Dear Editor Albrecht Weerts,

Thank you for sending me "Calibrating 1D hydrodynamic river models in the absence of cross-sectional geometry: A new parameterization scheme" by Jiang et al for review. The paper is overall well written, and the figures and the language of the document are of high quality. When looking at the abstract, I was really enthusiastic, due to my interest for ungauged catchments. However, while reading the paper, my enthusiasm gradually ebbed away, as the approach seems neither novel, nor superior to existing ones. The authors should revise the paper, to convince the reader about the advantages of their approach. The 1D model calibration needs to be described more in detail. I had to guess what the authors actually did. Moving some of the material from the supplement into the manuscript would help the reader.

Kind regards

## Major

- Novelty
  The proposed method is not novel. Power laws (log linear relations) are just fit to the cross sectional area and conveyance. Furthermore, a fit to the conveyance can be shown to be algebraically identical to fitting the roughness coefficient:

$$A = a\, d^{\beta} \tag{1}$$

$$K = \gamma\, d^{\delta} = \frac{1}{n} A\, R^{2/3} \tag{2}$$

$$\text{with } R \approx d \tag{3}$$

$$K = c\, d^{\delta} = \frac{1}{n}\, A\, d^{2/3} \tag{4}$$

$$= c\, d^{\delta} = \frac{1}{n}\, a\, d^{\beta+2/3} \tag{5}$$

$$\Rightarrow n = \frac{a}{c}\, d^{\beta-\delta+2/3} \tag{6}$$

$$= \tilde{b}\, d^{\tilde{\delta}} \tag{7}$$

  Furthermore, Manning's relation for n resembles itself just is a power law to determine the Chezy coefficient C:

$$n = \frac{1}{C}\, d^{-1/6}$$

  This is the essence of *Manning et al.* (1890).

- Model and performance

  - The two power laws for A and K have together four coefficients. The authors introduce two more coefficients into A and claim that the extended model superior (line 137). However, for almost any model, introducing more parameters will improve the fit to the calibration data, but it will lead to overparametrization and worse model performance during prediction. The authors should demonstrate that the more complex model performs better. This could be done on hand of the Akaike or Bayesian information criterion, or maybe in a less mathematically robust calibration-validation approach.
  - I do not think that the model performs better, as A and K are still determined by the same log linear relations from d. Thus the model is ill conditioned without need, requiring strong regularization. I can think of other 6 parameter models which could potentially perform better, for example
  $$log(A) = c_0 + c_1 log(d) + c_2 \log(d)^2$$
  - As the first author (Jiang 2019) already published a previous study where the roughness coefficient was fit to the same river reach, it would be interesting to see a direct comparison.
  - Area-Depth relations can also be obtained from satellite images at different stages. How does the method compare in performance?

- Parameter choice
  The authors assume the slope, and thus the bed level, to be known (line 91). From my personal experience, in large ungauged lowland rivers, it is the bed level and thus the slope, which is often the largest factor of uncertainty and much more difficult to determine, even with field measurements. Roughness typically varies little between rivers (*Latrubesse*, 2008), while width can be much sensed remotely much easier than levels. Virtual gauging stations from satellites are typical 100-200 river km apart, and neighbouring virtual gauging stations are not passed simultaneously, introducing uncertainties in the slope estimates due to changes of the hydrograph between passages of the satellite. It would be good to get practical advice on how the slope can be determined for an ungauged river, and how this uncertainty compares to the uncertainty of the A-d and K-d relation.

- Model calibration (line 150ff)
  A lot of information essential to understanding is missing here. The points below are purely guesswork by me and should be explained in the manuscript:

  - There is probably a 1D dynamic wave mode for each river, with K and A determined from the flow depth by the power-laws. There are thus two models, the 1D hydrodynamic model (Mike 1D) and a model to predict K and A used by the hydrodynamic model, this should be clarified much earlier in the manuscript, at least when stating the SWE (eq 1,2)
  - Parameters seem to be defined locally for each reach, and to vary along and between rivers.
  - The Levenberg-Marquard is used, but how are the derivatives and the Jacobian calculated? If parameters are defined on reach base, then there will be 100ds of parameters to calibrate, requiring hundreds of hydrodynamic model-runs to alone compute the gradient during one optimization step. There should be a note on the computational effort.
  - What is the value of lambda?
  - $\emptyset$ seems to be the objective function, but it is not defined,

- Results and presentation
  Fits are presented on log-log plots (e.g. Figure 1). This visually emphasizes low-flows, which might be meaningful for drought analysis but it is not suitable for flood risk estimates. Some plots in linear space would thus be insightful.

- Discussion Limitations of the method and sources of error should be discussed, and they should be connection to the physical processes.

  - The model cannot reproduce the hysteresis, i.e. different d-K and d-A relations during the rising and the falling limb of the hydrograph, caused by the dynamic wave. This is in particular the case for low sloping lowland rivers (*Hidayat et al.*, 2011), and strongly sloping mountain reaches.
  - Beforms dynamics can likewise introduce hysteresis and non-uniqueness in the relation between roughness (conveyance) and discharge (depth) (*Cisneros et al.*, 2019).
  - Many large rivers are anastamosing (Irrawaddy, Amazon) and consist of compound channels, and a log-linear relationship will probably not perform well there. The yellow river in the dataset shows this behaviour as well.

## Minor

73 eq 1,2

- The authors state the shallow water equations (SWE), but the equations used later (3,4,6-10, figure-1) are based on a kinematic wave approach, otherwise the K-d, K-A relation would vary in time. This limitation should be mentioned here and later be addressed in the discussion.

86 "K is much more sensitive to A" → "the model is much more sensitive to K?"
Even with this clarification, the statement seems to be a fallacy, as according to eq 4 and 14, K co-varies with A.

77, 90 "unknowns" is too vague here.

  - Variables (A and Q) and parameters (n, $S_0$) should be distinguished
  - Furthermore the set of "unknowns" is not minimal, A can be expressed as a function of d, and $S_f$ as a function of Q, given the appropriate relations.

130 "valid at river reach scale instead of individual cross sections"
This is an interesting practical aspect, as the thalweg can vary along a single sharp bend much stronger than the surface elevation varies over hundreds of kilometres. Did the authors sample the area in straight reaches between bends to avoid perturbations due to scours in channel bends, or did they average continuous bathymetry along a river?

S1 This section should be moved the manuscript, to help the reader understand the field site better.

S2 What is $z$? Bed level? So after all, the bed level (slope) is a model parameter which is fit together with the other parameters. This is essential and should be mentioned in the manuscript.

S2 I think this section, or at least parts of it, should also go into the main manuscript, to help the reader understand the model calibration.

# Typos and suggestions

32 a limited → only a limited

55 there is not just spatial variation, but also temporal variation, c.f. comment on the discussion

85 Sf → $S_f$

149 starting models → starting points?

165 remove somewhat

165 The paper is short, why not moving the map and some other illustrations from the supporting information into the paper?

- Punctuation at the end of equations is missing (, and .).

## References

Cisneros, J., J. Best, T. van Dijk, and E. Mosselman, Dune morphology and hysteresis in alluvial channels during long-duration floods revealed using high temporal-resolution MBES bathymetry, in *Proceedings International Conference on Marine and River Dune Dynamics–MARID VI*, pp. 1–3, 2019.

Hidayat, H., B. Vermeulen, M. G. Sassi, P. J. J. F. Torfs, and A. J. F. Hoitink, Discharge estimation in a backwater affected meandering river, *Hydrology and Earth System Sciences*, *15*(8), 2717–2728, 2011.

Latrubesse, E. M., Patterns of anabranching channels: The ultimate end-member adjustment of mega rivers, *Geomorphology*, *101*(1-2), 130–145, 2008.

Manning, R., J. P. Griffith, T. Pigot, and L. F. Vernon-Harcourt, *On the flow of water in open channels and pipes*, 1890.