Dear Prof. Yamazaki,

Thank you for reviewing our work and providing useful comments. In the following we will briefly reply (in blue) to your comments and suggestions. A more detailed reply will be provided along with the revision of our manuscript.

[General Comments]

This manuscript proposes a new concept to calibrate the channel parameters in hydrodynamic models, focusing on flow area and conveyance. The proposed method has a potential to be widely used, as it has less parameters to be calibrated compared to previous approaches, and the calibration works with satellite measurements.

However, I feel the manuscript requires substantial edits before acceptance. Especially, it describes the new methods very well, while its usefulness cannot be assessed because the new method is not compared by previous methods. I suggest to include "comparison to previous method" in the manuscript, to highlight the validity of the proposed method.

Related to above point, the current abstract only describes the idea on using flow-area and conveyance instead of the explicit channel cross-section and roughness parameters, and it does not contain any explanation on the result of experiments to test the applicability of the idea. The abstract should contain the summary of the "case study" part (and also some comparison to previous method). Without the explanation of the case study results, readers cannot guess whether the proposed idea is valid/useful or not. Please consider how to organize the abstract.

Also, I assume this manuscript was once prepared as a letter. I feel the information in the main text was a bit limited, and some model description can be added (or moved from Supplement), as HESS is the full paper journal. A more detailed explanation in the main text must improve the readability of the manuscript.

Reply: Thank you for the positive comments. We will add a more detailed comparison of the results to those obtained using previous method, and expand the abstract according to your suggestions.

Agree, we will also expand the main text by moving some details from the supplement.

[Specific Comments]

P1.L14: > However, strong correlations appear between cross-section shape parameters and hydraulic roughness in a hydraulic inversion approach.

What authors want to state from this sentence is not very clear. A bit more well-organized explanation is needed. I guess the authors intended to say cross-section and roughness are calibrated independently in previous studies, while they propose a new method which can handle them in a combined manner. I think the explanation can be improved by modifying this sentence together with the previous sentence.

Reply: Simultaneous parameterization of channel geometry and roughness is very difficult, and often there are some trade-offs between them. Instead of calibrating two sets of parameters, we propose a new method that combine them into one parameter, i.e. conveyance. Yes, we will revise this sentence to make it clear.

P1.L14: > That reduces ambiguity

I wonder whether "reducing ambiguity" is a proper word to describe the authors intention or not. I feel the proposed method will "allow some ambiguity" by abiding explicit channel-shape representation, while providing reasonable approximations by using flow area and conveyance (which are more conceptual/ambiguous compared to the cross section shape and roughness).

Reply: Our new approach does not need to consider the cross-sectional shape and roughness anymore. In this sense, we do not have ambiguity issue. To clear up this doubt, we will rephrase this sentence.

P1. L17:

> thus are assumed to be linearly related

The logic is not clear here. Even though the flow area and conveyance can be expressed by power-low functions at each cross-section, there is no reason that they can be linearly related at reach scale. The linear relationship is confirmed by the observations (and not derived from the analysis of the equations), I think "thus are assumed to be" is not logically explained.

Reply: Yes, agree with you. We will revise these sentences accordingly.

P1. L18:

> Data from a wide range of river systems show that the linearity approximation is globally applicable

The relationship is confirmed in 6 rivers, but I cannot get any analysis to support the statement that the similar relationship can be found in other rivers in the globe. I think more data analysis should be added to support this statement.

Alternatively, the authors may discuss why the linear relationship can be found along the river reach. I feel this relationship might have some similarity to AMHG approach (which assume along-reach relationship in cross-section parameters), and they analyzed why and when the relationship can be found by mathematical analysis (Brinkerhoff et al., 2019). I guess this is beyond the scope of this manuscript, but at least the authors should include some discussion on potential reasons on why linear relationship along reach is found.

Reply: Agree. Considering the difficulties to collect surveyed cross-sectional data sets, we will add some discussions and call for validations.

P2.L57:

> strong parameter correlation appears between cross section shape (wetted perimeter) and hydraulic roughness

It's better to provide some reference related to this statement. I'm not sure whether this is widely known/accepted by hydrology community.

Reply: We had calculated the correlation coefficients of roughness and river width and showed strong correlations in our previous study. We will add the reference.

P2.L63

> this ambiguity in hydraulic inverse problems

Please think about another word to replace "ambiguity" as explained above.

Reply: Will rephrase this sentence.

P5.L25:

> Due to the linear nature of logarithmic pairs of (A, d) and (K, d),

I cannot fully get the logic behind this statement. Why at-a-station parameters can be correlated as at-many-station parameters (i.e. reach scale relationship)? Is there any mathematical or geographical background reason to support this derivation?

Reply: Take the linear relationships of (A, d) and (K, d) as the start,

$$log\,A(x,t) = \alpha(x) + \beta(x)\;log\,d(x,t) \tag{9}$$
$$log\,K(x,t) = \gamma(x) + \delta(x)\;log\,d(x,t) \tag{10}$$

Substituting log *d* in Eq (9), one can get

$$\log A = \alpha + \frac{\beta}{\delta}(\log K - \gamma) \tag{R1}$$

By rearranging Eq (R1), we have

$$\alpha = \log A - \frac{\beta}{\delta}\log K + \frac{\beta}{\delta}\gamma \tag{R2}$$

Similarly, we divide Eq (6) by Eq (7), we can obtain

$$\frac{A}{K} = \frac{a}{c}d^{\beta-\delta} \tag{R3}$$

Taking log transformation and rearranging the equation, that leaves

$$\beta = \frac{\log A - \log K + \gamma - \alpha}{\log d} + \delta \tag{R4}$$

The above derivations (Eqs R2 and R4) are for each individual cross sections. However, because on the right side both equations have A and K that are dependent on water level, the intercept of linear equations (R2) and (R4) are not just constant.

One should not confuse Eq (R2) and Eq (R4) with Eq (11) and Eq (12) that are

$$\alpha = p_1 + p_2\gamma \tag{11}$$

$$\beta = p_3 + p_4\delta \tag{12}$$

These two equations are derived by fitting a linear function to observed data at the reach scale as shown in Figure 2. Because we see it in our dataset, we then use it to simplify the hydraulic inverse problem by tying some of the parameters together, i.e. reducing the number of fitting parameters. Due to the spatial variation of α, γ, β and δ, we have to constrain 4*N of parameters (N is the number of cross sections). By using equations 11 and 12, we can halve the number of parameters.

We will expand this section to clear up the doubts.

P7.L177:

> the residuals between model predictions and observations (i.e. water level and width);

It is not clear how the width is calculated in the model (as the model now only has flow-area without explicit width representations). How width and flow area can be related? Please explain in a detailed manner.

Reply: In the model, the width is calculated as the derivative of flow area and depth. Will add this in the model setup section.

P7.L163

> Case study

I think it is better to explain the method (experimental setting) of the case study in the method section, and explain in a more detailed manner by moving descriptions from Supplement to main text. As HESS is a full paper journal, it is obviously better to write the minimum explanations on the experiment settings to follow the results.

Reply: Will expand this section by moving materials from the supplement.

P8.L182

> Figure 3

Please inform that the "km" indicates the distance from the upstream boundary. It is not clear which is

upstream/downstream from the current explanations.

Reply: Thank you for pointing this out. Will explain it in the caption.

P8.L186:

"Figure 4" should be "Figure 3".

Reply: Yes, will revise it.

P8.L191:

> the best match with the observed cross sections.

It is not so obvious that the calibration #3 is the best match, as we can observe some exceptions in some reaches. Please provide some objective measure (e.g. RMSE of fitting) to support this statement.

Reply: Thank you. We will calculate the statistics.

P9.L192:

> compared to less dense one (one per 5 km, Fig. S8) although high-resolution imagery (30 m) can provide plenty of width observations.

This explanation appears suddenly, and it is difficult to follow. I suggest that the authors describe how width observations is prepared in the method section (i.e. how it was sampled), and make an analysis on the sampling sensitivity in the discussion section (not in the case study section) to improve the readability.

Reply: Agree, we had a text explaining the procedure for preparing width observations in the supplement. We will move the related materials to main text.

P9.L196:

We can see the proposed method worked with a certain accuracy, but it is difficult to guess how much it is useful compared to other/previous methods. Please include some comparison with previous methods (e.g. comparison to the method by Jiang et al. 2019). Can you say the result is improved? Or can you say the result does not differ so much even though the new calibration requires less parameter numbers?

Reply: Yes, the results are very similar, but this method does not need to assume any specific shape of the cross sections. We will further explain this and include more details.

P10.L201:

> the accuracy of simulation is acceptable and comparable to what was achieved using a different approach (Jiang et al., 2019).

Please explain

1) What was the characteristics of the different method? (If not explained, this sentence has no meaningful

explanation). I suppose the Jiang et al 2019 calibration was "explicit cross-section and roughness parameters for each section" and thus have "more parameter numbers to be calibrated".

Reply: The major difference is whether considers the cross-sectional shape or not. Our previous study assumed a certain shape (rectangle) while this one does not need to consider shape anymore.

We will make it clearer in the text.

2) Please provide some numbers for subjective comparisons. For example, how the RMSE values are different in these two methods? Without the subjective analysis, I cannot get how the authors concluded that "accuracy is acceptable and comparable".

Reply: Will added the RMSE values of both methods.

P11.L249:

> the relationship is generally independent of rivers

I think this is still the observation-based finding, and needs for some theoretical/geographical analysis should be discussed (in the discussion section).

Reply: We will discuss this point further.

Supplement:

Some contents are better to be moved to main text, such as Test S1, (a part of) Text S3 (except for sampling sensitivity experiments), Figure S5.

Reply: We will adopt your suggestions.