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Comment on esurf-2021-7

John Jansen (Referee)

Referee comment on "Global analysis of short- versus long-term drainage basin erosion rates" by Shiu-An Chen et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2021-7-RC3>, 2021

'Global analysis of short- vs long-term drainage basin erosion rates'

General comments

Chen et al. present a global meta-analysis of previously published data on denudation (erosion) rates from a wide selection of drainage basins. The data draw on two approaches to quantifying basin-scale erosion: suspended sediment (SS) yield and cosmogenic Be10 measured in fluvial sediments. Key findings are that short- and long-term erosion rates tend to differ significantly, and that this disparity is largely due to i) human activities, and ii) climate-related factors involving the role of plants, glacial history, topography, and scale effects associated with sediment storage in fluvial systems.

How timescale affects the quantification of erosion rates is a primary question in geomorphology that has received plenty of attention. Making use of large datasets to explore the problem is not novel, but to my knowledge the suspended sediment (SS) yield data has not been previously examined specifically in this way.

In my view the study would gain from some restructuring that clarifies the rationale behind each step of the analysis and leads to a more logical unfolding. At present the MS gives a jumbled impression, some passages are difficult to follow and several loose threads detract from the main arguments. The conclusions are mostly compatible with previous work, but my main concern is with a number of oversights that seriously weaken the standing of the work.

I have 5 main points that require some consideration (my other comments are keyed to line #).

(1) Treating Be10-derived erosion rates as long-term in comparison to the SS records is a valid approach, but it's also important to note that the Be10 integration timescale is a function of erosion rate. The integration timescale is conventionally calculated at one absorption depth scale \sim the time taken to remove \sim 0.6 m of rock under long-term steady erosion. This means Be10 integrates a continuum of timescales spanning 3 order of magnitude (10^3 to 10^6 y), and this may have implications for how the erosion rates are interpreted, as noted below in my comments on Fig. 3a.

(2) There is a striking omission here of the time-dependence of the distribution of hiatuses in the sedimentary record known as the Sadler Effect. This issue lies at the heart of comparisons of erosion and deposition rates over different timescales and cannot simply be passed over without comment. There is a stack of recent papers, but Schumer & Jerolmack (2009, JGR) would be a good start.

(3) In order to reflect short-term denudation, the SS data should be limited to specific sediment yields within the upland source zone only. In the transfer zone, downstream SS load is chiefly driven by sediment exchange between floodplains and channels: a function of sediment availability, not denudation (see Dunn et al. 1998, GSAB). If on the other hand, direct efflux from agricultural lands and plantation forestry is the driver then the authors need to make their case accordingly.

(4) I am not convinced the median is a legitimate choice of parameter for comparing datasets that overlap across several orders of magnitude. In such variable data, the median has no specific value other than being roughly in the middle. I would like to see a justification for the use of the median as the basis for comparing relative erosion rates.

(5) The topographic indices do not include mean hillslope gradient despite it being the strongest of all parameters tested by Portenga and Bierman (2011). The local river slope used here has practically no bearing on Be10-derived basin-scale erosion rate and there is no reason to expect it would either. Channel relief is not an effective substitute. The physical basis of the alternative approach presented here needs to be justified.

Specific comments [keyed to line #]

Intro

35- Perhaps add that SS flux records also reflect the availability of fine sediment, and to some extent the production of fines via weathering and transport; e.g. some lithologies like NZ greywackes break down remarkably fast.

36- '...basin-averaged exposure ages', is not the correct phrase. As used here, Be10 abundances are modelled to yield surface erosion rates, not exposure ages, which generally impose a zero-erosion constraint—it's a different equation. Please correct this error throughout the MS.

52-3. This assumption of uniform bulk density seems reasonable for the purposes here but should probably be propagated through the uncertainty analysis. I didn't find any evidence of that.

58-59 Good point. I agree the SS records are likely to yield transient rates in light of widespread riparian zone destruction and the global expansion of river bank revetment over recent decades.

63- Usually described as 'secondary cosmic rays'—mainly neutrons and muons by the time they get to ground level.

63- The integration timescale of cosmogenic nuclides varies with erosion rate.

68-73. I appreciate seeing an explicit statement of the method assumptions; however, I can suggest a few amendments. Sediment storage is not strictly a problem (reworking can be). Most important is that the sampled grains have been subject to long term steady erosion and continuous exposure to cosmic rays. Those two assumptions are violated by abrupt and deep erosion (e.g., landsliding), or long-term burial followed by erosion. Landscape-scale equilibrium is not really necessary (see Willenbring et al. 2013, Geol); if

it were, practically all of the OCTOPUS dataset would be invalid. The key is that the erosional processes acting are more or less steady and there is a minimum of deeply shielded grains (e.g., from landsliding) in the sample collected. Please clarify the meaning of 'no erosion-deposition cycle'.

74-76. See note above on the integration timescale of Be10. This statement is valid only for the longer-lived nuclides such as Cl36, Al26 and Be10 (not C14). It is not clear what is meant by 'stochastic events', but perturbations such as landsliding and extreme floods that erode old sediment storages can potentially affect Be10 abundances, and certainly will affect abundances of in situ C14.

107- '...stripping of rock underneath basal ice' is not how it is usually described. Perhaps rephrase to something like 'Glaciers erode bedrock via quarrying and abrasion wherever subglacial conditions allow basal sliding.' The legacy of deep and steep walled glacial troughs prone to mass failure is another key reason why glaciated landscapes yield high sediment load.

129- True, but deconvolving the effects of tectonics and climate is not really one of the aims of this MS. Perhaps confine the scope of this literature survey to the issues that are specifically addressed in the MS.

134- ... glacial and periglacial processes.

Methods

147- Was the Ray and Adam (2001) study used here because it classifies vegetation distributions at the LGM? Are there more up to date alternatives?

164- '...published literature' seems a bit general—perhaps refer to the Supp data here.

166-67. One of the strengths of the OCTOPUS dataset is that the nuclide data are recalculated from scratch with uniform methods and propagation of uncertainties. Has the same been done with the SS data including uncertainty analysis? e.g., the bulk density assumption? It would be good to see an effort in favour of reproducibility.

182- It is a bit unclear whether the original K-G zones or the new modified versions were used. Fig. 4 is not clear to me. What is the purple representing? I cannot determine clearly where the previously 'glacial and proglacial zones' are exactly. Is the map indicating that the Tibetan Plateau was ice covered during the LGM? That idea has long been discredited (e.g. Heyman 2014, QSR), giving the impression that these glacial extents are a bit outdated.

188-194. Some repetition could be cut here.

191- In addition to the regions directly covered by Plio-Pleistocene glaciers, it is worth considering the widespread distribution of frost weathering associated with periglacial activity. At high latitudes, this is a far more extensive and more persistent control on sediment production and transport than ice sheets per se. The direct effects of glaciation extend far beyond those regions directly covered by Plio-Pleistocene glaciers. The great northern ice sheets fed prodigious amounts of sediment into surrounding glaciofluvial landscapes, which were in turn surrounded by a vast periglacial domain. Even today, about 18% of the ice-free terrestrial surface has a mean annual temperature <0°C and of course this expanded greatly during the LGM.

196- Perhaps 'glacial-interglacial cycles' rather than 'ice ages' which is a bit general; 'last ice age' presumably means the coldest part of the last glacial cycle, the global LGM

(~27–19 ka). This is important in the context of the Be10 integration timescale noted elsewhere.

207-11. Owing to its scale dependence, estimates of 'mean slope gradient' i.e., hillslope gradient, have created some difficulties in previous meta-analyses (e.g., Willenbring et al. 2014, ESurf). I suggest you be specific about how this is calculated; expand on Chen et al. (2019).

A bit puzzling. The topographic data presented here include river profile concavity but not river slope, or some normalised version of steepness such as k_{sn} . That seems a bit odd given that river slope is one of the main drivers of fluvial incision (via bed shear stress). Is this saying that it was local river slope that was measured within 150 m of the Be10 samples, not mean catchment hillslope gradient? Local river slope has practically no bearing on a Be10-derived erosion rate. In any case, I suggest the term 'reach-scale channel slope' be used instead of 'mean slope gradient'.

226- Please expand on the description of the Kruskal-Wallis test. I understand the K-W is an old school ANOVA based method, but it needs to be justified here. Why is K-W the most appropriate tool to use among so many others?

Results

Fig. 2 is a nice and clear representation of the data. My first impression was that all these erosion rates overlap at the interquartile range and so demonstrate remarkable similarity despite spanning such different timescales! Fig. 2 shows that most sample sets span an order of magnitude in the interquartile range and 2–4 orders for the 5–95 percentile (I presume the whiskers are 5–95, though I could not find it stated anywhere). I have several concerns.

(i) How were those outliers defined? Is there a physical basis for excluding those data? In my view a solely statistical reasoning for defining the outliers is not justifiable because these are simply descriptive classes; they do not imply a model distribution that would dictate the shape of the tail, for instance. Excluding outliers implies that there is a problem with those data. But what would that problem be other than they are in the tail of the distribution? This is important because excluding data naturally affects the shape of the distributions—possibly a lot (the Portenga & Bierman 2011 study does the same).

Given that the outliers are all at the upper end of the data range, I would guess that the distributions are close to log normal. If so, then it probably also means they could benefit from log-transformation before plotting. Out of curiosity, I plotted the SS data provided in the Supplement using violin plots rather than the standard box-whisker. The violins have obvious advantages and I suggest the authors try these out.

(ii) If one includes the outliers for a moment, most of the classes span >4 orders of magnitude in erosion rate, which suggests a severely undetermined problem. In my view these climate classes are simply not discriminating enough—there are too many other factors at play.

(iii) I find it difficult to understand why the median is a legitimate choice of parameter for comparing datasets that overlap across several orders of magnitude. In such distributions, which are potentially polymodal, the median has no specific value other than being roughly in the middle. Further, the median ignores the magnitude-frequency of events that drive sediment yield. For instance, a good proportion of the sediment yield in the tropics is the result of tropical cyclones and another large fraction is related to seasonal burning which has been practiced by indigenous people over $\sim 10^4$ timescales. My guess is that the median would fall in between those two. Is that a good model? In my view, the

authors need to work a bit harder to convince the reader here and especially argue why the K-W is the best and most appropriate tool to use.

Table 1. Tables are never a nice way to present an argument. To clarify, the 'erosion rates between climate zones' are merely the median erosion rates, not the full distributions. Is that correct?

Fig. 3. Interesting plot; I like the LOWESS approach. Where are the uncertainties on the erosion rates?

Fig. 3a. Combining present-day precip rates with Be10-derived erosion rates, which integrate a range of timescales, might have some implications worth considering. Two related issues that come to mind with regard to the bump in the data at MAP <1000 mm: the higher erosion rates (~1000 mm/kyr) are integrated over a timeframe of ~600 y while the slower rates (~20 mm/kyr) are integrated over a timeframe of ~30 kyr. It is well established that presently arid parts of the American West experienced much wetter conditions over the transition from full glacial to the present-day interglacial (REF), and this is the same timescale spanned by the Be10 samples. The changes in MAP are sufficient to move these data to the right (possibly forming a cluster alongside the 'humid trough shown now). I'm just speculating here ...

274-275. This is good idea to exploit the region between the former maximum ice margins and the non-glaciated temperate zone but this is not well executed, in my view. A couple of things require clarification given that the glaciated and non-glaciated zones seem to overlap to a great extent:

(i) What is the purple zone in Fig. 4?

(ii) Note that the LGM ice limits were visited only very briefly; ice cover for most of the Pleistocene was a small fraction of the LGM max. In other words, how useful is the LGM limit as an index of glacial erosion given that most of the glacial forefield is mantled with drift in places hundred of metres thick.

(iii) Not all glaciers are alike. Polar ice masses erode slowly (<10-100 mm/kyr) owing to their frozen-beds. It is true that the ice sheets complicate the quantification of large-scale erosion rates, but short-term rates linked specifically to glaciers have been quantified (see Hallet et al., 1996, GPC; Koppes & Montgomery 2009, Nat.Geo).

277- I don't think it is reasonable to characterise this comparison as 5-fold discrepancy without mentioning that it is merely the medians being compared (as stated in the Fig. 4 caption). Most of the data (5-95) overlap across 2 orders of magnitude.

279- As noted above, this effect is not just driven by glaciers but the full range of cold climate processes.

290- Puzzling why so little difference! It suggests the classes are not effective. Can this be improved somehow—there must be a stack of global datasets describing anthropogenic activities nowadays.

304-305. Finding that $R_{S/L} > 1$ is a predicted outcome of the time-dependence observed in the sedimentary record known as the Sadler Effect.

Fig. 7. Aside from the issue that these histograms obscure the enormous spread of these data, I'm interested to understand what causes the differential between the tropical and arid datasets. Could these trends be the magnitude-frequency factors emerging? For instance, the flood frequency curves in the tropics are characteristically flat whereas those

in arid regions are steep.

Fig. 8. Not well conceived in my view. I cannot grasp what the right panel is aiming to show. In (c) no river slopes exceed about 0.05, unlike in (a), because SS load derives mainly from reworking of fine sediment fills found predominantly in the transfer and sink zone at low channel slopes. At higher slopes there is little availability of SS load, so I expect that this plot is largely reflecting landscapes with argillaceous lithologies or glacial settings—not an effective way of exploring how slope affects erosion rates.

Discussion

336- Given that this study is based upon comparing short- and long-term erosion rates, this part of the Discussion ought to include some consideration of the effects of the time-dependence observed in the sedimentary record known as the Sadler Effect. I suggest a brief outlining of how it has been recognised by previous workers, followed by some analysis demonstrating that the observed findings of $R_{S/L} > 1$ are not a simple outcome of the Sadler Effect.

It is my understanding that Be10-derived basin-scale erosion rates are not subject to this bias for the reason that they incorporate the erosional-depositional dynamics across a wide range of ground surfaces in the basin, some eroding some not, and this effectively neutralises the time-dependence. Whether or not the SS load data reported here is subject to a time-dependent bias is for the authors to demonstrate.

346- This could be rephrased to be less general. As it reads now, this conveys very little information and reports findings that more or less echo those of previous work. A major part of the analysis is not global, it is restricted to the USA.

352- Perhaps a bit too generalised. The non-linearity in this relationship is most likely a function of the response of plants (soils reinforced by roots, rainfall interception, weathering etc.).

362-64. The question is, why does Mishra et al. (2019) differ from the curve produced here? Perhaps the authors could raise some explanations here.

376-77. This statement does not accurately reflect the results. Note something like a 10-fold increase in MAP from hyper-arid to arid to semi-arid is accompanied by <2-fold increase in median erosion rates. Why is that? Across this range, ground surfaces typically go from being totally bare to having complete seasonal vegetation cover.

388- This statement needs some rethinking. I do not follow why erosion 'rates might not be expected to change much' after glacial retreat. What is that based on? I expect rather the opposite as bare sediment and bedrock surfaces are colonised by vegetation over the postglacial period and large areas in North America and Scandinavia are uplifted isostatically. The paraglacial regime is associated with a well studied trajectory of sediment flux over the postglacial period.

389-90. This also ignores that high erosion rates are integrated over short periods...

397-98. Please clarify.

413-414. But according to Fig. 1, it seems that few data are available for boreal regions. The regions for which data are available are some of the most agriculturally exploited on Earth. Could the low SS loads be the result of generally low relief and the intensively modified lowland riverbank revetment?

429-36. This is a reasonable deduction, but not a satisfactory resolution. Why not recompute the data using a range of different thresholds to evaluate the problem thoroughly? As for the second point, I don't follow the logic. How does this explain the discrepancy? You find an 8 vs 3.5-fold increase in the smaller basins but then point to the conservation efforts in upland (smaller) basins?

438-41. Yes, slope forms part of stream power law, but slope was not part of your evaluation of the influence of drainage area. It's difficult to see the point of this statement.

442- Be10 derived erosion rates are rarely affected by floodplain storage—the sediment would need to be buried deeply for a long time ($>10^5$ y), then large volumes would need to be somehow reworked into the channel. As for '...violating the detachment limited assumption within area-erosion relationships', it's hard to follow what this is getting at—certainly not the effects of sediment storage on Be10 erosion rates. Further, the Whipple et al. (1999) reference cited here makes no mention of cosmo.

438-52. A paragraph of confused thinking. E.g. 442-446, this is not an argument that accounts for differential erosion rates in large vs small basins. 447, What evidence is there that active plate margins have steeper relief than passive margins? The steepness of hillslopes is set essentially by rock strength such that mass failure occurs beyond a certain threshold of internal friction within the slope (Schmidt & Montgomery 1995, Science). Elsewhere in the MS, 126, it is stated that tectonic uplift lowers rock strength via increased fracturing (also not true as a rule: the tallest hillslopes commonly occur in tectonically active terrain, e.g., Nanga Parbat).

449- It is clearly true that lithology strongly influences erosion rates, but this analysis did not assess lithology. Why is this being raised here?

461- It is not clear whether this statement refers to short-term or long-term erosion rates but in the case of Be10 derived rates the sediment buffering is not very effective. Several studies by Wittman have shown this.

466-73. One of the most striking aspects of Fig. 7 is that tropical basins have much higher $R_{S/L}$ combined with a greater sensitivity to drainage area. I would expect to find some Discussion of that point but I can find no explanation for why $R_{S/L}$ ratios are notably higher, nor why such regions are more sensitive to drainage area.

487- Good to see mention of landsliding: the main driver of high erosion rates in mountain belts.

489- As noted above, the representation of local channel gradient in Fig. 8 conflates the Be10 derived basin-scale erosion rates with reach-scale fluvial incision rate. Be10 abundances measured in fluvial sediment are not closely related to basin-scale erosion rates.

492-499. Clearly true; agriculture is highly concentrated in lowland settings. But its effect in terms of soil loss is likely to be most destructive in steep terrain. This study sets out to compare erosion rates. And yet, the last few sentences reveal the failure of SS load data to capture soil loss where it actually occurs on hillslopes—due to sediment trapping in reservoirs and perhaps post hoc soil conservation efforts. Recent advances in isotope-based approaches (e.g., cosmogenic C14) mean that soil depletion can be quantified without the source-to-sink assumptions inherent with conventional sediment yield estimates.

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Please also note the supplement to this comment:

<https://esurf.copernicus.org/preprints/esurf-2021-7/esurf-2021-7-RC3-supplement.zip>