Geochronology Discuss.,
https://doi.org/10.5194/gchron-2020-14-AC2, 2020

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 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

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 different 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 2

General technical comment: There are numerous grammatical errors throughout the text. I recommend the authors read through the text carefully and fix places where there are missing words, or verbose text that could be made more concise.

With the helpful suggestions and guidance from the reviewers, we believe all grammatical errors are now fixed in the revised manuscript.

Title: change “through a comparison of complimentary” to “by comparing complementary”

A welcome change! Revised.

Line 23: How do these compare to the model fluxes of Heikkila and von Blanckenburg for the study area? Are they wildly different, or in close agreement? Would be good to mention this in the abstract for those readers who might use the modeled fluxes.

The calculated fluxes are both lower and higher than that estimated by Graly et al. (2011) for the Pinedale and Bull Lake moraines, respectively, and are lower than that predicted by Heikkila and von Blanckenburg (2015). This is a bit too specific for the abstract. Rather, we have revised the abstract text to note that a considerable discrepancy exists for both methods at this site, neither of which match the calculated fluxes within uncertainties.

Line 24: Can the authors add the ages of these moraines to remind the reader over what timescale they are averaging over for the fluxes?

Added

Line 30: add uncertainty of +/-0.01 to (readers unfamiliar with 10Be might want to know the certainty of this half-life

Added

Line 31: be more specific about which particles (i.e. 14N and 16O)

Added

Line 34: Add both Al- and Fe-oxyhydroxides (Graly 2010 show that Al has a stronger relationship to 10Be concentrations)

Added, along with citation.

Line 43: I would cite Graly et al 2010 who did an extensive analysis of the controls on 10Be concentrations in soil profiles from around the world.

Citation added.

Line 68: If it is windy, this implies either removal or deposition of fine particles over time, which could influence 10Bem concentrations. Can the authors say anything about dust

delivery to this site?

We note that dust delivery is insignificant to this site, based on Sr isotope measurements of these moraine soils and dust sources from previous workers, on line 88.

Line 93 and 99: can the authors give uncertainty estimates, as this should factor into the uncertainty of their 10Bem delivery rates?

The model of Schaller et al., 2009a does not permit for uncertainties in the independent age constraints when calculating denudation rates. These uncertainties only matter for the recalculated independent age constraints, and thus in situ produced 10Be effective erosion rates, which indirectly affect meteoric 10Be delivery rates. The flux we calculate solely depends on the estimate of denudation rate, not moraine age.

Line 108: change studies' to study's; and sites to site's

Fixed.

Lines 123-124: Why do the authors want to compare the Schaller denudations rates with 10Bem erosion rates? The 10Bem erosion rates (calculated using equations of von Blanckenburg et al., 2012) are not always comparable to denudation rates (they would need 9Be concentrations to calculate these rates). One could perhaps evaluate the chemical weathering component as the difference between the erosion and denudation rates.

We do not aim to compare the Schaller in situ denudation rates with meteoric erosion rates – we instead do as described – using the potential chemical weathering mass loss calculated by Schaller et al, 2009b to account for this component of the denudation rate of Schaller et al., 2009a in order to more properly compare "in situ-produced 10Be erosion rates" vs. meteoric 10Be erosion rates. Now that we use transient erosion rates for all calculations (see above), accounting for this potential chemical weathering mass loss is done so via the uncertainty for these transient erosion rates.

Lines 128-130: Are there no major element data or weathering indices calculated for different depths within these profiles? In the introduction, the authors stated that they had all the data they needed to evaluate loss due to leaching and weathering.

Major element data is available from Schaller et al., 2009b, however we are unable to determine if this potential mass loss occurred above or below the cosmic ray attenuation pathway. The weathering rate is based on weathering loss in profile and material removed by denudation – with the rate based on the average of the four samples in the surface layer for each moraine (Schaller et al., 2009b). Since we do not know at which depths the material removed by denudation came from, we instead take this chemical weathering mass loss to be the uncertainty in the in situ-produced 10Be transient erosion rates used in all calculations.

Lines 138-139: Please mention that the amorphous and crystalline oxide fractions were re-combined before the next steps.

Added.

Line 141: âĹij200 ul of 9Be carrier doesn't really provide any information because we don't know the concentration of the carrier solution. It's better to report the total mass of 9Be added to each sample.

We now report the 9Be mass added.

Lines 142-143: Rather than repeating the previous sentence, say "The samples were then dried down and dissolved in an additional 1 mL 50% HF solution, repeated once."

Revised.

Line 161: what unit do the authors use for erosion rate?

g/cmˆ2/yr. This information has been added to the text, good catch.

Line 165: rho is not used in equation (1), so the authors should introduce it in the next sentence, before equation (2). They also give the value for rho twice, but it is only needed once.

This has been fixed.

Equation (2): the authors should add in the correction for inherited 10Be into the equation.

We instead now explicitly describe the inheritance correction before presenting these equations.

Lines 172-173: It is best to include the decay effect in the equation. It might be negligible in this case, but may not be in older settings where this method may be applied in the future.

Agreed, we have now removed Eq. 3. The old Eq. 4 has replaced Eq. 3 and has density and inventory terms accordingly.

Line 181: use 'calculation' rather than 'back-calculation'

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 given 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.

Equation (4): This equation is dimensionally incorrect as written. By rearranging Eq. 3 of von Blanckenburg et al. (2012), the erosion term should be added to the discharge term, not multiplied. It is also unclear what units the authors used for the variables because a water flux in m/yr does not cancel out with the partition coefficient, which is in L/g, unless a density term is inserted.

The multiplication of the water flux term is a nasty typographical error. It has been fixed. Additionally, while we report discharge units as m/y, our calculations actually use L/m2/yr. Great catches – we have fixed these typographical issues and report units properly so everything is dimensionally consistent.

Line 186: The authors previously defined [10Be]reac, so they don't need to re-introduce it here. The authors also don't use the term 'Nsurf', which is from the Willenbring and von Blanckenburg (2010) equations.

This has been fixed.

Lines 160-194: The text would read more clearly if the authors first introduce the equations and variables, and then parameterize the equation in a paragraph following the theory. If the authors change the format to theory first, followed application, it will be easier for the reader to follow the theory and then understand why and how each equation is applied.

We chose to leave the format of this section as is. Aside from density in Eq. 2, we only directly parameterize Eq. 4, which already follows the theory at that point.

Line 195: The calculated atmospheric 10Be flux estimates should be reported in the results section. It seems that the authors mix methods and results throughout the manuscript. These pieces should be separated.

We have substantially re-organized the manuscript according to reviewer suggestions. Methods and results are now clearly separated – moving much of the background information (e.g. comment below) to the Introduction aided this process.

Lines 210-233: This is all background information that should go in the introduction. The authors should place this information into context. What do we know about 10Bem atmospheric fluxes in the study area (e.g. from previous estimates, if existing, or from the GCM/GISS -based models)? The authors should identify the knowledge gaps highlighted by this background information, then pose their questions and hypotheses, and then go into the methods.

The majority of this information has been moved to the Introduction and, in some instances, rephrased to reflect existing knowledge gaps (e.g. without a local calibration like we carry out in this paper, we do not have any way of knowing which production

rate estimation method is more correct – which is troubling for a site with such a discrepancy).

Lines 245-252: Similarly, the information about the variability in the geomagnetic field and its effect on 10Be atmospheric fluxes should e presented in the introduction, not the methods section. The authors should provide more detail on how the geomagnetic field strength influences the 10Be fluxes. Why is the modern solar modulation factor is much higher than the Holocene average? The authors should compare their Holocene-average flux of 0.92x106 at/cm2 yr to the value modeled by Heikkila and von Blanckenburg. If they are different, why? Could the dust flux make up an appreciable component of the Holocene-averaged flux? The authors should consider addressing this possibility in their flux reconstruction.

The average Holocene flux depends on variations in both solar modulation and magnetic field strength, which results in a flux that differs from modern.

Dust flux is insignificant at this site (as noted on line 88). We have moved the majority of this information to the Introduction, and instead present the estimated flux for this site in the Results section, and then speculate on differences between methods and the calculated flux in the Discussion.

Line 249: The authors should mention which Heikkila and von Blanckenburg flux map (i.e. the pre-industrial map).

We are using the pre-Industrial modeled flux, but we use the Industrial as an estimate of uncertainty. Text has been added to be more clear about this.

Line 280: The authors should include the inheritance correction in equations 1-4. Somewhere in the introduction, they should add that there is a high likelihood for inheritance since the concentrations were measured in reworked glacial till that may have been exposed to cosmic rays prior to burial.

We have added text that defines this explicitly when defining [10Be]reac, as well as for the measured inventory. We have also added a sentence to the Introduction explaining the likelihood for inheritance in these deposits, as follows:

"We utilize bulk samples sieved to <2 mm for our analysis, extracted from the lower mineral soil developed on each moraine, both mixtures of reworked glacial till (composed of Archean granite, granodiorite, and dioritic gneiss) that have a high likelihood for inheritance from cosmic ray exposure prior to burial"

Line 287: change parenthetical to: (e.g. Willenbring and von Blanckenburg, 2010)

Fixed.

Line 291: I believe the authors mean illuviation, rather than eluviation.

Correct, this has been fixed.

Lines 317-326: This paragraph raises a lot of questions about soil mixing, but leaves them mostly unresolved. Can the authors explore these questions in more detail? Because there is a low pH at the profile surface, can you estimate how much might be lost/mobilized down profile (e.g. based on Maher and von Blanckenburg, 2016 equations)? It appears that the grain size data in Tables 1 are from the <2 mm fraction only. How does the >2 mm size distribution change down profile? Could the relative abundances of pebble-sized clasts explain the difference between the in situ 10Be profile and the 10Bemet profile? It's possible that the fine fraction is relatively uniform down profile, but the coarse size fraction varies.

The equations of Maher and von Blanckenburg (2016) are for non-eroding settings. We can reasonably assume steady state for these profiles so using an upper and lower bound for Kd is sufficient and achieves the same goal.

We do not have specific data on the GSD of the >2mm size distribution aside from it being assumed to be unweathered and representing ~50% of the total material (Schaller et al., 2009b) – however, this wouldn't affect the in situ-produced 10Be profile any differently as those concentration measurements all came from the <2mm size fractions.

Lines 346-347: Can the authors provide some suggestions for resolving the influence of precipitation on F10Bemet? If this is identified as one of the key uncertainties influencing F10Bemet estimates, then they should provide a brief outlook for future research into this topic.

A hearty discussion on how to resolve this influence is beyond the scope of this work. However, new work by Deng and von Blanckenburg on this topic is about to appear in EPSL and we cite and summarize in a few sentences here.

Line 354: The authors do not make it exactly clear what two methods are being used to calculate the fluxes. Somewhere at the end of the introduction, the authors should state something along the lines of: "Here we estimate the atmospheric delivery flux of 10Bemet to the Wind River region using two methods: 1) . . ., and 2) . . .. Then we compare the results of these methods to determine the best estimate for the local flux, and gain insight into the key processes regulating 10Be accumulation and retention in soil profiles so we can improve soil residence time studies."

We have revised a couple of sentences to this effect at the end of the Introduction.

Supplementary material: In the paleo-precipitation rates section, the reported 10Be flux values are missing the 'x106' term. Instead, they are reported as 1.09 and 0.66 atoms cm2 yr-1, respectively, which is impossibly low.

Good catch! We have since removed this section, however.

Figure 2: It would help to show corresponding plots of grain size data for these profiles (e.g., wt% silt+clay). There is a typo after the semi-colon in the second sentence.

The inclusion of these plots tends to make this figure too busy. Instead, we report this data in Table 1, and if curious, the reader can compare to the GSD plots of Schaller et al. (2009a,b). Typo has been fixed.

Table 2: If the methodology for the in situ exposure age and denudation rate calculations are in the supplement, then Table 2 should also go into the supplement.

Table 2 has been moved to the Supplement.

Table 4: There are 10Bemet-derived erosion rates reported in this table, but neither the method nor the results are reported in the main text. The authors should add a section on the erosion rates and compare them to the in situ 10Be-derived erosion/denudation rates. This could make for an interesting comparison and ensuing discussion. The authors should also use numbers or letters for the superscripts in this table. Some of the chosen symbols could be confused with actual text.

We have decided to remove this section of Table 4, as we choose not to discuss them in the main text – erosion rates calculated from our study are circular, since we use them to calculate the depositional flux. They will always agree with the in situ derived rates. Any discussion therein is not warranted.

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