Geochronology Discuss., referee comment RC1
https://doi.org/10.5194/gchron-2021-31-RC1, 2021

Comment on gchron-2021-31

Erica Erlanger (Referee)

Referee comment on "Cosmogenic nuclide and solute flux data from central Cuban rivers emphasize the importance of both physical and chemical mass loss from tropical landscapes" by Mae Kate Campbell et al., Geochronology Discuss., https://doi.org/10.5194/gchron-2021-31-RC1, 2021

This study presents the first estimates of paired 10Be and 26Al cosmogenic nuclide denudation rates for catchments around Cuba, and these denudation rates are compared with chemical weathering fluxes derived from riverine solutes. The authors compare these two metrics across catchments within different lithologies (sedimentary, igneous, and metamorphic) and find that chemical weathering fluxes are often higher than total denudation fluxes. The authors interpret these results as evidence for deep chemical weathering that occurs below the upper couple of meters that where cosmogenic nuclides are produced. The high chemical weathering rates in this landscape are also consistent with other tropical landscapes around the world that generally find high chemical weathering rates, which indicates low rates of physical erosion. However, the long-term rates appear to generally be lower than short-term sediment yield fluxes, which the authors attribute to a period of enhanced agriculture during Soviet occupation.

The manuscript is well written and easy to follow, which is much appreciated! The goals of the manuscript are clear, and the background descriptions of methods such a cosmogenic nuclide dating were also well explained. The figures are all necessary and of good quality.

The results are certainly interesting and suggest that long-term denudation rates underestimate short-term denudation rates and chemical weathering fluxes. Most of my comments are minor, although I have two major comments related to the interpretation of the data and the decision to add the denudation and weathering fluxes, which I detail below. I hesitated between putting minor or major revisions for this manuscript, since I think they are actually moderate, but may also hinge upon further clarification of the methods and regional geology.

Moderate/Major Comments

The Methods section, specifically related to the calculation of the weathering fluxes, requires more detail. As currently written, it's unclear to me whether the authors partitioned the concentrations of Ca and Na for silicate lithologies. If not, then this could very well call into question the interpretation of the chemical dissolution data as reflecting deep weathering that is not captured by the cosmogenic nuclide data. It may be that a clarification of the Methods section and added detailed to the regional geology would address my concern.

In the case that the full Ca and Na concentrations are used to determine weathering fluxes, the authors would be essentially comparing a "quartz" or silicate denudation rate with a chemical weathering flux that includes ions derived from both silicate and carbonate rocks. There is little description of the lithologies present in the study area, although Bierman et al. (2020) state that there is likely carbonate in all sampled basins. It would be important for the authors to clarify in what form carbonate is present (e.g. as a cement, as discrete layers within sedimentary rocks, as individual units, etc). The reason this is important is because the authors need to understand whether the silicate and carbonate lithologies are weathering together—a cosmogenic denudation rates encompassing all lithologies could in this case be appropriate—or whether they denude separately. In the latter case, it would make sense that the weathering fluxes might be altogether higher, particularly in the marine sedimentary units, which might reflect a large carbonate weathering flux that is largely absent from igneous and metamorphic rocks (except perhaps ophiolites)? Even for landscapes (e.g. New Zealand Southern Alps) where carbonate is present only in hydrothermal veins, the calculated carbonate weathering flux is still higher than the silicate weathering flux (Jacobson and Blum, 2003). So, it could make sense that the dissolution rates are higher than the denudation rates, since they are in fact reflecting all lithologies (carbonates, silicates, and maybe evaporites) while the denudation rates reflect only a portion of this. Of course, perhaps it's also a combination of deep weathering and carbonate weathering that are driving these rates. I'm not familiar with other studies in the tropics that have used chemical weathering from riverine solutes, so maybe there are comparisons that can be made there. Without partitioning the weathering into silicates and carbonates, I'm not sure how the authors can exclude this possibility.

Since you have estimates for ion concentrations for precipitation, you should at least test how much this would alter your own data. You can also correct for cyclic inputs using global stoichiometric ratios for global average seawater. If these corrections are indeed minor, that would be justification to use the uncorrected data.

Your only mention of active tectonics in Cuba is on Line 283. The active tectonics, particularly faults and fractures, could be important structure that facilitate the circulation of deeper groundwater and weathering, so I think more information needs to be added to the "Study Area" section that gives an overview of the tectonic setting. You also mention evaporite deposits in the basins, and your sentence on line 303 suggests that they are not exposed at the surface. If they are indeed only present in the subsurface, this suggests that you may have deeper circulation of groundwater in the region. Are there perhaps any springs in Cuba, thermal or otherwise?

It's also not clear to me why the authors combine the dissolution rates with the denudation rates. The denudation rates already include the dissolution flux, since it is the total mass loss, so this seems somewhat redundant to me and goes beyond what a maximum denudation estimate could realistically be.

On Lines 357-364, you also compare the difference between your summed denudation and dissolution with the original denudation flux, and refer to that as the CEF. Perhaps I'm misunderstanding something, but how is your factor of increase comparable to the CEF? Riebe and Granger (2013) state that you need measurements of an insoluble element (usually Zr) to calculate CEF, which you also mention in section 2.2 but no insoluble elements were measured in this study.

Minor Comments

The authors define terms for cosmogenic nuclide denudation rates as "sediment generation rates" and chemical weathering as "rock dissolution rates". These terms are not used consistently throughout the paper. I found example where "erosion" was used for the cosmogenic nuclide data, or to refer to physical erosion. I also found examples where "chemical denudation" or "chemical erosion" was used instead of "rock dissolution" or where "denudation" was used instead of "sediment generation rate".

"Sediment generation rate" to me implies physical erosion, rather than total mass loss or surface lowering, which is what the cosmogenic nuclide data represent. I would highly recommend instead adopting the terms "denudation" and "chemical weathering", in order to avoid confusion, and to use them consistently throughout the paper.

I would mention already in the Introduction that you also compare long-term denudation estimates from cosmogenic nuclides with short-term estimates from sediment yield fluxes. This point was on my mind for a long time as I read the paper, until I reached the discussion where you do in fact do this.

In general, I found that the figures could be referenced more throughout the paper when referring to results that they illustrate. Examples include line 248, where you could reference Figure 4 and Figure 7, and line 325, where you could reference Figure 4.

Methods. More detail can be given as to the specific methods used to calculate weathering fluxes. You mention using the West et al. (2005) method, although this study defines a couple of different methods for calculating weathering. If your method is equivalent to his cation weathering flux, it would be good to mention this and include an equation to make clear which cations and anions went into your calculations.

Supplement. The lithologic classifications shown on Figure 1 are not consistent with the categories given in Table 2 of the supplement, and "ultramafic" is missing altogether. It would be useful to include an additional row in the supplementary table as umbrella terms (with names equivalent to the categories in Figure 1) that would cover the various columns from the supplementary table.

The dashes for ranges of numbers (e.g. Line 227) should be En dashes.

When using terms such as low slope to describe a basin or other feature, they should be hyphenated, so “low-slope basins” (e.g. Line 267), as you’ve done for “low-relief topography” on line 269.

Your conclusion is the first time you state that you made the first measurements of cosmogenic nuclides in Cuba! You should definitely mention this in the introduction, since this is a nice contribution.

Figures

Figure 1. The legend for the geology is also somewhat unclear. The terms uC and pE are never explained in the text or the caption; I only figured them out when looking at the Supplement. It’s also not clear to me from this Figure or from the supplement why the Upper Cretaceous marine deposits are differentiated from the Post-Eocene Marine deposits? If you insist on keeping them separate, it would be important to mention in the text what the differences in composition are.

Is the term “undivided” supposed to be “undifferentiated”? Finally, it would be helpful to the reader to know what “Other” stands for, at least whether they are sedimentary rocks, igneous, or metamorphic, since some of your basins in the center of the field area appear to have a large part of the catchment that drains these areas.

The map figures would benefit from having the river networks includes and combining a hillshade map with the elevation DEM (as in Bierman et 2020). Otherwise, it not clear where the sampling location is for each basin, since I cannot tell where they flow! I would also like to see the locations of the discharge stations plotted on this map. If they are far away from the sampling location, it could be that they do not accurately reflect the discharge passing through the sampling location for the solutes. You also state that only 3 of your catchments had both sediment yield and cosmogenic nuclide measurements, so it would be good to see a map illustrating the locations where the other sediment yield measurements were made within your field area. Otherwise, it’s difficult to make statements to this effect that sediment yield measurements are higher or lower than

cosmogenic nuclide measurements, since there is overlap between the two datasets in your figure 8.

In your figure caption you write the panels with uppercase letters A and B, but there are shown as lowercase in the figures themselves.

In Figure 3C, could you put one color boundary that separates a ratio between rock dissolution and sediment generation at 1? That way the reader can more easily see where one process has a greater magnitude than the other.

Line Comments

Line 66. The authors use the term “at depth” in the paper. I understand that this implies depths greater than the upper couple of meters where cosmogenic nuclides are produced, but it would still be helpful to the reader to put some quantitative bounds on this term.

Lines 71-76. This is a very long sentence that was a bit hard for me to follow. I think it would benefit from being split.

Line 73. Are these the same data that were used in Bierman et al. (2020)? If so, I would cite them here, since they are already published.

Line 87-90 I understand what you mean with this sentence, but found it to be a bit misleading when I first read it, since it specifically refers to only rock dissolution. My suggestion would be to say "…cosmogenic nuclide rates cannot provide insight into **denudation processes** -such as rock dissolution- **occurring below depths of…** "

Line 121. I think a title for section 2.2 is missing

Line 241. Do you mean "Supplement T1" here?

Line 243. Figure 7 is also a nice illustration of the fact that anything that lies left of the 1:1 line has higher chemical weathering rates relative to total denudation rates!

Line 265. You state that rock dissolution rates are strongly negatively correlated with slope, but your R^2 value 0.27, so this should be a weakly negative correlation.

Line 284. I was a bit confused by the beginning of this sentence. Does the strong negative correlation refer to the Ollier study as well? It might be clearer if you start the sentence with a mention of this study so that it's clear to the reader you are not referring to your own results.

Line 316. “of” is written twice.

Line 406. What do you mean when you say that “…neither consider the solutional component of denudation”. If you are comparing denudation estimates, then they do include chemical weathering as part of the total mass loss.

Line 406-408. It would be useful to cite the Bierman et al. (2020) study here to support your statement, since they made maps illustrating the locations of human influences.

Line 431. I think you mean “of” here?