Responses to compiled reviewer comments for manuscript titled: *Erosion rates in a wet, temperate climate derived from rock luminescence techniques*.

In this document, we detail changes to the manuscript in response to the reviewer comments for the **third round of revisions**. Please note that the ACs are our responses to the (unedited) reviewer and editor comments.

Reviewer 1 (Benjamin Lehmann) comments: RC1

Author comments: AC

Reviewer 1: Benjamin Lehmann (RC1)

RC: I thank the authors to address all my comments, to answer all my questions and produce the change in the manuscript. In its revised version, the manuscript gains in quality with a better explanation of metholodology, a deepening of the discussions on the results and a clearer retranscription of the results in the different figures.

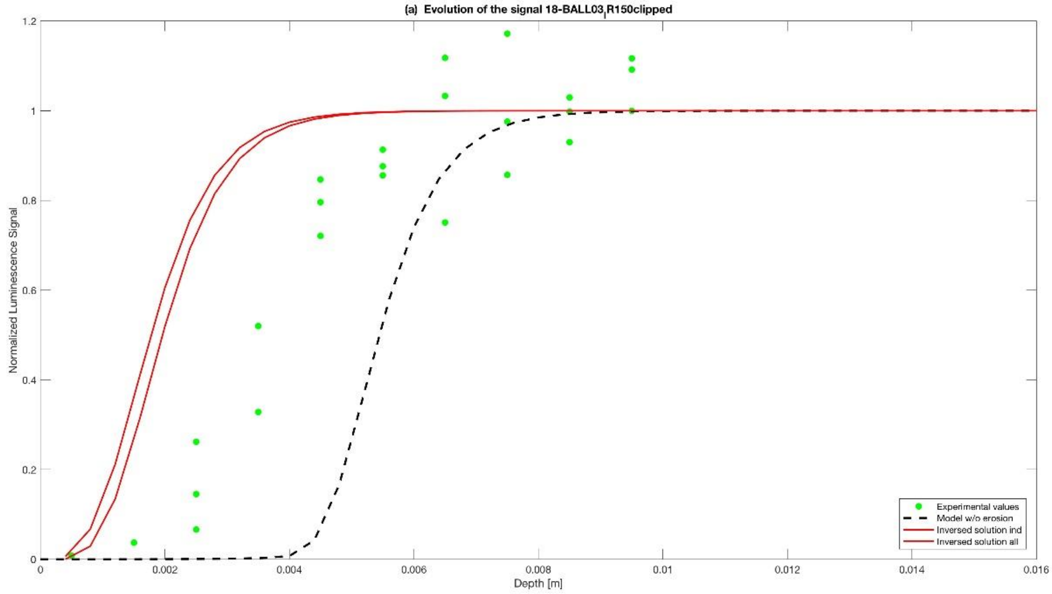
About the reconstruction of the the erosion history using Lehmann et al., 2019a approach, I acknowledge the efforts of the authors in discussing the difference between the experimental values and the inferred model (presented in Figs. 5G, 5F, 5F and 5I). All the points made by the authors are accurate and could explain the difference mentioned above. I still want to raise few questions on this part.

First of all, to which values of the inversion correspond the dashed black line in Figure 5, the maximum likelihood, median value, values above a certain threshold? In Lehmann et al., 2019a, the inversion results show the best-fitting profiles inverted for all numerical solutions with likelihood > 5 %.

AC: The data in Fig. 4 and Fig. 5 are the best fit of the inversion results and we have added this information into the captions of both figures.

RC: Secondly, I still think the model should be able to fit the experimental values. One explanation of the poor fit quality could be that the model gives too much importance of the in the plateau and too little importance of the values in the bleaching front. A solution to test this potential modelling problem would be to run the inversion using only experimental values at depths from 0 to 10 mm. Being an important part of this study, I think the luminescence as an erosion-meter should show more convincing inversion results.

AC: We have explored the effect of modelling just the data from 0 to 10 mm depths as requested by the reviewer to assess the importance of the plateau on the fit, and whether this may account for the poor fit currently calculated. As a sensitivity test, we performed this for the sample BALL03 $pIRIR_{150}$ signal and the output is shown below. Evidently, only using the data from 0 to 10 mm made little difference to the fit of the data.



As currently stated in our discussion, specifically the last paragraph of Section 5.3, "it is possible that surficial weathering products may have changed in thickness and composition over time, which in turn could slightly vary the attenuation of light (Meyer et al. 2018; Luo et al. 2018), meaning that the calibration of $\overline{\sigma\varphi_0}$ and μ from ROAD02 here introduced uncertainty into the inferred erosion model as it was not time-varying".

Luminescence as an erosion meter is still a new application of the method. No studies have yet derived transient erosion ages that can be compared to independent erosion estimates. As such, statements such as "luminescence as an erosion-meter **should** (our emphasis) show more convincing inversion results" are currently untested with real-world examples. We suggest a physical explanation for the lack of fit that is plausible and accords with circumstantial evidence from field observations. We would argue that a central aim of science is to take hypotheses (e.g. the erosion model) and test them against evidence (e.g. our measured profiles). Our findings thus set up important research questions that can refine the hypothesis. While beyond the scope of this study, such refinements can now be addressed in future studies thus improving the applicability of the luminescence erosion meter.