General Comment

This paper uses SWOT water surface elevation data and observed discharge data to calibrate Manning's coefficients in a 1D hydraulic model , with UAS bathymetric water penetrating radar data used to obtain river depths for the hydraulic modelling. The 1D hydraulic model is then used with a range of discharge values to estimate water surface elevations, from which a rating curve is generated. These rating curves are then used with additional SWOT water surface elevation data to estimate discharge. Overall, I found this to be a useful paper, employing data from several newly available technologies for river modelling. However, as outlined below, in my opinion the authors have oversold their claim that they have developed a method to estimate discharge in ungauged basins. I also had a few other comments that may help further strengthen the paper. Accordingly, I suggest the paper be returned to the authors for major revisions.

Response:

Thank you for the positive and constructive assessment of our manuscript. We appreciate your recognition of the value of combining SWOT, UAS bathymetric WPR, and hydraulic modelling for river discharge estimation.

We agree that our original wording regarding discharge estimation in "ungauged basins" was too broad. Our approach still requires some intermittent discharge observations at variable discharge levels for calibrating the hydraulic model and Manning's roughness coefficients. Therefore, it should not be interpreted as a method that can estimate discharge in a completely ungauged basin without any prior discharge information. In our paper, we used discharge from an in-situ station because it was available, but in the absence of in-situ discharge occasional UAS discharge surveys (https://doi.org/10.22541/essoar.175794211.10690810/v1) or hydrological models can be used. River managers in ungauged or poorly instrumented regions can make a one-off investment in a UAS hydrometry survey, build a hydraulic model of selected reaches and then regularly obtain high-accuracy discharge estimates from SWOT (and other altimetry missions) overpasses.

Following your suggestion, we will revise the manuscript to clarify the scope and applicability of the proposed approach. In particular, we will replace or qualify statements referring to "ungauged basins" and now describe the method as a framework for discharge estimation in rivers with limited conventional gauging observations, where SWOT WSE observations and UAS-derived bathymetry can be used together with hydraulic modelling to construct and apply a rating curve. We will revise the relevant statements in the Abstract, Introduction, Results, Discussion, and Conclusions to avoid overstating the contribution and to clearly acknowledge the requirement for discharge observations during model calibration.

We thank the reviewer for pointing this out, as this clarification has helped us present the contribution and limitations of the study more accurately.

Specific Comments

L62 I would appreciate citations here for all (any?) previous studies that have used UAV-WPR for river bathymetry. In my opinion this is an interesting and possibly unique aspect of the present paper, and the novelty of this approach should be identified by full review of previous literature. Similarly, it would be informative and helpful to review in detail previous papers describing UAS-greenLIDAR for bathymetry.

Response:

Thank you for this helpful comment. We agree that a more comprehensive review of previous UAS-based WPR and green LiDAR studies is needed to better position the novelty of our work.

Following your suggestion, we will expand the literature review to include previous studies using UAS-WPR for river bathymetry, as well as studies using UAS green LiDAR for underwater and river-channel mapping. We now discuss their applications and reported performance in greater detail. These revisions will be made in Section 1 Introduction, where the relevant previous studies and the distinction between existing UAS bathymetric techniques and the contribution of our study are now discussed.

'Bandini et al. (2023) demonstrated UAS-borne WPR for freshwater bathymetry, Zhou et al. (2025a) extended WPR-based bathymetry to integrated river hydrometry, and Eskandari et al. (2026) further advanced radar-based bathymetry through a UAS-based software-defined radar system. Meanwhile, Kinzel et al. (2021) demonstrated the potential and environmental constraints of compact UAS-deployable topo-bathymetric LiDAR, Islam et al. (2022) demonstrated the capability of UAS-borne green LiDAR to characterize vegetated rivers and map continuous terrestrial and shallow submerged topography across seasons, and Himmelsbach et al. (2025) demonstrated its application to resolving detailed submerged morphology in complex mountain rivers.'

L70 I appreciate that the intent of the methodology is to be applicable in ungauged basins. In Figure 1d it is not clear how discharge is selected during calibration, but L68 and L118 indicate this was the measured discharge at the time of the SWOT-observed water surface elevation. This begs the question: what would be the accuracy of eventual discharge estimation without the gauge station data? An answer to this question would be necessary to support the claim within the paper title that the methodology can produce discharge in ungauged basins. Without this, then I think the paper title, objectives and conclusions should be rephrased. Ideally, the authors would test their approach by modelling discharge (without use of the reference discharge for hydrological model calibration) and then test the accuracy of the eventual discharge estimation. However, the authors did not test if the model could be calibrated without the reference gauge station discharge data. Instead, the authors apply a Monte Carlo analysis (Section 4.5, L412) to test the outcome if random error is added to the measured discharge (with 30% error in discharge assumed for hydrological modelling). They argue (Line 430, Figure 11d) that the central tendency of the resulting confidence interval indicates that the method is applicable to ungauged basins. However, in my opinion, the central tendency is entirely the result of the uncertainty analysis employed, since only random error without bias was introduced. It is challenging to calibrate a hydrological model without bias in the absence of reference discharge data for calibration, and any bias in the hydrological model estimates of discharge used for calibration of their method would eventually propagate to their discharge estimation. I suggest that the large width of the confidence band shown in Figure 11d for a 30% relative error provides a better estimation of the outcome of a 30% bias in modelled discharge used for method calibration than the central tendency. This issue should be discussed thoroughly.

Response:

We thank the reviewer for this important and constructive comment. We agree that our original interpretation of the Monte Carlo results was too strong. In the original analysis, zero-mean correlated Gaussian errors were introduced into the discharge observations used for model calibration, with relative standard deviations of 5%, 15%, and 30%, respectively, and a correlation coefficient of $\rho = 0.5$among the discharge errors. The zero-mean assumption ensures that the perturbations do not introduce a prescribed systematic bias across the **full Monte Carlo ensemble**. However, because the perturbations are correlated, individual Monte Carlo realizations can exhibit coherent systematic shifts, with discharge observations across multiple scenarios being simultaneously higher or lower than their nominal values. This correlation allows the uncertainty analysis to account for plausible common-mode errors among the discharge observations, which can affect the calibrated rating-curve parameters and the resulting discharge estimates.

Following the reviewer's suggestion, we will revise the interpretation of Figure 11d and the related discussion. We will focus on the width of the uncertainty band rather than its central tendency. The widening of the uncertainty band with increasing discharge error will be interpreted as an indication of the sensitivity of the final discharge estimates to uncertainty in the discharge information used for calibration. We will also clarify that systematic bias in the calibration discharge is not explicitly represented by our zero-mean perturbation scheme, but could propagate through the calibration process and further affect the estimated discharge.

Accordingly, we will moderate our original statement regarding application to ungauged basins. The Monte Carlo analysis will be presented as a sensitivity assessment of the proposed framework to uncertainty and correlation in the discharge information used for calibration, rather than as direct evidence that reliable calibration can be achieved in the absence of discharge observations. The corresponding statements in Section 4.5, the discussion of Figure 11, and the Conclusions will be revised accordingly.

In addition, we found that the version of Figure 11d included in the previous manuscript submission was incorrect. This will be corrected, and the updated Figure 11d will be included in the revised manuscript.

L123 Similarly, SWOT elevations are bias corrected with gauge water levels, and there is a statement that for ungauged stations overlapping SWOT elevations could be used for bias correction. I can see how overlapping SWOT elevations could be used to reduce inconsistencies, but I do not see how this is equivalent to absolute bias correction.

Response:

We thank the reviewer for pointing this out. We agree that our original wording at Line 123 was not sufficiently clear and could imply that overlapping SWOT elevations alone were used for absolute bias correction at ungauged locations.

Our actual procedure does not rely on SWOT elevations alone for absolute bias correction. On days when SWOT observations are available, independent WSE measurements are collected using the UAS-based radar at the same selected reference locations. These UAS radar-derived WSEs provide the absolute reference for estimating the bias of the corresponding SWOT observations. The estimated absolute bias is then applied to SWOT observations from nearby dates, allowing the bias-corrected SWOT WSEs to be combined for subsequent analysis.

We will therefore revise the statement at Line 123 to clarify that in the updated version.

'Similarly, independent UAS radar-derived WSEs acquired during SWOT overpasses can provide an absolute reference for correcting SWOT WSE bias, even in the absence of conventional gauge observations, with the estimated bias subsequently applied to nearby SWOT observations.'

L173 I also would appreciate greater clarity as to which reaches were calibrated with discharge data measured at gauges versus obtained by hydrological model predictions.

Response:

We thank the reviewer for pointing this out. We agree that the original wording could cause ambiguity regarding the source of the discharge data used for calibration. In the present study, the discharge data used for calibration were obtained from gauge measurements; we did not use hydrological model predictions as calibration discharge for the reaches investigated in this manuscript.

Our original statement referring to "model-based estimates in ungauged reaches" was intended to describe a potential extension of the proposed framework to ungauged reaches, where model-predicted discharge could potentially be used as the reference information for calibration. However, this scenario was not actually tested in the present study. To avoid implying that such an approach was evaluated, we will remove this statement from the manuscript and clarify the source of the discharge data used for calibration.

'Simulations were conducted using upstream inflow conditions derived from in-situ gauges where available. For the reach investigated in this study, the upstream inflow discharge was obtained from gauge observations, and no hydrological model-derived discharge estimates were used.'

Figure 1. It may just be my pdf, but the resolution of Figure 1a is poor, so the scale bar is not legible. This makes it difficult for me to judge the study site river width. Can the resolution be improved? Also, please state the mean river width in the text Section 2.

Response:

We thank the reviewer for pointing this out. We will improve the resolution of Figure 1a to ensure that the scale bar and other map details are clearly legible in the revised manuscript.

Following the reviewer's suggestion, we will also add the mean river width of the study reaches in Section 2 to provide a clearer description of the measured XSs of the study sites.

'Between 3–9 September 2024, a total of 23 XSs were surveyed across upstream and downstream segments of the study area (Fig. 1a, b), where the mean river width across the investigated reaches ranged from approximately 135.7 to 524.9 m.'

L123 I appreciate the citation to Liang et al. (2025) for the point cloud filtering. For the benefit of the reader, please briefly describe how this filter works. Also, please clarify if all the red points in Figure 2 are included in and thus "selected" from the raw data set, or rather they are derived values obtained during the filtering process.

Response:

We will update that using

'SWOT WSE observations from different dates were first bias-corrected and then combined at the selected locations. Following Liang et al. (2025), the bias-corrected SWOT WSE observations from different dates were combined and filtered based on their WSE density distribution. Upper and lower boundaries were defined around the main high-density region to identify and remove outlying observations. The retained red points in Fig. 2 are directly selected from the original SWOT WSE observations rather than newly generated or interpolated points. Finally, WSE values from SWOT_SP and SWOT_Pixc at common chainages were averaged (Fig. 3a).'

Table 1. Please add a column with the actual gauge discharge during each SWOT measurement. This would clarify the range of discharges included within each discharge class. For example, what were the actual discharges for each day included in the Q=300 m^3/s discharge class?

Response:

We thank the reviewer for this helpful suggestion. We agree that reporting the actual gauge discharge corresponding to each SWOT observation provides a clearer description of the discharge conditions represented by each discharge class. We will therefore add a new column to Table 1 showing the gauge discharge measured at the time of each SWOT observation.

Table 1. How were the discharge classes selected?

Response:

We thank the reviewer for this question. The discharge classes were defined specifically for this study to represent the range of flow conditions sampled during the SWOT overpasses. Based on the actual gauge discharge observed around the SWOT observation times, the measurements were manually grouped into four classes: low, medium, medium-high, and high discharge. The class boundaries were selected to provide a practical and representative categorization of the observed discharge conditions rather than being based on a predefined hydrological classification scheme. We will clarify this procedure in the revised manuscript and add the actual gauge discharge values to Table 1.

'The selected 18-day SWOT observations were grouped into four discharge classes—low, medium, medium-high, and high—based on the actual gauge discharge recorded at the Pello gauging station around the corresponding SWOT overpass times (Table 1). These classes were manually defined to represent the range of flow conditions sampled during the SWOT overpasses and were intended for comparative purposes rather than as predefined hydrological categories.'

Figure 4a. Which WPR return was deemed to be the river bed? Please explain how this image was assessed to determine bathymetric depths.

Response:

Thank you for pointing this out. In Fig. 4a, the riverbed was identified by manually tracing the coherent and laterally continuous reflection corresponding to the water–bed interface. The selected reflection follows a consistent spatial pattern along the transect, whereas deeper and less continuous reflections were considered to be multiples or subsurface responses and were excluded from the bathymetric interpretation. The two-way travel time of the picked reflection was converted to water depth using the adopted electromagnetic wave velocity in water. The resulting WPR-derived bathymetry is presented in Fig. 4b together with the independent in-situ observations. We will add the following sentence in the revised version:

'In the WPR radargram (Fig. 4a), the riverbed was identified as the coherent reflector beneath the water-surface return and traced continuously along the transect.'

Figure 4d, L306. The measured section (Figure 4d) is for a different section than the constructed section (Figures 4a-c), The text says Figure 4 is provided to "compare" the measured and virtual sections, which I think is confusing. Instead, of "We compare" I suggest the sentence should be "Figure 4 presents both an in-situ surveyed section (XS9, chainage 99226.4 m) and a virtual XS (chainage 82043 m) using the same bathymetry."

Response:

Thank you for pointing this out. We agree that the original wording could be misleading because the in-situ surveyed section and the virtual section are located at different chainages. We will revise the text accordingly:

'Figure 4 presents an in-situ surveyed XS (XS9, chainage 99226.4 m) and a virtual XS (chainage 82043 m) constructed using the same bathymetry.'

L310-314. Despite this explanation it is still not clear to me how the virtual sections were constructed. How exactly were the vertical and horizontal offsets selected to place the WPR-measured bathymetry from a different location fit within the DEM? In Figure 4d there are red lines that indicate modelled portions of the near-shore section at each bank, which indicates to there was subjectivity in the offset selection. Greater clarity/detail in the offset selection would be appreciated.

Response:

We thank the reviewer for pointing this out. We will clarify how the virtual sections were constructed.

'The WPR-derived bathymetry from the morphologically similar in-situ XS9 (see Table 2) is then horizontally shifted to align its center with the center of the target virtual XS, while the vertical offset is determined from the SWOT-derived WSE. For example, in Fig. 4d, the WPR-derived bathymetry is shifted by +100 m horizontally and +11.96 m vertically. Because the shifted WPR profile does not cover the entire target XS, the uncovered near-shore portions at both banks are reconstructed by fitting the available DEM elevation data using univariate spline interpolation, as indicated by the red lines in Fig. 4d. This procedure combines the measured bathymetric geometry from the morphologically similar XS with the satellite-constrained WSE and DEM-based near-shore topography, thereby enabling reconstruction of the complete virtual XS despite the limited direct bathymetric observations.'

L197. The water surface slope measured over ~15km of SWOT data (0.0683 cm/km) implies a water elevation difference of ~ 1 cm. Are SWOT WSE data sufficiently internally accurate to measure this elevation difference?

Response:

Thank you for raising this important point. The reviewer is correct that the ~1 cm elevation difference inferred from the SWOT-derived slope over ~15 km is small compared with the uncertainty of individual SWOT WSE observations. We therefore agree that this value should not be interpreted as a centimeter-level absolute measurement of water-surface elevation change.

We will revise the manuscript to clarify that:

'Note that the SWOT-derived slope is used as a reach-scale indication of the overall water-surface gradient, rather than as a precise measurement of the ~1 cm elevation difference.'

L210-219. I understand this section to mean that Manning's coefficient for a given location in a section is constant as discharge changes, but distinct Manning's coefficients are selected for near-bank zones as they become inundated as water level rises. In other words, each subsection corresponds to an increment in wetted width as discharge increases. Is this correct? If not, then please clarify this section. A conceptual figure showing the subsections across a section would be helpful.

Response:

Yes, this interpretation is correct. In our approach, each XS is divided into a series of subsections based on the spatial distribution of hydraulic roughness. The Manning's coefficient assigned to a given subsection remains constant as discharge changes. As the discharge and water level increase, the wetted width expands and progressively inundates adjacent subsections, which then contribute to the wetted XS with their predefined Manning's coefficients. Therefore, the effective hydraulic roughness of the entire XS can change with discharge because different combinations of subsections become wetted, even though the Manning's coefficient within each subsection remains constant.

We will add a conceptual figure (Fig. 5) illustrating how the subsections are distributed across a XS and progressively become wetted as the water level increases.

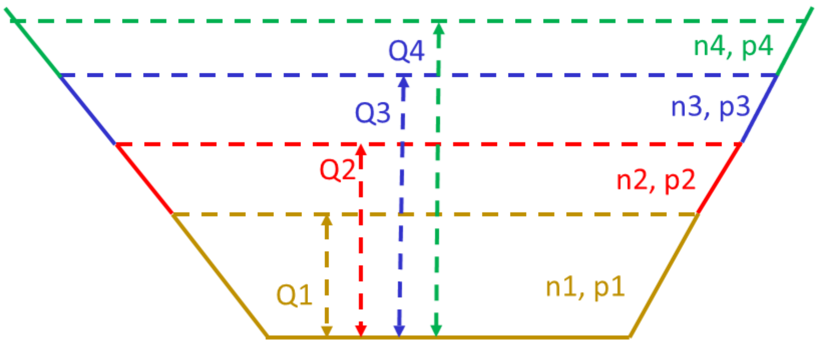

Figure 5: Conceptual illustration of cross-sectional subsections and progressive inundation.

Figure 11a-c. I do not understand Figures 11a-c. These are supposed to show 95% confidence intervals, but I see only single lines. Are these lines the mean value of the estimate or the confidence interval? Is the line thickness supposed to represent the confidence interval? If the reference discharge was varied (by 5%, 15%, 30%) randomly during Monte Carlo, then it is not surprising that the mean values of the estimates do not change. What matters is the standard deviation. What were the standard deviations of the estimates from the Monte Carlo runs? Please clarify.

Response:

Thank you for pointing this out. We agree that Figures 11a–c in the original manuscript did not clearly communicate the uncertainty associated with the Monte Carlo simulations. In the original figures, the solid lines represented the **mean WSE estimates**, rather than the confidence intervals, and the line thickness was not intended to represent uncertainty.

In the original analysis, the interval was calculated as $\mathrm{mean} \pm 1.96 \times \mathrm{SD}/\sqrt{N}$, where N is the number of Monte Carlo realizations. We recognize that this represents the confidence interval of the estimated mean rather than the variability of the Monte Carlo estimates themselves. Because each discharge level was evaluated using 300 Monte Carlo realizations, this formulation resulted in relatively narrow intervals, which made them difficult to distinguish from the mean curves in Figures 11a–c.

As the reviewer correctly noted, the reference discharge was randomly perturbed by ±5%, ±15%, and ±30% in the Monte Carlo simulations. Because these perturbations were applied around the reference discharge, the mean estimates are expected to remain close to the original estimates. The standard deviation and the spread of the Monte Carlo realizations therefore provide more relevant measures of the uncertainty introduced by the discharge perturbations.

We will consequently revise Figures 11a–c. The solid lines now represent the mean WSE estimates, while the shaded areas represent the **95% Monte Carlo uncertainty intervals**, defined by the 2.5th and 97.5th percentiles of the 300 Monte Carlo realizations. We will also calculate and report the standard deviations of the Monte Carlo WSE estimates. Across the selected cross-sections and discharge levels, the mean standard deviations are **0.1623 m, 0.4469 m, and 0.7627 m** for the ±5%, ±15%, and ±30% perturbation cases, respectively. These revisions clarify the distinction between the mean estimates and the uncertainty associated with the Monte Carlo simulations.

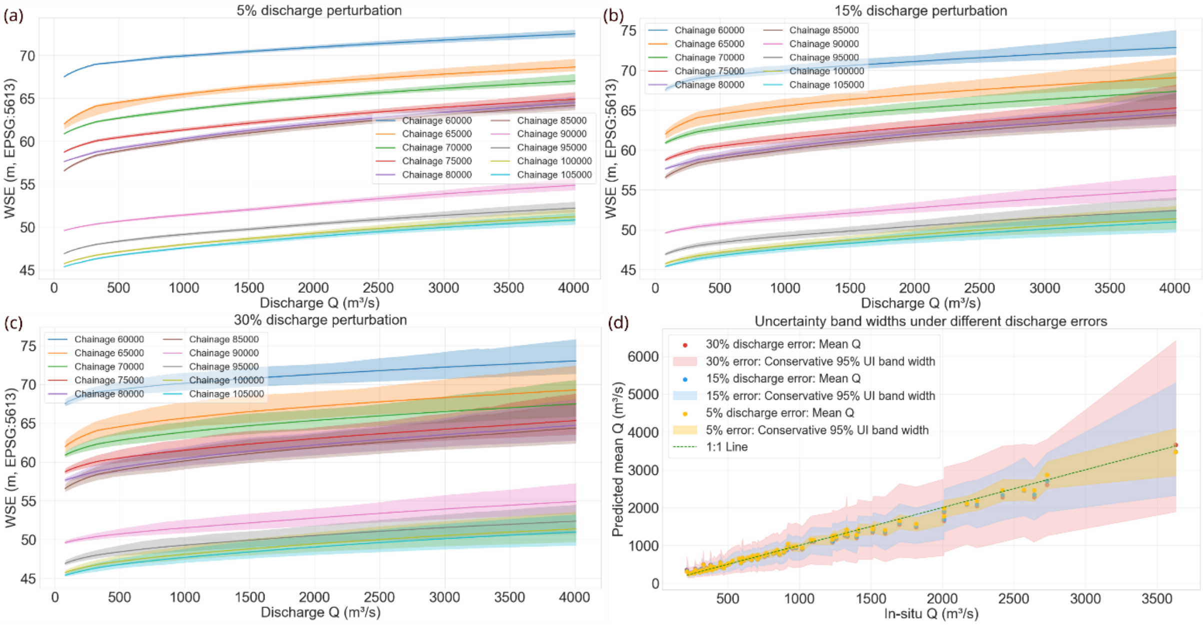

Figure 11: (a–c) Comparisons of the selected rating curves under relative discharge error levels of 5%, 15%, and 30%, respectively. (d) Comparison of the predicted mean discharge, derived from SWOT_SP observations acquired during the science orbit in combination with ICESat-2 WSE measurements, with in-situ discharge measurements under the three relative error scenarios. The predictions were obtained using Monte Carlo and Latin Hypercube Sampling (LHS) simulations. The dashed green line denotes the 1:1 reference line. The shaded areas represent the corresponding 95% Monte Carlo uncertainty intervals (UIs), defined by the 2.5th and 97.5th percentiles of the 300 Monte Carlo realizations for (a–c) and the 97 error realizations for (d).

Technical/editorial

L161 typo "triangles" not "triagles"

Response:

Thank you for pointing this out. We will update that.

Additional correction to Equation (4): During the revision, we re-examined the formulation and reference basis of Equation (4) and found that the original $1.5^{2/3}$term could not be traced to a clear and verifiable literature source. To ensure a clear theoretical basis and traceability, we have revised Equation (4) by replacing the $1.5^{2/3}$term with a first-power term, following a formulation with a clearly documented literature reference. **All results presented in the revised manuscript have been regenerated using the revised formulation. Although the updated calculations lead to some numerical differences from the original results, these differences are minor and do not materially affect the overall findings or conclusions of the study.** The corresponding equation, text, figures, and results have been updated accordingly in the revised manuscript.

$$n_{e,k} = \left(\frac{\sum_{i=1}^{k} p_i}{\sum_{i=1}^{k} \frac{p_i}{{n_i}^{1.5}}} \right)^{2/3} \qquad (4) \quad \text{(original version)}$$

$$n_{e,k} = \frac{\sum_{i=1}^{k} p_i}{\sum_{i}^{k} \frac{p_i}{n_i}} \qquad (4) \quad \text{(revised version)}$$