**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 #1

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1. Does the paper address relevant scientific questions within the scope of HESS?
The authors present a new method of estimating river discharge from earth observations (satellite remote sensing) by combining mass conservation of flow and the Strickler relationship to solve for Strickler's K and the level of the river bed. They compare their solution to the statistical regressions of Bjerklie et al. (2003; 2005) for the Amazon at Obidos and Manacapuru. This topic is within the scope of HESS.

2. Does the paper present novel concepts, ideas, tools, or data?
The approach is quite similar to that of Durand et al. (2010), which the authors should

probably cite; however, the Negrel et al. additionally solve for a friction parameter (Strickler's K) and incorporate surface velocity information. Durand et al. (2010) suggested that this would be possible with their method (which does not incorporate velocity) as well.

3. Are substantial conclusions reached?
Because the method only worked at one of two locations, I have a hard time saying that the conclusions are substantial. The technique is interesting, and the authors do lay out their assumptions and the possible error sources in their assumptions.

4. Are the scientific methods and assumptions valid and clearly outlined?
Aside from grammatical, editorial errors, the authors outline clearly the methods that they employ and what assumptions they make (which are referred to as "hypotheses" in the current version).

5. Are the results sufficient to support the interpretations and conclusions?
I find it disconcerting that the authors disparage the statistical methods of Bjerklie et al. (2003; 2005) on the basis of two river basins when the method presented here by Negrel et al. only performs well on one of the two basins. I find it almost more amazing that the regression models 1 and 4 of Bjerklie et al., which are developed from small rivers mostly in New Zealand and North America, are able to perform similarly well as the method presented here without recalibration. Of course, the key benefit of the method presented here is the estimation of depth and Strickler's co-efficient, which, if somewhat accurate, could be interesting for other global applications.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?
I do not understand why the authors have given only the raw data for the Manacapuru station (Table 1) and not the Obidos station. The number of measurements from the

Obidos station is not listed. The assumptions used in the SIC model are not explained here. It would be difficult to reproduce the simulated hydraulic data. More description of the hydrodynamic modeling should be given.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?
See answer to question 2. The authors should more explicitly state that the solution of Strickler's coefficient is unique to their methodology.

8. Does the title clearly reflect the contents of the paper?
Yes.

9. Does the abstract provide a concise and complete summary?
The abstract does not provide any information regarding the method developed (i.e., that the authors combine the equation of mass conservation and Strickler's equation to define an inverse problem that can be solved for river bed elevation and Strickler's coefficient, and that these can, in turn, be used to estimate streamflow).

10. Is the overall presentation well structured and clear?
The structure of the paper is good. The dataset descriptions should go in the methods section, rather than the results section, but otherwise, the order in which material is presented and the figures used make sense. The presentation of the implications of the model results on simulated data could be made much clearer.

11. Is the language fluent and precise?
This paper needs extensive grammatical editing; at present, the paper is quite difficult to read and is awkward in many places.

12. Are mathematical formulae, symbols, abbreviations, and units correctly de-

fined and used?
Not all symbols are defined (see detailed comments below), and equation 11 contains an error that appears to be a typo.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?
Equations 6 and 7 are unnecessary because the first part of equation 9 is well-known as the Gauckler-Manning-Strickler formula.

14. Are the number and quality of references appropriate?
The authors should cite Durand et al. (2010) and an additional reference, as recommended in comment 8 below.

Detailed comments:
1) On page 7843, lines 5-7: Bjerklie et al. (2003; 2005) do not validate against global discharge to the ocean; they validate instantaneous discharge estimates at each reach against the observations on that reach (i.e., they only consider "the ability to estimate a unique measurement on a given river"). Still, the regressions were performed on a small set of observations, which, aside from one measurement on the Amazon River at Obidos, are only located in the United States and New Zealand. As a result, one could argue that the applicability of these regressions to specific rivers outside of their data set is not assured.
2) Page 7843, lines 8-13: The authors state: "Depending on the dataset used, the different models give us contrasted results, excepted the last model which always gives a wrong estimation of the discharge." In all fairness, Bjerklie et al. (2003) acknowledge that some of their models perform better than others, and Bjerklie et al. (2005) make a point of the fact that river characteristics (for instance, single-channel versus braided) impact the quality of model results and may determine what model coefficients are appropriate.

3) Page 7843, lines 16-19: As far as I can tell, the first step does not express Q based on Saint-Venant hydrodynamic equations. The first step is the formulation of assumptions, which are then employed in simplifying equations in step 2.

4) Page 7844, line 4: width Ls and river width L (defined on p. 7842, line 4) are both top width, only one symbol is necessary.

5) Page 7844, line 12: What is a "reasonable number of measurements" required by the method?

6) Page 7844 and elsewhere: The "hypotheses" would probably be more aptly referred to as "assumptions".

7) Page 7844, line 16: "Permanent flow configuration at each measurement" is not clear. Do this mean that the hydraulic geometry is assumed to remain constant between measurements?

8) Page 7844, lines 17-20: The statement that "a river can be considered a thin film" with a width to depth ratio of around 50 should be cited.

9) Page 7844-7845: It might be more intuitive to list the mass conservation equation (8) directly beneath line 25 on p. 7844 and the Gauckler-Manning-Strickler equation directly under line 1 on p. 7845.

10) Page 7845, Vmoy should probably be Vmean and needs to be defined. S also needs to be defined as cross-sectional area.

11) Page 7846, equation 12: Should be $K3/2$

12) Page 7846, equation 13: It's confusing to use J as the minimization criteria here and as energy slope in equations 6-9. Also, if J is the root mean square error, the terms in the summation should be squared.

13) Page 7847, section 4.1: What assumptions are made in the 1-D hydrodynamic model? Any of the same assumptions (H1-H6) that are used in the estimation technique?

14) Page 7848, section 4.2.2: The standard deviation of the Strickler coefficient for the Obidos data set is a much smaller percentage of the mean than for the Manacapuru, which suggests that the assumption of a fixed Strickler coefficient would have a smaller

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impact on the Obidos estimates than on the Manacapuru estimates. I can't help but wonder how much of this variation could be due to measurement error.

15) Page 7849, lines 1-2: It would be interesting to know which variables discharge is most highly correlated with on Obidos.

16) Page 7849, lines 15-18: It would read better to say that the standard deviation of the slope at Manacapuru is "a noticeably lower proportion of the mean slope." This comparison is difficult to take seriously due to potential for errors in slope measurements (as described by the authors in line 19-23 on the same page). If temporal variability slope is considered a factor in whether or not the method works on a given river, it would be worth testing the sensitivity of these results to expected precision errors in slope.

17) Page 7850, lines 4-8: Is the "amplitude" of the bottom level from the ADCP measurements the maximum variation from the mean bottom level from the ADCP measurements? This is unclear. Same page, lines 9-10: Explicitly state that the bed on Manacapuru shifted more during the period of observation.

18) Page 7850, lines 19-20: Since all of the assumptions apply only to "Q2", it makes sense that the simulated data set would perfectly match "Q1", as long as alpha is 0.9 in the hydrodynamics model.

19) Page 7850, lines 24-26: The surface slope computed by fitting Eq. (9) is a theoretical uniform flow slope, "perfect" does not have much meaning here. I think that this means that the fitted slope is lower than the simulated slope by 620) Page 7851, line 17: should be "discharge larger than" not "superior to."

References
Bjerklie, D.M., S.L. Dingman, C.J. Vorosmarty, C.H. Bolster, and R.G. Congalton, 2003, Evaluating the potential for measuring river discharge from space, J. Hydrol., 278: 17-38.
Bjerklie, D.M., D. Moller, L.C. Smith, and S.L. Dingman, 2005, Estimating discharge in

rivers using remotely sensed hydraulic information, J. Hydrol., 309: 191-209.

Durand, M., E. Rodríguez, D. E. Alsdorf, and M. Trigg, 2010, Estimating river depth from remote sensing swath interferometry measurements of river height, slope, and width, IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, 3(1), 20-31, doi:10.1109/JSTARS.2009.2033453.

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