**Review of Topographic controls on divide migration, stream capture, and diversification on riverine life, by Lyons et al, submitted to Earth Surface Dynamics.**

This paper is about the relationship between landscape evolution in response to base level fall or heterogeneous uplift and the evolution of species richness, based on a large number of numerical simulaitons. The authors use a free-access LEM to generate the landscape and develop a new component for the LEM to solve for species richness.
This work addresses very interesting questions on the links between perturbations, landscape and species richness. However, I found that the current form of the manuscript does not support this work as it should. The text is sometimes vague because of the use of generic words and absence of quantitative data, and some sentences are a bit complex and could be more straightforward.
As a consequence, it is a bit difficult to follow the description and the arguments of the authors. I think the manuscript requires rewriting to clarify the context of this study, to ease the reading and to clearly support the purpose and the novelty of this work.

I hope my comments below can help,

Laure Guerit
Géosciences Environnement Toulouse, France

Your comments certainly did help. Thank you for your time. Our responses to your inline comments below cover the topics in your introduction to your review.

**Introduction**

First paragraph: I think the authors can present better what has been done before on drainage reorganization from field, lab and numerical studies. It seems that the real novelty of this work is the SpeciesEvolver they propose and the evolution of species within an evolving drainage network. This should be better presented and highlighted throughout the paper. In the current, this very interesting contribution is a bit lost among other things. Below is a small selection of papers that might be relevant for the general context and maybe elsewhere in the manuscript (sorry for the self-citation but it seems to be relevant for this paper. Note that I don't ask for reference to these papers, they are just some examples).

We agree the introduction should better highlight prior work in drainage reorganization along with species macroevolution and our key contribution of integrating the two through modeling. We added additional prior work context and references for both topics, while keeping paper length in mind, and emphasized our contribution more in the introduction, discussion, and conclusions.

Second paragraph: add a reference at the end of line 25 to justify this statement or explain it a little bit here.

Improved our explanation of dispersal here.

Third paragraph: the limits of the stream power model coupled to hillslope diffusion are discussed for quite some years (see for example Lague, 2014) and other models based on a different formalism have been proposed (see references below). As the choice of the model affects how the landscape responses to a perturbation (Armitage et al., 2018), this could be discussed in section 5.

In the revision we included a new section in the discussion regarding limitations of our model. We included how limitations of the stream power model and hillslope diffusion factor into our model.

**Description of modelling tools**

This section is too vague and it is difficult to get a correct idea of the numerical model used here. I suggest to be more specific, for example, name the fields, give the values, present the multiple components, etc. Also explain how the SolverEvolver is working: define what kind of species you are considering, how do you set the parameters, etc.

> This section provides a general description of the tools used to build the model of the study; therefore, the goal of this section is not to explain the numerical model used in this study. The following section, 'Experiment design' presents our application of the tools to create a numerical model, including descriptions of components, fields, etc. We find it preferable to separate these because (1) readers are guided from general to more specific, (2) this organisation separates what we are using in the study (the tools) from our application (the model), and (3) including the level of detail of components, etc here would greatly increase the length of this section because the components, etc would have to again be put into the context of the study.
>
> In this revision, we explicitly indicate the purpose of the section and more succinctly describe the tools to make clear it is an overview of the tools.

**Experiment design**

Here again, I suggest to be more specific and quantitative: what is the amplitude of the sea-level fall, of the uplift, how to you identify the variables and what are these variables (l.12). At the end of the first paragraph, you mention seven factors that are not listed below. Please name them and give range of values so that the following sections are easier to follow.

> The absence of introduction to the seven factors was addressed by including in this paragraph/section (3.0) a reference to Table 1 and indicating the precise section where the factors are described. This table includes both the magnitudes of base level fall and fault throw as well as the other sensitivity analyses factors. The values of factors vary in experiment trials, so these cannot be quickly summarized without great redundancy with later sections. The inclusion of Table 1 provides the introduction to the factor names to help readers link this section introduction paragraph to later subsections.
>
> As this paragraph is an overview of all of section 3, description of the factors is held off until they come into play in the 3.2. Section 3.1 explains the sensitivity analyses providing how factors in general are used and why there is a range in factors.

**Sensibility analysis**

Please clarify how you define the expected value of Y (l.4) and how the indices will be use in the following (end of the section).

> Clarified that the expected value is more precisely the conditional expectation, in terms of probability theory. Our use of Sobol indices in identifying the most influential factors on a response was added to the end of this section.

**Model trial progression**

The values used in this study must be presented in the manuscript (at least in Supplementary Material)

> This comment is in response to the following text in the manuscript: "The factor values for each trial are available in Lyons et al. (2019)." This reference is the data repository that contains the factor values of the 51,200 trials for this study. Using a data repository such as Zenodo follows the recommendations of the journal. In the revision, we explicitly indicate that the reference is a data repository associated with the paper.

**Initial conditions phase**

beginning of page 6: I don't understand how you generate the initial elevation grid. Please consider reformulate these sentences.

Two sentences were added for clarification beginning with, "The initial topography of each trial was generated in a two-step process…". The first paragraph containing the sentences in question describe how the initial random elevation values were set. The description of the initial elevation creation does not really begin until the second paragraph of this section. I find that the new sentences clarify the generation of the initial elevation values.

p.6 l.18 to 24 5mm/yr is also reported in New Zealand (eg, Jiao et al, 2017) while $10^{-5}$ corresponds to cratonic values. Maybe simply write that you consider uplift rates in the range of cratonic to orogenic values.

This is a fantastic idea. (thank you!) We added this as a simplification and we retained some of our prior explanation for those less comfortable with the terms cratonic and orogenic, given that this paper may appeal to those with less of a geoscience background.

Additionnal references for erodibility and diffusion suggested below.

We added an additional reference for erodibility. We sought references that were directly comparable, i.e., m/n is the same in the reference in our paper. For the diffusion coefficient, we cited a review paper with several references. Suggested references do not contain a comparable D, for example Perron et al. (2009) provides D/K ($m^{2m+1}$).

m and n: Kang and Parker (2018) suggest that the value of 0.5 should not be used as it leads to unrealistic behavior. Maybe the authors coudl run a few additional simulations to check whether they do observe the same behavior with m/n = 0.4 for example (this does not have to be part of the main manuscript).

The paper that you describe, Kwang and Parker (2017) states, "when hillslope diffusion is neglected, the choice m/n=0.5 yields a curiously unrealistic result...". We did incorporate hillslope diffusion; therefore, this model limitation does not apply here.

p.7 l.12 describe or add a figure to illustrate.

This comment refers to the following sentence: "Across the trials during this phase, factor values produced different initial stream networks and species locations." We removed this line that we now recognize is more of a result than a method.

**Perturb phase**

p.7 l.14 describe the steady state topography (for example the elevation and the number of catchments)

The steady state topography is described in the results section because it is an outcome of the model factor combinations. Metrics of the tens of thousands of unique landscapes are summarized in Table 2. The model responses, including relief, of each trial is provided in the data repository reference, Lyons et al. (2019).

p.7 l.21 describe how the landscape responses to the perturbation. Is it only by knickpoint propagation ? What happens on the hillslopes ?

We rewrote this sentence to be more direct in why this equation is presented. The landscape response to the perturbation is thoroughly described in the beginning of section 4. This description was improved in the revision.

p.8 l.11 the way to define steady state could be recall here.

Included steady state conditions here as well.

**Model response variables**

l. 13 what variables ?

In the revision we now recall the explanation of response variables directly under the header of this section.

p.9 l.1 the model descriptions must be within this manuscript.

This comment concerns a citation of Lyons et al. (2019), which is the dataset of this research in the Zenodo repository. It is now explicitly mentioned that the reference is a data repository associated with the paper.

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The comments within these dashes were generally addressed by focusing more directly on exemplary model trials. Exemplary were also used in the prior version, although our prior explanations were unnecessarily confusing by attempting too much to generalize with all experiment trials when the exemplary trials often suffice.

p.9 l.4 specify what minimally implies

The streams in the lower grid are now described as remaining fixed, which is the case for the exemplary trials, rather than minimally shifting.

p.9 l.5 unclear, consider reformulate this sentence.

We reformulated this sentence for clarity.

p.9 l.7 please give the size in meter

A newly included measure of main divide migration enabled us to include 250 m instead of "a few nodes".

p.9 l.7 the sentence is odd with respect to the previous one saying that the streams are minimally affected. If so, why is the main divide migrating ?

The idea being no lateral stream erosion while streams erode headward. This paragraph was rewritten in the revision to clarify this.

p.9 l.9 a quantitative value or a figure to support this statement would be welcome.

Improved in the revision is clarification that comparison of the analytically-predicted and numerically-modelled knickponts is illustrated in supplementary animated videos. Animations include quantification of knickpoint propagation using Eq. 6.

p.9 l.12 «sufficiently» please quantify

"sufficiently" was quantitatively put into context of the minimum perturbation magnitude required for main divide migration now described earlier in this section.

p.9 l.16 please consider reformulate. This sentence suggests that they are two main divides (the main one and the main on the upthrow block), which is odd.

We interpret this comment as a misunderstanding of our intent to compare main divide migration in the two scenarios. We rewrote this sentence to clarify the nature of the main divide in the two scenarios.

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**Topographic relief and landform change**

The first paragraph is a bit complexe to follow, it could be written in a more straightforward way to ease the reading.

> We have rewritten the paragraph for clarity.

l.25 11 000 m seems high for a terrestrial landscape.

> We included in the discussion that the maximum relief outputted in a trial is greater than observed, notably that mass wasting not included could contribute to the discrepancy. It is our opinion that the discrepancy is small especially given the simplicity of the model.

l. 29 the evolution of the topography is controlled by the stream power model (your equation 5). The main controlling factors are U and K so I don't think the total order Sobol indices analysis is required here. This would simplify this section.

> Perhaps those more familiar with the stream power model will understandably question the purpose of the analysis for these variables given that the control of U and K on relief is easy to understand given the simplicity of these variables in the equation. An intent of conducting the Sobol analysis on U and K in respect to relief is to allow readers to confirm their understanding of how the Sobol indices work, and that they do work, prior to using these indices in later sections on more complex relationships between factors and responses. Further, this analysis is to emphasis the primary influence of factors on relief, which is critical for later sections.

p. 10 l.3 please quantify «low relative»

> First, this sentence was rewritten with U and K (model inputs) instead of relief (model output) to indicate the control on divide and stream location change. Also, the paper puts forth that the relative values of U/K (or relief) vs perturbation is what matters. The values of the parameters relative to each other is more important than their absolute values. Text was added here to emphasize this.

p. 10 l.8 please quantify «high»

> Our response to the prior comment also applies to this comment.

p. 10 l.10 could you add a figure to support this statement ?

> A figure already exists. This sentence describes another detail about the figure referenced in the prior sentence. Text modified to help make this clear.

p. 10 l.14 please quantify «low»

> Maximum relief is now indicted for trials with < 30 % divide change.

p. 10 l.17 please quantify «sufficiently high»

> Rewritten to describe the relationship among the trials between perturbation magnitude/fault scarp and stream/divide location change.

p. 10 l.23 please define what is a divide change

> Stream and divide change, collectively referred to as landform change, is defined in detail in

the methods. In the revision, these terms are recalled early in this results section, “Topographic relief and landform change”.

**Stream capture occurrence**

This section is more about the controls of the occurence than the occurence itself so the title could be adjusted to better reflect the content of this section.

Adjusted section title.

p. 11 l.33 please quantify «moderately high»

Rewritten to describe the relationship of relief with stream change and capture when Pm:relief near 1.

**Species richness**

Here again, the section is more on the controls on the species richness than on the richness itself. The title should be adjusted to reflect the content of this section.

Adjusted section title.

l.9 unclear, please consider reformulate

Rewritten for clarity.

l. 16-22 this paragraph should come first in the section

We agree and reorganized the beginning of this section.

p. 12 l.21 please specify «less than» what ?

“topographic” was inserted before relief and the sentence was rewritten for clarity.

**Discussion**

p.13 l.8 a short description to the chi metric could be proposed here and a proper chi analysis could be performed to support the discussion.

Chi analyses are especially useful in real landscapes (i.e. not modeled) where natural topographic complexity/roughness is great. In modeled landscapes, analyses of other metrics (e.g. relief) lead to similar interpretations. Whipple et al. (2017) found relief to be a reliable predictor of drainage divide migration, and relief is already central to this paper.

p. 13 l.15 please quantify «greater increase»

We could not find “greater increase” at this line or elsewhere in the document.

p.13 l.17 define «a certain relief»

“a certain relief” is rewritten as “a given relief” to indicate the relationship described in this heading varies by the relief of the landscape, where the landscapes and their relief vary in the experiment trials.

p.14 l.4 did you work with higher Pm values ? Does it influence this behavior ?

The Pm of trials ranged 0.1 to 100 m. The Pm influences the proportion of divides that migrated as Pm is less than initial relief. Rewritten for clarity.

p.15 l.4 quantify «relatively high»

> We rephased sentence to describe the relationship of kd and other parameters in this relationship as the relationship is of greater importance than absolute values in this instance.

p.14 l.4 define what is an «elongated divide migration»

> We removed "elongated divide migration" in a rewrite of this sentence to better describe that divides migrated a greater distance when initial relief was less than the perturbation magnitude in a trial.

p.14 l.14 specify «more than » what

> Added "more important than the fault throw scenario" to indicate the scenario where Ac had a greater influence on model output factors.

p14 l16 captures should be captured

> We interpret this comment as a misunderstanding of the intended meaning that captures become increasingly more frequent as Ac decreases. We rewrote for clarity.

**Conclusions**

As suggested for the introduction, it seems that the novelty of this work is the relationship between species richness and drainage reorganization rather than reorganization itself. This should be better highlighted here.

> We appreciate you ensuring that the novelty of the work is highlighted sufficiently. In the first submission, the relationship between species richness and drainage reorganization was highlighted in the second paragraph of the conclusion. The centrality of this topic in our contribution was made stronger.

Can the authors comment on the value of 439% ? Is there a way to compare with natural landscape ?

This kind of quantification is missing in the rest of the paper to support the work of the authors.

> In the revision we describe the parameter and conditions of the run that put this great increase in richness. In our modeling we find singular values are likely less useful in comparison with natural landscapes as they are the outcome of the interaction of multiple factors.

**Table 2** considering the range of uncertainties, the statistics could be close to 0. Could the authors comment on that ?

> We clarify that the plus/minus values in this table indicate the range of values outputted by trials of the model experiment.

**Figure 6c-d** missing labels

> We included labels added in the revision. The labels were omitted in the first submission because the axes in c-d are the same as a-b. Consistency among the subplots will be clearer.

**Some references about drainage reorganization and chi**

- Bishop (2007) Long-term landscape evolution: linking tectonics and surface processes
- Bonnet (2009) Shrinking and splitting of drainage basins in orogenic landscapes from the

migration of the main drainage divide

- Perron and Royden (2012) An integral approach to bedrock river profile analysis
- Guerit et al (2018) Landscape 'stress' and reorganization from chi-maps: Insights from experimental drainage networks in oblique collision setting

**Reference to landscape and species evolution** (with references inside that might be very relevant to this work)

- Salles et al (2019) Mapping landscape connectivity as a driver of species richness under tectonic and climatic forcings

**Reference to the stream power model** (and references therein)

- Lague (2014) The stream power river incision model: evidences, theory and beyond

**References to other models**

- Armitage et al. (2018) Numerical modelling of landscape and sediment flux response to precipitation rate change
- Carretier et al. (2016) Modelling sediment clasts transport during landscape evolution: Earth Surface Dynamics, v. 4, p. 237–251
- Shobe et al. (2017) The SPACE 1.0 model: A Landlab component for 2-D calculation of sediment transport, bedrock erosion, and landscape evolution: Geoscientific Model Development, v. 10, p. 4577–4604,
- Langston and Tucker (2018) Developing and exploring a theory for the lateral erosion of bedrock channels for use in landscape evolution models: Earth Surface Dynamics, v. 6, p. 1–27
- Yuan et al. (2019) A new efficient method to solve the stream power law model taking into account sediment deposition: Journal of Geophysical Research: Earth Surface
- Jiao, R., Herman, F., and Seward, D.: Late Cenozoic exhumation mo- del of New Zealand: Impacts from tectonics and climate, Earth- science reviews, 166, 286–298, 2017.
- Kwang and Parker (2018) Landscape evolution models using the stream power incision model show unrealistic behavior when m/n equals 0.5

**References to m/n, K, Kd**

- Whipple and Tucker (1999) Dynamics of the stream-power river incision model: Implications for heigh limits of mountain ranges, landscapes response timescales, and research needs
- Snyder et al. (2000) Landscape response to tectonic forcing: Digital elevation model analysis of stream profiles in the Mendocino junction region, northern California
- Wobus et al. (2006) Tectonics from topography: Procedures, promise, pitfalls
- Perron et al. (2009) Formation to evenly spaced ridges and valleys.

We carefully considered which of the above references were appropriate, as well as other additional references, and added those that were appropriate. We thank you for compiling this list.