

Interactive comment on “Calibrating a long-term meteoric ^{10}Be delivery rate into Western US glacial deposits through a comparison of complimentary meteoric and in situ-produced ^{10}Be depth profiles” by Travis Clow et al.

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Received and published: 22 August 2020

Thank you for your detailed review of our manuscript (gchron-2020-14) entitled Calibrating a long-term meteoric ^{10}Be delivery rate into Western US glacial deposits through a comparison of complimentary meteoric and in situ-produced ^{10}Be depth profiles.

The three reviewers provided great, thorough reviews which will enhance the readability and impact of this manuscript after revisions are made. We largely agree with the

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majority of the reviewers comments and suggestions and summarize the final author 'major' comments for revisions as follows:

- The erosion rates used to calculate the meteoric fluxes are no longer the average between the constant and transient modeled denudation rates. Instead, we only use the average transient denudation rate (with uncertainties accounting for chemical weathering mass loss) for all calculations, as it is geologically incorrect to use the average rate between the constant and transient model runs – only one can be correct. We have added text to explain and justify this treatment, and have a note to the reviewers below that explains our rationale.

- Paleomagnetic intensity normalizations for the calculated fluxes for each moraine will now be calculated for the residence time of the soil profile down to the e-folding adsorption depth of meteoric ^{10}Be (20 and 30 cm, and thus 6 and 24 kyr, for Pinedale and Bull Lake moraines, respectively) to properly weight and capture paleomagnetic variation effects on the production of meteoric ^{10}Be over time (instead of over the entire ages of the moraines). The revised normalised meteoric fluxes now agree within uncertainty and are closer to the atmospheric model flux estimate. A table will be added to the Supplement that lists all factors employed in the Monte Carlo simulations, along with the MATLAB code used for the Monte Carlo simulations, so that future readers can also carry out calculations and normalize fluxes themselves.

- The Monte Carlo approach will be properly introduced and described before presenting results. We will remove precipitation rate uncertainty (previously through an overly credulous paleo-precipitation rate estimation) in the simulation and associated text in the Supplement.

- All typographical errors will be fixed and reported units corrected for the main equations used for this work. Equation 4, which previously had a typo by which an addition sign was instead a multiplication sign, has been fixed. Equation 4 will also now include radioactive decay and meteoric inventory terms, and equation 3 will be removed.

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This did not result in any appreciable change to our calculation results (as previously described).

- Soil mixing discussion will be combined with the section on Cosmogenic Nuclide Profile discussion and be expanded upon.

- The Introduction, Methods, and Results sections will be considerably re-organized so that there is no ambiguity between sections. This will enhance the readability and flow of this work substantially.

- We choose to leave our treatment of inheritance corrections as is, but will now explicitly define our treatment both qualitatively and analytically in the proper section.

See below for more detailed responses to your specific comments by line number. Please let us know if there are any questions about our suggested revisions.

Sincerely,

Travis Clow, Jane Willenbring, Mirjam Schaller, Joel Blum, Marcus Christl, Peter Kubik, and Friedhelm von Blanckenburg

Important note to reviewers and editor:

We have chosen to alter our approach regarding the known erosion rate for these moraines. Previously, we chose the known erosion rate as the average between the recalculated transient and constant denudation rate models of Schaller et al. (2009a) after accounting for potential chemical weathering mass loss. We have realized since our first submission that this is geologically incorrect – only one of the models can be valid – thus using the average between the two is erroneous. Instead, we now use the recalculated average transient denudation rates for all calculations, as this model is much more likely to be correct. Our justification is as follows:

Moraines are deposited in a triangular shape at the terminus of a glacier. Today they have more of a concave down parabolic shape. These two geometries have very dif-

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ferent slopes and curvatures to them, which means the erosion rates must change through time. If you apply a linear (or nonlinear) hillslope diffusion law to understand moraine erosion, then the erosion rate equals the hillslope diffusivity of the moraine multiplied by the second spatial derivative of the topography (i.e. the curvature of the topography, or $dh/dt = k \text{ grad}(h)$). Thus, the erosion rate depends on the curvature of the moraine topography.

Going back to the initial triangular shape of a moraine, the apex of the triangle (and the bottom corner where it sits on the ground) have the highest curvature when initially deposited. This part of the moraine will erode quickly at the start. As the apex flattens out and the bottom corners fill in, the curvature decreases, so the erosion rates will decrease. Erosion rates continue to decrease with time as a moraine flattens. Because of this, the erosion rate of moraines must be transient, with highest rates initially after deposition. All diffusion problems (e.g. temperature, hillslopes) respond this way (fast response at first, then slower response later) when adjusting to a non-equilibrium initial condition.

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Response to Reviewer 1

Line 25: Reword “Requires careful consideration” to something less vague.

Removed ‘careful’

Line 31: “Target atoms”?

Revised to “target nuclei”

Line 34: Also Al-OOH (e.g. Graly et al., 2010)

Revised to “Fe- and Al-oxyhydroxides”, citation added

Line 43: I might avoid implying that most previous work is flawed here. These issues have been discussed and debated since the inception of the method with the work of

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Pavich and Monaghan.

Revised to “not all of which was possible in many of these studies”

Line 58 (and elsewhere): A priori knowledge refers to knowledge derived from first principles, etc. Data from another study is not a priori knowledge.

Revised to “previous knowledge” here and elsewhere.

Line 60 (and elsewhere): Why “back-calculated”, why not simply “calculated”?

We initially chose to use the phrase ‘back-calculated’ as to be up-front that we are rearranging equations to solve for delivery rate (i.e. the calculated flux will always be a ‘perfect match’ for a known erosion rate), since all other meteoric studies to date utilize these equations to solve for erosion rate. However, this is a matter of taste, and can also be described as “calculated”. We have since revised this to “calculated” here and elsewhere.

Line 140: When you say “homogenized”, do you mean that two aliquots from the sequential extraction were mixed together? I assume you must, since nowhere in the results do we see data from the separate sequences. This needs to be more clearly stated.

Correct – the text now reflects this to be more clear.

Line 156 (and elsewhere): I think “e.g. Willenbring and von Blanckenburg, 2010” would suffice. They were hardly the first or the only authors employ this concept of steady state (or the other concepts they receive sole credit for throughout the manuscript).

Revised accordingly throughout the manuscript; added Brown et al., 1988 in this instance.

Line 173: You may ignore the decay effect, but must you?

No. Per Reviewer 2’s comment on this matter, we now remove Eq.3 and include decay

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and inventory in Eq. 4 for applicability to older settings.

Equations 3 and 4: I believe it is standard to use the interpunct for multiplication and a full line for division. Equation 4 is wrong. The density term from Eq. 3 has disappeared, and the water flux term should be added not multiplied. I sincerely hope this is a typographical error, not an error that was implemented in the Monte Carlo model. But the authors should certainly double check this.

The multiplication of the water flux term is a nasty typographical error. It has been fixed. Likewise, the density term should not be in Eq. 3. Density is factored into the erosion rate, which was not described with units (g/cm²/yr) on line 163 previously! This has been fixed as well.

Line 192: The authors need to explain how they treated inheritance (i.e. with an equation). The inherited fraction is also eroded and leached to depth, so it is not clear which approach was taken. I think inheritance should be included in equations 1-4, rather than tacked on separately without an equation.

Inheritance (lowest concentration measured) was subtracted from all concentrations measured. We have added text that defines this explicitly when defining [10Be]_{reac}, as well as for the measured inventory, in this section.

Section 3.3: This section, as written, belongs in results not methods. In its place, a proper description of the Monte Carlo methods is needed. As it is, I don't see what the Monte Carlo accomplished that could not be done with error propagation.

This section has been moved to results – in its place is a proper description of the Monte Carlo simulation, which we use to determine uncertainties for our calculated fluxes. Traditional error propagation could also accomplish this goal. However, we do not have great constraints on K_d and evaluating the equation over the entire range of possible values in this manner provides a more realistic estimate on the uncertainties.

Line 205: I am confused to as which equation (1,3, or 4) was actually used to generate

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the results presented. It sounds like all of them where, though the caption in figure 3 indicates eq. 4 was. The methods here need to be far more clearly presented.

In the interest of applicability of this method to both older and younger settings, we have removed Eq. 3 entirely and included inventory and decay in the old Eq. 4. We now explicitly state that this equation is used to generate the results presented.

Lines 210-220: This topic needs to properly treated in the introduction. The delve into the literature to characterize the “debate” and the various approaches is not appropriate to the methods section.

Agreed, majority of this section has been moved to the Introduction

Section 3.5: The authors seem to take it as granted that paleomagnetic intensity exerts linear and predictable control on paleo ^{10}Be flux. From what I can tell, this is far from certain. Looking at global datasets such as Frank et al. 2008, the two correlate but with significant deviation and scatter, including time periods (such as OIS 5e) where the correlation seems to break down entirely. I can't help but notice that the depositional fluxes derived from the two moraines are far closer to each other in raw form (Figure 3) than after paleomagnetic correction. What the authors seem to have done (line 259) is to simply use the average paleomagnetic intensity over the moraine age. But because erosion and leaching effects are cumulative, this should actually be weighted towards the more recent flux. If they wish to keep it all, the authors need to propagate the paleomagnetic flux correction through their model. This section also seems to mix introductory background with methods and results.

The production rates dependence on paleomagnetic intensity is certainly not linear – even if calculated from paleomagnetic stacks using the “Elsässer formula”. However we avoid doing this by instead reconstructing paleo-production from the measured ^{10}Be stack (from marine cores) from Christl et. al (2010).

The comment that before correction, the depositional fluxes are quite close to one

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another, yet deviate more after the correction however made us revisit the estimated time scale over which we have done this correction. This point is a great one that we agree with – that one should weigh these corrections towards the most recent flux. We now normalize over 6 ka and 24 ka for Pinedale and Bull Lake moraines, respectively, based on the residence time for the soil from the surface to the e-folding depth (~ 20 and ~ 30 cm, respectively). This is a more realistic correction. Now, the corrected depositional fluxes for each moraine stay relatively close to each other (which one would expect for moraines so close to one another) and overlap within uncertainty.

After considering all reviewer comments and internal discussions, we have also decided to use the average transient erosion rate, recalculated from Schaller et al. (2009a), for all calculations, as it is more geologically correct than using the average between the constant and transient denudation rate model runs. Please see our note to the reviewers above with a detailed justification.

Lines 269 & 276 / Table 1: The inventories should be reported at an appropriate precision and include propagated error calculations.

This has been fixed.

Line 278: I don't think the lowest concentration is the inheritance. The inheritance is the average of all of the values measured below the 60 cm (in this case).

We chose to keep the lowest concentration as the inheritance to avoid negative inheritance-corrected 10Bemet measurements at depth.

Line 287 (and elsewhere): I personally find the need to call out other sections in advance to be a symptom of poor organization. The paper should flow naturally without the need to do this.

After re-organization of sections as advised by both reviewers, we have greatly reduced section callouts throughout the manuscript.

Line 293: Graly et al. 2010 tested this claim and found that grain size effects could

explain subsurface maxima in none of the 29 soil profiles analyzed. A far better explanation is that ^{10}Be is incorporated into the lattices of newly forming clays and oxyhydroxides at depth (e.g. Barg et al., 1997). Though in this case, the increase is fairly trivial and the depth and clay content small.

This information has been added to the text as follows:

“This subsurface maximum could be the result of smaller grain sizes within this horizon, as these grains have a higher surface area per unit mass and can exchange ions more easily (Brown et al., 1992; Willenbring and von Blanckenburg, 2010). An alternative explanation invokes enhanced ^{10}Be incorporation into the lattices of newly formed clays and oxyhydroxides at depth (e.g. Barg et al., 1997), though the increased clay content at this depth is not appreciably large.”

Section 5.1.2: This section would greatly benefit from having the Monte Carlo approach properly explained in the methods. As it is, the Monte Carlo is something of black box that gives surprising results on its own accord.

We now include a paragraph introducing and summarizing the Monte Carlo simulation in the Methods section. We are also now explicit that the Monte Carlo simulation is used to determine the uncertainties on our calculated fluxes.

Line 319: Remove “At first inspection, it appears that”.

Removed

Line 320: Remove “In either case”.

Removed

Line 322: This is a surprising and novel observation that deserves further depth of treatment. Could you possibly mix coarse sand and fail to mix silt and clay? In some cases, patterned ground will mix pebble and cobble sized clasts at the hexagonal boundaries, excluding smaller grain sizes. Some delving into the cryoturbation liter-

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ature seems warranted. Likewise, the second explanation needs further treatment. It is true that you only need to mix a declining profile for the in situ, whereas everything drops in at the top for the meteoric. But could you really homogenize one but not the other from these initial shape considerations alone? The reactive flow explanation proffered seems a bit wanting as well. How would reactive flow transport everything to the top of the otherwise mixing layer? This section would be much richer if a numerical model/calculation could be provided for any of these possibilities .

It is curious that smaller grain sizes wouldn't be mixed, but larger grains would – as finer grains are thought to have higher mobility than coarse grains and a tendency to migrate upward in a soil profile (e.g. Gray et al., 2020). A cryoturbation-related explanation would likely only explain a lack of mixing in the uppermost soil, however based on the in situ-produced ^{10}Be data, we should expect a mixing signal down to ~ 40 and $\sim 50\text{cm}$ for these profiles.

Reactive flow wouldn't transport everything to the top of the “in situ mixed layer”, rather we are referring to a continual input of $^{10}\text{Be}_{\text{met}}$ at the surface overwhelming any potential “meteoric mixed layer”. In this case, even if there is mixing of these smaller grains going on in a similar fashion as the larger size fractions analyzed for in situ produced ^{10}Be , before this mixed concentration can be set in stone, it gets ‘reset’ by the addition of newly-delivered $^{10}\text{Be}_{\text{met}}$, starting with the uppermost soil interval and then propagating to depth via reactive flow or possibly through macropore permeability. This requires reactive flow timescales to be much shorter than mixing timescales, which is a reasonable assumption given that typical soil mixing rates are low (cm's per century [Kaste et al., 2007]) versus the rapid adsorption of beryllium (~ 1 day [Boschi and Willenbring, 2016]) and fast permeability rates (as a rough proxy for reactive flow rates; $\sim 5 \times 10^{-3}$ m/s) for soils with these grain size distributions. We have revised the text to be more explicit about this. We are not certain how to numerically model such a scenario beyond back of the envelope style calculations by arbitrarily assigning diffusion and reactive flow rates (as described above), neither of which are known for this

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site. Modeling using an existing framework like Be2D or LSD Mixing Model would be fantastic, but is not possible at present due to a higher degree of model sophistication needed, which is beyond the scope of this work.

Please note that we have now decided to merge the Soil Mixing and Cosmogenic Nuclide Profile discussion sections for consistency and readability.

Line 348: An 100% additive precipitation control on flux is almost certainly not possible, as some dry deposition will occur, and complete scavenging and thereby dilution is likely in the largest storms. However, I think this is the wrong framework to consider. The paleo-precip factor is from a glaciological model and therefore quite uncertain. Nor is there any certainty in assuming that the “Graly Curve” for the Pleistocene was the same shape as that of the modern. Only after several more studies of this nature, will these sorts of things start to flesh out. I would recommend simply comparing to the modern and mentioning the paleo-precipitation estimate in the discussion. But the second line on figure 3 and the “uncertainty” term on Table 4 seem to attribute too much to something we still know too little about.

Agreed, we have removed paleo-precipitation as an ‘upper bound’ in our calculations and instead mention the potential for paleo-precipitation rate to be higher in the text.

Discussion: The deposition of recycled ^{10}Be on dust is neglected in the analysis. Are there any estimates of Pleistocene dust flux in this region? If not, the uncertainty introduced by this unconstrained parameter should be at least mentioned. The authors don’t make any mention of the fact that their two moraines differ by a statistically significant margin. As I mention above, the difference is almost entirely due to the paleo-flux correction. So, if they keep the paleo-flux correction, they need to come up with something that varies in opposition to paleo flux to explain their results.

We mention on line 88 that eolian flux is insignificant, as determined by Sr isotope measurements of the moraine soils and dust sources from previous workers. Please see above for our new paleomag intensity normalization treatment.

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Line 636: “0Be”

Fixed

Table 4: There is uncertainty inherent in the Graly curve apart from the + 20% attributed to paleo-precipitation. I believe this is true of the Heikkila GCM output as well. Per above, I think that simply treating the paleo-precipitation model as an upper bound is an overly credulous approach.

We have removed paleo-precipitation as an upper bound in the text, the table, and the MC simulation.

Supplement: I don't know why this information needs to be supplemental. The paper is not over long and I see no reason why this information cannot be integrated into the main text.

We have removed the section on paleo-precipitation rate, but have chosen to leave the Updated Independent Age Constraints section in the supplement as it is (necessarily) too detailed for the main text.

References Cited: Barg, E., D. Lal, M.J. Pavich, M.W. Caffee and J.R. Southon 1997. Beryllium geochemistry in soils; evaluation of $^{10}\text{Be}/^{9}\text{Be}$ ratios in authigenic minerals as a basis for age models. *Chemical Geology*, 140: 237-258.

Egli, M., D. Brandová, R. Böhlert, F. Favilli and P.W. Kubik 2010. ^{10}Be inventories in Alpine soils and their potential for dating land surfaces. *Geomorphology*, 119: 62-73.

Frank, M., B. Schwarz, S. Baumann, P.W. Kubik, M. Suter and A. Mangini 1997. A 200 kyr record of cosmogenic radionuclide production rate and geomagnetic field intensity from ^{10}Be in globally stacked deep-sea sediments. *Earth and Planetary Science Letters*, 149: 121-129.

Graly, J.A., P.R. Bierman, L.J. Reusser and M.J. Pavich 2010. Meteoric ^{10}Be in soil profiles – a global meta-analysis. *Geochimica et Cosmochimica Acta*, 74: 6814-6829.

Interactive comment on Geochronology Discuss., <https://doi.org/10.5194/gchron-2020-14>, 2020.

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