Comments from the Editor 3rd round (July 2026)

Thank you for addressing my comments. Some short comments below, that are mainly technical.

Lines 15-17: Please correct typos and messed up text fragments.
Line 22: Same here.
Line 36/37: Would change to something like: "Prior to its deposition on Earth ´s surface by precipitation or dry fallout, 10Be resides in the stratosphere and in the troposphere due to atmospheric circulation for 2 years and 55 days, respectively".
Line 40: ".. the solid phase at approximately...?
Line 57: The citation of von Blanckenburg et al., 2014 refers to weathering rates measured from marine sediments. I would not cite this here, as you are referring to watershed-scale rates.
Lines 211: ..desorption losses "according to Clow et al., 2020:" ?
Line 268 (heading" Would suggest to change it to "Comparing inventory-based fluxes against...".
Line 296: Correct to CaCl2.
Line 460: factor of 12? Is this the full range in variability?

Table 2: Sure that the 10Be concentration given is in "1000" (10^3) atoms/g? Let ´s also reduce the precision of the numbers in the last two columns (e.g. change to 578x10^3 atoms/g). In the text, you call it "atom".
Fig. 4, superscript Meteoric"10"Be flux in y-axis
Fig. 7, The IC flux "origin" in SWE appears twice. Is one of them supposed to be the "standard"?

Thank you for submitting your work to Geochronology.
Hella Wittmann, July 7th, 2026.

Comments from the Editor 2nd round (May 2026)
Lines refer to the "authors track changes" version.

I decided to give the authors some more feedback, instead of sending the MS again to the reviewers. This will save them some time, but I would expect to treat my comments as if from a reviewer. The manuscript itself is in my opinion still in parts unstructured and needs clarification. Also, discussion of the results of at least one major issue is pending. I would strongly suggest that the authors follow these instructions, as the MS is in its current form not very accessible.

Major comments
I think the introduction needs some re-structuring. It ´s too detailed, and with the sub-heading removed now, reads more like a "methods" section. The balance between the soil-based inventory approach and the other approaches is not even, hence I would move the equation and the detailed description to the methods part. The paragraphs dealing

with the assumptions (half-life, 84-90), retentivity & dust (92-96; 97-104) I would also move to the methods, or create a subheading "testing assumptions" and clearly explain your approach. Reading the 3.1. methods / laboratory methods, it´s just weird to have no equation at this point. Are Tables S1 and S2 in the data repository? Please provide a link to the ESS-DIVE archive, it´s not available.

Regarding soil pH, I ask to cross- check the pH values derived here by using ultra-pure water with a method using $CaCl_2$ (and using a reference material for pH, such as IRMM433-7).

I don´t understand why the measured fluxes correlate so well with precipitation and precipitation-related variables, but then the rainfall-relation from Graly et al. (2011) gives much higher fluxes. How can that be? The watershed itself is not that large, any rainfall-dependence should not change drastically. Was there perhaps something lost without accounting for (see pH comment)?. I would like to see a more in-depth discussion on this issue, perhaps geared towards additive versus dilution effects?

Overall confusion results for me for several aspects centrered around the term "predictions". In that regard, please explain how the "watershed-scale fluxes" are calculated, what the "forced-to-zero" predictions actually are. I had asked the first question already in my comments in the first round. Please address.

Minor comments
31: remove "garden variety" (whatever that means)
35: "circulated through the atmosphere" – not sure what this statement is good for? Please remove or improve clarity, eg. by referring to the residence time of Be in the stratosphere vs troposphere
36: "binds with minerals" is equally vague. It is coprecipitated, not only "adsorbed".
165: pH measurements: I would ask you to again check your pH measurements. Using ultra-pure water can result in readings to be very unstable. Using CaCl2 leads to more reliable results (e.g. Minasny et al. (2011)).
215-220: I would move the list of uncertainty treatment to a footnote of a table.
244- units for erosion rate now differ from those used above. Streamline, please
247: "We also modelled F_corr-e using e vales ranging from…." is statement showing up well below the eq. 4, but in the caption of Fig. 4, I get the impression that you used eq. 4 for the modelling. So, what have you exactly done and can you please explain more properly how erosion rates were modelled and how a single model erosion rate (Fig. 4) can lead to regression lines shown in Fg. 4?
253: Now you present other methods. Subheading?
275: I don´t quite understand what you mean with "generate spatially continuous predictions of F_met"…. Are you simply referring to correlations in Fig. 5?
At the end of this paragraph, you mention details on the GCMs used for comparison. This is out of place here. Move to e.g. results or even discussion section.
307: Please refer to as "AMS-measured $^{10}Be/^{9}Be$ ratios".
411&412: Graly "et al."
449: This is only unsurprising if MAP, snow depth and SWE equivalent haven´t changed through time. How sure is this actually?

452: I don´t see a factor of 2-3 in table 4. Or I am I misunderstanding? Is this the reason for giving "forced" values so that they are comparable among the different variables? So, why are these then all different if they represent the "same" forcing?
454: So that means that the integration time scales of the data is much shorter than the F_met from soil inventories....
488: Zheng´s flux values are for the modern atmospheric conditions only. Perhaps also use the modern value only from Heikkilä & FvB instead of using the average?
501: Now you refer to the "pre-industrial" value. I´m confused. If at all, I would compare the modern values between the GCMs (mainly because there are no pre-industrial values from Zheng´s model).
505: there is something missing in that sentence.
573: Your measured fluxes are lower, Graly rainfall-prediction gives higher fluxes....Can you give the Graly values in some comparison?

Eq. 3: I thought the erosion correction approach was from Clow et al., 2020. Please cite.

Table 4: May be I am ignorant but what does a forcing to zero mean ? It gives quite different fluxes, so I would wonder about the physical meaning of these values.
Also, I don´t get these values in general: Have you simply calculated the mean values for each variable from the 4 different sites which you then termed "predicted"?

Fig. 6: The 4 different figures all look pretty similar. Choose a different color -scheme?
Fig. 7: The citation of Graly et al., 2011 is not in the citation list.
Fig. 8: Given that I am confused regarding your "predictions", I would certainly spend some words on explain what the "watershed-scale" fluxes are that you present here, and how they are calculated.

Comments from the Editor (first round)

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 10Be flux and environmental variables in Fig. 5. All these regression lines are strongly affected by the extremely high 10Be 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 adressed. How solid is this regression? Do the symbols contain the uncertaities (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 10Be_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 >2mm 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 10Be 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 10Be. Converting this meteoric 10Be 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 10Be in Holocene Taiwan river terraces. Quaternary Science Reviews 265.

I don ´t quite understand the term "watershed-scale" 10Be 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 10Be/9Be(AMS) should be corrected for the natural 9Be concentration?

Minasny, B., McBratney, A. B., Brough, D. M., and Jacquier, D., 2011, Models relating soil pH measurements in water and calcium chloride that incorporate electrolyte concentration: European Journal of Soil Science, v. 62, no. 5, p. 728-732.