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 (→ 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 ε (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 ε 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 ε (i.e. for T6-8, Dm are actually lower than ε). 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.

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.

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

Line comments:

100: “mass loss from” (typo)

Fig. 1: Please include a lat/long grid

118: 10Bem is desorbed under LOW pH conditions.

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).

141…,located in northeastern Tasmania,

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)?

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

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

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).

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

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

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

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.

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.

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 ε (conclusion), which is why I would present such data somewhere.

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

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

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 ε/ in situ cosmogenic nuclide concentrations are at steady state or not in the basin, and then present the arguments.

Actually, if you discuss whether ε 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).

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

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

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). Line 454: would suggest to replace “mass loss” with “mass produced”.

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.

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?

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.

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?)