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 #1

Reviewer #1

General comments
It is my pleasure to review the manuscript by VanLandingham titled "Comparison of basin-scale in situ and meteoric ^{10}Be erosion and denudation rates across a rainfall, slope, and elevation gradient at George River, northeast Tasmania, Australia". Quantification of millennial-scale background erosion rate is crucial to understand landscape evolution over time and to assess human-induced land degradation. Although the driver of long-term erosion is commonly attributed to tectonic uplift/topographic relief over large scales, such pattern may be less clear on local scale due to small variability in these factors and distinct variability in other factors. Here the authors studied the background erosion rates in the George River on the island-state of Tasmania. The major goals of this study include two parts: 1) to find the controlling factors of millennial-scale denudation rates in the study region and 2) to compare between denudation rates derived from a well-established method (in situ 10Be) and a relatively new method (meteoric 10Be/9Be). The first part is a piece of standard work and the highlight should be the second part.

In brief, the authors found that in situ 10Be-based erosion rates are positively correlated with precipitation (R2=0.82) and only poorly correlated with slope (R2=0.17), which is (surprisingly) different from the pattern derived from dataset of mainland Australia (slope control of erosion, Fig. 9b). The authors also showed that the denudation rates based on 10Be_i and 10Be_m/9Be_reac agree within a factor of 2 (except TG-7, Fig. 8), supporting the meteoric 10Be/9Be applications in basins with minor geological heterogeneity and little human-induced disturbance.

In general, I think it is a nice case study regarding inter-method comparison (in situ 10Be vs. meteoric 10Be) and this study on determining rates of catchment-scale denudation processes meets the scope of GChron. Nevertheless, the interpretation on precipitation control requires more lines of evidence especially quantitative constraints and more details need to be added regarding calculation of 10Be_m/9Be_reac-based denudation rates. I provide specific comments and technical corrections below and hope that these comments can help to improve the manuscript.
Author Response: We thank the Reviewer for their thoughtful considerations on our manuscript and address them individually below.

Specific comments
1. Control of precipitation vs. other factors. The correlation between 10Be_i-based denudation rates and precipitation rates looks sound. However, a key quantitative link is missing here. Please note that the variability of precipitation among all sampling basins is quite small as 1.4-fold (0.97-1.26 m/yr), compared to ~5-fold variability in denudation rates (4.8-24.5 m/kyr).

Author Response: We now acknowledge this in the first paragraph of the Discussion.

Erosion rates (E) are commonly assumed to scale with precipitation rates (P) as $E \propto P^m$ (D'Arcy and Whittaker, 2014), and m is commonly assumed to be 0.5 (using m/n of 0.5 and n of 1) or may be a bit higher as e.g. 1.2 (using m/n of 0.5 and n of 2.4) based on global data fitting (Harel et al., 2016). However, in both cases the large variability in erosion rates cannot be explained by the small variability in precipitation rates. If such scaling is applicable in this study area (if not, please justify), it means that the majority of erosion rate variability should be explained by factors other than precipitation. The authors provided several other alternative explanations (around Lines 338-345), which I appreciated, but then they rejected these scenarios later. From my perspective, it seems that the denudation rates are controlled by certain processes related to elevation (6-fold variability, Fig. 6). Although glacial processes (elevation-related) may not play a major role in such low-elevation regions as the authors mentioned, what about other processes? For example, discharge variability may also play a role in river incision (Lague, 2014). I think the WorldClim global dataset includes similar parameters (precipitation seasonality?) that can be extracted for analysis. In short, the authors should provide more alternative scenarios to explain the variability of denudation rates, which may not be mainly controlled by the small precipitation gradient.
Author Response: D'Arcy and Whittaker (2014) demonstrate that the normalized channel steepness index – often used as a proxy for erosion in tectonically-active regions – is linked to precipitation in the sense that uplift in the Andes creates a topographic barrier, which is reduced via precipitation-driven stream incision. The relationships that D'Arcy and Whittaker model may be appropriate for other tectonically-active, high-elevation regions; however, this is not the setting for George River in Australia. As such, we hesitate to fit our observations into a landscape evolution model derived for a significantly different topography and tectonic setting. As the Reviewer suggests, we argue that erosion rates are controlled by some factor related to elevation, and as noted, we review the likely possibilities (e.g. periglacial weathering, mass movements, etc.) and ultimately use our observed relationships between longitude, rainfall, and elevation and erosion rates to suggest that in this specific region of the world, erosion rates are primarily driven by precipitation. The coastal setting of George River and Tasmania's temperate climate are not highly seasonal, and Tasmania lies outside the track of tropical cyclones which seasonally make landfall in far northern Australia.

2. Terminology (epsilon, E and D_m; erosion vs. denudation). It is quite confusing to the audience (or at least to me) when reading rate estimates of different meanings, from different calculation methods, with different units (mm/kyr vs. Mg/km2/yr) and different from the terminology used by previous studies. First, I think both 10Be_i and 10Be_m/9Be_reac methods derive denudation rates, i.e. removal of whole rock by physical erosion and chemical weathering. Second, I think the unit should be unified in the text (either mm/kyr or t/km2/yr) for reading purpose. Third and more importantly, I do not think 10Be_m based E is needed in the discussion. Meteoric 10Be concentration alone is very sensitive to grain-size effect (Singleton et al., 2016; Wittmann et al., 2012). When the analyzed sample is dominated by coarse materials (250–850 microns of bedload rather than suspended load), meteoric 10Be concentrations will be low as expected (less adsorption capacity and/or quartz dilution) and thus the calculated erosion rate will be biased towards higher values as shown here (e.g. Fig. 8). Hence, I would suggest to simply remove all the content related to 10Be_m based E (also in figures) as it has not been

discussed in detail anyway and does not contribute to the key conclusions. If the authors insist, including such estimates in the supplement would be more than enough.
Author Response: First, in this study, as with past studies, we find it very important to maintain a distinction between erosion and denudation and which is measured by each $^{10}Be_i$ or $^{10}Be_m$. Here, and in our previous work, we acknowledge that $^{10}Be_i$, which is measured on weathering resistant quartz grains, is an apt measure of erosion (the total physical mass loss from a landscape). Given that some mass in any landscape may be lost to chemical weathering a different method is needed, and this is where the von Blanckenburg et al. (2012) measure of total mass loss – i.e. denudation – is derived.

Presenting erosion rates in units of length/time (i.e. mm/kyr) is consistent with nearly the entire literature of $^{10}Be_i$ studies; if there is no mass loss to chemical weathering, the product of erosion rate and the density of rock should yield an adequate measure of denudation as well mass/area/time (i.e. $Mg/km^2/yr$), and should be equal to the result of methods that directly measure denudation. Similarly, presenting denudation rates in units of mass/area/time (i.e. $Mg/km^2/yr$) is consistent with the units of denudation presented in von Blanckenburg et al.'s equation derivations. In our previous work (Portenga et al., 2019, GSA Bulletin), we ask the question, "When should erosion and denudation be the same?" and the field area of this study at George River is ideal to answer this question: homogenous lithology, relatively thin soils, and pH conditions that do not promote desorption of $^{10}Be_m$ from sediment grain coatings. We do, following revisions and Reviewers' suggestions, estimate the amount of chemical weathering – based on water quality data from a water intake station in the town of St. Helens (cf. TG-9) – and make it more clear that pH conditions for George River, in soils and in stream water, are such that desorption of $^{10}Be_m$ is unlikely. Understandably, many previous studies measuring landscape dynamics with $^{10}Be_i$ use the term "denudation" because the assumption of little mass loss to chemical weathering is made, or the amount of chemical weathering is measured independently.

Lastly, given that both reviewers take issue with our inclusion of the geomorphological meaning of E presented in this study, we now highlight in the Discussion that our calculated values of E are likely due to unresolved grain size bias (citing suggested Wittmann and Singleton papers), and therefore are inaccurate measures of landscape dynamics at George River. We choose to retain calculated values of E in our data tables and in our Figures to visually highlight this inaccuracy. We focus all remaining discussion on the comparison of D_m and ε.

3. Choice of ^{10}Be depositional flux. First, it is better not to use Q as 10Be_m delivery rate. I think it may cause confusion as it means water discharge for many geomorphologists, and it is inconsistent with the original framework (von Blanckenburg et al., 2012) (cited by the authors) or the co-authors' previous paper (Portenga et al., 2019). Second, it is appreciated that the authors mentioned several different approaches to determine10Be_m delivery rate. However, the authors then decided to only use Graly et al. (2011)'s approach. Graly et al. (2011)'s equation is based on fitting of modern precipitation 10Be dataset and might cause flux overestimation in some cases when applied to millennial timescale (Deng et al., 2020). Since there is no 10Be_m delivery rate measured in the studied basin (e.g. using dated soil profiles as Reusser et al. (2010)), I would recommend to also calculate denudation rates using 10Be delivery rates from GCM that indeed integrate over millennial timescale (Heikkilä and von Blanckenburg, 2015). This approach can also provide latitude- and longitude- specific 10Be fluxes. As such, readers

can get more comprehensive information on the utility of both methods in this specific region by comparing resulting 10Be_m/9Be_reac based denudation rates with those from 10Be_i. If the authors still decide to only use 10Be delivery rates from Graly et al. (2011)'s equation, the denudation rate results using GCM-based 10Be delivery rates should at least be included in the supplement.

Author Response: We thank the Reviewer for suggesting that we consider using values of ^{10}Be_m delivery from other studies and deriving denudation rates from those values. First, we now refer to Q as $^{10Be}F_{met}$ to be consistent with recent works. Second, we've revised our methods to acknowledge the many different ways that $^{10Be}F_{met}$ can be measured or modelled. The CGMs that the Reviewer directed us to do not have the spatial resolution that is required to differentiate values of $^{10Be}F_{met}$ for individual study basins. We prefer to use Graly et al.'s (2011) $^{10Be}F_{met}$ estimations because it allows us to present a specific value of $^{10Be}F_{met}$ for each studied basin. We revised our Methods section to better present the possible values of $^{10Be}F_{met}$ from each model, and we show that our estimated values of $^{10Be}F_{met}$ based on Graly's work is similar to $^{10Be}F_{met}$ predicted from GCMs. While it would be an interesting exercise to measure Dm from each $^{10Be}F_{met}$ value, we think this would distract from the narrative of this study; because of the similarity of all $^{10Be}F_{met}$ values, which we already present, the spatial patterns and relationships to basin metrics shown in Figure 6 and Figure 7 would not change. We therefore do not believe it is necessary to calculate denudation rates based on each of the GCMs to interpret the spatial variability of meteoric ^{10}Be-based measurements of erosion and denudation.

4. Long-term trend in denudation rates (millennial-scale vs. decadal-scale). The authors mentioned in several places that the sediment input increased due to land use prior to 1990s and later decreased (?) afterwards, and the sediment input nowadays should be generally higher than millennial-scale denudation rates. At least this is my impression after reading the text. So I am wondering if there is any gauging data (e.g. sediment yield) in the studied catchments so that comparison between rate estimates that integrate over different timescales can be made and thus support the authors' claim. Although I am not sure about data availability, such comparison seems to be important as the authors emphasized this point as a major implication at the end of the abstract.

Author Response: The Reviewer is correct that historical sediment yield increased and has since decreased back to pre-disturbance conditions (Knighton, 1991), but it is not accurate to say that sediment input nowadays is higher than millennial-scale conditions. Most of the excess historical sediment has either been sequestered in floodplains or delivered already to Georges Bay, which we discuss and cite appropriate literature. Unfortunately, sediment gauging data are scant and generalized when referred to in the literature at all, and to our knowledge there are no long-term records of sediment yields for George River or any of its tributaries. We are limited to brief mentions of sediment yields in Knighton (1991) and recorded variations of sediment supply shown in Figure 1, therein. In order for Knighton (1991) to quantify variations in sediment supply, sediment yields must have been recorded at some time; however, we have not been able to ascertain where such data exist. We have revised our text to note that our knowledge of pre-disturbance channel conditions, or grain size measurements (i.e. 30-50 mm) come from a limited number of sites in the field area.

5. Low D_m caused by topsoil erosion. In Lines 409-432, the authors argued that the low D_m in the headwaters are caused by significant 10Be_m-rich topsoil erosion. I do not necessarily

disagree on this argument. However, I am confused why such process does not affect 10Be_i data. Both in situ and meteoric 10Be should show a decline profile with soil depth and thus are enriched at the surface, and if bioturbation plays a role and a mixing layer is established, it should also affect both nuclides.
Author Response: Meteoric ^{10}Be profiles in soil have been shown to re-establish over short-term periods following bioturbation events whereas in situ ^{10}Be in bioturbated soils remains homogenous within the soil mixing zone (Jungers et al., 2009), which would explain why recent and intense disturbance to topsoil affects only meteoric ^{10}Be and not in situ ^{10}Be. We make this clearer in the manuscript.

Technical corrections and minor scientific comments
Main text
Title: I am not sure if a range of precipitation rate of 0.97-1.26 m/yr can be considered as a gradient. The variability is relatively small compared to that in the eastern Australia coastal rivers (Fig. 9). How about "… denudation rates in felsic lithologies at George River…"? The studied catchment is indeed dominated by Devonian felsic intrusions and the authors emphasized in the text that the simple lithology in this catchment makes the inter-method comparison easier. I will leave the decision to the authors.
Author Response: We appreciate the reviewer's suggestion about focusing on the lithology, and we agree that this is important for the inter-method and comparison. We recognize that the rainfall gradient observed across George River is not significant when compared to other regions, which we already note in the text. However, of all studies that compare $^{10}Be_i$ erosion rates to basin metrics, including mean annual precipitation, there are few correlations as close as that which we observe for George River. Instead, we think it is more impressive that such a small rainfall gradient seems to influence erosion in this low-elevation, post-rift margin. Nevertheless, we revised the title of this study to remain inclusive of precipitation, but also add the inclusion of felsic lithologies as the reviewer suggests.

Lines 59-60 There are too many references here. Can they be assigned to each specific topic? E.g., mining (ref), fishing (ref)…
Author Response: The activities referred to in this list are interconnected for Tasmanian estuaries, and the studies cited in this list are comprehensive assessments of all activities leading to degradation of Tasmanian Estuaries. However, Augustineus et al. (2010) focuses mainly on mining and Nanson et al. (1994) focuses mainly on tourism. As such, we separated these studies from the rest of the list, per the reviewer's suggestion.

Lines 97-98 Please separate references on 10Be delivery from those on catchment applications.
Author Response: $^{10}Be_m$ delivery citations are now separate from application studies.

Line 100 "non-cosmogenic" should be "stable"?
Author Response: We use "non-cosmogenic" to refer to ^{9}Be because it is found terrestrially on Earth and to differentiate it from the ^{10}Be, which is produced only through cosmogenic reactions. While tempted to describe ^{9}Be as "naturally occurring," this would suggest that cosmogenic production of ^{10}Be is not a natural process, which it is. We are unsure how else to describe ^{9}Be, so we added "stable" to our description of ^{9}Be per the reviewer's suggestion.

Line 103 Harrison et al., 2021 only measured 10Be_m instead of 10Be/9Be. Hence, it should be placed at the beginning of this paragraph.
Author Response: We revised this part of the paragraph to better distinguish between which studies used which $^{10}Be_m$ method.

Line 108 "pH…high (>3.9…)" I do not think 3.9 can be considered as a high pH and the partition coefficient of Be can be low (You et al., 1989).
Author Response: We thank the Reviewer for directing us to You et al. (1989). This comment and the other Reviewer's suggestion that we reference Aldahan et al. (1999) with regards to $^{10}Be_m$ mobility in the environment. We clarify that soil and stream pH levels are all within the realm of $^{10}Be_m$ retentivity in sediment grain coatings and that desorption of $^{10}Be_m$ in George River is unlikely.

Lines 119-120 "soil pH". Could you also provide river water pH data if available?
Author Response: Both Reviewers' comments encouraged us to take a second look at pH values in stream water. We now present long-term, decadal pH values for George River and Ransom Creek (the only two sites in our field area for which data were available [DPIPWE, 2021]), which is consistently >5 and mostly >6.

Line 146 "drain" should be "drains".
Author Response: Changed.

Line 189 It is hard to imagine that the average grain-size of alluvium sediment can be 30-50 mm with moderate precipitation rate of ~1 m/yr and gentle slope. Are there any field photos on the sampling sites (perhaps included in the supplement)? Besides, these are the materials left behind and can not represent most materials that have been transported to the sea, which should be much finer.
Author Response: Unfortunately not. We searched for sediment yield/grain size data in both published and grey literature, and we reached out to people in Australia/Tasmania who might know whether such data exist without success. The only data we were able to find were numbers from Knighton (1991), which we cite in this study, although we now clarify that the pre-disturbance grain size data are limited in their applicability across the field area.

Line 206 Please give a brief description on the acid used here.
Author Response: Revised to be more detailed: 6N HCl.

Lin 245 Which type of regression? Linear?
Author Response: Linear. Sentence is now revised.

Line 247 Here TG-1=1.1 km3/yr, but in the text above TG-1=3.8 km3/yr. Please use different terms for both values.
Author Response: In this instance TG-1 = 1.1 mm kyr^{-1}, which is the average of modelled erosion rates for the TG-1 subcatchment based on linear regression equations for longitude, elevation, and precipitation from Fig. 6, of the subcatchment (area upstream of TG-1 sampling site and downstream of tributary sampling sites). The 3.8 km3/yr value is the product of the measured $^{10}Be_i$ erosion rate and the total catchment area upstream of TG-1. We, however,

discuss the accuracy of the measured $^{10}Be_i$ erosion rate for TG-1 (and TG-9) in this paragraph, demonstrating that it is dominated by erosion in the tributaries and missing a significant contribution of erosion from the TG-1 subcatchment. We make revisions to this section of the Discussion to make our logic and reasoning here more clear, especially as it relates to the value we believe best-reflects the average erosion for the entire catchment area of George River.

Line 303 I checked Mishra et al. (2018) and they actually claim that "the regime between ~1000 and ~2200 mm/yr is dominated by opposing relationships where higher rainfall acts to increase erosion rate, but more water also increases vegetation/tree cover, which slows erosion". As such, there is no correlation or even negative correlation between precipitation and erosion rates within the precipitation range of 0.97-1.26 m/yr (Mishra et al. (2018)'s Fig. 7). Hence, this point needs to be rephrased.
Author Response: We clarify in the first paragraph of the Discussion that Mishra et al.'s findings also suggest that increased rainfall leads to increased vegetation cover which can slow erosion. We maintain our argument that precipitation drives erosion in George River, which is a highly-localized area of Earth's surface and what controls erosion here may not be reflected in erosion studies at the global scale.

Lines 326-327 and Fig. 9 The close relationship does not mean 10Be_i denudation rates must be correct, especially when the variability in precipitation rates cannot explain the large variability in 10Be_i denudation rates. I think Fig. 9 shows that the 10Be_i measurements in this study should be ok as the George River data can fit in the general pattern over a large spatial scale. However, Fig. 9 also shows some evidence against the precipitation control: although precipitation/elevation may play a role in controlling erosion rates on local scale, such relationship can not be found on a larger spatial scale (east Australia). Besides, the control of mean slope seems to be clear on the same (large) scale. If this is correct, it means that the pattern found in the George River is a very local phenomenon and its applicability is very limited. One suggestion may be that the authors simply claim that their denudation rate data do fit in the large-scale pattern in east Australia and spend much less text on its controlling factors, as I think the highlight is the inter-method comparison anyway. Otherwise, the authors need to explain such inconsistency to convince readers that their conclusion is not only of local impact.
Author Response: It is unclear what the Reviewer is suggesting here. It seems the Reviewer accepts the validity of our $^{10}Be_i$ data only because they are consistent with erosion across the remainder of the Great Dividing Range. We discuss in the paper that we observe no relationship between $^{10}Be_i$ erosion and slope for George River whereas $^{10}Be_i$ erosion and slope are closely related across mainland Australia. Between this comment and a previous comment, it seems the Reviewer wishes to reject our primary interpretation because a modelled relationship between channel steepness and precipitation in a vastly different landscape and tectonic setting says it shouldn't exist. We maintain our primary interpretation that erosion is driven by rainfall in George River and is not related to basin slope, as it is elsewhere along the Great Dividing Range (Codilean et al., 2021). The similarities and differences of erosion in George River compared to erosion on the Australian mainland is already discussed in detail in this paper. We disagree with the Reviewer that our findings have limited applicability. George River may be small and not important for most, but our dataset presents erosion data for a part of the world that previously had none.

Tables & Figures

Table 1 Please clarify if slope and precipitation provided here are basin-averaged values

Author Response: Fixed per the Reviewer's suggestion.

Table 2 Q's unit: atoms/cm2/yr

Author Response: Fixed (column width was too-narrowly adjusted)

Table 3 Please add a note to explain the meaning of epsilon, E and D_m.

Author Response: Fixed.

Fig. 1 Please provide a color bar to the precipitation map.

Author Response: Added.

Fig. 2 Caption text is incomplete. Also, what does the white star (St. Helens) mean? City?

Author Response: St. Helens is the town of St. Helens and this is indicated in the main text and previous figures.

Fig. 3b The color of the text (Elevation) is different from that of the corresponding symbol.

Fig. 9 Caption text: "B. Comparison" should be "C. Comparison"

Author Response: There is no map symbology for elevation because the background map is a shaded relief map. We've changed the elevation color symbology to grey, however, since that is the same as the shaded relief map.

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