Review to ESurf-2021-7 by Chen et al (R2)

I am reviewing this paper now for the second time, so I am familiar with it. The general idea is that a global comparison between decadal-scale suspended sediment yields and longer-term cosmogenic nuclide-derived "erosion" rates allows to draw conclusions about what is controlling either. Although the manuscript has benefitted from a general streamlining of the overall message (i.e. now mainly focusing on the influence of climate), I still find it wordy, the discussion needs re-structuring (see below) and the overall suggestions by the reviewers have in my view not been adequately addressed. Below, I focus on 6 major issues that should be dealt with before the paper is ready to be anything close to being published.

1) One issue (still) regards the integration time scale of cosmo rate: The reviewers suggest to go into more depth e.g. regarding the fact that the integration time scale of cosmo rates is a function of the "erosion" rate itself, and also includes weathering (being hence a denudation rate!), in contrast to sediment yield data. As far as I read the text, the cosmo integration time scale is only very generally treated by saying that these rates "integrate over 10^3 to 10^6 years" (l. 234-235), and hence the "data covers several glacial-interglacial cycles". Perhaps the authors are not aware of the fact that by diving by 60 cm (rock) or 100 cm (soil) depth scale, the integration time scale can be calculated from the denudation rate, and this type of information can be used to discuss the data and the trends with e.g. climate proxies. The authors say they have "clarified" that issue, but fail to include this anywhere in the actual discussion of the trend in D vs Precip. The authors ignore a great paper by Schaller & Ehlers, 2006, that looked at the "limits of quantifying climate driven changes in denudation rates with cosmogenic radionuclides"- although the paper is more focused on denudation records through time, a lesson the authors could learn from it that cosmo rates have this inherent variable integration time scale, which should affect the pattern with recent (!) MAP they observe. In my view, they should first show that climate hasn't changed for the regions they use data from, and then they can start making the argument that cosmo rates and MAP follow some trend. Just simply saying that they use recent MAP data because everyone else does that seems a bit too oversimplified. There are global climate models that could be inspected for that purpose. One way of dealing with the integration time scale of cosmo rates would be to include a smoothing function in Fig. 3 (i.e. the "error bar" of the smoothing function could be linked to the integration time scale of the y-axis, the denudation rate). Where higher rates occur, integration times scales are shorter, and longer for lower rates. How does that affect the trend shown in Fig 3a?

2) Issue 2 is on the Sadler effect. Note a paper by Wilkinson 2005 (The Journal of Geology) that uses precipitation amounts and duration as analogue for the effect of hiatuses on sedimentation rates. It is generally describing the effect of hiatuses on datasets, such as any rate determined over some length of time (e.g sediment accumulation rates, erosion rates, bedrock incision rates, but also precipitation rates etc..). Hence, it would be necessary in such a global comparison (that does explicitly NOT compare 1:1 sites of where both cosmo and sediment yield have been measured *at the same location*) to test whether the sediment yield data and/or

precipitation rates are biased by the Sadler effect (e.g. by plotting the rate versus measurement interval). I assume that not all sediment yield data nor precipitation date have the same measurement interval ?! As one reviewer puts it: “Whether or not the SS load data reported here is subject to a time-dependent bias is for the authors to demonstrate.”

3) I guess the most important issue regards vegetation cover. The authors motivate their findings (cosmo trend with MAP) with the early findings of e.g. Langbein & Schumm, where peaking erosion rates fall together with a transition from dry to wet precipitation and sparse to extensive vegetation. Although this interpretation is generally fine with me, I am wondering why the effect of recent vegetation (not land use) was not inspected in more detail, by using recent global vegetation maps?. Using a LGM vegetation map the authors explain the higher cosmo rates in the cold zones by glacial and periglacial influence. In my view, this finding is nothing new and does not need to be discussed in detail. I would rather have expected a deeper analysis of vegetation versus precipitation effects in the other climate zones. A recent paper by Starke et al (2020) in Science showed that vegetation and precipitation co-vary and interact, with different feedback strengths. Therefore it would be highly interesting to include vegetation datasets into the analysis.

4) Overall, I find the organization of the discussion still wholesale confusing, and still not much to the point. They set out with their “key finding” (l. 389) regarding the mentioned trend in cosmo rates with MAP. Without relying (again) on the interpretation made by others, I would suggest to include the vegetation dataset and try to develop the discussion from that- mainly because I think that the discussion points that follow do not help (as organized in this order, they need to be re-arranged!) and only add confusion. At this point (i.e. after mentioning the k”ey finding”), I would have expected a discussion on why cosmo rates may follow this trend (Fig 3a). Perhaps the authors are saying that some of this trend is controlled by glacial/periglacial processes as suggested what they write in lines 433 ff? (motivated by the long averaging time scale of cosmo rates?) But how would that affect the trend shown in Fig 3a? Are they saying that the peak around 500-700 mm MAP is an artifact of glacial processes inherited because of the long integration time scale? I guess the fact that this discussion is interrupted by talking about the absence of this trend for the short-term rates (lines 402-416) does not help either. In general, it does not help that the authors jump around in the discussion between the trend shown in Fig. 3a, and the observations drawn from comparisons between long- and short-term rates in the other figures.
I guess a summary suggestion on this trend shown in Fig 3a is: The authors need to more thoroughly inspect possible mechanisms, including: effects of integration time, effects of changes in MAP, vegetation cover, and lastly, possible bias within cosmogenic nuclide analysis itself (yes, sorry). The latter must include some discussion on weathering underestimated from cosmo data because of thick weathering profiles in the tropics (e.g. Riebe et al., 2001, Dixon et al., 2009). In tropical basins, denudation rates might be underestimated as mineral dissolution at depth is not “seen” by cosmo. Naturally, this effect could help to explain why short-term rates are so much higher in the tropical climate zone than cosmo rates. So, simply acknowledging that there are biases in cosmo rates does not help, either- they need to be explicitly mentioned and discussed.

5) 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.

6) I think the last two paras of the discussion would benefit from being clearly separated from the rest of the discussion. But that would not solve a profound issue that some of these controls cannot be separated by other effects. A way of dealing with different components that might interact with each other would be to do a principle component or factor analysis, that might reveal which component is more influential. I question hence the separate treatment of these factors such as channel gradient, catchment area, in comparison to vegetation, climate and so on. Perhaps I should have mentioned this as first point in my list, so, mentioning this at last does not mean it is the least significant.