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Comment on esurf-2021-101

Anonymous Referee #2

Referee comment on "Short communication: Forward and inverse analytic models relating river long profile to tectonic uplift history, assuming a nonlinear slope–erosion dependency" by Yizhou Wang et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2021-101-RC2>, 2022

In this paper the authors propose an analytical model for knickpoint migration, and a methodology for inverting river longitudinal profiles when the slope exponent, n , is not assumed to equal 1. Overall, the research is well presented, with clearly stated general research motivations, and the methodology well documented and explained. The figures are overall clear and well presented, however the captions and in-text references to figures could benefit from further explanation of what is actually being shown. The methods and results presented in this paper are novel and I believe it would be well suited for publication in Earth Surface Dynamics. I have a few minor comments, which are mostly suggestions for expanding the discussion and typo corrections.

General points:

The inverse model proposed in the paper, solving for an uplift history under the assumption that $n \neq 1$ is not the first one. Paul et al. (2014) invert river profiles for an uplift history and vary the value of n between 0 and 2. The model themselves are different but there should be some acknowledgement that this paper is not the first to invert for an uplift history without the assumption of $n = 1$. For example, for rivers draining the Angolan dome, how might the results from the inverse modelling presented in this paper differ from those in Roberts & White (2010), JGR Solid Earth or Pritchard et al. (2009), GRL? Perhaps the analysis or comparison is beyond the scope of this paper, however some discussion might be warranted.

The analytical solution and inverse model requires that uplift is spatially uniform. The authors point out that "slope-break knickpoints are commonly associated with a step change in the tectonic uplift rate" (Line 106-107), but only in the context of a spatially uniform change in uplift rate. Given the assumption that we are looking at a very specific case where knickpoints are formed along a river channel in a tectonic setting where

changes in uplift are uniform throughout the whole length of the channel, the methodology presented in the paper is rather elegant. However, one can easily picture a scenario where a knickpoint is generated by a spatially varying uplift rate, such as those formed in rivers draining active fault systems. In such cases, the position of the slope-break knickpoint is not associated with a migrating knickpoint. Or at the very least it is a complex result of a migrating knickpoint as well as the spatial distribution of uplift rates. The scenario where whole catchments are affected by the uniform change in uplift rates is very unique in that this is unlikely to happen over very large spatial scales. I think this manuscript could use some discussion about the lengthscales over which such analysis is applicable. It is perhaps unreasonable to expect that changes in uplift rate are uniform in space on the lengthscales of 100s to 1000s of kilometers. In such cases, knickpoints are not expected to form at the coast and migrate inland, but rather be localized to where the uplift signal is inserted along the river. I am not arguing that merging of knickpoints due to $n \neq 1$ does not happen at such lengthscales, in fact they probably do. But given a requirement of the methodology is that the uplift is spatially uniform, it might be more adequate to include some discussion of the lengthscales over which it is applicable.

The inverse model presented is only applicable in the case where knickpoints have not yet merged. When looking at real rivers, that is an assumption that one has to make to be able to apply the inverse model. I don't see a problem with making such assumptions and inverting for an uplift history in this way. However, I wonder how these results are different from those using a linearized inversion (i.e., $n=1$). How are the uplift histories predicted from using the inverse modelling strategy presented here different if n is assumed to be 1? It is also not clear from the text or the figures whether the inversion requires an a priori determination of the value of n , or if the best-fit value of n is calculated as part of the inversion. I understand that the ratio of m/n is derived from the data for each river segment, but without any other information on the value of m , the value of n must be determined a priori. In this case, is there an objective way to determine the value of n in natural landscapes? Given a river longitudinal profile, how do you know what value of n to use? It appears that the example shown in Figure 5 assumes that $n = 2$, and it provides a good match to the applied $U(t)$ because we know that the profiles were known the n value used in the forward model. However, in natural landscapes, we do not know what the uplift history was, or what is the true value of n to use. Perhaps exploring what are the implications of using different values of n on the modelled uplift history.

Regarding the inverse modelling, I commend the authors in both their choice to add in noise to the data in order to demonstrate the applicability of the method, as well as their decision to invert for the number of division points in the data. Real data is noisy and discrete, and creating synthetic examples that also possesses these characteristics makes a better case for the applicability of the model. In the model, the rate of knickpoint migration is dependent on the slope and the ratio of adjacent slopes of the river profile in χ - z space. If this slope is poorly constrained (i.e. the data is noisy) this has major implications for the resulting uplift history (see Roberts et al., 2012, Tectonics supplementary information for a further discussion on the implications of differentiating discrete and noisy data). Some acknowledgement of these effects when working with real river data is warranted.

Line by line comments:

Line 11: Typo – “record” instead of “recorded”.

Line 26: Should be “divides the profile into reaches”.

Line 34: Typo – “mediated” instead of “mediates”.

Line 36: Should be millennial.

Line 38: (L/T) is used without specifying what the letters mean. I can only assume that they refer to the units being in dimensions of Length/Time. Maybe a clarification is needed?

Line 63: Rearrange for clarity – “ ‘stretched zones’ form along the river profile that are not [...]”

Line 80: “a simple and easy implement forward analytic model” – phrasing doesn’t make much sense. Implement a simple and easy forward analytical model? Simple and easy to implement forward analytical model?

Line 129: Equation (12) uses the terms U_f and U_i , which haven’t been introduced before. Perhaps it’s more intuitive to keep the equations in terms of U_1 and U_0 as in previous equations.

Line 154: Should be “depending on the kickpoints’ relative celerity” (add apostrophe after the s).

Line 222: Sentence starting with “To illustrate long-profile and knickpoint time evolution...” is a bit too long and convoluted. Perhaps some further description of what is shown in Figure 4 is warranted, together with a better description of how the modelling was set up.

Line 223: Typo - “consistency” instead of “consistence”.

Line 231: “The model infers the best fit $U(t)$ based on the long profiles of the tributaries

and basins." How is this achieved exactly? Are you minimizing the misfit between the observed and modelled river profiles? What is the form of the misfit function you are using? I think this is suggested later in the manuscript (Line 264)...

Line 250: "Linear regression is applied in the chi-z domain." This method is ok for an idealized dataset but becomes increasingly difficult for discrete and noisy data.

Line 504: Should be "final steady-state channel profile under uplift rate U_1 "

Line 505: A bit more description of figure 1(b) is required. What is the black dashed line, as it seems to have a negative slope?

Line 527: "Inverted uplift history" means something different to uplift history from the inverse model/inversion of river profiles.