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

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 10Be (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 10Be 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 themselvess.

- 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.

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-

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 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 3

Line 34: Al-oxyhydroxides, too? See both Jungers et al., 2009 and Graly et al., 2011 in your references.

Correct, this has been fixed; citations added.

Lines 48-51: Consider rewording the sentence starting with " 10 Be met shares a. . ." To me it is a little confusing and I think I only understand it because I'm already familiar with the differences between in situ and meteoric 10 Be.

This sentence has been reworded as follows:

10Bein situ shares a cosmic ray origin with 10Bemet but differs in production method; it is produced within crystal lattices in surface rocks and soil, rather than in the atmo-

sphere, with a well constrained total production rate of 4.01 atoms g-1 yr-1 at sea level, high latitude (Borchers et al., 2016), and is characterized by full retentivity and known production pathways with depth

Line 68: I think you mean a posteriori here since the knowledge is based on empirical evidence. Could just simplify it to "...utilize previously determined effective. . ." Same spirit goes for other instances of a priori later in manuscript.

Great catch – we have decided to change this to "previous knowledge" in all instances.

Line 68: "...50-year. . ." There are small grammatical and punctuation errors peppered throughout the manuscript. Nothing that derails the reading, but the authors should do a couple proofreads. I'll point out ones that jumped out. Not really being a grump here - just want to help.

Thank you – we have carefully re-examined and edited the text for these errors thanks to suggestions and catches like these from all reviewers. It is a bit embarrassing!

Line 68: When talking about precipitation here, you are really reporting an annual depth rather than rate (as written).

We have added explicit units of m a-1 here.

Line 69: To me, the use of "proximal" here is confusing since that word has facies implications in geology. Just saying "nearby" might be clearer.

Good call, this has been changed to "nearby" in all instances

Line 78: Just to be clear, it sounds like you did not measure pH of your samples? I think it's reasonable to use the nearby measurements, although in situ pH measurements would be nice considering the potential impact on 10 Be met mobility.

pH was unfortunately not measured in these samples :-(

Line 83: The suggestion here that the deepest samples are unweathered seems some-

what counter to the later argument that inherited meteoric concentrations are due to reworked material. Is there another model for inheritance that could work?

Not that we are aware of. That the deepest samples are unweathered is actually an assumption of Taylor and Blum, 1995 and is in reference to the >2mm size fraction, which is not what is analyze for either in situ-produced nor meteoric 10Be.

Line 92: I find “proximal” confusing again here, too (cf., Line 69). Do you mean nearby terraces or terraces that are proximal to the rangefront. Perhaps it doesn’t matter, but I’d encourage precision with the language in both cases.

We have replaced proximal with nearby, as suggested.

Line 95: The section that starts with “We recalculated. . .” seems like it should be part of the Methods section. There are several instances of methodology being presented either too early (such as here) or too late (such as the treatment of inherited concentrations), and I think that restructuring where these bits are presented would improve the clarity of the manuscript.

Agreed, we have since restructured and re-organized this manuscript considerably based on all reviewer suggestions. This entire section is now in the Methods section.

Line 102: Consider removing “...are likely. . .” All the moraines have experienced erosion since Deposition.

Removed.

Line 103: Stray hyphen in “...for-contiguous. . .”?

Removed.

Lines 105-110: It seems like the averaging times of the methods may also play a role in the different results.

Indeed. We have added text to this effect.

Line 113: "...were recalculated. . ." again suggests a section that may better fit in Methods. Some or all of the approach outlined in the Supplementary Materials could be integrated into the main text to good effect.

This information has been reduced and moved to Methods – we chose to keep the Supplementary Material related to this there as it is (necessarily) overly detailed for the main text.

Line 116: I appreciate the consideration of transient denudation that you discuss here (in terms of a sensitivity analysis of your results), but you don't clearly justify why you set up the transient denudation the way you do. Why waning instead of cyclical, for example? Just justify your approach with a sentence and/or reference.

These scenarios are not prescribed by us, but rather by the model of Lal and Chen (2005) that Schaller et al. (2009a) uses. Nonetheless, we have added some additional text here describing the rationale they used in considering each scenario. As described in our note above, we have now chosen to use the average transient denudation rates (accounting for potential chemical weathering mass loss) for all calculations, instead of the average between the constant and transient denudation rates, as it is a more geologically sound approach. We now describe our rationale in the text.

Line 130: "...erosion rate decrease. . ." From the original pub? Or is this the sum decrease of both recalculating and accounting for mass loss due to chemical weathering. Not immediately clear to me.

We have removed this sentence from the manuscript.

Line 147: "...minor adaptations. . ." Like what? You are so detailed in the preceding sentences, why not report your specific adaptations? Inquiring minds want to know!

We have added this information to the text as follows:

The Be in the water leach solution was extracted and purified by a form of the ion exchange chromatography procedure from von Blanckenburg et al. (2004) that was

adapted for meteoric 10Be purification by passing the leachate through anion (2 ml of BioRad 1x8 100-200 mesh resin) and cation (2x 1 ml BioRad AG50-X8 200-400 mesh) exchange resins, precipitated at pH ∼9 using NH4OH:H2O (1:1), washed twice with 2 ml ultrapure water with centrifugation in between, mixed with AgCl, centrifuged and dried overnight, and finally oxidized over open flame (>1000 °C; modified from Kohl & Nishiizumi, 1992).

Line 157: "...residence time...less than the depositional age..." I wonder if you can quantify this in some way to show that it holds for your site (seems like it certainly does). Can a residence time be inferred from the difference between your modeled flux rates and a "naive" flux rate determined by just dividing total inventory by moraine age. The discrepancy between those two numbers may be telling you something about how much 10 Be met is being "lost" since deposition. Perhaps this isn't important, but it could be interesting in comparison to some of the diffusion modeling and other prior work that tried to quantify degradation rates for the moraines.

We now calculate the residence time of the soil from the surface to the e-folding depth as 6 ka and 24 ka for the Pinedale and Bull Lake moraines, respectively, and use these timescales for paleomag normalizations.

Line 163: Units for E?

Added in (g/cm2/yr).

Line 165: No ro term in Equation 1. I would recommend going through equations carefully to make sure they are correct. I imagine this is in the realm of typos rather than anything that made it into your modeling.

It is factored into the erosion rate (which we neglected to define the units of) – this has been fixed.

Line 175: Check unit analysis of Equation 3.

Fixed.

Line 186: There is no N surf in Equations 1 & 3.

A remnant of an earlier draft of this manuscript – this has been fixed to 10Be[reac].

Line 195: Section 3.3 reads more like Results rather than Methods.

This section has been moved to Results. In its place is a proper explanation of the Monte Carlo simulation.

Line 196: Nice agreement between flux rates! Remarkable stability over these timescales. Encouraging for future application of this isotopic system if one's local flux rate is known. Good stuff.

It was quite a welcome surprise to us! Even after using the transient erosion rates instead of the average between the constant and transient erosion rates for our calculations, the raw flux rates still agree remarkably well.

Line 204: I feel like I've lost track of what equations you are now reporting the results from. Perhaps a small table could clarify the differences between the outputs of Equations 1 vs. 2 vs. 4?

We have removed Eq. 3, revised Eq. 4 to include decay and inventory, and are explicit that this equation is used for the MC simulation.

Line 216: "...type of estimate. . ." not "...type of estimates. . ."

Corrected.

Line 253: Should this bit about rescaling other approaches go into Methods?

Indeed – it has been moved to Methods.

Line 269: I think you really need to bring the discussion of potential inheritance into how you build your equations in your Methods. Can you just treat inheritance explicitly there? Then, in Results, you can certainly report apparent inheritance and discuss how that may occur.

Inheritance is now directly factored into the equations and reported accordingly in the Methods section.

Line 270: I believe there is a typo in your units for 10 Be inventory in Table 1. Check and correct.

Fixed.

Line 291: Think you mean “illuviation” not “eluviation” here. You are referring to removal of clay from above (eluviation) and the concentrating of clay in this horizon (illuviation).

Indeed, good catch!

Line 304: “...reworked till. . .” Just another flag to consider whether this idea of reworked till jives with the composition and state of weathering in your deepest samples.

See response to line 83 comment.

Line 320: “...different diffusion coefficients. . .” Seems like this would manifest itself in some way beyond just the 10 Be met depth profile. You’d see a trend in grain size with depth from the surface within the mixing layer or something. I think the difference between mixing timescales and the rate at which 10 Be met is being translocated from the surface is more likely. For that matter, the formation of distinct clay horizons in at least the Pinedale suggests that soil horizonation happens faster than mixing (as inferred from the 10 Be is profile). These are cool results with neat geomorphic and pedogenic process implications. Jungers et al., 2009, see a similar thing in hillslope soils of the Great Smoky Mountains.

Great inference – we agree that this is indeed a likely possibility and have added a couple sentences to this effect in the text. Please note that we have also decided to combine the Soil Mixing and Cosmogenic Nuclide Profile discussion sections for consistency and readability.

Line 338: Where does the value of 128 cm/yr come from (in terms of both geography

and a citation)?

Citation added

Table 1: Check units for inventories in the final column.

Fixed.

Nice work - this is very cool stuff!

Thank you!

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