Comments from Associate Editor and Principal Editor on MS geochron-2021-23

Dear Eric et al.,

As you know, I have received two reviews for your MS and I´ve carefully checked your responses. Unfortunately, due to the system setup of the journal, I do not get the track-changed version of your replies at this stage, only *after* I have made my decision. For the record- this is nothing I can change, but it´s simply due to the system, but it makes, in my view, my life as an Associate Editor harder. The reason is that I cannot in detail see your changes, and some of them are hard to judge without the connection to the text.
One of these issues where seeing the track-changed version would clearly help me is your treatment of terminology, an issue that both reviewers commented about. I agree that back in Devendra´s time, the term *erosion rate* was used for *in situ*-derived rates. But I think that the community has clearly advanced on this in the meantime. I don´t mean that one should *per se* call *in situ*-derived rates "denudation rates", because weathering rates might be very very low, but without seeing the numbers and changes, this is difficult for me to judge. I however have a hard time believing that we are dealing with a landscape where chemical weathering is totally absent. (I read from your response that there is some water chemistry data available, but even a multi-decadal record in TDS still has time scale issues when comparing to millennial-scale cosmogenic rates). So, even if it´s only a super small portion of total denudation, the *in situ*-derived rates should be called denudation rates and are thus directly comparable with $^{10}Be/^{9}Be$-derived denudation rates (btw, I´ve calculated the reac/min fraction (eq. 9 in my 2015 paper) using your data, and the values are around 0.25-0.3, which is a range in Be-related weathering degrees found by others, i.e. indicating 25-30% of a weathering degree, meaning that weathering cannot be absent). If it wasn´t for this comparison, I would not care too much, but I stress here that it would make the MS much easier to digest if the terminology was adjusted. Also on this matter, I would suggest that you present either in mm/kyr or in t/km^2*yr, at least in the text (I would not opt for "Mg/km^2*yr", for reasons of common acceptance. Also, it´s in my view totally unnecessary to provide D_{insitu} AND in situ-derived sediment fluxes in the Figures, as these essential only differ by density). Tables could provide both values. Note that for D_{insitu}, one also needs a density assumption, so one could easily convert these to t/km^2*yr, or, if lithology is uniform, as claimed, one could use the same density to convert $^{10}Be/^{9}Be$-derived denudation rates from t/km^2*yr to mm/kyr. Given that you only have one major lithology, this would be a fair assumption. Regarding erosion rates from single ^{10}Be concentrations, I agree with both reviewers, and welcome the removal of their interpretation from the Discussions, as their presentation did not help to clarify controls among the variables. I hope that Fig. 6 and 7 will benefit from this removal and adjustment of terminology, as these are particularly hard to digest. Regarding the comparison between denudation/erosion rates, I think it would help to add the "Appendix" equations into the main text, at least the two main ones (Lal, von Blanckenburg). I´m not sure where the "Appendix" equations will end up, hence if they are not instantaneously visible for the reader, this would help an audience that is not so familiar with the topic (assuming that the general audience of GChron is not familiar with this).

I´m happy to see that you take the effort and compare *in situ* and $^{10}Be/^{9}Be$ denudation rates. I am a bit puzzled however by the overall scientific outcome of this comparison. D_{insitu} show a clear trend with precipitation (but please see Greg Balco´s comment on this at the end here, in support of the concerns raised by Reviewer #1.), whereas $^{10}Be/^{9}Be$ denudation rates are all over the place if one takes the entire dataset. Why is that so? Regarding the methodology, please clarify in a response if the used rainfall-derived $F^{10}Be_{met}$ could play a role in obscuring a trend between denudation rates and precipitation. I understand that you favor Graly et al. (2011) values, but that makes me a bit worried if there could be some intrinsic dependence. Btw, are you using a local rainfall-relationship or a global one? I don´t think that is specified in the paper. Further, you use the 2.5 ppm value suggested by the von Blanckenburg paper (2012) for $^{9}Be_{parent}$. Unfortunately, we have seen that this is probably only valid for larger basins, hence I would really welcome some measured values, even if it´s one sample per lithological unit….The basalts will clearly deviate from that value, but also granites can show some heterogeneity (see Dannhaus paper) and in such small basins (your tributaries), this might really be an issue. In order to check out the potential magnitudes, the percent areal coverage of each lithological unit per basin might give a clue? At least discuss please in which direction the associated changes would go for the $^{10}Be/^{9}Be$-derived denudation rates, if you either over- or underestimate $^{9}Be_{parent}$ (but, in order to resolve the missing trend, there must be some systematic pattern). Another way to assess the accuracy of $^{10}Be/^{9}Be$-derived denudation rates would be to do the Q/E calculation suggested by reviewer #2 (note that even at "high" pH values of 5, the bias might be between 10-100% on D´s). At least that would give you an idea where the problem might be. Lastly, there might the geological reasons for the absence of this trend with precip (if it´s at all there?), which might be e.g. overland flow? However, I´m overall not too worried, since the overall numbers are not so far apart between in situ and $^{10}Be/^{9}Be$-derived denudation, but still a bit puzzled.

In conclusion, I think this paper needs another round of revisions. Given that one reviewer opted for "major", the other for "reject", I will send it out again. I´m looking forward to a new version.

All the best,
Hella

Below I give some more technical / structural advise that could improve your paper:

Line 101: Just to be accurate here: At least the studies I was involved in, we use the "$^{10}Be_{reac}/^{9}Be_{reac}$" ratio. We do not perform bulk extraction of meteoric ^{10}Be.
Line 187: "No native Be" was detected. It´s hard to believe that there was "no" native Be found, because this is a matter of which instrument you use. We measure ^{9}Be in seawater ranging down to 1 ppt and lower using ICP-MS. Please re-phrase such that "relative to added ^{9}Be carrier amounts, no significant Be was found" or something like that.
Line 240: This definition of sediment fluxes derived from D_{insitu} should come in the paragraph above, where these are mentioned.
Line 242: How were the "average modelled" rates derived? Not clear to me. If these are from a simple mass balance. i.e. summing up the respective yields and then dividing by summed area, then please say so.
Fig. 4 The "historically burnt areas" are really hard to make out in the Figure. Would suggest that you use some other color / make larger.

In addition, editor Greg Balco had the following comments:

Reviewer 1 points out that there is not an obvious physical means by which the relatively small variability in precipitation could cause a relatively large effect on the erosion rate. This is true, and there are some additional aspects of this part of the paper that seem to need attention. First, the smoothed data set used to calculate precipitation for all the basins (Worldclim) appears to have been generated by using elevation and distance from the coast as smoothing parameters, so precipitation is not an independent variable, it is already defined to be correlated with elevation. Thus, for example, the R^2 of 0.93 in Fig. 3C appears to just be reproducing how the smooth data set was created. In other words, precipitation and elevation appear to be effectively the same independent variabie in these regressions. Second, their Figure 3 shows that the smoothed data set is doing an extremely poor job of reproducing rain gauge data -- the true variability from rain gauges is twice as big as predicted by the smooth data, so the question brought up by the reviewer about the range of precipitation is already based on an incorrect range. Overall, the aspect of the paper dealing with whether or not precipitation is the main control on erosion needs rethinking. Clearly a precipitation control is possible, but I am not sure how you would exclude the possibility that elevation is actually the controlling variable, either through, for example, a relationship between elevation and local relief or perhaps between elevation and temperature, perhaps involving freeze-thaw processes. A possible approach to this would be to take

Reviewer 1's observation that there is a physical limit on how much variation in the erosion rate could be accommodated by the available range in precipitation, and evaluate whether or not that is adequate to explain the observed variation, or if an additional factor related to elevation would be required. In any case, this aspect of the discussion needs more careful attention.