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Comment on esurf-2021-7

Anonymous Referee #2

Referee comment on "Global analysis of short- versus long-term drainage basin erosionrates" by Shiuan-An Chen et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-7-RC2, 2021

The manuscript by Shiuan-An Chen et al. describes a compilation of cosmogenically derived denudation rates and a compilation of short-term erosion rates from gauging data and couples these together to ask/answer questions about long- and short-term controls on erosion rates. I commend the grad student, first-author on a lot of work done in getting these datasets put together but I think the paper could be radically improved. I like the general idea of the paper but I found a couple of issues that I just can't get past in order to believe the results. First, denudation from cosmogenic nuclides is a combination of weathering products and erosion fluxes, but there is no mention of this in the paper or accounting for it by combining solute fluxes, or alternatively, convincing me that these data don't require this. Second, the paper reads like a choose-your-own topic where the authors pursue, in my opinion, too many different avenues. I would have preferred if they had decided, after doing all the background analyses that appear in the MS currently, which is the most interesting of the findings and then focusing the MS around that idea. Sadly(?), sometimes science involves doing work behind the scenes that doesn't need to appear in print anywhere. Personally, I find it really interesting that the analysis shows something very different from the Kemp et al., 2020 Nat. Comm. paper and would focus on that aspect given the strengths of the methods here and the flaws in that Kemp paper. Then, all the information/figures would be circling around this single topic. So I recommend a substantial revision to make the MS more readable/interesting to the reader. Currently, it has a PhD dissertation-chapter style, but not the style of a MS that tells a story or makes a compelling point through several lines of evidence.

Abstract comments

"Measuring erosion rates, analysing their temporal variations, and exploring environmental controls are crucial in the field of geomorphology because erosion through sediment transport in drainage basins shapes landforms and landscapes." <-- This is a confusing first sentence because it seems to imply that erosion is important but then that sediment transport is important. Erosion of sediment and transport of sediment are different things when viewed through the cosmogenic lens.

"unpicking" <-- perhaps use 'unpacking' or better 'unraveling' instead? In any case, are they really controls or are they simply correlations? I'm guilty of doing this leaping myself so I know it is tempting.

Introduction
I think the authors should take another try at creating a compelling abstract. This current abstract goes into details that are both irrelevant and either incorrect or imprecise. Fortunately, the authors don't need to correct each statement necessarily. It should be tightened up to focus on the paper and analysis done and conclusions drawn. I suggest writing papers backward: i.e. write the conclusions, then the discussion necessary to support them, then the results, methods, only parts of the intro necessary to tell the story at hand and then the abstract. This paper seems to be written the opposite way. It should really help rewrite the paper a bit more logically. Missing from the analysis are the other ways long and short-term data can be different including aliasing and the likehood of rare events (a la Kirchner, 2001 already cited) as well as the likely timescales of sediment storage and purging, which are relevant for sediment gauging data.

Here are some things that should be fixed and in some cases removed especially if irrelevant in a revision:

"The erosion rate of a drainage basin is an important geomorphic quantity because it reflects the net flux of sediment from source to sink in drainage basin and correspondingly, the rate and spatial pattern of landscape evolution." <-- There are a lot of concepts mashed together without regard for precise language or concepts here in this first sentence, which paints a negative first impression of the work, unfortunately.

Line 50: I take issue with these statements. There are many indications that sediment storage can be on the order of millennia in floodplains. Also, it's unclear exactly what timescale they are thinking is negligible. There are very few cases where bedload and suspended sediment have both been quantified to a degree that one could actually say this. I would feel uneasy making this statement for the whole world from just a handful of studies.

Line 65: This whole paragraph is riddled with incorrect statements about the basin-wide cosmogenic method and missing are some critical parts that should be included. Cosmogenic nuclides don't assume no sediment storage. This is another example of imprecise language. If that was the case, no one would ever publish any cosmogenic nuclide data, because there's obviously sediment storage in watersheds. Cosmogenic nuclide basin-wide erosion rates do not assume no decay. It is often negligible but it fact we *know* that decay exists and is ongoing. Also, there is a lot of data to show that in many cases there *are* discrepancies with 10Be-derived erosion rate with differing grain size. In fact the earliest paper (Brown et al., 1995 that the authors cite) showed that this was the case. No erosion–deposition cycle? No, that's not how I understand it. Those papers don't say cyclicity doesn't exist anywhere that I remember. Quartz is not assumed to be equally present throughout the catchments - that is something that is checked, and if it isn't the case, it is corrected for or we don't use the method if it cannot be accounted for.

Cosmogenic nuclide analyses do actually need to assume that catchments have been receiving cosmic radiation throughout the entirety of the time they have been eroding the layer that has moved through the production zone and that the eroded sediment is coming from the surface. So, if a catchment has been glaciated - especially if only part of it has been glaciated - the concentration generally can't be used as reliable indicator of the erosion rate. Unless(!), the erosion rate was so high that the timescale of averaging is less then the glacier retreat age and it is only scraping off the very surface - not below the attenuation length. This is maybe the case in a couple places (New Zealand?) but it also creates circular reasoning given that the averaging timescale is determined from the erosion rate that might be too high owing to the glaciation itself. What a pickle! Another strategy is to not assume that t->infinity (in steady state) and to assign a deglaciation age to the "t" (time) in the erosion rate equation. Unfortunately, these conditions for "ok cosmo erosion rate data" from glaciated catchments are not met in the OCTOPUS database to the extent that you should trust them to make a definitive statement about glacial vs. non-glacial erosion rates. Any small difference you find in the two datasets could easily just be due to violation of the 10Be method. If the rates are lower for the glacial catchments, then you could maybe qualitatively infer something (I don't know how it would be quantified) since the glacial bias would cause the observed rates to be too high - not too low.

Line 80: The authors go from 'global' to 'nation' a little too quickly. (Also, I know this wasn't the intent but it sounds like the authors are saying the US is the only nation in the world.)

Line 84: Typo: "of sediment yield" not "on sediment yield" but also saying non-linear here evokes the wrong concept. At mid-MAPs, the erosion rates are highest and are lower for very high MAPs and very low MAPs. Non-linear is a very vague way to say this. It is also non-exponential. You might say, there's a mid-MAP maximum or something like that. A similar fit is described much better in the later section that talks about the Misra et al., paper.

Line 95: "Global analyses of short-term erosion rates from suspended sediment records suggest that a change to agricultural land cover has enhanced erosion rates by one to two orders of magnitude (Dedkov and Mozzherin, 1996; Montgomery, 2007; Wilkinson and McElroy, 2007; Kemp et al., 2020)." I don't think this is exactly right since these authors don't show a timeseries with agriculture imposed at some point. They simply show that agricultural rates are higher than different areas with other rates - sometimes at discrepant timescales, which is a fraught topic. Of the places currently in the literature (before this paper) where long term and short term rates are compared, in Covault et al 2013, which the authors cite, >50% of the long-term rates are higher than the short term rates.

Line 100: To my knowledge, nobody has definitively shown with data that vegetation actually plays a role like the authors are suggesting here (except perhaps Vanacker et al., 2007), it is a hypothesis.

Line 110: Similar comment to that above: 'higher rates are *associated* with glaciated terranes' might be a defendable statement (but not using cosmogenic nuclides, since they actually don't show this.) Are the authors actually saying that the decreased infiltration that fires create in soil surface geochemistry (that last for only ~1 week to months at the most) are responsible for higher long-term rates of erosion? There's really no good way (with cosmogenic nuclide data) to show which areas are burned more or less over the millennia of averaging.

Line 118: Good paragraph!

Line 136: topology? I don't know that this is actually achieved.

Line 210: "To extract river profiles from the database for comparing topographic parameters with erosion rates, we chose a subjective distance threshold as 150 m between river profiles and erosion rate sampling points (i.e. selecting river profiles which are within 150 m to the closest erosion rate points), and calculated the mean slope gradient and total relief of river longitudinal profiles."

I don't see why such a large window was used here. Usually, people sample for 10Be right on the river itself and this highlights a potential problem with the geoid or projection used, if the points are this far away from the actual river on the map. The OCTOPUS sites have made sure that the sites are approximately on river sites.

Line 225: To a reader not familiar with kruskalwallis, could you describe the method and why it was chosen? This is fairly important because so many variables here interact so the authors could be/are conditioning on a collider. I think the kruskalwallis function does not help to eliminate this issue. I hope another reviewer covers some suggestions for stats that would be helpful to the author because I don't know what to use with so much nonlinearity and dependencies in the data. For example, higher precipitation and lower temps are actually *created* by mountain ranges. So how would one disentangle slope and elevation from those climatic effects. This is not the way to account for that.

Discussion and conclusions:
I assume these will radically change in scope/focus with submission of a major overhaul revision.