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Comment on esurf-2021-101

Philippe Steer (Referee)

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-RC1>, 2022

The manuscript by Wang et al. describes a theoretical and modelling approach, based on analytical developments, to simulate the dynamics of river profiles under a non-linear stream power law. The paper is interesting, well written and proposes a significant development compared to the state of the art. The developed theory could be used in forward analytical landscape evolution models (e.g. Steer et al., 2021) or in inverse models (e.g. Goren et al., 2021), which are currently mainly restricted to the linear stream power model. However, the paper fails to provide a general answer to the issue of developing an analytical model for the non-linear stream power law by dismissing the case of stretched river segments, which appear for certain values of n and changes in uplift rate. This also possibly explains why the model has not been tested against natural settings, which limit the significance of the paper. Despite these comments (see my main comments below), I am truly convinced that this paper represents a timely and useful addition to the literature and will deserve to be published after some significant changes. I below list my main comments and some more minor comments.

Best regards,

Philippe Steer

Main comments:

1) The paper could gain some significance by applying the inverse model to a natural setting. This is somewhat lacking, in the current form of the manuscript, as the model suffers from several restrictions (U increases and $n > 1$ or U decreases and $n < 1$ – knickpoints should not have merged) which questions its applicability to natural settings.

2) The paper does not focus on the case of stretched river reaches (U increases and $n < 1$ or U decreases and $n > 1$). This clearly represents a main limitation of the paper, as the developed model can not be used in an inverse approach for rivers having experienced non-monotonic variations in uplift rate (which likely represent the vast majority of rivers worldwide). What are the methodological and theoretical barriers that prevent the authors to also develop a model for stretched river reaches? The paper would benefit from either developing a more general model (including the case of stretched river segments) or explaining why it is not doable in the framework of this paper.

3) The paper strongly focuses on knickpoint tracking and migration (including merging), while ignoring the recent experimental and theoretical works on knickpoint and waterfall dynamics (mainly by Scheingross and Baynes), including this paper (Scheingross, J. S., & Lamb, M. P.: A mechanistic model of waterfall plunge pool erosion into bedrock. *Journal of Geophysical Research: Earth Surface*, 122(11), 2079-2104, 2017.) I would like the current paper, despite a fully understandable simpler approach based on the SPIM, to discuss 1) how it could integrate a more mechanistic approach to knickpoint dynamics and 2) what are the limitations of the developed model with respect to the state of the art. The paper should also better address in the introduction the need for a non-linear SPIM. Indeed, if observations of the scaling of slope with erosion rates in steady-state part of rivers point towards a $n \sim 2$, observations of transient features such as knickpoint retreat mostly point towards a linear SPIM, (e.g. Lague et al., 2014).

4) Discussion and conclusion: this is the weaker part of the paper as the discussion remains rather superficial and does not mention the limitations of the approach, its applicability to natural settings, or the fidelity of the model to knickpoint dynamics ... (see previous comments). I fully understand this is a "short communication" format, but in its current form, the paper fails to really demonstrate how this new model could be of broad use for the geomorphology community.

5) Shape of the paper: I found the figures of the paper were generally not of the highest standards in terms of clarity and quality. Figures 2 and 3 for instance use some symbols while it is simply representing results of equation 16. It is therefore probably recommended to use some plain lines. The legend of Figure 4 a should be in the caption (except maybe for the uplift history). The equations (starting from section 4) could be made easier to exploit for other numerical models by using general indices such as i and $i+1$ instead of the 1 and 2 indices. Some references were lacking or not appropriate.

Minor comments and edits (no need for replies):

Line 25: replace "equilibration" by "equilibrium" or "dynamic steady-state" (which I think is the "reference" formulation)

Line 25: the profil "to" reaches – change "to" by "in"

Line 37: the appropriate references are probably: Howard and Kerby, 1983 [and not 1989]; Howard, 1994; Whipple and Tucker, 1999; Lague, 2014; Venditti et al., 2019

Line 47: "Notably, the formulation of equation (2) represents many simplifications of the processes of river bedrock incision." No, equation (2) is simply a mass balance equation, it should be equation (1) that represents many simplifications.

Line 85: I guess there are some anterior references than Wobus et al. (2006) and Cyr et al. (2010) for the slope-area relationship in river.

Line 107: "step change in tectonic uplift rate" – the general case is the one of a change in the rate of base level variation.

Lines 107-108: I would replace "below" by "downstream"

Equations 14 and 19: Why not simplifying the k_{s_1} and $k_{s_1}^n$ that are located at the numerator and denominator?

Line 160: "T2_m" - I find this variable name for the merging time a bit confusing. Why not using Tm_1-2 to insist that it corresponds to the merging of KP1 and KP2?

Page 10: Maybe a figure showing a flowchart of the operations involved in the inverse modelling approach could help to clarify it.

Equation 29: I do not understand why this not simply $z_i = z_i + A(\text{rand}(1) - 0.5)$, with A the amplitude of noise. Moreover, a noise below 1 m is really low (most global DEMs have higher noise). I suggest having a figure in supplementary testing the sensitivity of the

inversion model to the amplitude A of noise, considering noise values for common DEM (SRTM, ASTER, ...).

Line 275: "Inversion in applied" – replace "in" by "is"