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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.

Anonymous Referee #1

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Having seen the presentation of this material at GSA 2018, I'm quite happy to see the authors have moved along to publication. The development of additional long-term records of 10Be retention in soil is one of the key pieces still needed for the robust application of the meteoric 10Be chronometer. This paper represents an important advance in that it factors erosion into the study. Previous work of this kind has relied on zero erosion assumptions that often complicate the interpretation of the results (e.g. Egli et al. 2010). I think the manuscript should be acceptable with minor to moderate changes. I detail my concerns by line or section below:

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Discussion paper

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

Line 31: “Target atoms”?

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

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 Pavich and Monaghan.

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

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

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.

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).

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

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.

Line 192: The authors need to explain how they treated inheritance mathematically (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.

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.

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

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.

Section 3.5: The authors seem to take it as granted that paleomagnetic intensity exerts linear and predictable control on paleo 10Be 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.

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

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).

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.

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 10Be 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.

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.

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

Line 320: Remove "In either case".

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 literature 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.

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-precipation 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.

Discussion: The deposition of recycled 10Be 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.

Line 636: "0Be"

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.

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.

References Cited: Barg, E., D. Lal, M.J. Pavich, M.W. Caffee and J.R. Southon 1997.

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