

This is my second review on the manuscript entitled «Topographic controls on divide migration, stream capture, and diversification in riverine life» by Nathan Lyons et al, submitted to Earth Surface Dynamics.

As a general comment, I think the manuscript has been greatly improved and most of my questions and comments have been addressed. However, I still have two main comments on the revised manuscript :

- My main concern is about the topography of the simulated landscapes. In the first review, I mentioned that 11 000 m of elevation is quite high for a terrestrial landscape. The authors argued that this is related to the simplicity of the equations used in LandLab and that the discrepancy is thus small. I'm not a user of Landlab myself but I've been working with several numerical models based on the same kind of equations. Such models are able to produce realistic landscapes in terms of topography but some combinations of parameters can lead to unrealistic terrestrial landscapes.

One conclusion of the article is that the relative magnitude of perturbation to relief limits the landscape susceptibility to reorganisation. The total relief thus seems to be of importance for the conclusions of this work.

Accordingly, I would appreciate that the authors explicitly state that they do not work on terrestrial landscapes, or that they limit their analysis to landscapes that are similar to Earth. Given the exceptionally large number of simulations performed for each scenario (which is really impressive), this should not affect the conclusions of this contribution.

- In line with my first review, I still think that the main contribution concerns the co-evolution of species together with drainage reorganisation and that these key and new results could be better emphasized, in particular in the title and abstract. In the current manuscript, I still miss a proper paragraph in the introduction dedicated to the mechanisms that drive drainage reorganisation or to the questions that are still open regarding this topic. Such a paragraph would introduce the novelty of the results presented in section 4.2 for example.

Below are some additional line-by-line comments:

p 2 | 30 Please correct this sentence as the work of Bonnet (2009) is not computational but experimental.

p 6 | 7 Because there is life in the model, could a different time step (shorter, longer) affect the results ?

l 7 | 7 this rate is not exceptionally high, please remove exceptionnaly

p 7 | 16-17 the sentence of m and n should appear first as it justifies the units of K.

p 7 | 20 and following : this paragraph is a bit complex to follow, please consider rephrasing.

p 8 | 14 please add the Berlin and Anderson worked on the Roan Plateau as the values given here might not be universal.

p 8 | 19 and following : this paragraph is a bit complex to follow, please consider rephrasing.

p 10 | 26 : see my general comment about the maximum elevation. Do you mean that all simulations reach 11 000 m in elevation ? Given that the modelled landscape is at maximum 20 km long, this is really high.

p 11 | 21 - and similar sentences - Please clarify 30% of what

Section 5.1 references and discussions with respect to previous works on this question would be welcome in this section to emphasize the novelty of this contribution (see also my second main comment).

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