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Interactive comment on "The role of frost cracking in local denudation of steep Alpine headwalls over millennia (Mt. Eiger, Switzerland)" *by* David Mair et al.

Anonymous Referee #2

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General comments-

The manuscript by Mair et al considers erosion rates and processes on the Eiger Mountain in the Central Alps using five new 10Be measurements from a depth profile. They compare the results to modelled cosmogenic nuclide profiles and investigate frost cracking intensity using previously published Matlab programs, with available temperature data coming from an established network. They discuss the new measurements in relation to their previously published 36Cl dataset that consists of 5 depth profiles and with regards to field evidence for bedrock fabric and jointing.

The topic is certainly worthy of investigation, with a nice summary given in the background section (at least for those not well acquainted with frost cracking literature like myself) and the paper is generally well written with only a few minor quibbles about the language and presentation of the figures. While the paper reads well, necessary information is often lacking to properly assess what is being done and there is too much reliance on a previous publication (Mair et al., 2019), which the reader is essentially forced to read if they want to understand this paper. While I have no problem with referring to standard procedures published elsewhere the paper should be able to stand alone.

I have some major concerns about the work which I will detail below. Mostly these relate to the cosmogenic nuclide application, the modelling of profiles and their interpretation, as this is more my area than the frost cracking modelling. Based on these concerns I suggest the paper needs significant revisions to produce a suitably rigorous investigation of denudation processs/histories from the cosmogenic nuclide data. I'm not sure the authors will be able/be willing to provide this.

One concern is that there is little new data offered here and what is presented is close to the limits of what might be considered acceptable in terms of noise-to-signal. This leads me to be unconvinced that what data is presented support the findings. I do not agree with the authors that the relative analytical uncertainties of 11-69% at 1 simga are small (as is claimed in Line 296), instead they are hampering a sound interpretation of a small set of data.

Modelling of the limited dataset is valid to try and extend the approach and investigate erosion in a more general sense, but the profile modelling is either missing crucial information, or is inappropriately used. The authors apply a published model (Hidy et al. 2010) that to my knowledge has been mostly used in order to extricate age/erosion information in situations where variable pre-exposure could be a concern. This has been suitable for sedimentary deposits, where samples have a pre-depositional exposure history (inheritance). In the case of bedrock, as sampled here, any inheritance must have other origins. I'm confused as to why the authors consider production by muons to be an inherited component in a study of erosion (e.g. L176). Muon production at depth as the rock erodes is not 'previous exposure', as the authors state, but part of the ongoing exposure that is being used to constrain the erosion rate. Assuming inheritance values equivocal to the concentration of the surface sample and relating this to muons (L168 and L180) would seem to suppose a large enough landslide occurred to entirely remove the spallogenic component. This, however, would go against what is claimed on L305, that the 'inheritance' is too large to support the notion of a deep landslide, and instead they mention multiple dm thick rockfall events (see also below on this point). On L300 it is mentioned that the inherited component likely comes from greater exposure at depth, before the current exposure period. Unless the authors are arguing for some kind of intervening burial, which I'm pretty sure is not the case, these would not be different periods of exposure, but one period perhaps separated by a hiatus (i.e. non-steady-state erosion). If non-steady-state erosion is the case it would go against a model that tries to fit a smooth production profile with depth; though erosion via stochastic mass wasting would arguably better explain why there is difficulty fitting a smooth profile through the data than some notion of inheritance. Perhaps I'm missing something fundamental here but if so the authors need to do a better job of explaining why they are including inheritance in the first place, and then why they are relating it to muogenic production.

The authors claim dm sized rockfall erosion. I suspect with dm size erosion events one could sample a metre or so away and get different results (i.e. these blocks fall from a specific site stochastically). Whether this is an issue depends on what is meant by dm; 10cm, 90cm? I don't see the support for this claim of erosion thickness other than the jointing would suggest it. That is, the bedrock structure data would be better used as a parameter constraining possible mass loss depth in an erosion rate modelling exercise, rather than being an assumed outcome of the cosmo profile analysis that it would likely be causing trouble for anyway (see above RE fitting a smooth profile to stochastic erosion events). Approximate fits to the data can be gotten by assuming the simplest case of a large rockslide 2.2 kyr ago setting surface concentrations to zero.

Admittedly the fit is not as good as shown by the authors as I use a much simplified approach but my point is the claims based on the cosmo data are weak (non-unique outcomes are clearly possible), not fully explained and are specific to certain sites.

The results are sensitive to the blank correction due to the low 10Be concentrations. Blank corrections as high as 19% could be acceptable if the authors can show the subtraction is robust. This probably requires several in-batch blanks, rather than a long-term lab background average, which needs to be justified here. The vague nature of the blank subtraction as it's reported lessens the confidence in such low concentration data, i.e. what is the uncertainty in this long-term blank value (not given on L140 or in table 3); was a blank/s measured in the batch, or at the same time in the lab, and if so what of the results? If the authors are forced to use a long-term average as no in-batch measurements were made I would expect to see some discussion of how variable this value has been over time (long-term averages would mask occasionally high/low values which is a problem when it comes to measurements close to the lab background). Is there any idea of what inter-batch background variability is?

More specific comments-

L61- The way this is written makes it appear as though new 36Cl data will be presented, rather than the inclusion of previously published data in the discussion. Same goes for the conclusion section L433.

L64 – Saying the long-term denudation of the mountain will be quantified sounds a bit too grand and is incorrect, as the rates reported are pretty short term and for a few specific locations only.

L100- Some discussion of the issues that might relate to sampling a constructed tunnel would be appropriate. How pristine were the surfaces sampled, especially for the zero depth sample, was it near the lip of the tunnel? L158- The shielding correction is high (0.55), so sensitivity of the results to the exponent used in the topographic shielding correction ('m' in Dunne et al 1999) should be considered.

L168- If the maximum likely age is 20 kyr why then use 75 kyr?

L207- Applying values that are 'slightly higher' is vague and seems arbitrary.

L298- The 'clear minimum' for denudation in the different simulations is zero. I'm not sure this suggests a clear minimum, or a problem, as it implies the model wants to go below zero. I also see no justification for using these 3 values?

L301- I don't understand how the uniformity of the 'cut-off' depths suggests a robust measurement. Time simply extends by a proportional amount to allow for the greater amount of denudation (i.e. Table 4)?

L311- This statement probably needs to cite the Mair et al 2019 study.

L304- Define 'large' inheritance? The deepest sample is within zero at 2 sigma. I don't think these arguments about concentrations at depth are sound for such large uncertainties. Also, 'lower' should be 'higher', or the statement needs to be written more clearly.

L315 and L411- If the argument is being made for steady-state erosion (though what steady-state means in relation to dm size chunks is unclear) the rate should persist for several multiples of the attenuation length (see the Lal 1991 paper cited). I'm not sure if it's appropriate to talk about the minimum age, based on assuming the sample concentrations represent exposure ages measurements, as being the time over which the measurements are appropriate. This point needs more explanation.

Minor points-

Fig 1A could be the same orientation as the diagrams (i.e. it's currently a mirror image of 1B).

L155/L158- What are spallogenic particles?

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