We thank the reviewer for spending time and effort reviewing our work. Below the reviewer's comments are in black and the replies in blue.

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:

$$A = ad^{\beta} \quad (1)$$

$$K = cd^{\delta} \quad (2)$$

$$\text{with } R \approx d \quad (3)$$

$$K = cd^{\delta} = \frac{1}{n} A d^{2/3} \quad (4)$$

$$= cd^{\delta} = \frac{1}{n} a d^{\beta + 2/3} \quad (5)$$

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

$$= \tilde{b} d^{\tilde{\delta}} \quad (7)$$

  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).*

  Reply: we partially agree with the statement.

  Firstly, we should point out that $R \approx d$ is an approximation that is valid for wide rectangular channels and does not necessarily hold for more complex cross-sectional shapes. So, in the general case, conveyance integrates both Manning roughness and cross-sectional shape parameters.

  We agree that the power-law relationships between flow area / conveyance and flow depth are not new. The novelty is to use these relationships in a hydraulic inversion, i.e. estimating the parameters of these relationships by fitting simulated water surface elevations/ river widths to observed water surface elevations/ river widths from satellite data.

  Previously, most studies focusing on hydraulic parameter estimation using satellite water surface elevation data, if not all, have used surveyed bathymetry data or parameterized the geometry of channels to calculate flow area and conveyance. This new parameterization bypasses this difficulty and thus does not require the assumption of any specific cross-sectional shape. Therefore, this approach is new and fundamentally different from previous studies. This has been clearly described in the Introduction section.

- 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 overparameterization 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.

Reply: The reason to introduce four coefficients, i.e. p1 ~ p4, is to reduce the number of parameters to be calibrated. Originally, as shown in Eq. (9) and Eq. (10), there are four parameters, i.e. α, β, γ, δ, which are varying with chainage (i.e. river coordinate). That is, there are totally 4*XS parameters (XS is the number of cross sections). Because of the correlation between α and γ (also β and δ) at reach scale, four spatially constant coefficients can be introduced. Therefore, the number of parameters is reduced to (2*XS+4) as shown in Eq. (13) and Eq. (14).

→ 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

$$log(A) = c_0 + c_1 log(d) + c_2 log(d)^2$$

Reply: As explained above, the introduction of four coefficients is simply to reduce the number of free calibration parameters. This improves the stability of the hydraulic inverse problem.

→ 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.

Reply: Yes, we have performed a comparison of model simulations. The simulation accuracy using this approach is similar to and even better than that achieved using a different approach shown in Jiang 2019. This has been described in lines 200 – 205.

→ Area-Depth relations can also be obtained from satellite images at different stages. How does the method compare in performance?

Reply: Please note that we used both river width and water surface elevation data in the inverse problem. We have conducted an experiment to investigate the power of different data sets in constraining model parameters. Two methods (river width only and WSE only) show similar performance in terms of RMSE of WSE at two gauging stations as shown in Figure S7 in supporting information. However, if a channel is embanked, for instance, model parameters may not be sensitive to the small changes of river width. Comparatively, WSE would be more informative in this case. This has been discussed at the beginning of the Discussion section.

- 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.

Reply: The slope is not assumed to be known. For each cross section, there is a parameter Z_0, representing the local datum, i.e. river bed elevation. Bed slope is then determined from the difference of adjacent bottom elevations.

Regarding the distance of virtual stations, yes, it is about 80 km for Envisat while 315 km for Jason series. But, it is about 7 km for CryoSat-2 data. Therefore, we can obtain more information from densely distributed CryoSat-2 derived WSE data. The advantages of this kind of altimetry datasets have been reported by several studies (Domeneghetti et al., 2021; Jiang et al., 2019; Schneider et al., 2018).

- 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)

Reply: During the calibration, the second model you refer to is an integral part of the Mike 1D hydraulic model. That is, the new relationships d-A and d-K, which change in each ieteration of the inversion, are updated in the Mike 1D model. Therefore, we have only one model. This was described in the supporting information in detail.

→ Parameters seem to be defined locally for each reach, and to vary along and between rivers.

Reply: Correct, Z_0, γ and δ are spatially varying, but p_1, p_2, p_3, and p_4 are constant. This is explicitly described in line 151.

→ 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.

Reply: Normally, in each iterative step, the Jacobian of the objective function is calculated or approximated using finite difference. However, computing the Jacobian in every iteration is computational expensive and in some cases the Jacobian does not change and thus, evaluation of the Jacobian can be unnecessarily costly. Instead, Broyden's rank-one update of the Jacobian (Broyden, 1965) is more efficient (Madsen et al., 2004). We use the Immoptibox toolbox (Nielsen and Völcker, 2010) to optimize the objective function.

The number of model runs is around 200. The time consumed for this optimization is about a few hours (1 ~ 4 hours). The calibrations were conducted on a windows server 2016 (Inter® Xeon® Gold 6154 CPU @ 3 GHz, 2993 Mhz) using 4 cores.

→ What is the value of lambda?

Reply: The regularization parameters, i.e. λ_γ, λ_δ, λ_{p1}, λ_{p2}, λ_{p3}, λ_{p4} are empirically set as 0.1, 0.1, 0.15, 0.15, 0.15, 0.15, respectively, by trial and error.

A detailed description is given as Text S2 in the supporting information.

→ Φ seems to be the objective function, but it is not defined.

Reply: Correct, that is Eq. (15). We will define it in the revision.

- 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 food risk estimates. Some plots in linear space would thus be insightful.

  Reply: Yes, but the purpose of this figure is to demonstrate the linear relationship. We will also add a linear scale plot to the supporting information.

- 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.

  Reply: We are not clear about what type of hysteresis the reviewer refers to. Hysteresis in rating curves does not occur because of d-K and d-A relationships but because of the pressure terms in the Saint-Venant equations, which contribute differently in the ascending and descending branches of flood waves. The Mike 1D hydraulic model uses dynamic wave model, thus, is able to simulate hysteresis. It should be noted that we did not directly calculate flow by multiplying conveyance (K) with bed slope (S_0).

  → Bedforms dynamics can likewise introduce hysteresis and non-uniqueness in the relation between roughness (conveyance) and discharge (depth) (Cisneros et al., 2019).

  Reply: Yes, bedform changes will lead to time variable d-A and d-K curves, which our approach does not allow. However, there are many situations where stable bed is a good approximation. We will acknowledge this point in the revision.

  → 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.

  Reply: Yes, one linear regression may be not able to describe both the low flow and high flow for certain rivers, especially those with a significant floodplain. We have reported this issue at section 2.2 (lines 110-115) when introducing this approach.

Minor

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.

Reply: Thank you for pointing this out. We will add this in the method section and mention this 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.

Reply: K is a function of both roughness and area. Even when the flow area is constant, the conveyance can be different depending on the roughness.

The statement is valid for kinematic wave model and diffusive wave model specially. The momentum equation is simply as below when it is in steady state:

$$\underbrace{gA(d)\frac{\partial d}{\partial x} - \underbrace{gA(d)\left(S_0 - \frac{Q^2}{K^2(d)}\right)}_{Kinematic\ wave}}_{Diffusive\ wave} = 0$$

Therefore, A(d) can be eliminated from the equation. In this way, it will not affect the momentum equation.

77, 90 "unknowns" is too vague here.

→ Variables (A and Q) and parameters (n, S0) should be distinguished.

→ Furthermore, the set of "unknowns" is not minimal, A can be expressed as a function of d, and Sf as a function of Q, given the appropriate relations.

Reply: We will revise it accordingly.

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?

Reply: We did not intentionally avoid any bends. The cross sections are evenly distributed at an interval of ~20 km along the centerline of main channel.

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

Reply: We will revise it accordingly.

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.

Reply: Correct, as explained in previous reply, we did not assume the slope but treat it as a parameter. We will revise it.

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.

Reply: We will revise it accordingly.

Typos and suggestions

32 a limited → only a limited

Reply: We will revise it in the revision.

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

Reply: We will mention this point in the discussion.

85 Sf → S_f

Reply: We will revise it accordingly.

149 starting models → starting points?

Reply: Yes, we will revise it.

165 remove somewhat

Reply: Agree.

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 .).

Reply: Thank you. We will revise it accordingly.

References

Broyden, C. G.: A class of methods for solving nonlinear simultaneous equations, Math. Comput., 19(92), 577–577, doi:10.1090/S0025-5718-1965-0198670-6, 1965.

Domeneghetti, A., Molari, G., Tourian, M. J., Tarpanelli, A., Behnia, S., Moramarco, T., Sneeuw, N. and Brath, A.: Testing the use of single- and multi-mission satellite altimetry for the calibration of hydraulic models, Adv. Water Resour., 151, 103887, doi:10.1016/j.advwatres.2021.103887, 2021.

Jiang, L., Madsen, H. and Bauer-Gottwein, P.: Simultaneous calibration of multiple hydrodynamic model parameters using satellite altimetry observations of water surface elevation in the Songhua River, Remote Sens. Environ., 225, 229–247, doi:10.1016/j.rse.2019.03.014, 2019.

Madsen, K., Nielsen, H. B. and Tingleff, O.: Methods for Non-Linear Least Squares Problems (2nd ed.)., 2004.

Nielsen, H. B. and Völcker, C.: IMMOPTIBOX: A Matlab Toolbox for Optimization and Data Fitting, 2010.

Schneider, R., Tarpanelli, A., Nielsen, K., Madsen, H. and Bauer-Gottwein, P.: Evaluation of multi-mode CryoSat-2 altimetry data over the Po River against in situ data and a hydrodynamic model, Adv. Water Resour., 112(August 2017), 17–26, doi:10.1016/j.advwatres.2017.11.027, 2018.