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# Comment on egusphere-2022-709

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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., EGUsphere, https://doi.org/10.5194/egusphere-2022-709-RC2, 2022

**Review of "River incision, 10Be production and transport in a source-to-sink sediment system (Var catchment, SW Alps)"**

This paper presented an exciting landscape evolution model that incorporated in-situ cosmogenic production with erosion processes. The model was relatively well presented and the model was demonstrated to be able to explore the relationship between imposed erosion rate and observed nuclide concentration in sediment. The authors used this model and explored multiple factors, including glaciation, precipitation, and sea-level fluctuation to the Var catchment in Southwestern Alps where an increase in erosion rate since ~40 ka was interpreted from cosmogenic nuclide concentrations. Overall, I think this paper contributed a model that can be used to explore the causal mechanisms such as climatic, or tectonics for the interpretation of cosmogenic nuclide concentrations in submarine deposits. However, I find some parts of the model and model results are still confusing and I wish the authors can explain them in the revisions.

**1 Major questions:**

(1) The calibration model that is presented in section 3.3. I wonder how representative the smoothed topography from DEM is. For example, how much the high peaks in the massif area are smoothed. This is particularly important because these are likely the major contributors to Be10. If it is a matter of computation efficiency to decrease the grid resolution, did the authors try with some of the small catchments, for example, the Tinee river or the Vesubie river, to evaluate the smoothed topography and see if the same

model parameters calibrated from the whole Var catchment can produce the same results in these small catchments?

A second question is what is the uplift rate imposed in the model? Is it zero uplift? I didn't find any clue about it. The authors should clarify it and give reasons. Flexural uplift is explored later in other models (section 3.4 and 4), given a 20 km of EET is used, the wavelength of flexural uplift is longer than the Var catchment size, that is saying, perhaps the spatial variations of flexural uplift is minimal. So if zero-uplift is used, why bother to test the flexural uplift later? Please give reasons, for example, is some area uplifted faster than the other regions due to the flexural uplift, and therefore a change of topographic slope (this is important for sediment transport) is expected? The flexural uplift is never tested alone when keeping the other factors constant in models of Figure 6. The authors could either provide arguments about the necessity of imposing uplift in the model, or make an additional model test of flexural uplift only to see what the model tells us.

(2) I wish the authors can clarify how the inferred denudation rate from the marine sediment and the in-situ sediment is calculated in section 3.4 and 4. Especially, whether or not the inferred erosion rate is corrected from ice-cover and quartz-fertility or not. In practice, the calculation of a basin-averaged erosion rate from a cosmogenic sample should make these corrections when they vary dramatically in the catchment. And this shouldn't cause any discrepancy from model data, at least not the discrepancy between the erosion rate and the inferred-rate from in-situ sediment (the blue dots and red line in Figure 6A). Please correct me if I miss some processes. Figure 6A, I see an acceptable match between the marine sediment, the "real erosion rate" and in-situ sediment from 85-100 ka, so what does it mean considering the absence of any variations of precipitation, ice cover, etc.?

(3) The so-called lag time that is observed in the models in Figure 6. In line 220-223, the authors argued there is an apparent lag time for an abrupt increase in precipitation. Actually, from Figure 6A, the lag time is more or less the same as well for an abrupt drop in precipitation rate. It is not "less visible" as the authors argued in the text. I wonder whether it is a numerical error because the production rate exponentially decays in a depth profile, but the erosion rate more or less linearly changes with the precipitation rate (to the power of m in Equation 2). (A little distracted from the main point, but the authors should put the term of precipitation in Equation 2 for the purpose of this paper.) If the timestep is significantly small, would the authors still expect the same lag time in the model scenarios of Figure 6? This might propagate into the lag time (10-15 ka) of the marine sediment when the ice cover significantly affected the Be10 production area (Figure 6C, D). I wonder what the authors think about this problem.

(4) Residence time of sediment in the system. Following question (3), if we assume the time lag due to numerical errors is quantifiable or ignorable, why the offshore marine sediment showed a constant value after 40 ka regardless of the fluctuated erosion rate in Figure 6A? I think the sediment residence time is underexplored in section 4. Since sediment is trackable (?) in Badland, why not track the sediment thickness, sediment patches, and the number of nuclides carried in sediment throughout the modeled domain? A temporal inventory of sediment and nuclide (eroded material, sediment deposited in the Var basin, and in the offshore channel and depocenter, and the nuclide carried within the

sediment) will clearly reflect what is missing in the offshore sediment. The authors might have tried in this direction because the model they presented in Figure 7 was also trying to understand the lag time by imposing a slow erosion rate (and therefore high Be10 concentration) on a certain part of the basin. But the models in Figure 7 were presented very briefly in the text which from my perspective, should rather be expanded in the text. Models presented in Figure 7, what is the substantial difference between this model and a model of applying the ice cover only (no sea level fluctuation, no flexure, no precipitation temporal variation)? Figure 6 also missed a model showing the net effect of ice cover only. If the sediment storage is not explored, it is really hard to understand the text line 277-279 "The observed increase in 10BeMS around 40 ka is due to the presence of a patch of 10Be-rich sediments deposited in the basin in the early stages of the model, which slowly migrates towards the lowest areas of the basin during the run" because it was not presented in Figure 8, or Figure 9 and this is a very necessary proof for the authors to make a conclusion here.

**2 Some suggestions on figures:**

(1) Figure 2. Since the authors already named the 9 sites with numbers in Figure 1, I would suggest the authors put the site numbers together with the site names in the key (at the top right of the figure). So Line 91- 94 can be phrased to refer to either the site numbers, or the site names, and causes no confusion.

(2) I suggest the authors put the outline of the Var catchment and the submarine drilling cores (B2017) in Figure 3a, Figure 5, and Figure 8 for a better spatial reference, similar to Figure 1.

(3) The colorbar of Be concentration in Figure 5, and Figure 8 (the three sub-figures in the second row) doesn't show the spatial variation well. I suggest the authors adjust the colorbar so that the variations within the sediment patches are shown with more distinguishable colors. I see some spatial variations of concentration in the marine sediment in Figure 5. Is it showing one timestep or the integral of all steps? The authors could clarify it in the captions and explain why there is a spatial variation if it is showing one timestep.

**3 Suggestions for presenting models parameters:**

(1) A table of parameters and values for each model run will be necessary for understanding the model results. For example, in section 3.3 (Line 179), what are the "initial parameters" that were calibrated, and what are the values used for each parameter? Important parameters of models presented in Figure 4, Figure 5, Figure 6, and Figure 8 should all be listed in a table, either in the main text or goes to the supplements. It might be helpful if the authors can refer to each model run, for example, model 1, model 2, model 3,… consistently throughout the main text and the tables.

(2) Precipitation is missing from Equation (2).

**4 Suggestions on the paper structure:**

A discussion section is definitely needed to discuss the results. The discussions can focus on some of the topics that I put in the **Major questions**.

**5 A few line-by-line comments:**

Line 7: "nearly" ï□ "nearby"

Line 79: The estimated value of sediment flux is missing.

Line 87: $^{10}Be_{MS}$, what does it mean?

Line 91: "red-brown dots on 2", should be "red-brown dots on Figure 2"

Line 93: "3, 4, 5, 6,8, orange, yellow and green dots on 2", should be "site 3,4,5,6 and 8 on Figure 2".

Line 94: "… blue dots on 2", the same problem as Line 93.

Figure 2: Since the authors already named the 9 sites with numbers in Figure 1, I would suggest the authors put the site numbers together with the site names in the key (at the top right of the figure). So Line 91- 94 can be phrased to refer to either the site numbers, or the site names, and causes no confusion.

Line 161: give refs here.

Line 163-164: symbols are missing.

Line 179: which parameters? Need to present clearly.

Equation (8): how the initial long-term erosion rate is determined? Is the long-term erosion rate everywhere the same for all catchments?

Line 200: $^{10}Be_{MS}$, is it Be10 concentration of marine sediment? I see this symbol is used throughout the paper till the end, please give the exact meaning of this symbol in the first place where it appears.

Line 202: (as shown in 5) ï□ (as shown in Fig.5)

Line 203: "use the same equation…denudation rate variations ". Figure 5 presented the denudation rate, so please delete the "variations".

Line 226: delete "specific"

Line 324: (9) ï□ (Fig. 9). This is not the last place of the same problem as found in Line 202. Almost every figure was referred to in this way. Please correct the same problem throughout the paper.