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Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimating river discharge from earth observation measurement of river surface hydraulic variables" *by* J. Negrel et al.

Anonymous Referee #2

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Review of manuscript "Estimating river discharge from earth observation measurement of river surface hydraulic variables" by J. Negrel, P. Kosuth, and N. Bercher submitted to HESSD

The authors present an approach to estimate river discharge solely based on earth observation data and compare the results to measured discharge data, hydraulically simulated discharges and other similar approaches, mainly the work of Bjerklie et al. The main idea is based on the shallow water equations (St.-Venant equations), which are comprised of a continuum and momentum equation. The authors simplify the equations, mainly the momentum equation, by making several limiting assumptions on the flow characteristics, which is acceptable and in fact often done in hydraulics. By this they ended up with the basic discharge calculation flow velocity * flow area and another one equal to the Gauckler-Manning-Strickler formula for uniform channel flow. In hydrodynamics the flow velocity in the continuum equation is unknown and computed by the momentum equation.

The innovative aspect of the work is then the approach to compute the discharge with the continuum and momentum equation separately with parameters directly derived from earth observation data, which includes surface flow velocity. Because not all parameters can be derived from remote sensing, i.e. flow depth and flow resistance (roughness), the authors propose a method to estimate these by minimizing the difference between the two discharge calculations. The results are then compared to measured and simulated discharges of the Amazon and evaluated for their accuracy. They show that at one station the approach yielded acceptable results, while poor at another station. The authors then discuss possible reasons for this error.

General comments

In principle, the presented approach is surely valid and genuine and could possibly present a step forward to a direct estimation of river discharge from earth observation data. However, as the approach and the manuscript in general are presented at the moment, it is not acceptable for publication for two main reasons:

1. The derivation of the method and the presentation of the results are substandard, because many important information are missing and some errors exist in the manuscript.

2. The discussion of possible error sources is lacking the discussion of the errors caused by the uncertainties of the remotely sensed hydraulic parameters and the parameter estimation methid. This is vital for the validation of the approach and

the identification of error sources.

Ad 1)

The rationale of the method as presented in section 3 should start from the fact, that the flow velocity can be estimated directly from earth observation. Then it becomes obvious the continuum and momentum can be calculated independently from earth observation. This is not entirely clear from the manuscript at a first glance. I would also recommend to describe the fundamental assumptions of river flow and the St.-Venant equations first and then introduce the rational of the approach and the limiting assumptions. The St.-Venant equations should also be cited in order to enable the reader to follow the derivation of the equations presented. Also, more effort should be made to clearly state which variables are derived from earth observation and which from ground based measurements.

Section 4 “results and discussion” should be structured more clearly. At the moment the reader is a bit at a loss, because the sections are not properly introduced and the aim of the sections becomes clear after repeated reading only. This is particular the case when the discussion on possible error sources start. Also, the comparison with the simulated discharges is hard to understand at all. The following points are not clear:

1. What is the actual purpose of the comparison?
2. What is actually compared? Discharges of a whole river stretch or at a single station?
3. What are the assumptions and boundaries/boundary conditions of the hydrodynamic simulation?
4. What parameters were fixed in discharge simulation and estimation?

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5. What is exactly meant with “noise” in this context?

The whole section 4.3 needs to be re-worked in order to enable the reader to follow what was done and deducted by this comparison.

In addition to this some more information on results and assumptions in general should be given in terms of tables and figures. More to this in the specific comments below.

Ad 2)

As mentioned before, the approach heavily depends on the fact that flow velocity and the water surface elevation can be estimated directly by remote sensing. The authors correctly state in the introduction that “the accuracy of these data is still limited” and “should be carefully taken into account”. But exactly this is missing in the discussion of the possible error sources. In order to assess why the accuracy of the results is significantly different at the two gauging stations, this discussion has to be conducted by quantifying the uncertainty of the remote sensing products and their possible effects on the discharge calculation. This would surely clear some question marks about the general validity of the approach that remain after reading the manuscript.

Also the errors that may be caused by the minimization procedure need to be investigated. This is also lacking. You try to estimate two parameters with a single equation (13), which is prone to yield ambiguous solutions (local minima). This may also be a reason for the poor performance in the Obidos test case.

In general, more care should be taken to distinguish clearly between data from remote sensing and ground data. Both are termed measurements and it is often hard to follow which data is meant in the different sections.

Another general remark concerns the language. Although not a native English speaker, I would strongly recommend to improve the language in order to enhance the readability of the manuscript.

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Specific comments

p. 7840, abstract: The abstract does not well summarize the work done in the paper. This should be improved by clearly stating the approach, the methods and the outcome.

p. 7841, introduction: In a revised version of the manuscript I would also include the recently published paper of Sun et al (2010) in the review of methods for discharge estimation from earth observation.

p. 7481, line 25 ff.: This is exactly the problem: the method cannot be evaluated properly without considering the accuracy of the remote sensing products. Also, the abstract does not state the aim of the manuscript as given here.

Section 2: It should be made clear that in this section previous studies are cited. This includes the title of the section as well as the text. E.g. it should be clearly cited that the equations (1) – (5) stem from Bjerklie et al. (2005).

Section 3: In order to enhance understandability and readability of the manuscript, please use definitions and symbols commonly used in hydrodynamics:

1. "A" for flow area (cross section surface)
2. "S" for friction slope (linear energy slope, which is equal to bed slope for uniform flow -$>$ your hypothesis H6)
3. "Z_s" should be termed "water elevation" to clearly differentiate to water level or stage, which usually the flow depth above the datum of the gauge.
4. "J" is used twice: as energy slope in equ. (6) and minimization criteria in equ. (13). Please differentiate.

p. 7844, line 6: include symbols for surface and mean flow velocity here (V_s and V_m). This is missing.

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p. 7844, line 15 ff.: I would recommend to term your hypothesis “limiting assumptions”, because this is what they are.

p. 7845: in Equ. (8) V_{moy} is not explained. I guess that the mean (moyen) flow velocity is meant. Also Z without subscript appears for the first time and is not explained. From the text I assume it should read Z_s, i.e. water surface elevation. Please clarify and use the same symbol throughout the manuscript.

p. 7845, explanation of equ. (8) and (9): the approximation R $\approx$ (Z-Z_b) follows from the limiting assumption of applying the method to wide and shallow rivers, not only from the rectangular cross section. For rectangular cross section R = flow area / wetted perimeter = (depth * width) / (width + 2*depth) $\approx$ (depth * width) / width = depth for wide and shallow rivers. In general these approximations should be explained in more detail in order to give the reader less experienced in hydraulics the chance to follow your formula derivation.

p. 7856, equ. (11): in the equation the expression should read $(\alpha/K)^{3/2}$, otherwise the expression of A below is not correct. Also, another symbol should be used for A, see comment above on standard symbols in hydraulics.

p. 7845: equations (6) and (7) can be deleted and the Gauckler-Manning-Strickler equation can be cited directly (which is the square root of equ. (7)), because you state above that the river discharge can be calculated by the Gauckler-Manning-Strickler equation given the assumptions you made above.

p. 7856, sentence above equ. (13): I assume you mean the “least squares” method, which is normally used in minimization problems? Please clarify. Also, state which method you use for the actual minimum search (Levenberg-Marquart, etc. . . .). The formula for J in equ. (13) is not the definition of the root mean square error, as stated above. It is the sum of the residuals. Here another fundamental problem arises: If the formula is used as stated in the minimization, the solution may not converge to a global minimum, because you do not use <u>absolute</u> residuals. This means that large positive differences can be compensated by large negative differences. Please clarify what you really use for minimization. If equ. (13) is used as stated, I would strongly recommend to conduct the minimization again with the absolute or squared sum of residuals or the root mean square error as objective function. Also, you should test the minimization for local minima by starting with different initial guesses of Z_b and K, in case you did not use a global optimization routine. Unfortunately this is not stated in the manuscript. In general this is a crucial point of the method, see general remarks.

p. 7846: the last paragraph should me moved up after the reference of Costa et al. for logical argument reasons.

p. 7847, section 4: in general an overview of the test sites would have helped, e.g. location, range of flow depths and discharge to be expected. Also, the test data (ADCP measurements and the hydraulic simulation) need to be introduced better. Especially the purpose of the latter is not clear. The introducing paragraph of section 4 is in part repeated in 4.1. This could be written in a single (sub-)section. The discussion of the possible errors in sub-sections 4.2.x should be introduced properly to give the reader a chance to follow the argumentation. This is hard in the presented form.

p. 7848, line 26 ff.: the fact that your estimation yields values for K and Z_b far from the values derived from the ADCP measurements is an indication for ambiguous parameter estimation, i.e. that your optimization routine might got trapped in a local minimum, see comments above. A sensitivity analysis of the discharge calculations varying K and Z_b within reasonable ranges would be helpful in this context.

P. 7848, section 4.2.1: The statement that the mean value of α “appears to be 0.9” should be substantiated by the data or a plot of the values derived from the ADCP measurements. The appropriate place would be section 3 and in section 4.2.1 it should be referred to this part. Also, it is stated that the discharge calculation for Obidos improves significantly if the measured (I assume from ADCP measurements, again this is not properly differentiated) bottom elevation is used. What K value was used for this calculation? The estimated or the one derived from the ADCP measurements? This needs to be specified in order to derive or to follow your conclusions based on this.

p. 7849, section 4.2.2: The standard deviation of the K-values derived from the ADCP measurements is small in relation to the mean value, therefore it would not be surprising that discharges calculated with varying K-values based on this standard deviations do not differ considerably. But in any case, in section 4.2.2 there is no evidence for this statement. Include a plot showing the discharges calculated with varying K-values or delete this statement. Simply using the mean value from the ADCP-measurements and finding that the results improve does not prove the statement of low influence of varying K-values on the discharge calculation.

p. 7849, section 4.2.3: Another possible reason for the invalidity of the uniform assumption for one or both stations could be the flow depth. The higher the flow depth and flow depth variations, the less valid is the uniform flow assumption. But since no information about the flow depth variations at the two stations are given, this cannot be discussed. Please include this information.

p. 7852, conclusions: It is actually no wonder that the formulae and estimated parameters of Bjerklie do not perform well on the Amazon, because they were developed and estimated on a dataset including a lot of rivers much smaller than the Amazon. A thorough comparison should have fitted the parameters of Bjerklie's equations to the Amazon data set. Therefore I would recommend to weaken the statement of applicability of Bjerklie's method, that the parameterization derived in his paper does not work for the Amazon. The equation itself could work, if parameterized differently. In general I find the conclusion too short. I would prefer to write this section in a way that it can be understood without reading the whole manuscript, but this decision I would leave to the editor.

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Minor remarks & typographical errors

p. 7840, line 24-25: one could add also "different accuracies due to different methods and quality standards" as a problem in trans-boundary hydrologic monitoring.

There are a number of typographical errors in the manuscript. But because I expect the paper to be re-worked thoroughly, I do not list them in detail.

References

Sun, W.C., Ishidaira, H., Bastola, S., 2010. Towards improving river discharge estimation in ungauged basins: calibration of rainfall-runoff models based on satellite observations of river flow width at basin outlet. Hydrol. Earth Syst. Sci., 14(10): 2011-2022.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 7839, 2010.