

Interactive comment on “Mapping landscape connectivity under tectonic and climatic forcing” by Tristan Salles et al.

Anonymous Referee #2

Received and published: 29 July 2019

In this manuscript, the authors combined geomorphological, meteorological, and ecological models to explore how biodiversity patterns evolve over a geological timescale. The work appeared to me to be an intensive, computational undertaking for which the authors should be applauded. The authors adopted the so-called landscape elevational connectivity (LEC) as a simplified proxy of biodiversity and analyzed how the LEC's spatial distribution changes over a geological timescale. They then extracted a number of insights from these LEC patterns. The manuscript is generally well-written and reads well. The topic should appeal to the ESD audience. Below I discuss a few reservations and make some suggestions which I hope will help improve the manuscript.

The extent of validity of LEC as a measure of biodiversity in mountainous regions. At many places throughout the manuscript, the authors state that LEC can explain “to

C1

the first order” the biodiversity found in mountainous regions. This claim seems to be based on Fig. 6. But the claim will only be valid if the y-axis of Fig. 6 is EMPIRICAL biodiversity; as it is, it is from model results. While this type of biodiversity metacommunity model has been shown to produce realistic biodiversity patterns for a range of ecological systems, I am not sure if it has been done for mountainous regions with such a wide range of environmental conditions and niches. A citation or two that show this is the case will strengthen the above claim. Otherwise, a caveat/caution should be placed earlier in the manuscript.

The qualifier “neutral” is not necessary nor accurate. In your model, individuals of different species have different optimal elevations (z_{opt}) and therefore are not equivalent. Consequently, some readers may be confused by calling this model “neutral.” Indeed, the reference to the neutral model is not needed nor helpful here, in my opinion.

Is there a more intuitive way to explain/understand χ (Eq. 4)? I found it a bit difficult to interpret and appreciate results associated with this quantity.

Other minor comments:

Fig 1's caption: “Two scenarios are ran” -> “Two scenarios are run”

P7, L3: “In this first example” -> “In this first set of results”? For me, this is not an example, but an experimental setting or something along that line.

P8, L2: “inhomogeneous” -> “heterogenous”

Fig 5's caption, 4th line: “are defines” -> “are defined”

P12, L13-14: I suggest changing “In addition and for simplicity as we assume a neutral approach, the parameter. . .1” to “The parameter f_{max} does not affect the system dynamics and is, without loss of generality, set to 1.”

P14, L25: “Orographic precipitation fosters faster isolation than the uniform precipitation”—I must admit that I had a hard time finding the figure that supports this

C2

statement.

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2019-32>, 2019.