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Earth Surface Dynamics Discussions

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

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GENERAL COMMENTS

This submission combines a stochastic model of earthquake occurrence with a stream power landscape evolution model to study the spatial distribution and detectability of co-seismic knickpoints generated by a buried thrust fault.

The authors explore the effects of seismic coupling, channel spacing, and exogenous sedimentation on knickpoint expression in isolated and neighboring channels. Among the most significant findings is that a fault with a large seismogenic area and limited aseismic slip may produce a similar number of channel-rupturing earthquakes at all magnitudes. They also identify a channel spacing beyond which parallel channel profiles are poorly correlated due to limited rupture extent, and quantify detection limits for knickpoint identification as a function of data resolution. The authors close with a discussion of the impacts of cyclic sedimentation on knickpoint preservation.

Overall, it is a novel contribution and the methodology is reasonably well documented. I think the manuscript could be accepted after moderate revisions and editing for length and clarity.

SPECIFIC COMMENTS

MAJOR POINTS

1. In general, the writing could be made more concise. One easy change might be to expand the appendix with some of the details and background. I've tried to indicate sections I felt could be moved to the appendix in the minor points below.

2. A summary table of symbols used would make the study more accessible (included in main text or appendix). This is particularly important because some of the notation (χ) has conflicting uses in geomorphology (drainage-area-normalized channel length) and seismology (seismic coupling coefficient calculated as a ratio of velocities or moments). It would also help to clarify model parameters like σ not always found in physics-based earthquake cycle models, to aid comparison to something like the half width of the seismogenic zone in other studies.

3. A simple schematic figure would be helpful: either a map view of the model domain showing rupture extent and channel spacing, or a profile view of the channel and fault geometry, or both. This may be available in previous work by the authors, in which case a citation pointing to the model set up figure would be very helpful early in the text.

4. I believe the LEM described starting on page 10, line 20 is better described as a one dimensional model of the channel profile. Lateral transport in the y direction is not considered. Regarding this model, a non-linear stream power rule ($n \neq 1$) is worth

including in this paper. What if higher slope knickpoints migrate faster than low slope knickpoints? I was surprised to see this discussed at length in Section 5.4 without a comparison of linear and non-linear model results.

5. The methods used in Section 4.4 should be clearly described in the opening paragraph. The analysis counts known co-seismic knickpoints in down-sampled river profiles rather than detecting knickpoints without prior information (which "detectability" might imply to some readers). This provides a useful baseline which should be emphasized early in this section's text. I appreciated the resolution testing summarized in the closing paragraph of this section.

6. The results shown in Figures 10 and 11 are quite interesting and I hope they inspire future work. Could the authors determine something like the maximum detectable tectonic knickpoint height as a function of sedimentation rate and periodicity for tectonically active catchments whose climactic history is well understood? What about superimposing several earthquake cycles with climatically varying sedimentation?

MINOR POINTS

1. To justify the Poisson process claim in Section 4.2, a best-fit exponential function and standard error value (or other measure of goodness of fit) could be provided for each of the distributions of inter-event times in Figure 4. It appears that the decay is not necessarily exponential for the most aseismic model (4a). It might be better to weaken this claim if the decay is not exponential in all cases.

2. Figure 5 and parts of Section 4.3 could be moved to the appendix. It is important to justify the choice of VR, but this distracts from the central result in this section.

3. Figure 8 and parts of Section 4.5 could be moved to the appendix as Fig. 9 + A1 and Section 4.5 include sufficient detail.

4. The model fault is a moderately dipping thrust fault, but the knickpoints are generated as vertical discontinuities. How might knickpoint detectability, preservation, and

the profile cross-correlations change if the knickpoints have a finite initial slope or if knickpoints are displaced horizontally? This is addressed in Section 5.1, but it would be particularly interesting in the case of a non-linear stream power rule if knickpoint slopes vary.

5. The link to the GitHub repository is nice to see. For a more direct citation, it would be best to archive the current version of the code and provide a DOI through Zenodo (https://guides.github.com/activities/citable-code/).

TECHNICAL CORRECTIONS

I have tried to avoid duplicating editorial points raised by reviewers 1 and 2. I encourage the authors to proofread the revised manuscript.

Page 1 Line 17: “range magnitude” > “magnitude range”

Page 2 Line 25: “following” > “following work”

Page 4 Line 1: “intersect” > “intersection”

Page 20 Line 10: “fundament” > “fundamental”

Page 22 Line 9: “abrupt changes” might be more appropriate than “brutal changes”

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