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

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We would like to thank the anonymous reviewer for his comments. Below we provide a point by point response. We have also attached the updated manuscript based on the two reviews.

Main comments:

MC1: 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 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 metacom-

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Discussion paper

munity 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. R: The reviewer is right when mentioning that the y-axis of Fig. 6 is based on model results and it is fair to acknowledge that a recent study from Liu et al. (2018) investigated the relationship between species richness and elevation on ant community within the Hengduan Mountains region and did not find a similar relationship as the one from Bertuzzo et al. (2016). The authors agree that environmental gradients dominate variation in both alpha and beta diversity but in their case ant alpha diversity strongly declines with elevation. However, several empirical observations often show a hump-shaped rather that monotonically decreasing pattern such as in Lomolino (2001), Rahbek (1995, 2005), McCain & Grytnes (2010) or Kessler et al. (2011). As suggested by the reviewer we have added references to these studies. We believe it highlights the power of a pluralistic approach integrating field surveys with conceptual, statistical, and theoretical frameworks to understand the drivers of species distribution patterns. Future research bridging the gap between theory and the real-world systems will enhance our understanding of the mechanisms that govern biodiversity patterns.

MC2: 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. R: We agree with the reviewer and have removed the qualifier neutral in the text when referring to the metacommunity model. We have also removed the reference to Hubbell 2001.

MC3: Is there a more intuitive way to explain/understand chi (Eq. 4)? I found it a bit difficult to interpret and appreciate results associated with this quantity. R: Chi analysis is a method of extracting information from channel profiles that attempts to compare channels with different discharges (Perron & Royden, 2013). The longitudinal coordinate chi has dimensions of length and is linearly related to the elevation z(x). Therefore, if a channel incises based on the stream power incision model like in our landscape evolution model, then its profile should be linear on a plot of elevation against chi. As well as providing a method to test whether channel profiles obey common incision models, chi-plots could also be used in the field to provide means of testing the appropriate m/n for a channel (Mudd et al., 2014). In the paper, we have added first a reference to Mudd et al. (2014) as well as additional explanation in regard to the use of chi parameter.

Minor comments:

C1: Fig 1's caption: "Two scenarios are ran" -> "Two scenarios are run" R: corrected.

C2: 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. R: corrected.

C3: P8, L2: "inhomogeneous" -> "heterogenous" R: corrected.

C4: Fig 5's caption, 4th line: "are defines" -> "are defined" R: corrected.

C5: P12, L13-14: I suggest changing "In addition and for simplicity as we assume a neutral approach, the parameter ...1" to "The parameter fmax does not affect the system dynamics and is, without loss of generality, set to 1." R: corrected.

C6: 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 R: We have added in the revised manuscript the associated figure panel as well as the lines within the plots that support this statement (red lines in Fig. 10a).

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2019-32/esurf-2019-32-AC1-

supplement.pdf

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