

Comments from the Editor

I carefully reviewed the replies to the comments made by the reviewers. I do see the need to go into more detail, and need to see this manuscript again after these comments. I would also send this out again to the reviewers, as I think that in parts, the reviewers (mainly reviewer 1) comments were not adequately addressed.

The comment “Regression between ^{10}Be flux and environmental variables in Fig. 5. All these regression lines are strongly affected by the extremely high ^{10}Be flux data from Copper Creek. If this data point is removed (as demonstrated above), the regression becomes much less obvious”. “ was largely ignored, but I do agree with the reviewer that this needs to be addressed. How solid is this regression? Do the symbols contain the uncertainties (hidden?) or what uncertainty has been propagated? In Fig. 5b, it looks to me as if the 4 samples cluster, and there is the one at high fluxes (the middle 3 values are certainly the same within uncertainty). It’s clear that the other variables are just a derivative of precipitation, so that these, too, showing the same correlation does not support the argumentation. I would not elevation here, it might be totally misleading to people from the *in situ* community who might think that there is a sort of “scaling” with elevation like there is with *in situ*-produced nuclides. Please remove it accordingly. I can see however that snow depth and SWE might be relevant, as these could present a more time-average of MAP. Within this possibility, could you discuss why the MAP-derived flux is lower than the snow-based ones? (see comment by reviewer 1). Why would MAP data underpredict precipitation? Could you discuss this?

Regarding pH, I also agree with reviewer 1 that at a pH lower than 6, “only minimal chemical loss of $^{10}\text{Be}_{\text{met}}$ ” cannot be assumed. The reason I do not agree with the reply (which is based on Graly et al.’s soil compilation) is that in Graly compiled data, only 1 profile is included that is developed on glacial till, but there, pH hasn’t been measured. Hence, the quoted pH cutoff is likely valid for other materials, where humic acid (shifting the pH dependency) and grain size (supplying adsorption space) might all matter. Clow et al.’s moraines by the way have quoted pH values of 5.5 to 8, so on average (much) higher. I would therefore expect to see a calculation on potential loss due to low retentivity, and its effects on the depositional flux.

Next, as mentioned by the reviewer, the clast size $>2\text{mm}$ were not quantified, and I am not satisfied by the reply. What is then the estimated proportion of the large clasts (and what is the consequence for the ^{10}Be concentration?). Please clarify. I cannot see any discussion on this in the MS, although it reads as if there is data on this. Related to this is the assumption on bulk density value: How good is this assumption? The argument here is that the same bulk density as a till from a previous study in the valley (is this now the Copper Creek till?) was used. How variable is the bulk density in tills? Is there any effect of post-depositional compaction in tills, and if so, could that increase/ change with age?

What rationale is being used for assuming that the measured values at depth correspond to the inheritance? (Concentrations could further decline, no? You don’t know, you haven’t sampled). In a scenario of temporally-uniform erosion, the

measured inheritance should represent the erosion rate measured by in situ ^{10}Be . Converting this meteoric ^{10}Be concentration into an erosion rate is possible, but might be prone to bias (grain etc.). But, perhaps worth a try?

Some minor comments:

Fig. 3: Please label each profile at the top with the name of the till.

I would support the citation of Deng et al., 2021, Deposition and retention of meteoric ^{10}Be in Holocene Taiwan river terraces. Quaternary Science Reviews 265.

I don't quite understand the term "watershed-scale" ^{10}Be fluxes- how is this calculated? Is this based on the distribution of elevation within the watershed and then the average flux is computed? (So, sort of an average of each map shown in Fig. 6?). Please clarify.

As soils can have relatively high ^9Be concentrations, I wonder if the $^{10}\text{Be}/^9\text{Be}$ (AMS) should be corrected for the natural ^9Be concentration?