This manuscript presents a careful attempt to use meteoric ^{10}Be ($^{10}Be_{met}$) concentration-depth profiles in deposits with known ages and denudation rates - Pinedale and Bull Lake moraines in Wyoming, USA - to calibrate long-term delivery rates (i.e., fluxes) of $^{10}Be_{met}$ to those sites. The authors leverage previous *in situ* ^{10}Be ($^{10}Be_{is}$) concentration-depth profiles and surface exposure ages (from boulders) to parameterize their calculations of $^{10}Be_{met}$ flux over tens to hundreds of millenia (the ages of the landforms being studied). This study carefully updates past dates and erosion rates according to advances in our understanding of both the half-life and production rate of ^{10}Be, and it also diligently considers how factors such as precipitation rate and paleomagnetism may have varied over millennia affecting $^{10}Be_{met}$ flux, in turn. The authors then compare their site-specific results to two commonly used, empirically-derived methods for estimating $^{10}Be_{met}$ delivery rates, and they demonstrate that both methods overestimate flux rates for this site in Wyoming.

This work is one of only a couple studies that successfully constrain a delivery rate for $^{10}Be_{met}$, and (as the authors point out) knowing the delivery rate for a landscape of interest is of utmost importance if $^{10}Be_{met}$ is to be used as a tracer of surface process rates or as a geochronometer. This study represents an important contribution as a model for how long-term delivery rates for this isotopic system can be determined even when there is erosion of one's benchmark landform. That is to say that the authors show that it is not necessary to have all $^{10}Be_{met}$ retained since deposition in order to use a landform of known age as a calibration site. I think this article will be of interest to the readers of *Geochronology* from a methodological standpoint and also of interest to a broader audience interested in these iconic moraines of the Western United States.

I recommend this manuscript for publication after some minor to moderate revisions and clarifications. I lay out my thoughts/questions line-by-line below.

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

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.

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.

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.

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

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.

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.

Line 83: The suggestion here that the deepest samples are unweathered seems somewhat counter to the later argument that inherited meteoric concentrations are due to reworked material. Is there another model for inheritance that could work?

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.

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.

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

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

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

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.

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.

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.

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!

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.

Line 163: Units for E?

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.

Line 175: Check unit analysis of Equation 3.

Line 186: There is no N_{surf} in Equations 1 & 3.

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

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.

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?

Line 216: “...type of estimate…” not “...type of estimates…”

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

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

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).

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.

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.

Line 338: Where does the value of 128 cm/yr come from (in terms of both geography and a citation)?

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

Nice work - this is very cool stuff!