Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2019-5-RC4, 2019

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

George Hilley (Referee)

hilley@stanford.edu

Received and published: 4 April 2019

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

Summary:

This paper combines a stochastic model for releasing accrued moment (by earthquakes, aftershocks, and creep) with a geomorphic model to advect surface-rupturing offsets upstream at a constant velocity along channels. The modeled fault is a 30° dipping thrust fault slipping at 15 mm/yr. The upstream advection rate of offsets generated by earthquakes is set to 10 cm/yr. Using these parameters, the authors investigate the range of generated profile forms, with an eye towards detectability of knickpoints produced by seismogenic offsets and the role that creep plays in river profile development. This speaks to the utilization of the distribution of knickpoint heights as a paleoseismic tool, and the use of constant (or smoothly varying) uplift rate boundary conditions versus individual offset events in modeling river profiles.

Recommendation:

This paper presents an interesting new approach to addressing several important questions in tectonic geomorphology: 1) under what circumstances are profiles approximated by constant or smoothly varying uplift rate conditions versus individual earthquakes, 2) given data sampling and accuracy, under what conditions might river profiles resolve past earthquakes, 3) how does the distribution of surface rupture magnitudes affect the interpretation of knickpoints in river profiles, and 4) what role does creep play in generating the profile forms of rivers traversing faults. I think the work has the potential to provide clarity to many of these issues, at least to the extent that it could nicely frame these problems and lay out a path for future field and modeling studies. As written, the paper contains the seeds of this, but could be reorganized, recast, and extended to be more impactful in this regard. Below, I make several observations and suggestions that are aimed at this purpose. The authors may argue that these suggestions are outside of the scope of this work given their aims. Yet, I worry that in its current form, the paper may not garner the impact it deserves. These are issues of preference, and so I leave it to the authors and editor to decide on whether or not these suggestions are appropriate for the scope of this work. Nonetheless, at a minimum, the paper needs some efficiency improvements and clarifications, which together probably constitute MODERATE TO MAJOR REVISIONS.

General Comments:

1) I have to admit that I found Section 2 confusing and largely unnecessary. I think it is being used to motivate the model that has been developed. But, an alternate approach would be to move directly from the Introduction to the Methods and use the literature

cited to support the model selection. The discussion could then be used to cite and discuss literature that may complicate the model, and what impacts these studies might have on the fundamental results that are reported.

2) I think the presentation of the model in Section 3 could be greatly simplified. It seems as though the following is done: 1) Earthquakes are drawn out of a G-R distribution whose patch sizes and offsets are determined by the Leonard scaling relations (aftershocks are then generated using BASS). 2) The hypocenters of the patches are located according to a Gaussian depth-distribution centered at the midpoint of the model domain, and are uniformly sampled along strike. A uniform slip rate is maintained, such that aseismic creep takes up what the earthquakes do not. 3) From these, surface-rupturing patches are identified, used to uplift surface channel points, and then the profile is advected horizontally at a prescribed rate. I'm not sure there is a need to bring the power-law incision model into this, since it is not really used, to the extent that the advection velocity is assumed constant.

3) I appreciate the comments of the reviewers with regard to examining more flexible geomorphic incision rules. I think that these criticisms are rooted in the fact that the paper is cast in terms of the power-law incision model. As mentioned in (2), the incision model is not really being used here anyway, at least not in any explicit way - the perturbations in the profile are simply being advected headward at a constant velocity. Thus, I would suggest taking the approach that you are using a zeroth-order geomorphic model of constant advection rate to match the simplicity of the offset model (see below for suggestions on this). These limitations can then be discussed in the Discussion, and a path toward future work can be laid out. This avoids issues related to geomorphic model selection, since you would simply be using a kinematically prescribed description of knickpoint creation and migration. The appropriateness of this simplified model could then be discussed later in the paper and put into the context of the incision rule.

4) It seems that with a little more thinking, the spirit of this work could go a long way

in clarifying the role that individual earthquakes play in producing channel profiles. First, I think that the dimensionality of the problem could be reduced and the results could be clarified. The current study uses a single slip rate, which is implemented through events created by the G-R relations, and completed by creep to advect these knickpoints at a velocity, Vr, which is either constant or randomly sampled. It seems as though the profile geometry will be approximated by uniform uplift rate when the spacing between successive knickpionts is small, and will be detectable as individual offset events when this distance is large. This suggests a horizontal length-scale L that varies with Vr * t, where t is the recurrence time of some earthquake. Additionally, the vertical dimension of the model might be normalized by the vertical displacement (d) that occurs during this earthquake. Normalized time in this case would simply reflect the number of earthquakes that have occurred and the fraction of the earthquake cycle that has been experienced. Given this parameterization, if one starts with the simple case of a time-predictable characteristic earthquake, the two length scales can be related through the slip rate along the fault as t = d / (Vs sin(dip)), meaning that L = d (Vr / Vs) / sin(dip). The extremes of this length scale should revert to the single-event, and constant uplift rate cases as t* becomes large.

I understand that this is not what the current study has done, as it is a stochastic model that is used here. But, I wonder if it would be more illustrative to restructure the paper to start with a simple toy model that demonstrates this point. Beginning with a characteristic earthquake that ruptures the surface in this way, one could use the procedure for uplifting and advecting the surface profile in normalized space (x* = x / L; z* = z / d). You could show, in normalized coordinates, that this is just a unity increment in z* with each unity increment in x* at the beginning of each earthquake cycle. When you do this when t* is large, for a range of x* between 0-5, you'll see the earthquakes, when x* is between 0-500, you'll see a constant slope (more-or-less). I think that these are the two end-members that are being sought. Since the uniform uplift case will produce a slope of one in this space, you could even use the deviation from this line as a measure of how far / close to the uniform condition you are. You could then define a horizontal length-scale that defines the "detection limit" that one might expect to observe in the field, and cast this in terms of the multiple of L at which individual earthquakes bleed into continuous uplift to see those d*(Vr/Vs)/sin(dip) conditions for which one might expect to be able to see individual events clearly.

After this simple exercise is completed, then the stochastic model would be a nice extension of the basic idea. In this case, the productivity rates of the a-value of G-R could be cast in terms of the seismogenic fault slip rates, and this could be used with the maximum magnitude event to define recurrence time, which could serve as the normalizing time-scale. Experiments could be performed, results normalized, etc. to examine the impact of the stochasticity on the character and range of knickpoints generated by such an exercise. Finally, W could then be varied in a way that creep could be added into the analysis, with its impact on the profile form (and knick point detectability) analyzed.

Such an incremental and non-dimensional casting of the problem might help to distill the problem to its essence for illustration, show how increasing realism in the way in which seismic moment is released impacts the profile forms, and broaden the applicability of the analysis to a wide range of fault geometry and slip rate conditions through the non-dimensionalization. The discussion could then be focussed on: 1) How Vr might actually relate to the power-law incision model (since the analysis can be understood without the context of the incision rule), 2) What the impacts of more complicated variations of Vr with things like channel slope might be (i.e., $n \neq 1$), 3) what lithologic / climatic conditions might be appropriate for archiving meaningful paleoseismology information (ranges in K for different watershed areas that produce Vr/Vs ratios that yield detectable knickpoints), 4) what the impact of heterogeneities in lithologies and transport processes (e.g., transport-limited alluvial conditions) might be on profile form and detectability, and 5) other model limitations.

5) I was not altogether clear on how the horizontal motions produced by the 30° dipping thrust fault were actually treated in the knick point evolution model. I think that

the authors are probably resolving the component of slip into the vertical direction, and advecting this offset upstream. This creates two related issues. First, the horizontal motions produced by fault slip will act in an opposing direction to the knickpoint migration, and so some adjustment to Vr needs to be made to account for this. Given Vr and Vs used in the base-case simulation, this will not be a large contributor to the net advection velocity, yet it will constitute punctuated motion during offset events (or constant during creep). Nonetheless, as Vr approaches the 1mm/yr end-member shown in Figure 5, this effect will be important. Related to this, it is worth some text in the discussion that, if the location at which offsets are generated remains fixed, this assumes that the constant-elevation boundary condition is not advected by the horizontal motions. While this is likely the case in many circumstances, I'm not sure it can be regarded as universally true. Advance of the hanging wall of thrusts over the topographic surface in a ramp-flat geometry requires motion of the boundary condition. This will probably happen when Vr - Vs cos(dip) produces a negative value. In these cases, knickpoints will be created and will advance into the hanging wall, but the coordinate system needs to move toward the footwall at this velocity. As the thrust sheet advances over the topography, the ramp-flat geometry might cause motions to parallel to the topographic surface, and so discrete vertical offsets may not exist in these situations as a fault-bend fold develops at the ramp-flat transition. I'm not advocating that the authors implement this, as it is clearly beyond the scope of this work. But, it is an opportunity to frame future studies in the Discussion, which might track down some of these issues.

6) In the spirit of matching the complexity of models to one another, I wonder if the inclusion of aftershocks in the earthquake model creates a mismatch to the constant-advection-velocity geomorphic model. Indeed, I might argue that the appropriate match to the complexity of the geomorphic model that is used would be a time-predictable characteristic earthquake offset. But, I think there is real value in extending the model to creating events according to a G-R distribution. Analyzing the effect of creep also seems important. What was not completely clear was how much of a difference the aftershock component of the model makes in the end analysis. Could it be eliminated

without loss of insight? I am not saying that this is the case, but the process by which the authors build the study – that all of the complexity is introduced at the same time rather than an incremental inclusion of effects to assess each's importance – makes this difficult to determine.

Specific Comments:

I started marking up the PDF in Hypothes.is. Given that the paper may need some restructuring (if the authors find any of the above commentary valuable), I tapered off the detailed editing after Section 2.

https://hyp.is/go?url=urn%3Ax-pdf%3Ace60be1c82ce3c4e151f9f5839f60e0c

Summary:

I hope that the authors find these comments helpful in preparing their revision. Again, I think that this work has the potential to provide clarity as to when, and under what conditions individual offsets may be resolved within the profile of channels. It also has the opportunity to frame many future modeling and field studies focussed on tracking down and field-testing some of the assumptions made in the approach. In this regard, I hope that the authors understand that my comments are aimed at seeing that the work ultimately has the impact that it deserves.

George Hilley.

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

Printer-friendly version

Discussion paper