Comparison of basin-scale in situ and meteoric 10Be erosion and denudation rates across a rainfall, slope, and elevation gradient in felsic lithologies at George River, northeast Tasmania, Australia

VanLandingham et al. submitted to *Geochronology*
Author Responses to Reviewer #2

Reviewer #2

This manuscript by VanLandigham et al. reports in-situ and meteoric beryllium-10 (^{10}Be) based erosion and denudation rates for a small catchment located in Tasmania. The study is presented as aiming at two main goals: evaluate the long-term sediment delivery to the estuarine zone, which has implications for policy regarding land use and coastal ecosystem preservation; compare the estimates of erosion / denudation yielded by the two "varieties" of ^{10}Be.

Although I find the overall manuscript nicely written and well structured, I have concerns about (1) unclear terminology; (2) interpretation of meteoric ^{10}Be based erosion rates - see detail provided below. In addition, I must say that I noticed is some "dissonance" between the way the scientific question is set up in the introduction (revolving much around the issue of increased delivery of sediment to coastal ecosystems under land use change in modern times), and what is discussed in the rest of the paper (controls on long-term erosion rates, and more methodological aspects about the comparison between in situ and meteoric ^{10}Be). As a consequence, the manuscript would benefit from a clarification of its goals.
Author Response: We thank the Reviewer for their thoughtful comments on our manuscript. We agree that there is dissonance in the Introduction and we revised Section 1.1 "The Importance of Erosion of George River" to focus on understanding the drivers of erosion here and less on how erosion rates might be used to help ecological restoration efforts of Georges Bay.

1) Terminology
The first concern I have about terminology is about the fact that the authors keep on calling the $^{10}Be_i$-derived "epsilon" an "erosion rate" (*e.g.* Appendix A and throughout the manuscript), in apparent opposition to $^{10}Be/^{9}Be$-derived D_m called a "denudation rate" (*e.g.* Appendix A). Both are denudation rates (sum of physical and chemical removal of matter), really, except if a significant fraction of the chemical weathering occurs at depth < 2 m, typically. I understand that the difference between erosion and denudation rates might be very small if the chemical weathering rate is negligible, but this is discussed nowhere in this paper. In addition, in tectonically stable landscapes like the one under study here, and at erosion rates of ~10-20 mm kyr^{-1}, it's very possible that chemical weathering is a significant component of total denudation. Anyway, this might be a purely terminological issue, but one that relates to fundamental understanding of the proxies, such that I think this needs to be fixed before the manuscript can even be considered for publication.
Author Response: The Reviewer is correct that (1) both ε and Dm are denudation if there is no loss of mass to chemical weathering and that (2) chemical weathering can be significant in settings such as George River. However, we do not know *a priori* that chemical weathering is negligible and precisely why we compare an erosion metric to a denudation metric in this study.

It is true that we presume there to be negligible chemical weathering in George River, based on the topography, geological setting, and bedrock, and therefore we also consider George River to be a location where the two independent measures of ε and *Dm* can be compared. We revised our Introduction to make it clear how we define erosion and how we define denudation in this study. Given that $^{10}Be_i$ is used and has been used to measure erosion since Lal's (1991) seminal paper, in which ε is defined as the erosion rate, we continue its use here, and because $^{10}Be_i$ is measured from quartz minerals, which are highly resistant to chemical weathering, it may not always reflect total denudation. For this reason, we choose to distinguish ε in this study from $^{10}Be_m/^{9}Be_{reac}$ based measures of total mass loss (von Blanckenburg et al., 2012; i.e. denudation), and we choose to use *Dm* as our abbreviation for this measure to differentiate it from $^{10}Be_m$-based measures of erosion, *E* (Willenbring and von Blanckenburg, 2010)

With regards to chemical weathering, we have, as the reviewer suggested, obtained chemical and physical measurements of river water and flow at a drinking-water intake station for the town of St. Helens near the outlet of George River basin as well as discharge measurements; this site is comparable to TG-9. The chemical and suspended load data extend over 6 years, and the flow/discharge data extend over 60 years. This appears to be the only site sampled in the basin. Considering the measurements of major anions and cations as well as total suspended solids, the chemical export and suspended solids export rates appear similar to the erosion rate determined using ^{10}Be measured in the sediment exiting the basin at TG-9. When revising the manuscript, we will include details of these data we obtained, the flow records, and the procedure we used to estimate chemical and suspended sediment export rates for the basin.

Second, it appears surprising to me that the $^{10}Be/^{9}Be$ ratio is here called "$^{10}Be_m/^{9}Be_{reac}$", whereas in most recent studies about this ratio the term "$^{10}Be/^{9}Be_{reac}$" was used. I think I understood why the authors have done so: in this study $^{10}Be_m$ is measured by digestion of the bulk sample (l. 203), rather than on the chemical leachate of the "reac" fraction on which ^{9}Be is measured (l. 206-207). This is fine as long as $^{10}Be_i$ - which is also released during bulk digestion - is negligible compared to $^{10}Be_m$, which seems to be the case (Table 2 shows that the latter us typically two orders of magnitude smaller than the former). If I am right, I now feel like it would have been nicer if the authors have explained that themselves in their manuscript, instead of leaving the job to the reader / reviewer. If I am wrong, this terminology is simply very misleading.
Author Response: We revised the methods to make it clear that $^{10}Be_i$ is incorporated into the $^{10}Be_m$ measurement, but that the amount of $^{10}Be_i$ is two orders of magnitude smaller than the overall $^{10}Be_m$ measurement, thus being negligible.

Finally - and this is a concern of lesser importance - "*Q*" is an unusual notation for the flux of meteoric ^{10}Be, at least in the most recent literature. I also note that it is not really defined in the text: l. 211 is the first of this term, with no definition. On a more general note, having the equations just at the end of the manuscript makes the reading and the evaluation very difficult. To come back to this *Q*-notation, this is particularly misleading since in the recent $^{10}Be/^{9}Be$ literature, *Q* was used to denote discharge, which might be an important parameter for estimating denudation rates for $^{10}Be/^{9}Be$ ratio in cases where Be retentivity onto particles is not complete (see below).
Author Response: We had chosen to use *Q* to denote meteoric ^{10}Be delivery to Earth's surface because this was the notation being used in older literature (i.e. Brown et al., 1988); however, we

recognize that Monaghan et al. (1986) used *F* as the notation for $^{10}Be_m$ delivery to Earth's surface and since this is what other researchers, including authors on this paper, have used in recent work, we changed all instances of *Q* to $^{10Be}F_{met}$.

2) Interpretation of $^{10}Be_m$- and $^{10}Be/^9Be$-derived rates

The main point made by the authors from their comparison of $^{10}Be_i$- and $^{10}Be_m$-derived rates is the 5-6 times greater rates (leaving aside the fact that one might reflect total denudation and the other erosion rates, which in itself would deserve some discussion in the manuscript) obtained from the latter method (Figure 8). To me, this is simply a grain size effect. Indeed, and as acknowledged by the authors, 10Be_i tends to weakly depend on grain size itself (l. 181-182), $^{10}Be_m$ is strongly grain-size dependent (e.g. Wittmann et al., 2012). Hence any erosion rate inferred from ^0Be_m will be affected by grain size effects. I understand from section 3 that both varieties of ^{10}Be were measured on the 250-850 µm fraction, which is much smaller than the average grain size of sediment delivered to the alluvial plain of the George River (l. 179-180).

Now, I admit that one could expect much greater $^{10}Be_m$ concentration in the analyzed, relatively fine fraction (meteoric Be being enriched in fine fractions offering large mineral surface areas) than in the "representative" sediment generated in the catchment, thereby likely leading to an understimate of erosion rates by $^{10}Be_m$ (see equation in Appendix A) compared to what could be deduced from $^{10}Be_i$ - even if the latter includes some fraction of chemical weathering. The opposite observation is made in Fig. 8. But in my opinion this apparent contradiction shows even more how these estimates need to be discussed in the frame of the limitation / inherent assumptions of each proxy, an aspect of the discussion that is critically missing from this manuscript. I note that the equation used to calculate D_m in Appendix A accounts for such grain size effects through the term $^9Be_{min}/^9Be_{reac}$ and leads to estimates of *denudation rates* (hence more directly comparable to 10Be_i-based estimates) that, although with some significant scatter, lie uniformly around the 1:1 line (between 1:3 and 2:1) in Fig. 8, rather than showing a systematic overestimate.

Author Response: We agree with the Reviewer that the differences between ε and *E* are due to grain size bias in our $^{10}Be_m$ measurements. As described to Reviewer 1 above, we now justify our disregard for geological meaning in our calculated values of *E* and remove interpretation of *E* from the Discussion altogether. We are not concerned about the grain size effect on $^{10}Be_i$ data, as noted in the manuscript, because $^{10}Be_i$ in low-elevation, temperate settings is rarely affected by grain size bias (van Dongen et al., 2019) and evidence for deep landslides that could otherwise dilute $^{10}Be_i$ in stream sand is not present in the field area.

Another issue with the use of the $^{10}Be/^9Be$ ratio here is the potential bias induced by loss of Be to solution. The equation for D_m in Appendix A here does not account for such poor retentivity. And in contradiction to what the authors say, a pH in the range 4.0-5.5 (l. 119-120) entails significant loss of Be to solution (*e.g.*, Aldahan et al, 1999). This loss can in turn lead to a strong bias in both *E* and D_m estimates, particularly in situation where the ratio between discharge and erosion rates (*Q/E*, "Q" here being understood as water discharge, see my comment above about terminology) is high. I think this field setting, which is reasonably wet and tectonically quiescent, is one where this ratio is expected to be high. Taking the $10Be_i$-derived estimate of erosion rate of ~20 mm kyr^{-1} (Table 3) and the precipitation of ~ 1000 mm kyr^{-1} (Table 2), and

assuming a evapotranspiration factor of 0.5, I get a *Q/E* ratio of around 10^4 L/kg, which at pH 5 corresponds to an overestimation of *E* from $^{10}Be_m$ of ~100% and of D_m from $^{10}Be_m/^{9}Be_{reac}$ by 10 to 100% depending on the fraction of ^{9}Be in the "min" fraction (see von Blanckenburg et al., 2012). Now, these back-of-the-envelope calculations might well be wrong, and surely can be refined, but clearly this potential issue clearly has to be discussed in more detail in the manuscript before a comparison with another proxy can be made.
Author Response: We thank the Reviewer for directing us to the Aldahan et al. (1999) study. This comment and the previous Reviewer's suggestion to refer to ^{10}Be partition coefficients in Yiou et al. (1999) led us to try to better understand streamwater pH throughout our field area. To this end, we find that streamwater pH in the main stem of George River at St. Helens and in the Ransom Creek tributary is >5 and has been since measurements began in the 1980s. This supports our interpretation that pH conditions in George River basin are not likely to induce $^{10}Be_m$ loss to solution.

*** Other comments ***
- l. 43: "northern tropics" is a bit misleading (to me, the "northern tropic" is the Tropic of Cancer, which is not what the authors are talking about here, I guess).
Author Response: We mean the northern reaches of Australia's Great Dividing Range, which is north of the Tropic of Capricorn. We edited the text to better reflect this geography and not mislead the reader.

- l. 319: "to not have not had" -> problem with this sentence.
Author Response: Revised to fix the wording problem.

- l. 376-383: Not sure this paragraph (about controls on long-term rates) belongs to this section (which is supposed to be about trunk stream vs. tributary sediment supply.
Author Response: The first paragraph is a discussion of where sediment that is passing through trunk channel sites originates, from the tributaries, from below the tributaries, or from the entire catchment? The first paragraph describes our interpretation that sediment at trunk channel sites originates in tributaries, and it describes how we derive a value for ε for the whole George River basin. The second paragraph describes how this whole-catchment value for ε compares to whole-catchment values of ε across the rest of the Great Dividing Range.

- Table 2: Strictly speaking, "$^{10}Be_{met}$" is not defined anywhere - I think the authors mean "$^{10}Be_m$", which is used throughout.
Author Response: Fixed.

- The end of Figure caption 2 is missing.
Author Response: Text-box issue. This is now fixed.

- Fig. 5B and pie chartes in Fig. 7: I must admit I did not understand exactly what purpose the calculation and presentation of this "hillslope erosivity" serve in the manuscript.
Author Response: We mention and use Kidd et al.'s (2014, 2015) "hillslope erosivity" metric because we did not want to neglect their work and the maps of soil erosivity for Tasmania they produced. Despite being based on a calculated value via a multivariate equation, Kidd et al.'s

“erosivity” designations (i.e. Extreme → Very Low) are qualitative in nature, which makes it difficult to summarize “erosivity” at a catchment scale or to compare our numerical catchment-wide erosion rates to Kidd et al.’s “erosivity.” By determining that “erosivity” is ultimately significantly related to slope (Fig. 5B), we are able to acknowledge Kidd et al.’s work and use hillslope angle as a proxy for erosivity. We’ve reworded this last paragraph of the Methods to explain this better.