Geochronology Discuss.,
https://doi.org/10.5194/gchron-2020-14-RC2, 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.

Anonymous Referee #2

Received and published: 30 June 2020

General comments

The goal of this study was to constrain the delivery rate/flux of meteoric 10-Beryllium (10Bem)to the Pinedale and Bull Lake glacial moraines at Fremont Lake in the Wind River range of Wyoming. The motivation was to improve the method of estimating the atmospheric 10Bem flux by implementing an erosion rate correction to previously established methods. The study area was selected because the deposit ages are known, and there is existing data on sediment grain size, weathering indices, soil properties, and erosion rates. The authors report results that both agree and disagree with pre-

Printer-friendly version

Discussion paper

vious flux estimates derived from other methods, which raises interesting questions about the controls on 10Bem in soils. This study is novel because it is the first to compare 10Bem and in situ 10Be for the same soil profiles. The proposed methods and results are important for the field of meteoric 10Be geochronology, and the results provide an opportunity to learn more about the behavior of 10Bem in soils and build on these new findings.

I believe the study merits publication after the authors have had the opportunity to make minor revisions. The manuscript text needs to be improved. The authors mix background information and results into the methods section, which makes it difficult to follow their approach. One of the primary equations used in this study, Equation (4), is incorrect as written, and it is unclear if the associated calculations were affected by this issue. The authors should re-check their calculations and edit this equation to make it dimensionally correct. The discussion section is fairly weak as is, and could be improved with sensitivity analyses that aim to identify the factors that most influence their results. The difference in results between the two moraines should also be further addressed (a sensitivity analysis could help with this comparison).

Specific and technical comments

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.

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

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.

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?

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

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

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

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.

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?

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

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

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.

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.

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

Line 141: ~200 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.

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."

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

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.

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

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.

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

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.

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.

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.

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.

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.

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.

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

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.

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

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

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.

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 suture research into this topic.

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."

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.

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.

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 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.

References cited

Graly, J. A., Bierman, P. R., Reusser, L. J. & Pavich, M. J. Meteoric 10Be in soil profiles - A global meta-analysis. Geochim. Cosmochim. Acta 74, 6814–6829 (2010).

Maher, K. & von Blanckenburg, F. Surface ages and weathering rates from 10Be (meteoric) and 10Be/9Be: Insights from differential mass balance and reactive transport modeling. Chem. Geol. 446, 70–86 (2016).

Willenbring, J. K. & von Blanckenburg, F. Meteoric cosmogenic Beryllium-10 adsorbed to river sediment and soil: Applications for Earth-surface dynamics. Earth-Science Rev. 98, 105–122 (2010).

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