Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2019-32-RC1, 2019

Earth Surface Dynamics Discussions

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

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Received and published: 12 July 2019

Review of Salles et al. "Mapping landscape connectivity under tectonic and climatic forcing" Reviewer: Phaedra Upton

I was really interested to read this paper which aims to bring together landscape evolution models and speciation via landscape connectivity. Despite some concerns outlined below, this is a very worthy contribution. After revision, this paper will be a well cited link between biology and geomorphology. I can only select minor or major revisions but consider my suggestions to be moderate rather than major revisions.

Specific comments:

I don't think the boundary conditions chosen for the landscape model do justice to the

study. The model used is a block of rock uplifting at a fixed rate with potential outlets on all sides. The resulting landscape is predictably a mountain with four low angled ridges ending at the four corners of the model. It best reflects an island rising out of the sea. The paper that proposed geomorphic controls on elevational gradients of species richness (Bertuzzo et al. 2016) uses a region of the Swiss Alps. With slightly different boundary conditions, for example, outlets along only one boundary (e.g., Roy et al. 2015 JGR ES 120 homogeneous model), I think the model would have produced results that matched the alpine example more closely. The minimum I would like to see is a discussion of why these particular boundary conditions were chosen and how the authors think that the boundary conditions impact on the model.

I'm concerned about some of the geomorphic statements in the paper. This model is completely erosional, yet it is stated that the topography on the leeward side is constructional (page 9). Constructional topography is formed by block movement, for example, a range is uplifted along a thrust fault and a river may then be constrained to flow along the valley formed by that block uplift. An example is Central Otago, New Zealand where rivers flow parallel to the main ranges. The relief associated with the topography in this region is a result of the block movements along the faults. In this model, the whole block is being uplifted as one and any relief that arises is because of differential erosion. The asymmetric topography and reduced erosion on the leeward side in the orographic model is still an erosional topography.

I think the differences between the two models presented here are overstated. Page 10, line 2 "…has profound effects on the geomorphic expressions of the simulated landscape". The two landscapes are different in detail, but they are both dendritic, erosional landscapes.

These last two points raise an interesting question, which may be beyond the scope of this study, is there a significant difference in the LEC for erosional landscapes (e.g., this study, Bertuzzo et al 2016) and constructional landscapes (e.g., Central Otago – Craw et al. 2016, Nat Geosci, 9; or landscapes further north in New Zealand – Craw et al., 2019, Geomorphology, 336)? Inclusion in the discussion that this model only addresses one type of landscape (erosional) and how other landscapes might differ would add weight to the manuscript.

I'd like to see more like-for-like plots. Figures 2 and 4 are just different enough that it is hard to see the differences between the uniform rain and the orographic rain models. Similarly, an equivalent to figure 5 for the uniform rain model might be useful.

Figure 2: The caption to (c) states "..generally low LEC for valleys and mountain tops and high LEC on the most elevated flanks of the mountain ranges." I don't think this reflects what is being shown. I see low LEC for valleys and minor peaks lower on the ridges, some higher LEC on ridge flanks but the highest LEC along the highest elevations and along river valleys at 5.5Ma. This is quite different to what is shown in Bertuzzo et al (2016) where the highest elevations are very low LEC. I suspect a plot of LEC vs elevation for this model would not be a bell curve centred on mid-elevations but would be dominated by higher elevations – as is my reading of figure 3A. I think this is a function of the landscape model chosen, as outlined above in the first comment. This may also be being accentuated by the colour map chosen. I find figure 2C hard to reconcile with the text, figure 3 and Bertuzzo et al. (2016). Going from red to blue with a small band of white between them means a lot of the grid appears to be very high LEC when maybe it isn't. I would recommend a different colour map be used.

Technical corrections:

Page 1, Line 17: replace 'This competition converts. . .' with "These competing processes convert. . .

Line 20: timescales

Line 24: distances

Page 2, Line 3: change geological to tectonic

Line 8: reword 'highest frequencies for mid-elevations'

Line 8: over geological time? Implies a very long time. Reword to entire orogenic system

Lines 8-12: expand to make clearer

Line 19: as outlined above, I don't think the geomorphic processes are changing, there is a difference in rates due to the change in the distribution of the precipitation driving the model, but it is still based on a stream power/hillslope diffusion model, no change in process.

Line 31: allows us to

Page 4, 1st line of caption: check tense here, are run or were ran

Page 5, Line 16: spelling - cycle

Line 17: check tense here, are run or were ran

Line 25: is this a series of simulations or two simulations?

Lines 32-33: references for this assumption

Page 7, lines 20-21: '. . . model predicts that, on average, higher LEC are found at intermediate elevations.' None of the figures really show this, Figure 3a at 5Ma has a broad peak across most of the elevation range and (c) has a peak at very low elevations.

Line 27: is Fig. 2c the correct figure reference?

Lines 29-30: again I don't see an obvious peak at mid-elevations, rather a broad peak across most elevations.

Page 10, 4th line of caption: defined not defines

Page 12, line 3: . . .the offspring can come from the. . .

Line 8: ?allows us to model

Line 9: . . . a uniform

Page 13, line 9: why is this? Because these slopes aren't as steep therefore allowing for more distance with change in elevation?

Page 15, 1st line of figure caption: replace in at end of line with of

Figure 3: Plots (a) and (c) appear to have a moving horizontal scale, that is zmin and zmax change with time. State in the caption for clarity.

Figure 4: (d) is a map of chi over the whole region. Willett et al. (2014) show chi calculated along streams not the whole region. I think it would be much easier to see what the authors are trying to show here if this plot was similar to Willett et al Figure 3 or 4. The scale bar being pushed toward lower values also makes it difficult.

Figure 5b 5.5My. Given that the area shown in (a) is very small compared to the entire domain, I'm surprised by the distribution of the grey dots beneath those of catchment C1 and C2. Do these represent the whole domain? Shouldn't there be many more grey dots around the elevation range of C1 and C2? This plot suggests that the chosen region is unusual compared to the rest of the domain, but this is not evident in figure 5b.

Figure 8: How is an isolated region defined? The text only specifies low LEC and the figure caption lower connectivities. The final two sentences of the caption appear to refer to (a) not (b). The third plot of 8b doesn't look as asymmetric as the second plot of figure 7b.

There are some inconsistencies between the figures.

The region of 4d shown in figures 7 and 8 don't appear to be the same. The third plot of 8b doesn't look as asymmetric as the second plot of figure 7b.

Scale bars for LEC, figure 8 has more white around the median value of 0.5 than in other figures. This makes comparisons difficult.

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2019.