Dear Dr. Wittmann-Oelze,

Thank you for a prompt return and positive comments with regards to our revised manuscript. We recognize there are a few minor issues remaining before you consider the manuscript for publication, and we address those below.

Additionally, the References list should now be in concordance with the EGU Journal guidelines for reference style.

Kind regards,

Dr. Eric Portenga (corresponding author)
Leah VanLandingham, B.Sc.
Dr. Edward Lefroy
Dr. Amanda Schmidt
Dr. Paul Bierman
Dr. Alan Hidy

Most recent correspondence from Geochronology:

Comments from Associate Editor Wittmann to Van Landingham et al., GChron

Dear Eric et al.,

Many thanks for the thorough revision on this manuscript. I really appreciate your efforts, which in my view have significantly improved the MS. I think you have in most cases corresponded adequately to my and the reviewers comments. There is one issue remaining, but I think that can be addressed with minor efforts. I would suggest, for time-efficient handling, that you address the (minor) comments I have below, after which the MS could be published.

With best wishes, Hella

General comments:

1) “Sensitivity analysis” of Dm under changing 9Beparent and F10Be_met. As mentioned in my and a reviewers first comments on V1, I am missing a short discussion in which directions Dm´s would go if, e.g. 9Beparent would be higher than 2.5 ppm (à Dm would decrease), or lower (basalt units), how changes in F10Be_met would affect Dm, and how retentivity would affect them (see the Q/E approach mentioned by reviewer #2). Of course, this assessment would be largely hypothetical (at least for 9Be because there simply isn´t any data on 9Be_parent for this catchment). However, previous studies have shown that a factor of 2 in 9Beparent even in granites, and something in the order of 30% for F10Bemet can be easily the case. Specifically for F10Bemet, wouldn´t the use of precipitation-derived [elevation-controlled…] meteoric flux counterbalance any elevation dependency? (Perhaps one way around this is using the basin-wide average F10Bemet value from the CGM. Having values for each catchment doesn´t benefit the discussion if their reliability is unclear. Also, this would normalize at least this effect, and the remaining effects could be better identified, perhaps…). I think looking at this carefully would put the Dm into the right frame, as a factor of 2 disagreement with e (or agreement, however you want to call it) is okay for a method that is still in its infancy. That´s why my main critique is that I would not generally call the 10Be/9Be-derived denudation rates as being sensitive to land use. We simply don´t know how the 9Be_reac plays into this, whereas 10Be_met-derived erosion rates can be clearly affected by e.g. soil loss. Also it seems like from Fig 7 that the way in which Dm and e scatter around the 1:1 line does not follow a trend with erosivity. When comparing to the landuse data, it looks like not all tributaries that have high landuse (erosivitiy) values also have Dm that are higher than e (i.e. for T6-8, Dm are actually lower than e). This clearly points in my view at some random effects (for which in my view catchment-wide variability in 9Beparent would be a good candidate). For these reasons, I would ask you to tune down these statement about Dm and land-use affected areas.

Author Response: With regards to the various requested sensitivity analyses.

$^{9}Be_{parent}$: Knowing that we are unable to measure bedrock from the field area at this time, we dug further into the literature to see if we could find other studies that measure natively occurring beryllium in similar lithologies. To that end, Grew (2002; *Reviews in Mineralogy and Geochemistry*) compiled a compendium of everything we (he) knew about beryllium at the time. In the introductory chapter, Grew writes that Earth’s crustal average concentration of Be is 3 ppm, that most Be-bearing minerals contain it in <10 ppm range, but it is not unheard of for Be to be found >100 ppm, in terrestrial systems Be is found at <1 ppm in (ultra)mafic lithologies, and that Be can range 10-fold within the same igneous complex. In the same compendium on beryllium, London and Evensen (2002) present a table of typical Be concentrations from felsic granites ranging from 1.6–160 ppm, but for S-type granites or those that are tin-bearing – the classification of those in our field area (Higgins, 1985) – the range of Be is from 2.3–130 ppm (n = 11, average = 18 ppm). Yet another study on tin-bearing biotite granites in Alaska (Sainsbury, 1964) illustrates a range of Be concentrations from 2–26 ppm (n = 5, average 16.6 ppm). In this study, Sainsbury cites work done by Beus (1962; *Beryllium: Evaluation of deposits during prospecting and exploratory work*), who measured beryllium concentrations of >200 granites across the former Soviet Union and China (average ~5 ppm), noted that the average concentration in the complete subset of biotite granites is 4.1 ppm. In our study, we initially used the crustal average value for Be of 2.5 ppm, which was used in von Blanckenburg et al. (2012), who therein also cite Grew (2002); this 2.5 ppm value is clearly on the low end of estimates for beryllium content in felsic biotite granites.

We now believe our original use of 2.5 ppm was not the best-estimate (although reasonable) of beryllium content in our field area based on lithology alone, and we know that Be could, in rare cases, be as high as 130 ppm. Thus, we now make the following changes to our analytical approach: (1) We use a $^{9}Be_{parent}$ concentration of 4.1 ppm in our final calculations of D_m because this is the average of biotite granites comprising a subset of >200 granites (Beus, 1962). (2) We discuss that we did not measure bedrock and that bedrock estimates of Be could be as low as 2.5 ppm (Grew, 2002; used by von Blanckenburg et al., 2012) and use this value to calculate *Dm* for all samples based on this low estimate; these values of *Dm* are those from the previous versions of this manuscript. (3) Given that Beryllium concentrations can exist >100 ppm – albeit rarely – it is unlikely (but possible) that Be in our field area is as high. Thus, we take a more conservative approach to estimating a high end of the possible range for Be in lithologies similar to those in our field area and calculate *Dm* for all samples using a value of 18 ppm (average of 11 S-type granites and tin-bearing granites compiled in London and Evensen [2002]). We show all of these values in a redrafted Figure 7 (now figure 8) with most likely *Dm* values, calculated from 4.1 ppm, compared to ε values for the same sample sites, and we also show a likely possible range of *Dm* values if Be concentrations were low (2.5 ppm) or high (18 ppm). The overall result is that *Dm* values are very sensitive to $^{9}Be_{parent}$ concentrations in bedrock – an important finding – and that *Dm* for individual samples in our field area could range from being 2x higher than ε to being consistently <3x lower than ε. The higher the concentration, the lower *Dm* values become.

$^{10Be}F_{met}$: We also carried out a sensitivity analysis on the meteoric ^{10}Be delivery rate and how it affects measured values of *Dm* (assuming a similar, constant value of $^{9}Be_{parent}$ of 4.1 ppm). In doing this, we identified another study from New Zealand that presents an additional measure of $^{10Be}F_{met}$ (Graham et al., 2003; *Geochimica et Cosmochimica Acta, 67(3)*) and now include their data in our sensitivity analysis of how different measures or estimates of $^{10Be}F_{met}$ affects our calculations of *Dm*. In our sensitivity analysis, we find that measured values of $^{10Be}F_{met}$ from New Zealand (Reusser et al., 2010a; Graham et al. 2003) yield *Dm* values that would be consistently greater than those we present in this study using the Graly et al. (2011) precipitation-based estimates of $^{10Be}F_{met}$. The $^{10Be}F_{met}$ based on atmospheric-depth integrated rates (Masarik and Beer, 2009; Willenbring and von Blanckenburg, 2010) yield *Dm* rates that are consistently lower than those we present in this study. Using Holocene-averaged $^{10Be}F_{met}$ rates (Heikkilä and von Blanckenburg, 2015) yields *Dm* that are consistent with those that we present in this study using basin-specific estimates of $^{10Be}F_{met}$ from Graly et al. (2011).

Thus, barring the low end-member models for $^{10Be}F_{met}$ (Masarik & Beer; Willenbring and von Blanckenburg) and the measured values of $^{10Be}F_{met}$ for New Zealand, hundreds of km away (Reusser et al., 2010a; Graham et al., 2003), we find confidence in the way that our estimates for $^{10Be}F_{met}$ from Graly et al. (2011) and measured rainfall from Australian Bureau of Meteorology precipitation gauging stations yield *Dm* values that are consistent and within the range of those we would have calculated had we used the Holocene-averaged $^{10Be}F_{met}$ rate (Heikkilä and von Blanckenburg) from a dataset that has a much coarser resolution. We present a new figure in the

Discussion (Fig. 11) that shows the comparison of *Dm* from Graly et al. versus *Dm* calculated using $^{10Be}F_{met}$ from GCMs and measured rates from New Zealand.

It was suggested that calculating *Dm* using a global climate model would be preferable to using elevation-dependent mean annual precipitation, but we find in our sensitivity analysis that *Dm* using Graly et al.'s $^{10Be}F_{met}$ values plot squarely between *Dm* using $^{10Be}F_{met}$ values from either global climate model, in which case the choice of source of $^{10Be}F_{met}$ values does not matter, at least for this study. These details are now included in the discussion.

Dm calculations are clearly sensitive to the values of $^{9}Be_{parent}$ used, but not necessarily the choice of $^{10Be}F_{met}$ values. With regards to land use, no matter the $^{9}Be_{parent}$ used, the result is simply a shift in the overall magnitude of *Dm* but the relationship to the % of land-use classified as "High" to "Extreme" erosivity does not change, in which case we are unable to suggest that measurements of *Dm* are not affected by land-use.

In the spirit of transparency and openness to the broader geomorphology community who might be interested in using von Blanckenburg et al.'s (2012) denudation method – one that the Associate Editor expresses is in its infancy – it is not appropriate for us tone-down our discussion on land use and *Dm*, but we have presented the information required for readers to come to their own conclusions about these findings (e.g., R^2 value, p value, description of "erosivity", etc.).

2) Regarding the use of "Mg/km2*yr", I can follow your argument about metric vs. imperial tons and SI units. However, I would still suggest to use the more commonly used "t(metric)/km2*yr" (simply define "t" as being metric tons" somewhere). "Mg" is just very clumsy and not often seen in use.

Author Response: As American authors writing to an international audience, it is important for us to be clear in the units we use, and we know that some readers will inadvertently misinterpret our use of "t" to mean imperial tons, whereas there is no ambiguity in using "Mg." The use of SI units, such as Mg, is also the required unit notation for EGU journals, including *Geochronology*, so we are just being consistent with standard Author Guidelines. We have retained Mg.

3) The Methods part is very long- Would suggest that you use subheadings to give the section more structure.

Author Response: Done

Line comments:

100: "mass loss from" (typo)

Author Response: Done

Fig. 1: Please include a lat/long grid

Author Response: Done

118: 10Bem is desorbed under LOW pH conditions.

Author Response: Done

125: I know this sounds picky, but this should be “10Bem/9Bereac”-derived denudation rates (Just to avoid confusion with “10Bem”-based erosion rates). Here and elsewhere (e.g. heading section 4.2, line 278).

Author Response: Done

141…,located in northeastern Tasmania,

Author Response: Done

Fig. 3: Caption sais that “at least one full year of recorded data…”. From Table 1, I get the impression that the data coverage is much better (at least 4 years)?

Author Response: Yes, there are four years of data, and each has data for the entire year for at least one year (as opposed to a year where there is data available, but not a full year).

Table 4: Please change “Q” in D_m eq. to “10BeFmet”.

Author Response: Done

172: over which time period has this mass been removed?

Author Response: Decades since this is the timing of disruptive land use within the field area.

195ff: Note that grain size effects may impact the 9Be_reac/9Be_min ratio when measured in river sediments due to sorting (not for soils!), because 9Be_reac is potentially enriched over 9Be_min in finer particles. Hence, the D equation is not fully grain size independent (even though grain size variations in the 10Be_reac/9Be_reac are minor).

Author Response: We clarify that normalization minimizes grain size dependence in stream sand.

Again 195: “10Bem and the reactive as well as silicate-bound 9Be phases (9Bereac, 9Bemin, respectively)….

Author Response: Done

200: That equation/approach was originally suggested by Brown et al., 1988 (ESPL).

Author Response: We added a citation to Brown et al. (1988) here as well.

217: What does “to a single point” mean?

Author Response: Because we use the CRONUS online calculator (Balco et al., 2008), which requires a single set of coordinates and a single elevation for input in order to scale in situ ^{10}Be production, we need to find a way to summarize the elevations throughout the basin and a way to summarize the latitude. Rather than taking a mean value for elevation and latitude, we use the hypsometry of the basin to find an effective elevation, and we use the specific spatial geometry of the basin’s drainage divides to determine an effective latitude, all of which is described in Portenga and Bierman (2011).

280: The depth of regolith per se does not tell anything about mobility of 10Be_m. You could use that depth to estimate the integration time scale of Dm (Willenbring & fvB 2010), but mobility itself can only be evaluated from pH, discharge, resulting Kd, dissolved v. reactive measurements, reactive 9Be vs. 10Be….So, I am not sure that the text for regolith depth (around lines 285) is needed.

Author Response: The depth to regolith does not tell anything about mobility of $^{10}Be_m$, but it does indicate whether the potential for $^{10}Be_m$ mobility is low or high. Deeper regolith offers more potential for $^{10}Be_m$ mobility below the spallogenic $^{10}Be_i$ production zone. We therefore believe we needed to let the reader know we considered this possibility.

Table5: If I type in all numbers that you give, I get slightly lower values for Dm- e.g. 54 instead of 60 t/km2*yr for TG5. I think that lower value was the value presented in the first version of the MS (Table 3). I note that some of the values (Version 1 vs this version) changed. Why ? Flux numbers are the same as before, even though you used now different precipitation rates? Please check all calculations again.

Author Response: The previous version of Table 3 had not been updated with Graly et al.’s $^{10Be}F_{met}$ values based on measured rainfall rather than WorldClim. This is now fixed.

398: frost-cracking: this reads like there is basin-wide temperature data (and not only data from point stations). Would you like to put that into a map? Seems like frost cracking plays a dominate role on e (conclusion), which is why I would present such data somewhere.

Author Response: Temperature data (Figure 3) are given in Table 1.

411: remove the second “of”. What is “stymy” erosion?

Author Response: Stymy, as in to slow down; Mishra et al. (2018) suggests that increased rainfall leads to an increase in vegetation cover, which serves to slow erosion down.

414: Has % of bedrock outcrops been mapped in the George River basin, or is there data from landslide occurrence?

Author Response: Not that we were able to find.

424: The part of the sentence on absent long-lasting dilution effects needs a justification. The whole section (420-430) would benefit in my view from a topic sentence around the question whether e/ in situ cosmogenic nuclide concentrations are at steady state or not in the basin, and then present the arguments. Actually, if you discuss whether e are at steady-state, why not do the same for Dm? Clearly, 10Be_m concentrations are more sensitive to e.g. recent soil loss/general anthropogenic disturbances (see e.g. Belmont et al., 2014, ESPL, as an example for the available literature body on this topic). So, for 10Be_m concentrations this could be discussed. In this respect, the paper would benefit in my view from a sort of sensitivity analysis of Dm for changes in F10Bemet and 9Beparent (see my initial comment from the first round, mentioned above, too).

Author Response: We added a figure to the Field Area showing that our samples come from streams with concave-up profiles (Fig. 5), an indication that they are in steady state. We also reiterate this in the Discussion where requested by the Associate Editor. We also agree with the Associate Editor here, that $^{10}Be_m$ is sensitive to soil disturbance, which is why we present a figure showing the relationship between $^{10}Be_m/^{9}Be_{reac}$ denudation rates and erosivity (former Figure 8, new Fig. 9).

429: I don´t understand the part about "normalized along with bedload characterisitcs". Are your referring to the decrease in mean particle size ?

Author Response: This was unclear and we've revised the sentence for clarity.

443-446: That is a long sentence….almost "German". Please split. Is "Conservation and Protective Native Land Cover" a government project? (Why capitalized?)

Author Response: Revised to break that long sentence down and de-capitalized the land use designations.

Section 5.2: First, the heading isn´t very informative. Second, you may wanna consider half a sentence on saying that you now calculate sediment loads (Mg/yr) (i.e. area-normalized).

Author Response: We believe the header sufficiently describes the content of this section. We did add an introductory sentence qualifying how we derive the mass loads from ε and basin area.

Line 454: would suggest to replace "mass loss" with "mass produced".

Author Response: Done

463-465: You use the word "similar/similarity" three times in the same sentence. Please consider re-phrasing. Actually, the similarity with mainland Australia given their common geologic history not so surprising, but still a finding worth mentioning.

Author Response: Done

Fig. 9: I really like that Figure. A small issue for improvement, still: Could you indicate the trunk stream sample also in B/C, please?

Author Response: Done

483: This is in general a valid statement. But, it really depends where in the soil the clay rich horizons are, where 10Bem will be mostly located. So, I guess, this could be improved by relating to the local soil type and characteristics.

Author Response: Unfortunately, that specific of detail for soil depths is not available, which is why our comments here were more generic in nature.

511: It´s not clear to me where the bedload estimate is coming from. (Not from Table 6, it appears, where only suspended load is mentioned?)

Author Response: Bedload = ε at TG-9 minus the measured dissolved load minus the measured suspended load. This is described in the previous sentence.