

Geochronology Discuss., referee comment RC2  
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## **Comment on gchron-2021-31**

Claire E Lukens (Referee)

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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.,  
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This manuscript presents new denudation rates from Cuba, and compares denudation measured with cosmogenic nuclides to solute loads in rivers from a previous study to infer substantial deep weathering in this low-relief, tropical setting. Using a paired nuclide approach, the authors are also able to constrain vertical soil mixing and quartz enrichment in some catchments. Overall, I found this paper to be well-written and interesting, and the implications for understanding global weathering fluxes and deep weathering make it both important and timely.

My concerns lie mostly with the way the data and analysis are presented, rather than with the underlying approach. I do think the required revisions are substantial, but relatively straightforward (more like "moderate revisions" - I'm in agreement with the other reviewer on this).

### **Major comments:**

Rock dissolution rates are inferred from solute loads in modern rivers and discharge measurements, which are from a previous study (Bierman et al 2020). The methods really aren't described here, and they need to be explained in more detail. In the 2020 paper, they're described very briefly in a supplemental file. As I understand it, solute fluxes were measured once, from samples taken at moderate discharges. These fluxes were then used to calculate weathering fluxes using average river discharges. However, if average discharges used to calculate fluxes were different from river discharges at the time samples were taken, or if fluxes vary substantially from rainy to dry seasons (which is very likely, given the seasonality of precipitation), these measurements could be way off. Incorporating some additional info on the variability in surface hydrology would be useful, and that variability should also be incorporated into uncertainties on the rock dissolution rates. I am concerned that there are no uncertainties plotted on rock dissolution rates in the figures, which suggests that these fluxes are known much more precisely than is

probably true.

It's also not clear how carbonate dissolution was handled, or how substantial evaporite contributions might be. These methodological details need to be included, as they will have substantial impact on how the high weathering fluxes are interpreted.

I realize the authors are trying to use clear terminology, but the use of less-jargony terms here actually creates some confusion. The big one is "sediment generation", which is used for denudation measured with cosmogenic nuclides; this includes mass lost via rock dissolution in the top several meters of weathering profiles, so terming it sediment generation is potentially confusing because it could be interpreted as just the physical part of the flux. It is clearly defined early in the manuscript, but becomes problematic later in the discussion.

There's a timescale mismatch between the solute fluxes and denudation rates, which isn't really discussed. Solute fluxes in rivers may represent a very brief snapshot, or may integrate over longer timescales depending on the groundwater residence time. Cosmogenic nuclides integrate over thousands of years in slow-eroding places, which is presumably a much longer timescale. There is also potential for a spatial mismatch that makes these rates inappropriate to directly compare – the denudation rates reflect parts of the landscape that contribute quartz to rivers, and the solute fluxes reflect parts of the landscape that are dissolving. Given the diversity of rock types, including carbonates and mafic rocks with little quartz and high dissolution rates, the denudation and dissolution fluxes may be biased toward different parts of the landscape (and potentially different spatial extent). I don't think these potential mismatches are a manuscript-sinking problem, but it would be good to acknowledge the limitations and assumptions somewhere in the discussion.

Groundwater – is it possible any of it exports directly to the sea, without going through rivers? In this case there may be even more mass loss via weathering.

The explanation of  $^{26}\text{Al}/^{10}\text{Be}$  ratios via soil mixing is very interesting. Why are these particular soils mixed, and not others in the study? (Is there a mechanistic reason, or something about their position on the landscape? This is touched on briefly, and could be expanded if there's room – this is certainly not a requirement for publication, I just think it's interesting!)

I don't think it's appropriate to sum sediment generation and rock dissolution rates at the end – this is stated to be a max estimate, but discussed as though rates could actually be that high. This is why I'm not enthusiastic about the use of the term "sediment generation" – it implies that it's just the physical part of the flux, but it includes all the weathering fluxes that occur within the top couple of meters of weathering profiles (including all soil weathering). Summing sediment generation and rock dissolution counts near-surface weathering twice, which means these max denudation rates are an

overestimate, and perhaps a substantial one.

At line 405, the authors state that sediment yields and cosmogenic nuclide-derived denudation rates is directly comparable because the latter does not include mass loss due to dissolution. This is not true, as argued above. It is also interesting that the discrepancy between modern and long-term rates could reflect either changes in weathering depth or agricultural inputs (likely the latter), and teasing apart the two influences is potentially complicated in this deeply weathering setting.

The discussion around Figure 7 could be expanded to include differences amongst rock types, which seem to be driving most of the variability. There's a lot more information in this plot than simply saying erosion and weathering aren't correlated in Cuba, which is how it currently reads.

### **Technical comments:**

This manuscript is very well-written and easy to follow, and the figures are generally quite good.

Line 121: 2.2 needs a title

Line 150: "Cuba's climate is tropical wet and dry" – it seems odd to describe something as both wet and dry like this. Maybe better described as strongly seasonal?

Figure 2: using a different color bar (or even trimming/stretching the grayscale) would make the elevation map more useful. Alternatively, a DEM hillshade overlain with the geology could simplify this to one panel, and make it easier to determine how lithology and topography correlate. "uC marine" and "pE marine" labels should be explained in the caption.

Fig. 6: trends in precip plots would be easier to see if the x-axis started at some higher value (750mm? 1000mm?) rather than zero. I appreciate that you didn't include trendlines on these plots, but just gave us the stats instead, especially where the trends are statistically significant but not necessarily meaningful.