erosion, I would expect the authors to have more thoughts and interpretation of those samples that determine transient erosion, rather than just dismissing them as is stated in Line 324. For example, do these samples derive transient erosion rates because the technique/analysis is not reliable? Do these samples have different surficial characteristics than other samples? Is there any evidence of transient erosion for these samples (e.g. frost shattering) that is not present for the other samples? What natural*

processes could have caused transient erosion in this setting? What even is transient erosion? This could be its own discussion point in the discussion before the steady-state erosion is discussed (Sections 4.1, 4.2).

Thank you for the suggestion, we have added a section (4.1) that attempts to explain the steady state vs transient results. Unfortunately, we were unable to confidently identify the specific cause of all three signals being in a transient state with erosion for GG04. For the moment we have stated it is due to a "localised stochastic process" as any more detail would be purely speculative, although Fig. S1 suggests possible evidence of surface spallation.

However, for samples which only have either their OSL_{125} or post-IR $IRSL_{225}$ signals in a transient state, we believe this could be due to the fact that they are more difficult to bleach, and as their bleaching profiles are thus necessarily closer to the surface, are therefore more susceptible to erosion and transient erosion states. This does not explain the transient $IRSL_{50}$ for sample GG03 which is interesting and requires further investigation that is beyond the scope of the present work.

3. *One of the main findings from this study is that "at present glacial erosion is assumed to have a greater influence on landscapes, yet a global compilation of both glacial and non-glacial erosion rates in deglaciated environments shows that erosion rates during interglacial times could be equally important" (Abstract, Lines 21-24). This is very interesting and is reflected in the data presented in this study. However, the discussion lacks discussion about glacial vs non-glacial or interglacial erosion rates. It could further unpack what natural processes differ between glacial and interglacial conditions that may or may not modulate the rock surface erosion (e.g. climate). Kirkbride and Bell (2010) do this well in the discussion of their study with respect to changing temperature and precipitation in glacial vs interglacial periods. Perhaps the discussion here could provide more insight into this as it is largely unknown due to the difficulty in determining glacial and interglacial erosion rates (i.e. deriving erosion rates on such resolution). The new data presented in this study on timeframes that were previously difficult to measure erosion rates on, therefore offers a great opportunity to explore these themes.*

Reading over the manuscript, we came to the realisation that our use of the word "glacial" was misleading and that it was unclear what exactly we were referring to. In fact, what we meant was subglacial erosion and we have now amended the wording to reflect this. Since we are looking at subglacial rates, which are influenced by the presence of ice, this makes it difficult to extrapolate to interpretations on the sub-aerial processes that differ between glacial and interglacial conditions. While it is a very interesting idea, to do so in this paper is beyond the scope of our research but we hope to see it included one day in a future study.

Specific comments:

Please could the authors explain what they are referring to when they use the term "non-glacial erosion". Is it referring to the interglacial period (i.e. it has a time dimension) or a deglaciated setting (i.e. it has a space dimension)? It is a minor comment but it would help to *clarify this in the introduction before the reader continues on through the paper, perhaps around Line 34 where it is first mentioned.*

> This is a good point. In this case, we are referring to any erosion in a glacial environment that is not related to glacial erosion (so a space dimension). Following on from this comment, we have added the sentence: "Here, non-glacial erosion refers broadly to any erosion occurring in a glacial environment that is not related to glacial erosion" (lines 38-39) for clarification purposes.

Line 42 – here you refer to erosion studies during interglacial times and state that they are mainly limited to catchment-wide erosion rates but you could add 1-2 sentences to highlight that there are a few papers that have quantified interglacial erosion rates (e.g. Kirkbride and Bell, 2010; Sohbati et al. 2018; Lehmann et al. 2019; Smedley et al. 2021), which you will later expand upon in Section 1.1.

> Thank you for your recommendation, we have done this.

Line 51-54 – it is useful to set up the aim of the study here, but I find it a little confusing that you report the main findings before presenting the data. Perhaps this is a feature of the journal and if so, that is fine as it is. If not, you might want to consider waiting to report the findings later in the paper.

> We understand why it might be confusing to report the main findings at this stage in the paper, however we prefer to keep the introduction as is.

Line 81 - Smedley et al. (2021) also measured erosion rates over the last 4 ka so derived interglacial erosion rates and suggested that the transient nature of the erosion could have been caused by climate fluctuations over this time period. This is probably worth adding given the scarcity of papers that use TCN and OSL surface exposure methods to derive erosion rates.

> We thank the reviewer for bringing this paper to our attention and have now included a few sentences on this study to the paragraph (lines 89-92).

Line 94 – technically Jenkins et al. (2018) performed burial dating, which is quite different from the exposure dating techniques mentioned. Discussing burial dating here is not necessary, but if you wish to demonstrate that it can be used for burial dating, I would be explicit about it and also add a reference to Freiesleben et al. 2015, for example: "In recent years, the application of OSL to rock surface dating has proved successful in a variety of settings for exposure dating (e.g. Sohbati et al., 2015; Liu et al., 2019; Lehmann et al., 2018) and burial dating (e.g. Freiesleben et al. 2015; Jenkins et al., 2018)."

> The reviewer raises a good point. We have now removed the Jenkins reference from this sentence.

Line 100 – calibration for what? I suggest you add "after calibration to account for the rock-specific light attenuation rates" or something similar.

Thank you for your detailed reading of the manuscript, we have amended the sentence so it reads "…after calibration to account for rock-specific <u>bleaching</u> rates" (line 116). We chose to use the term "bleaching" rather than "attenuation" as we were worried attenuation might be misleading to a reader who could think we were only calibrating for μ (the light attenuation parameter) which is not the case here.

Line 105 – add reference to Smedley et al. (2021) as it is possibly the only other reference that has used multiple luminescence signals specifically for deriving rock erosion rates with 10Be and OSL measurements as you are doing in this study.

Good point. We have now added this reference.

Line 181 – please could you add a few words as to why you were sampling areas with minimal lichen cover and red, iron-oxide staining to explain to those who may wish to sample using this approach in the future. Why is it important?

We have inserted the following ".. that would have otherwise impeded light penetration and impacted the luminescence signal" (line 199) at the end of the sentence.

Line 188 – please could you add a line to explain why the approach of Elkadi et al. (2021) was beneficial for these measurements and so demonstrate the importance to the reader, e.g. does it dramatically improve the measurement reproducibility? Are the measurements more accurate?

Thank you for your suggestion but we would rather not over-expand methodology. However, we have moved the Jenkins et al. (2018) reference to the more relevant part of sentence, so readers know which paper to refer to specifically if they want more information on the approach of Elkadi et al. (2021).

Line 188 – it would be worth stating explicitly here that you will derive three signals per sample for comparison, so OSL signal of quartz, IR50 and pIRIR225 signals of feldspar. It would also be helpful to non-experts/users to explain why analysing multiple signals is useful in this context. It is really unique and interesting so worth emphasising.

Excellent recommendation. We agree it would be beneficial for non-experts and so have added the relevant information in lines 207-210.

Line 193 – subscript the n in Tn in both occurrences.

Thank you to the reviewer for bringing this to our attention, we have now done this.

Line 194 – please explain why the slices were excluded from further analysis? Does it mean the results would not be reliable? At present, to a non-expert the sentence makes it sound a little like they are just rejected and could be better explained (although very briefly!) why these criteria are applied.

We have incorporated that monotonic signal decay is indicative of good heating (line 217) and also specified that slices which did not meet the criteria mentioned were excluded because they were not considered reproducible (line 218-219).

Line 219 – here you may wish to also consider the work recently published by Furhmann et al. (2022) on the incidence angle of light given your interest in the orientation of the sample for calibration (https://doi.org/10.1016/j.radmeas.2022.106732).

Thank you for bringing this study to our attention, we have added it to the paper.

Line 222 – here you state that you have provided sample-specific calibration parameters by returning to each site after a year. Presumably this is for all three lithologies, so hornfels, schist and gneiss, AND for all three signals, which would be worth highlighting here for clarity.

Thank you for highlighting that this might be unclear. We have now improved the sentence so it now reads: "…to calculate the unknown $\overline{\sigma\phi_0}$ and μ values for all three lithologies and luminescence signals" (lines 252-253) and we hope this makes it sufficiently clear.

Given the infancy of the technique, the variability in lithology and the fact that you're using quartz and feldspar, I think this would be of great interest to the community and so would be worth including Table S1 into the main manuscript but this is the authors discretion.

We understand why it could be beneficial to include the table in the manuscript but in the end, we feel it is better suited in the Supplementary. This is mostly due to the fact that, following on from the reviews, we have now expanded the information it contains and it has been split into 3 separate tables (Tables S2-S4) which we think is a lot to include in the main text.

Lines 312-313 – it is unusual to include some interpretation in the results section but given that the discussion is focussed on the erosion rates rather than the specifics of the luminescence technique, it is reasonable. However, if you are going to offer some discussion of the OSL unknown parameters in Section 3.2, it would be useful to discuss how the quartz and feldspar attenuation rates compared given that no (or few) other examples exist in the literature showing such data and it would be interesting to unpack this unique data, especially relative to the variability in lithologies of the samples.

We thank the reviewer for her interest in our results, but as mentioned in the general comments, we prefer not expand on this matter since we don't have pure quartz/feldspar signals although this is certainly worthy of further research.

Line 356 – "Several factors, often working in combination with each other, modulate bedrock surface erosion rates. These include temperature, elevation and surface slope". This makes it sound like only three factors modulate erosion rates, which is not the case as explored by Portenga and Bierman (2011) amongst other studies. Presumably temperature, elevation and surface slope are factors you will focus on in this study? If so, either state all the factors that may modulate erosion and then say explicity that you'll only consider these three, or just re-phrase to "Several factors, often working in combination with each other, modulate bedrock *surface erosion rates. These include, but are not limited to, temperature, elevation and surface slope".*

Good point, we have rephrased the sentence as suggested.

Line 364 – lithology is known to have a dominant control on rock surface erosion (e.g. Ford and Williams, 1989; Twidale, 1982; Moses et al. 2014), but this is not explicit from this section. It would be worth adding a sentence or two discussing the dominant role lithology has in modulating rock erosion rates, and then perhaps discussing whether you observe this in the erosion rates you measured for hornfels, schist and gneiss, or are they all similarly resistance to weathering and subsequent erosion? Given the metamorphic origin or these rocks, it is possible that they are more resistant than other lithologies (e.g. sandstones, limestones). Either way, it would be interesting having this discussion relative to your measured erosion rates, which are difficult to obtain.

We agree that the effect of lithology was not sufficiently developed in the original manuscript and have now added a few sentences that we hope has done this (lines 422-428). However, we found no relationship between lithology and erosion rates for the samples in this study, likely due to the metamorphic nature of the samples as suggested by the reviewer. We have stated this in the manuscript (lines 428-431) while also adding a figure in the supplementary for additional information (Figure S5).

Lines 379-390 – you state here that the anti-correlation between erosion rate and elevation is likely reflecting the lack of frost crack weathering in this setting, which is very interesting and new information, but where do your samples that derived transient erosion rates fit into this picture? Could these samples be reflecting frost crack weathering given that presumably frost cracking processes would be more stochastic over time and so more likely to be reflected by transient erosion, rather than steady-state. It would be interesting to have a better understanding of what transient erosion rates may be recording from the natural environment in general.

As mentioned in the "General comments", while we agree with the reviewer that it would be very interesting to have a better understanding of what the transient erosion rates are representing with regards to the natural environment, we believe at this stage that the specific cause remains hypothetical and requires further investigation. We have added a section (4.1) that discusses the transient vs steady state erosion results, and in it we have included a sentence that says the transient state of sample GG04 is likely due to a localised stochastic process (line 400).

Lines 391-394 – I find this a little confusing so perhaps you could better explain it for the reader. How do the observed patterns of glacial erosion in a valley due to quarrying and/or abrasion (that occur when the ice is present) control the interglacial erosion rates (when the ice is not present)? Are you suggesting that the rock has been weakened more during the glacial and so the interglacial erosion rates are higher at lower elevations? I think it would help the reader follow your arguments and interpretations better in this section if you provided a little more explanation for this.

Yes, indeed this is what we were suggesting. Thank you for bringing to our attention that it might be unclear, we have now expanded the explanation and hope that it is clearer (lines 471-475).

Line 399 – you give an example of frost crack weathering despite stating in Line 387-388 that "frost crack weathering is perhaps not a dominant form of post-glacier erosion in these areas", and rather "bedrock erosion is most likely occurring through continuous grain-by-grain erosion". I feel like these two interpretations do not align. Alternatively, have you considered the role of moisture via precipitation in this setting? Do lower elevations receive more rainfall/snowfall and therefore are subject to greater chemical weathering and subsequent erosion? It has long been known that precipitation can be a driver of rock weathering and subsequent erosion (e.g. Hall et al. 2012; Merill, 1906; Moses et al. 2014; Swantesson et al. 1992). Furthermore, in the 'global' compilation of rock outcrop erosion rates by Portenga and Bierman (2011), multi-variate statistical analysis showed that 32% of the variation in the global population of outcrop erosion rates could be explained by the five environmental parameters considered (latitude, elevation, relief, mean annual precipitation, mean annual temperature and seismicity), with mean annual precipitation being the most important parameter, accounting for 14% of the variability in this 'global' dataset even across many different settings. As such, it might be worth considering precipitation in your discussion. Although palaeo-precipitation records will be almost impossible, perhaps there are at least contemporary observational data of mean annual rainfall and snowfall from an elevation range of the alps for contextualisation?

Thank you for bringing this contradiction in interpretation to our attention, and for raising this interesting precipitation hypothesis. With regards to the former point, we have removed the bracket that mentions frost crack weathering and for the latter, have now added precipitation as a potential explanation. While we weren't able to find contemporary observational data of mean annual rainfall and snowfall from an elevation range in the Alps, we investigated the Clausius-Clapeyron relationship which estimates a roughly 7% increase in water holding capacity of the atmosphere for every 1°C rise in temperature. The temperature difference between the lowest and highest elevation sites for this study is ~3.5°C, equating to a ~25% increase. We have included this information in the manuscript and expanded upon the calculation in the Supplementary Materials.

Line 409 – Given the scarcity of studies, it is worth adding Smedley et al. 2021 as an OSL application, and then potentially expanding upon the findings of this study in Lines 411-425, given the authors determined interglacial erosion rates. Although the erosion rates derived were transient, it would be worth considering the erosion rates in the range that were lower and could be sustained for longer time intervals as these are more comparable to your steady-state erosion rates, in comparison to the higher erosion rates that can only be sustained over shorter timeframes.

We agree that it is useful for a reader to have the Smedley et al. (2021) reference added to the sentence given the scarcity of OSL applications to erosion rates, and we have expanded upon its findings later in the manuscript (lines 519-524).