

It is my pleasure to review “Meteoric beryllium-10 fluxes from soil inventory measurements in the East River watershed, Colorado, USA” by Marmolejo-Cossío et al. Meteoric cosmogenic ^{10}Be has been applied to determine rates and dates of various Earth surface processes in recent decades. A key prerequisite for its accurate application lies in constraining the meteoric ^{10}Be depositional flux. Here, the authors aim to quantify the millennial-scale ^{10}Be deposition in the East River watershed using soil inventory ^{10}Be measurements from five glacial moraines with known ages; this approach will provide important insights into meteoric ^{10}Be applications in this area. In general, this is a valuable ^{10}Be dataset worthy of publication in Geochronology. However, several major concerns need to be properly addressed before further consideration. I provide the major comments first, followed by line-by-line comments.

Thank you for taking the time to review our manuscript and provide constructive feedback.

Major concerns:

1. Consideration of >2 mm fraction in ^{10}Be flux calculation. The authors only measured the <2 mm fraction for ^{10}Be . However, glacial moraines may contain a non-negligible fraction of coarse grains (>2 mm). How did the authors treat this fraction during ^{10}Be flux calculation? As coarse grains may bear very low [^{10}Be], their volume percentage needs to be constrained and accounted for in the ^{10}Be flux calculation.

We have revised the manuscript to incorporate > 2 mm clasts following the approach of Deng et al (2021, QSR) using volume estimates of large clasts digitized from field photos and volumes of smaller clasts determined from sieving. The methods are described in lines 183-192. In re-calculated the inventories, we discovered an error in the way measured concentrations were corrected for inheritance. We have corrected that error and propagated the new results through all analyses. Hence the results in our revised manuscript differ both because we have handled bulk density and large clast volumes differently than in the original manuscript, but also because we corrected the error. However, the new results are comparable to the original results and the overall interpretations of the data have not changed.

2. Correction for erosion impact. Since the authors can constrain the erosion rate to some extent (using in-situ ^{10}Be or topographic curvature), I encourage them to correct for its impact on the ^{10}Be flux estimation. Such practice has also been applied in previous ^{10}Be inventory studies (e.g., Clow et al., 2020, Geochron.; Deng et al., 2021, QSR).

Clow, T., et al. (2020). "Calibrating a long-term meteoric ^{10}Be delivery rate into eroding western US glacial deposits by comparing meteoric and in situ produced ^{10}Be depth profiles." *Geochronology* 2(2): 411-423.

Deng, K., et al. (2021). "Deposition and retention of meteoric ^{10}Be in Holocene Taiwan river terraces." *Quaternary Science Reviews* 265: 107048.

We did correct for the impact of erosion in the original manuscript, but in our revised manuscript we have more fully explained the results and discussed the implications. For example, Table 3 includes erosion-corrected results, we have included an erosion-corrected version of Figure 6 as a supplementary figure, and Figures 7 and 8 show results that are corrected for erosion.

3. Representation of the Copper Creek profile (Fig. 3a). This profile has 1) a very low pH (~4), 2) poorly constrained ^{10}Be inheritance, and 3) an incomplete inventory (sampled depth is too shallow). It seems unlikely to derive a realistic ^{10}Be flux estimate from this profile. I suggest the authors focus only on the dataset from the other four profiles in the Discussion.

We focus on all of the profiles in the discussion, including the Copper Creek profile and potential limitations of the data from that profile. Because of the presence of the stone line in that profile, it is unclear whether the sample depth is too shallow, as the stone line may act as a physical barrier that impedes downward movement of meteoric ^{10}Be .

4. 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.

We agree that removing the point would change the empirical relationship, but we don't necessarily think there are grounds for removing the point. The comment above suggests the inventory may be underestimated ("sampled depth is too shallow"), but underestimation of the inventory would lead to a lower flux, rather than an "extremely high ^{10}Be flux", suggested in the comment above. The inheritance is less well-constrained, but unless the inheritance were radically different than what was measured at the other sites, the estimated flux would be or similar order to what we have estimated. Moreover, there are process-based reasons why the flux should be greater at that site (it is at a higher

elevation and hence revives more precipitation). Hence we have opted to retain the point in our regression analyses.

Furthermore, while the precipitation effect is known, it is unclear why the authors plot ^{10}Be flux against other variables—they do not control ^{10}Be flux, and correlation does not imply causation.

Elevation (one of the other variables) is a proxy for precipitation, due to well-known orographic influences on precipitation rates in mountain landscapes. The other variables (besides mean annual precipitation) are snowfall-based data (e.g., they are precipitation data, just from a different data source) and hence are also expected to correlate with meteoric ^{10}Be fluxes. Hence there is a causal mechanism that explains why fluxes should increase with elevation and snowfall.

Minor comments:

Line 55: If there is no Section 1.2, why is Section 1.1 needed? The text could be considered part of the Introduction.

We eliminated the section break.

Line 181: Bulk density should be measured rather than assumed, as it should generally increase with soil depth. If such data are impossible to obtain now, the authors should still propagate an uncertainty for bulk density (e.g., based on prior measurements; Quirk et al., 2024) into the ^{10}Be flux data.

We have incorporated bulk density measurements (reported in Tables S1 and S2). Those measurements do not include clasts too large to fit in our soil corer. Hence we adopted the approach of Deng et al (2021, QSR) and now incorporate > 2 mm clasts using estimates of large clast volumes digitized from field photos and volumes of smaller (but still > 2mm) clasts determined via sieving. We have propagated uncertainty in those measurements following (Deng et al, 2021, QSR) in quadrature, assuming a 5% relative uncertainty in bulk density and the volume of large clasts.

Lines 186-189: It must be emphasized that the sampled soil profile is too shallow and does not reach the depth with ^{10}Be inheritance, and thus the ^{10}Be flux is a lower-limit estimate

in the Copper Creek watershed. Otherwise, readers may believe that the data point in Fig. 3a is a very accurate estimate.

Because of the prominent stone line, it is unclear that the Copper Creek depth profile should look like the other profiles. The stone line appears to be a barrier for physical mixing and it may also limit infiltration/aqueous transport of meteoric ^{10}Be . We discuss the possibility that the inventory is underestimated in lines 371-375.

Lines 234-235: The assumption of a zero y-intercept in the MAP- ^{10}Be flux relationship requires further justification, as only the additive effect can be characterized by this feature (Willenbring and von Blanckenburg, 2010, ESR).

Without forcing the y-intercept to be zero, many of the regression relationships result in negative y-intercept values (Table S5), which results a negative meteoric ^{10}Be flux for some (positive) precipitation values. We used the regressions with the intercepts forced to zero because a model that predicts negative flux values is physically implausible. We added clarifying text to lines 245-247.

Table 3: Does MAP take snowfall into consideration? In general, is the effect of snowfall on ^{10}Be deposition the same as the effect of precipitation?

Yes, precipitation includes both rain and snow. ^7Be deposition has been shown to scale linearly with snowfall (Ishikawa, Y., Murakami, H., Sekine, T. and Yoshihara, K., 1995. Precipitation scavenging studies of radionuclides in air using cosmogenic ^7Be . Journal of Environmental Radioactivity, 26(1), pp.19-36.) and presumably ^{10}Be behaves in a geochemically similar fashion.

Lines 311-315: Why does the MAP-based regression derive an average F_{met} lower than most measured data, while other regression models derive an average F_{met} above most measured data?

There are simply different relationships between flux and the different variables. Because these are empirical relationships, it is difficult to assign physical meaning to the differences, but it is possible that the MAP data product underpredicts the true precipitation. We discuss this possibility in lines 409-410.

Lines 345-350: The soil pH in Copper Creek is much lower than 6, and thus I do not think the authors can claim “minimal chemical loss of $^{10}\text{Be}_{\text{met}}$.”

The pH values are less than six. However, a pH of six or less is not diagnostic of chemical loss of meteoric ^{10}Be . In lines 377-384 we indicate:

Pedogenic loss of $^{10}\text{Be}_{\text{met}}$ has been documented or suspected in low pH soils with substantial chemical alteration (e.g., Pavich et al., 1984; Bacon et al., 2012; Dixon et al., 2018). The partition coefficient for $^{10}\text{Be}_{\text{met}}$ is high when pH is greater than ~ 4.5 (You et al., 1989) but declines at lower pH values. Analysis of a compilation of soil $^{10}\text{Be}_{\text{met}}$ and pH data suggest loss is most likely to occur below a pH of 3.9 (Graly et al., 2010). The 0-10 cm interval sample at Copper Creek site had the lowest measured pH value of 4.3, but pH increased with depth to a value of 5.4 at the base of the soil profile, and pH values for the other sites are higher. Hence we infer $^{10}\text{Be}_{\text{met}}$ loss via chemical leaching is minimal and that mobilized $^{10}\text{Be}_{\text{met}}$ is likely confined within the sampled horizons. Minimal chemical loss of $^{10}\text{Be}_{\text{met}}$ was also inferred for moraines near Pinedale, Wyoming, USA, which has a climate and ecosystem broadly similar to the East River (Clow et al., 2020).”

Based on the findings of Graly et al (2010), and the measured pH values, we think it is acceptable to infer that there is minimal chemical loss of meteoric ^{10}Be from the Copper Creek profile, especially because the pH increases with depth to values that are well within the range where meteoric ^{10}Be retention is expected.

Line 379: Why should elevation control F_{met} ?

The flux of meteoric ^{10}Be is strongly influenced by precipitation rates. Due to orography, precipitation rates increase with elevation. Hence elevation is expected to influence the flux of meteoric ^{10}Be .

Line 392 and Fig. 7: Should this be “reduce,” not “increase”?

Thank you for this comment. We have corrected the issue, as explained in our response to the next comment.

Lines 395-397: Please describe the solar modulation function used by Heikkilä and von Blanckenburg (2015). Additionally, the authors have used incorrect data from Zheng et al. (2024). A higher solar modulation function (500 MeV) should lead to lower ^{10}Be flux, not higher as described here. After briefly checking Zheng et al. (2024), the 500 MeV scenario

may actually be a geomagnetic minimum scenario and is not consistent with modern conditions. The authors must re-check the paper to get the correct model values for comparison. In any case, the authors are encouraged to normalize all fluxes (model and measured) to the same solar modulation function for comparison.

Thank you for this comment. We have rechecked the relevant papers and now explain the solar modulation functions used in the different models and explain how the predictions were normalized for comparison purposes in lines 258-264.

Line 459: According to Fig. 7, it is “higher,” not “lower.”

Given the revision to the flux values based on the comments regarding bulk density and large clast volumes, this comment no longer applies.

Others:

Fig. 7: The uncertainty of the measured data is too small to be realistic. I assume only the analytical uncertainty of ^{10}Be is considered here. However, given the uncertainties in bulk density, fraction of >2 mm grains, soil erosion, and chemical leaching (pH effect), the real uncertainty should be much higher but is not included here.

The error bars in Figure 7 are now larger, as they include uncertainty in: AMS measurements, bulk density, and large clast volumes.

Fig. 8: I suggest adding another line—the average measured F_{met} of all four profiles (excluding Copper Creek). This average value is useful, and may be very close to the MAP-based estimate and the model from Heikkilä and von Blanckenburg (2015).

Given our re-analysis to account for bulk density and large clast volumes, the model-based flux from Heikkilä and von Blanckenburg (2015) are fairly similar to the watershed-average values based on several of our predictors (specifically elevation and snow water equivalent) and we point out those similarities in the Discussion (lines 430-440).

7 Data availability: Data must be made available upon publication.

The data are available in the U.S. Department of Energy ESS-DIVE repository and the manuscript includes a doi that links to the dataset.

This work quantifies meteoric ^{10}Be fluxes averaged over the last ~20,000 years within an alpine watershed of Colorado's (USA) Elk Mountains. The authors determine these fluxes by sampling meteoric ^{10}Be inventories in glacial moraines of known age (from previously determined *in situ* ^{10}Be exposure dates) across an elevation gradient of 714 meters. To successfully capture full ^{10}Be inventories, the authors must be sure that that: soil pH is not too low (i.e., <4), erosion is minimal or quantifiable, inherited ^{10}Be is minimal or quantifiable and samples are continuous and deep enough to capture full inventories. Sample grain size is also significant and sediment in the <2 mm fraction is ideal since meteoric ^{10}Be is known to preferentially adsorb to finer grains. I am satisfied that the authors carefully account for these factors, and, to the best of their ability, capture complete ^{10}Be inventories. Their Copper Creek site offers some complications/challenges, but the authors explain how they account for those challenges.

The results from this manuscript demonstrate a strong elevation-dependent relationship for ^{10}Be flux in the East River watershed that is closely tied to patterns of orographic precipitation (rain and snow). This flux is vulnerable to local-scale influences, and the authors demonstrate that measured inventories are significantly different than fluxes by much coarser resolution, global models. This work underscores the need for locally calibrated meteoric ^{10}Be fluxes if the nuclide is to be used as a tracer of surface processes.

This manuscript is well-polished in its current form. I offer some minor comment and points of that require clarification below. Following the minor revisions associated with those comments, I think this manuscript will ready for publication. My recommendation is "Accept with Minor Revisions."

Thank you for taking the time to review our manuscript and provide helpful feedback.

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Line 50: Add references to Nicole West's work in the Shale Hills CZO

We have made this addition.

Line 148: This does not seem sufficiently deep. See note for Figure 3.

We could not make the shallowest bit deeper by hand because of the presence of a stone line, and due to the remote location, could not bring in heavy equipment to make the pit deeper. We have discussed the implications (also in response to reviewer 1 comments) in lines 371-375.

Line 152: This combining of splits is not clear. It sounds like samples from multiple depths were combined, but that is not what I think you are describing. Clarify.

Samples from multiple depths were combined. We have edited the text to clarify in lines 149-153.

Line 181: Are there not bulk density measurements from your own sites?

There are bulk density measurements for each soil horizon, which we now report in Table S1. Based on this comment and those from reviewer 1, we have incorporated bulk density and uncertainty in bulk density in our calculations.

Line 209: Report your assumed value of D here?

We now report the diffusion coefficient ($5.8 \times 10^{-4} \text{ m}^2 \text{ yr}^{-1}$) in line 226.

Figure 3: Despite relatively shallow profiles, it does seem that you sample to a sufficient depth for the majority of moraines. Your deepest samples generally approach concentrations representative of inheritance. Copper Creek (with the stone line) is the obvious exception.

We agree with this interpretation.

Figure 5: Compelling regressions. These seem like solid relationships. The relationship with elevation is very closely intertwined with the other three variables due to orographic effects, though.

We agree. We specifically note the inter-related nature of the four independent variables in lines 396-397: “ F_{met} is highly correlated with elevation, MAP, snow depth, and SWE, which is unsurprising because these variables are related to one another via orographic controls on precipitation rates.”