Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2019-5-RC1, 2019

Interactive comment on "Statistical modelling of co-seismic knickpoint formation and river response to fault slip" *by* Philippe Steer et al.

Anonymous Referee #1

Received and published: 11 March 2019

Review of Steer et al: "Statistical modelling of co-seismic knickpoint formation and river response to fault slip"

Landscape evolution models (LEMs) are in most cases driven by simplistic boundary conditions. Rock uplift (or translation in the case of strike-slip) due to fault motion may change through time, but is generally treated as a time-averaged rate, and the effects of individual uplift events at the earthquake scale are generally not considered. Yet there is a growing sense in our community that rivers may be valuable indicators of paleoseismic activity, and potentially current seismic hazards. In this contribution, Steer et al make a well-considered and important step towards a better understanding of how rivers respond to sequences of earthquakes. The authors present a simple model for how earthquakes shape river profiles under idealized conditions (i.e., under the

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Discussion paper

assumption that knickpoints propagate unchanged upstream at a constant velocity). They use the model to make several tests of resulting river profile form, including exploring the effects of 1) the variance of the normal distribution of earthquake depths, 2) along-fault distance of a set of parallel rivers, and 3) various sediment cover scenarios.

The authors make several interesting findings. They find that incorporating both seismic and aseismic slip leads to only larger earthquakes being expressed in the river profile. They find that the degree of seismic coupling (seismic vs aseismic slip) also yields diagnostic changes in profile form. They assess the appropriateness of modern DEMs for extracting knickpoints in different seismic situations, and suggest that in fully coupled faults DEM resolution would have to be unreasonably high to extract knickpoints associated with individual earthquakes. They find a length scale above which correlation between the profiles of parallel rivers along the fault should no longer be expected due to the limited reach of a given earthquake along the fault plane. Finally, they identify situations in which sediment cover dynamics dominate seismically induced knickpoints and vice versa.

GENERAL COMMENTS

I really enjoyed this paper. The study is very novel in considering how the specifics of seismicity are expressed in the river profile. It is also timely as it affects both landscape evolution modelling and the inversion of river profiles obtained from high resolution DEMs. I find the study to be well thought out and well executed, and very appropriate for ESurf. My recommendation is to publish after minor revisions, which have very little to do with changing the science and mostly deal with the presentation. I have four main comments:

1) Explanations of key terms and variables in the seismic modelling portions of the manuscript could be improved. I am a surface processes person, so it may be that I am exceptionally deficient in this department. However, since this paper is submitted to ESurf, I doubt that I am alone in wishing for clearer explanations in the sections

dealing with seismic modelling. For example, the concepts of aseismic and interseismic deformation are not defined where they first occur on line 17 of page 2. I have marked in the specific comments other places where I think some easy additional explanations would help typical ESurf readers understand the paper.

2) These comments pertain to the organization of the manuscript.

The results section is very clear for sections 4.1, 4.2, and 4.3. However, we then jump to "4.4 Knickpoint detectability," which is really not itself a result but an implication of the findings related to knickpoint height and spacing that the authors reported in Figure 6 and section 4.3. To me it seems that section 4.4 and Figure 7 belong in the discussion section. To be clear I think that this discussion of knickpoint detectability is great and should certainly remain in the paper, but since the authors don't actually do any detection of knickpoints in the paper it belongs in the discussion.

Figure 9 shows a very interesting (and important!) result, with major implications for studies using river profiles to infer seismic history. But Figure A1 is important to understanding Figure 9. I suggest that Figure A1 be combined with Figure 9 so that all of this information is in one place in the main text.

Much of section 5.5 describes and contains results of a set of model simulations addressing the role of sediment cover. These are again great, but it does not make sense to me to locate them in the discussion after such sections as "model limitations". Most of section 5.5, including figures 10 and 11, should be relocated to the results section, because these are really just the results of simulations. Any remaining text that does more than describe the results (e.g., the very helpful writing on lines 21-30 of page 23) can remain in the discussion.

3) There are some places in the manuscript where awkward phrasing or sentence structure make reading difficult. I have marked many of these below in "technical corrections," but I would encourage one last thorough proofreading by the authors before resubmission.

4) I was excited to see that the model is available on Github. I ask the authors to consider associating the exact version of the model used for the paper with its own DOI and reporting that DOI in the code availability section. That way, if the model ever gets changed, interested readers can always find the version associated with this paper. This is easily done (~10 minutes) with GitHub and Zenodo: https://guides.github.com/activities/citable-code/.

SPECIFIC COMMENTS (page.line)

2.17: as noted above, it is important to define seismic deformation, aseismic deformation, and interseismic deformation. If it is too cumbersome for the introduction, use more common wording in the introduction and define the terms in section 2.

2.32: same comment as above for the term "seismic slip"

3.2: Just another clause or sentence here about the BASS model would be good. I know it is explained in detail later, but saying something like "This model uses a standard earthquake sequence model, the branching... "

3.20-3.24: This discussion of Flint's law does not fit into the discussion of earthquake magnitude distributions. It could be moved to section 2.3, or could be eliminated entirely as it is fairly obvious (e.g., if we consider steepness indices rather than slope, there would be no confusion between tectonically driven slope changes and slope change due to decreasing drainage area) and the authors hold drainage area constant in their model anyway.

4.5: define "fast earthquakes"

5.18: There is also a recent field study that should be cited here. Brocard et al (2016) argue that there is poor correspondence between drainage area and knickpoint retreat rate at their sites.

6.12: in the caption for part b, say what squares and circles mean for people reading in grayscale.

7.16: the parameter "chi" in the Figure 2 caption is not defined or explained until page 8, but Figure 2 is referenced on page 7.

8.22: a slightly more thorough explanation of what "seismic coupling" means would be helpful for most ESurf readers.

10.20: I am not sure I would describe the river profile modelling as 2D. Generally the number of coordinates needed to describe a point sets the number of dimensions. I would be tempted to say this is 1D modelling because every point along the profile is associated with an elevation (i.e., no consideration of river width). I understand that where seismically induced knickpoints occur some distances can be associated with two elevation values, but this still seems like a 1D approach, as opposed to for example Croissant et al (2017) in which channel width is also considered.

13.1: "river backward erosion" is awkward. Try an alternate phrase like "upstream knickpoint migration" or something else.

13.11: erodibility is spelled two different ways (also erodability) in this paper. I prefer the first way (and I suspect that the Copernicus typesetters do too).

14.11: as mentioned in the general comments, this subsection seems better suited to the discussion section.

14.19: is there a citation to back up the statement that resolution gets worse in gorges? It seems intuitively true, but a citation would be good.

15.11 and onwards: This is a very nice and important result!

17.1: Add to the caption that panels a/b and c/d correspond to simulations of different variance. I see it in panels a and b, but having it in the caption as well would be helpful.

17.21: as stated in general comments, please consider combining Figure A1 with Figure 9 and keeping it in the main text.

17.26: a novel and important result; this decay in correlation is not often considered by the surface processes community.

19.13: it would be good to also reference Brocard et al (2016), which provides field evidence compatible with Baynes' experimental result.

21.12: as stated in general comments, please consider moving this to the results section as it is describing an additional set of model simulations.

Figure 11: it might be helpful in panel A to add a label and arrow showing which profile is associated with which amplitude signal. I know it may be fairly obvious, but it would aid interpretation of the figure. Just an arrow with the label "increasing amplitude" pointing in the appropriate direction on panel A would suffice.

TECHNICAL CORRECTIONS

1.21: sub-meter

1.29: interactions among

2.13: builds

2.19: viscous mantle flow

6.16: awkward; consider rewording

7.9: is equal

12.7: typo "nd"

14.18: Resolutions of topographic...

14.22: delete "to"

15.3: last sentence is awkward; rephrase for clarity

16.9 and 10: no hyphen needed

16.10: independent means

17.13: delete "or"

18.9: awkward: rephrase for clarity

20.2: knickpoints were found

21.28: sentence fragment

23.13: a rate? or rates?

23.14: "from no to a large sediment" is awkward

23.17: delete "in turn"

25.13: scales at best linearly with

Caption of Figure A1: two typos: should be "panel a" and "panel b"

REFERENCES

Brocard, G. Y., J. K. Willenbring, T. E. Miller, and F. N. Scatena (2016), Relict landscape resistance to dissection by upstream migrating knickpoints, J. Geophys. Res. Earth Surf., 121, 1182–1203, doi:10.1002/2015JF003678.

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