Dear Prof Mudd,

Thank you for your insightful comments and suggestions on the latest version of our manuscript. Below we address all your comments. In addition, we have carried out some extra analyses to make the paper more consistent in the comparison between long- and short-term erosion rates. Specifcally, we plotted all the global long- and short-term data against MAP (derived from the GPCC gridded dataset) – previously we had just plotted these for the USA. We further plotted median erosion rates from each basin (both long- and short-term) against MAP to check for any biases in spatial sampling (no significant biases found). We fitted a LOWESS regression through the short-term vs MAP relationship (as for the long-term) and found a similar pattern (albeit with higher overall rates). We have edited the manuscript to reflect these changes. We hope these corrections are to your satisfaction – but please do get back to us if there are any further issues or queries.

Best wishes,
Shiuan-An Chen, Katerina Michaelides, David Richards, and Michael Singer

---

1. Co-located short- and long-term erosion rate data.
We had originally done this analysis but did not include it because the *n* of co-located points was so small (*n* = 79). However, we have now added a new figure with the co-located points (new Fig. 3) which shows a similar pattern as the global dataset (Fig. 2b) which combines all data points within each climate zone.

2. The data of Figures 3, 4, and 10 are from the USA only.
We originally only analysed the relationship between MAP and erosion rates in the USA because of the widespread systematic availability of gauge rainfall data across the USA. We have now constructed this scatterplot between long- and short-term erosion rates and MAP globally based on the GPCC rainfall dataset. So the new Figs 4, 5 and 11 (the old 3, 4, and 10) now present global relationships. We have accordingly updated the LOWESS fits for both long- and short-term data and globally, there is now a similar pattern between the two, albeit the short-term erosion rates are typically higher than the long-term rates.

3. Spatial bias of long-term erosion rate sampling.
We have now computed the median long- and short-term erosion rates for all drainage basins globally and the overall relationship between MAP and erosion rate remains the same (new supplemental Fig. A1). Therefore, the sampling bias does not translate into any significant influence on the shape of the relationship.

Line 71: high uplift rates do not have to result in threshold slopes. They could just result in higher relief. I would add "increased relief" before "threshold slopes".
Done (Line 69)

Line 73: Tectonics itself doesn't lead to rapid production of sediment during rainstorms. It increases gradients, and the steeper gradients are the cause of the increase sediment production (or actually sediment transport…increased sediment production is because the increased sediment transport results in thinner soils, which leads to increased sediment production). Anyway, this sentence glosses over a lot and could be edited to add some depth.

Done (Line 72)

Line 94: typo: "erosion via"

Done (Line 93)

Line 141: It is not the erosion rate that determines the CRN concentration, but rather the denudation, or total mass loss rate. Which includes chemical weathering. For accuracy you should say this and then refer back to the point from the previous paragraph that dissolved load is usually quite small. You do say this on line 176 so why not here?

Done (Line 141)

Line 151: Say something like: "10Be erosion rates average erosion rates over a characteristic timescale determined by the nuclide concentration divided by an average nuclide production rate. This equates roughly to the time it takes to erode approximately 60 cm of material (Kirchner et al 2001). Therefore a rock lowering rate of 1 mm per year equates to a 600 year timescale, 0.1 mm/yr 6000 years, and so on."

Done (Line 148)

Line 265: Typo: "summed"

Done (Line 254)

Figure 3: Why are only the USA data used?

See point 2 above

Figure 4: Why only the USA?

See point 2 above

Figure 5: There us a light purple and a dark purple in the figure but not in the legend. Add both purples to the legend.

Done (new Fig. 6)

Line 403: I'm working from a paper copy so can't search, but is R_S/L only defined previously in a figure caption? It has been a while since it was mentioned so I might reiterate what it means here.

First mentioned in Line 301 (and also in the caption of Figure 2), but we have added in Line 426, too.

Line 440: I don't think you have really addressed the sampling bias in the CRN data. I am quite confident that if you took a bunch of CRN samples from the Sahara you would get a very low erosion rate. In any climate region you could find a low relief landscape to sample if you wanted to pull the erosion rates down, or a high relief landscape to pull the erosion rates up. Also is this not from the USA data only? That is what is suggested in Figure 3.

See point 3 above

Line 443: Again, sampling bias. How do you know it is not driving this trend? This statement appears to be making a global comparison, but figure 3 says the data is from the USA only. If it is from the USA, is the trend because most of the very high erosion rates are from the mountains around Los Angeles, which are heavily sampled and uplifting the fastest of any sampled region in the USA? Also, the large numbers of lower points around 1000-1250 mm/yr: are these not the data from the Appalachians collected by Paul Bierman's students? How were the data aerially weighted? If someone collected large numbers of data from small basins (e.g., in the San Gabriel Mountains) vs a handful from large basins e.g., the handful of sites in Texas, which cover basins that are cumulatively much larger than the dozens of sites in the Transverse Ranges, do these get a greater weighting in the LOWESS regression?

See point 3 above

Line 459: This is an interesting point because the earlier papers had a much more limited dataset than you do. As you have collected a very large suspended sediment dataset, you only see noise when relating the flux rates to climate. So the trends from those earlier papers appear to be from sampling bias. How do you know the climate trend you see in the long-term rates is not from sampling bias?

See point 3 above and the intro to the rebuttal for changes made to the analysis. The LOWESS regression for the short-term erosion vs MAP is now consistent with the long-term LOWESS.

Figure 10: Again, is this USA data only or global data?

See point 2 above

Line 541: Delete reference to stream power law (stream power law uses local, not basin gradient to determine local erosion rate). Also the sentence doesn't need it.

Sentence removed

*Line 561: This is, at best, misleading. Basin area only affects erosion rate in the stream power law if everything else is equal. But everything else is not equal: gradients get steeper as drainage area goes down. So under the steady state assumption there is **no** dependence of erosion rates on basin area if you are sampling the same basin at two different places. If you are in a active mountain belt where the basis are expected to be eroding at different rates, you cannot just compare the slopes of two basins, or the gradients of two different basins. You have to combine both of these things: they are inseparable if you assume stream power is driving erosion rates (they are also inseparable for virtually any proposed model of channel incision). This is the reason why people use the channel steepness index rather than either gradient or drainage area.*

This sentence about stream power has now been removed.