Geochronology Discuss., referee comment RC1
https://doi.org/10.5194/gchron-2021-23-RC1, 2021

Comment on gchron-2021-23

Anonymous Referee #1

Referee comment on "Comparison of basin-scale in situ and meteoric ^{10}Be erosion and denudation rates in felsic lithologies across an elevation gradient at the George River, northeast Tasmania, Australia" by Leah A. VanLandingham et al., Geochronology Discuss., https://doi.org/10.5194/gchron-2021-23-RC1, 2021

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.

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). Erosion rates (E) are commonly assumed to scale with precipitation rates (P) as E ☐ 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.

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.

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.

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.

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.

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.

Lines 59-60 There are too many references here. Can they be assigned to each specific topic? E.g., mining (ref), fishing (ref)…

Lines 97-98 Please separate references on 10Be delivery from those on catchment applications.

Line 100 "non-cosmogenic" should be "stable"?

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.

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).

Lines 119-120 "soil pH". Could you also provide river water pH data if available?

Line 146 "drain" should be "drains".

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.

Line 206 Please give a brief description on the acid used here.

Lin 245 Which type of regression? Linear?

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.

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.

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 can not 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.

Tables & Figures

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

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

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

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

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

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”

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