Earth Surface Dynamics Discussions

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# Comment on esurf-2021-90

Anonymous Referee #1

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Referee comment on "Spatio-temporal variability and controlling factors for postglacial denudation rates in the Dora Baltea catchment (western Italian Alps)" by Elena Serra et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-90-RC1, 2021

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The paper "Spatio-temporal variability and controlling factors for postglacial erosion dynamics in the Dora Baltea catchment (western Italian Alps)" presents new 10Be-dervied catchment averaged denudation rates and investigates the parameters driving differences in 10Be nuclide concentrations. The authors find that lithologically controlled topographic differences control denudation rates, and that the sediment input from the Mont Blanc massive is the main source of sediment along the Dora Baltea.

I think the paper has potential and the data are good. However, the paper needs revision in terms of structure and language to improve readability. I also found that a more vigorous analysis is required to support the main findings. I therefore suggest major revisions.

Several sentences were long and hard to follow, had wrong punctuation, missing words, or wording that should be revised. I highlight only a few of the mistakes in the line-by-line comments. I think that the authors can improve the readability and language of the manuscript with a thoroughly revised version of the article.

The observation of almost constant 10Be concentration along the main stem is very interesting and I think it deserves much more attention within the paper. The authors show that the same signal can be seen in sediment gauging data, which indicates that this is not due to differences in quartz fertility. I would suggest to put more emphasis on this observation, expand the mixing model analysis, and provide more detailed discussion on this observation. Currently, the mixing model is in the discussion, but I think it should be in the results.

The authors often make links between erodibility of a rock (e.g. its mechanical strength), topography, and the denudation rate, where lower erodibilities are inferred to support

steeper topography AND higher denudation rates. It is important to note that this would not be the case in a steady-state landscape. In a steady-state fluvial landscape, differences in erodibility would only be expressed as differences in topography, and denudation would be constant throughout the entire landscape. I understand that the studied landscape was recently glaciated and is likely far from a steady-state topography, however it is important to note that steeper slopes do not necessary equal higher denudation rates.

The authors argue that the lower erodibility bedrock units allow higher geophysical relief to form, which in turn increases denudation rates. However, the slope distributions between the rock units only show minor differences. The authors point to the higher geophysical relief in external and internal units, but it remains unclear why that parameter should be a better predictor of gravitationally driven physical erosion processes than slope. In the current version of the manuscript, it comes across as if the authors choose to ignore the fact that all rock units exhibit similar slopes. I think a better way of presenting the data would be to either, to try and argue that the differences in slope, while minor, are still close enough to erosional thresholds that they actually matter, OR that the denudation rates mostly depend on elevation.

All regressions in this study seem to be done with an ordinary least squares regression (OLS). The results from an OLS depend on which variable is defined as dependent and which as independent. I suggest to revise all regressions and use a total least squares (TLS) approach. A TLS is independent of variable definition. This will probably change the $r^2$ and p-values of the regressions.

Figure 7 should be presented in section 4.3, otherwise there is a missing piece in the logical flow. The authors show that denudation rates vary between different rock types, but this variation could just be circumstantial because the distribution of topographic and climatic variables may be heterogeneous among the different rock units. As a reader, I need to know if the faster eroding external units are also steeper, to assess if the external units erode faster because they're steeper or because they have a higher erodibility.

Below are some line-by-line comments:

Line 29: I suggest to cite the first study applying this technique (Brown et al., 1995)

Line 44: I think there are much earlier papers than Godard 2014 to make the point that an increase in tectonic uplift increases denudation.

Line 44-45. Please, be more precise in your formulations. In a steady-state fluvial landscape, erodibility would only govern the steepness of the topography but not denudation. I kind of get what you mean, but here and in other places the formulations

should be more precise.

Line 45-47: This statement needs to be revised (see above). Commonly, we assume that denudation rates tend to balance rock uplift rates. If this is true, erodibility only controls topographic steepness and not denudation rates. I understand that this region was heavily glaciated and does not represent a steady-state fluvial topography. However, the way the statements in this paragraph are set up, this is unclear. The paragraph discusses controls on denudation rates, but does not indicate what spatial and temporal scales are being discussed in the second part of the paragraph. I can guess that the authors refer to millennial scale denudation rates on a catchment/landscape scale, but it's better to be precise to avoid misunderstandings.

Line 76: "precipitation is"

Line 98: I assume glaciers covered the entire catchment except for some peaks that were sticking out. If so, this point should be made more clearly, or the LGM ice extent could be on figure 1 (unless it would cover the entire fig. 1 area).

Line 107-109: an "a" is missing close to the beginning of your sentence.

Line 132: It has been argued that topographic shielding corrections should not be performed in most settings (Di Biase, 2018). Personally, I do not use it anymore and would suggest the same for this study, unless there is a particular reason to stick to the correction. Also, the abbreviation LIA is only defined later in the same paragraph.

Line 141-144: This sentence needs some English revision.

Line 144-146: I would appreciate a short sentence explaining how this shielding correction looks like, what percentage area it affects, its magnitude, and how it is calibrated.

Line 165: "Average"

Line 169: "subjected to more erosion" or simply "erode faster". The paragraph contains several long sentences that would benefit from some language revision.

Line 171: The influence "on" instead of "to". The word "rates" is missing after "10Be

derived denudation".

Line 176: for THE studied catchments.

Line 238: For a bivariate regression the r-value is written in lower case.

Line 241-2: You could instead simply calculate Cook's distance to evaluate if DB01 significantly affects your regression results.

Line 330-335: Similar to my comment above, I would not call this trend "counterintuitive". Differences in erodibility can be expressed through surface slope, and therefore do not require any impact on denudation rates.

Line 339: As a reader, it's unclear to me what exactly your hypothesis is (based on the sentence before). There are several places were the text is written imprecisely, in the sense that I can guess what the authors mean but it's not formulated explicitly. I suggest to revise the text carefully to avoid such ambiguities.

Line 354-364: This paragraph should be revised. The authors suggest that there should be a glacial imprint on denudation rates but have trouble arguing for it based on the regression and eventually leave the reader hanging. Please, be more explicit in your interpretation and feel free to speculate as to why the correlation may not be as good as expected.

Line 365: The authors sometimes start paragraphs with a sentence that ends without being finished. Here the paragraph starts by stating "we propose a hypothesis" but the sentence ends without the proposal. If you want to state the hypothesis in the subsequent sentences you need to add something like "in the following".

Line 393-396: It is a valid hypothesis. Please, add a calculation to test this hypothesis. It seems like you know all the necessary parameters to set up a mixing model.

Line 400-412: To me this is the most interesting part of the paper. The mixing model has to go into the results. The high contribution from the Mont Blanc massive is obvious in figure 3, and the reader needs to go through many pages of text to finally see this point addressed. Moreover, if there is a way to get quartz bedrock content estimates for the catchment, the quartz fertility should be included in the mixing model (especially since the higher quartz content of the Mont Blanc massive was already mentioned before).

Line 456: A strong time-scale bias on erosion rates has been shown for glacial environments (Ganti et al., 2016), where decadal scale erosion rates have been shown to be an order of magnitude higher than millennial scale rates due to the stochasticity of erosional processes.

Tab 1: I do not understand why the caption of table 1 lists all of the details that are already described in the main text. Please, reduce the text within this caption significantly.

Tab 2: Same as for Tab. 1. Do not repeat all the methods in the caption.

Tab S2: I appreciate the detailed reporting of production and denudation rates.

Figure 1: Increase line width of rivers, and size of sampling dots for better visibility. Do not use the colors green and red for the sampling dots, since this is the most common color blindness.

Figure 2: Please, increase line width of rivers and symbol size. I suggest to change the color palette, because the current colors are not color-blind friendly.

Figure 3: Maybe enlarge symbol size a little bit.