Dear Prof Mudd,

Thank you for providing us with further detailed and helpful reviews of our manuscript. We fully agree and acknowledge that this manuscript is attempting something different and that the analysis contains many uncertainties arising from the data sources and their assumptions. However, we aim for this paper to be a stepping stone in a wider conversation about the links between climate and erosion – not a definitive answer. We hope it sparks a debate in the community and provides some new perspective. We have therefore, attempted to make all the uncertainties and caveats as clear as possible and we have restructured the Introduction (and added subsections) and the Discussion. However, please do let us know if we need to clarify anything further. Below, we summarise briefly the requested revisions and the corresponding lines in the manuscript in response to each. There are several requests from the Reviewer which we feel are out of scope with our paper and we also justify this below. We hope the new revisions add clarity to our paper.

Best wishes,

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

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In a nutshell the requested additions that we address in the manuscript are:

1) Acknowledge and explain Sadler effect.

We have added discussion to manuscript (new lines 167–174).

2) CRN integration timescales. (AE and Reviewer)

We have added discussion to manuscript (new lines 188–192)

3) Unpick tectonic from climate signal (AE)

We have added a new section that analyses the relationship between climate and erosion in tectonic and non-tectonic regions (new lines 352–363, 442–450, and new figures 4, 10b, A1, A2)

There are several suggestions from the reviews that we believe are not in scope with our paper:

4) Use climate models to "inspect" past climate change (Reviewer)

This is not a trivial request and the huge effort required to do this would yield very little useful quantitative information that could be used in the paper. Paleoclimate models are full of uncertainties themselves, and while they can indicate where climate was different in the past, we would then not be able to use this information in any quantitative way (e.g. extract meaningful MAP) with the erosion data. I assume the Reviewer would want us to look at climate at different points in the past (e.g. 1000, 5000, 10000, 30000 years ago etc.). This is not as simple or trivial as they imply! There are numerous paleoclimate models, each with their own assumptions, limitations, and uncertainties. We explicitly state our assumption (and its limits) about climate change in the manuscript.

5) Correct for chemical vs physical weathering (AE)

While it does make sense to make this correction to allow for a more direct comparison between sediment flux measurements and CRNs, this is not a trivial exercise due to lack of relevant data. If the average offset between erosion rate and denudation rate could be provided, we'd be happy to correct our plots accordingly. Sadly, the OCTOPUS database does not contain the relevant info on erosion rates (correcting for chemical weathering), and we have no intention in going back to the thousands of entries in this database to compute the chemical weathering component, especially because the relevant info would not be available for most studies. More to the point, this step is not necessary to broadly compare denudation rates across different landscapes, as chemical weathering is undoubtedly small compared to physical erosion. It is also not germane to exploring the relative differences between erosion rates across different climate categories, which is what we have done here. Furthermore, we don't have co-located sediment flux and CRN data, so using sediment flux (and associated dissolved load) is not viable. We have added a sentence or two explain how climatic variations in chemical weather might be (or not) reflected in the CRN data.

6) Use global vegetation map to investigate role of vegetation on erosion (Reviewer).

The focus of our paper is on the role of climate. We discuss vegetation in this story as a likely factor that co-varies with climate, but it is beyond the scope of the paper to include a whole separate analysis of global vegetation as suggested by the reviewer. This would be something to follow up with potentially. Moreover, the Köppen-Geiger climate index includes vegetation within it.

7) Do principle [sic] component analysis for all factors (Reviewer).

We do not feel such an analysis would add much to this paper. In fact, we feel it would simply add more confusion to an already complicated story. Many authors infer too much explanatory power to PCA, but it is only a statistical method and does not clarify causal mechanisms. Anything further would lengthen (not shorten) the paper and add confusion, given the large number of variables included.

Other reviewer comments:

- *I think I raised this concern in my first review that a 1.4-fold increase in short-term erosion rates when comparing anthropogenically and non-anthropogenically impacted regions is in my view not a "significant" increase (Fig. 5). Especially in the light of what is written in lines 402-416 that make it sound like the USA dataset, and also the entire short-term dataset because of this high variability, may not be representative enough to draw such a conclusion.*

As we explained to the reviewer's comment before, the magnitude of a difference between medians does not indicate whether or not the difference is significant. Significance in this case is tested via the Kruskal–Wallis test, which showed that these two distributions are indeed statistically different (see manuscript). However, we also discussed the potential reasons why the difference is smaller than was shown in the literature.