



EGUsphere, referee comment RC1
<https://doi.org/10.5194/egusphere-2022-709-RC1>, 2022
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Comment on egusphere-2022-709

Sebastien Carretier (Referee)

Referee comment on "River incision, ^{10}Be production and transport in a source-to-sink sediment system (Var catchment, SW Alps)" by Carole Petit et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-709-RC1>, 2022

The study by Petit and co-author is, to my knowledge, the first study that couples the dynamics of relief with the evolution of the ^{10}Be cosmogenic nuclide concentration in continental and submarine river sediments. This approach seems fundamental to link the detrital signal of cosmogenic isotopes and the upstream topographic evolution, in particular to confirm or improve the interpretation of this signal in marine sediments at river mouths. Its application in a real case, based on existing ^{10}Be concentration data, is particularly relevant. This study concludes that this signal at sea can be strongly modified and may not reflect variations in the rate of erosion of the continent.

However, despite the interest of this study, several weaknesses in the presentation of the model, the results and their discussion make it difficult to judge the robustness of the results, which I detail in the following.

Description of the model and parameter values

The interest of the model is that it couples surface, terrestrial and submarine processes, isostasy by flexure, and the evolution of the cosmogenic isotope concentration. This implies a choice of processes, and a consequent number of parameters. These choices are not sufficiently explained (at least a table with all parameter values is needed). I list here a few points to illustrate my point but this applies to all the ingredients of the model:

- The river erosion law is classical but the values of the parameters K , m and n are not all indicated nor justified, whereas it is known that the response of the rivers strongly depends on these values (especially n).

- The slope erosion law is a simple diffusion law whereas it is stated that it describes linear or non-linear diffusion. The non-linear component, which would indeed be better suited to describe mass wasting on steep slopes, does not appear in equation (3). The values of K_d are not justified in the paper, either for bedrock or sediment (is there a distinction?).

- Sedimentation occurs below imposed thresholds, on land and at sea, but these thresholds are neither justified nor quantified in the manuscript. However, the dynamics of rivers depend in reality on the cover and tool effects of sediment. How are these effects treated here? It seems that sediment can be re-eroded: in this case, how do they erode? With the same K , K_d , m , n parameters as for the bedrock? This point is particularly crucial because the results seem to indicate that the modelled mismatch between continental erosion and the offshore detrital signal comes from this recycling. It is therefore necessary to explain and justify the choice of simplifying sedimentation to a simple threshold, and to justify the value of this threshold (and to give it explicitly in the manuscript). A better description of the sediment layer behaviour (deposition, sediment recycling), onshore and offshore sediment dynamics would help interpret the model results. For example the authors interpret the bad record of continental erosion by offshore detrital ^{10}Be concentration as the result of sediment recycling in the land rivers. This interpretation may be tested by running a simulation without sedimentation on land and verifying that the offshore detrital signal is different.

- The model seems to calculate the sediment flux Q_s using an empirical law (equation 1). Why do we need an empirical law to calculate Q_s when it should be derived from the difference between erosion and sedimentation upstream of each pixel? Is this empirical law used to distinguish between fine (which is what it was designed for) and coarse sediments on which ^{10}Be is measured? Is it verified in the model that the flux calculated with equation (1) is lower or equal to the one corresponding to the sum of the net erosion and sedimentation upstream?

- The parameters of the flexural model should also be given.

- The 'best-fit' model must be better characterized (method and parameter values), as well as the method used to determine the parameters of this model (Monte Carlo approach with minimisation of a misfit function, for example). Given the number of parameters to be adjusted, I assume that choices have been made to keep some constant while varying others. Details of this procedure are needed.

- An important result is the fact that onshore sediments give a good representation of erosion, while offshore sediments may have a cosmogenic signal de-correlated from erosion. What is happening on the offshore slope that changes the signal? Is it explained by sediment recycling from the offshore fan? Is this result the same if we sample the offshore fan apex?

The calculation of cosmogenic isotopes

The calculation of the cosmogenic isotope concentration is based on the differential equation (6) which is a simplification of the general equation involving the concentration gradient with depth. This simplification allows the evolution of the concentration to be written as a function of the surface concentration (Eq. 6 of Knudsen et al. 2019). However, I confess that I did not understand how equation (7) was established... It seems to come from Knudsen et al. (2019) but I did not manage to find this equation in their article. If this equation is correct, I think it is important to demonstrate it and explain why it has an advantage over a simple numerical solution of equation (6) (where $dN/dt=(...)$ could simply be approximated by $[N(t+dt)-N(t)]/dt=(...)$ so that $N(t+dt) = N(t) dt + (...)dt$ could be computed on each pixel at each time scale). I think it would also be useful to explain how one can avoid from calculating a depth concentration profile on each pixel at each time step since at a time $t+dt$, the surface concentration takes into account the

concentration that was previously at a depth ($\epsilon \times dt$). Different previous approaches based on grain tracking may be worth to cite (Repka et al., 1997; Codilean et al., 2010). Similarly, a comparison with the approach of Yanites et al. (2009) may be useful.

It is not clear to me how to calculate the ^{10}Be concentration in the sediment on each pixel. It is stated that the ^{10}Be concentration of the deposited sediments is a kind of average weighted by the eroded volumes upstream, but how do we know the origin and composition of the locally deposited sediment? Is the ^{10}Be concentration of the sediments transported from one cell to another tracked from upstream to downstream? This seems necessary to take local upstream sedimentation into account for example. This is why some other authors have developed modellings in which landscape evolution is coupled to grains tracking.

Absence of a "Discussion" section

The choice of simplification of the processes involved and the parameters used requires a discussion on the robustness of the results with respect to other choices. For example, how does the choice of sedimentation threshold value change the result? Are the results dependent on m and n ? Does the absence of attrition or different grain sizes have an implication on the results? Variation in river width is not taken into account: is this a problem?

Specific comments

Line 79 How were a and b estimated? With what data? On what grain size?

L 103-105 This is not inconsistent. The incisions are local compared to the average erosion estimated by ^{10}Be in detrital river sediment.

L117 Define and explain how this threshold is chosen (from what?) and the choice of its value

L 130 Is this very simple model able to reproduce the main characteristics of the alluvial dynamics (avulsion, recycling, etc) which seem to be determinant for understanding how the ^{10}Be signal is modified on land and in the sea?

L147 Could you justify that glacial erosion is proportional to the slope?

L178 How is this calibration done in practice? Is it done by a Monte-Carlo type approach? Is there only one solution (one set of parameters)? Please, give a table with the parameter values.

L180 What do you mean by "evaluated"?

L183 The sample Ves161-1 is also very different from the model result. Do you have an explanation for these deviations from the model?

L190 Why is it necessary to use an empirical relationship to calculate the sediment load when it can be derived in the model from the net erosion/sedimentation calculated upstream?

L191 What is the value of this threshold?

Equation (9) Q is already used for water flow.

L199 Equation 7 is not a solution of the general equation with stationary erosion rate. Do you mean equation 8?

L209-210 Since the method is precisely based on the ratio of quartz fluxes to ^{10}Be fluxes, there should be no overestimation. Unless I have misunderstood: do you mean that the concentration of sediments from glacial erosion are under-concentrated because the ice protected them from cosmic radiation?

L 216 Could you clarify what the "in-situ" and "actual denudation rate" are?

L220 What are the analytical solutions?

L235 Is it possible to test this hypothesis by preventing the sediment from settling on land (with a different threshold for deposition)? I think this would be convincing.

L239 Source of what?

L241 But the effect of glacier cover is well known. In general it is taken into account when calculating erosion rates.

L256-259 This requires a Figure

Figure 7: where is the second model? I see only one blue curve...

L278 This is an interesting point

Figure 9 Where is the best-fit model? Is it the reference one? The text is too small in these panels. I suggest to split it into different figures or increase the text size and reorganize the panels.

Good luck with the modifications.

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