Uniform flow formulas for irregular sections in straight channels

Reply to review of J.B. Faure

We first thank the reviewer for his valuable job.

General Comments:

Point 1: *The paper is out of the scope of HESS.*

Reply: We don't agree with the reviewer for the following (see http://www.hydrology-and-earth-system-sciences.net/about/aims_and_scope.html): a) among the others, the scope of HESS encompasses the following:

2. **the study of the spatial and temporal characteristics of the global water resources** (solid, **liquid**, and vapour) and related budgets, in all compartments of the Earth system (atmosphere, oceans, estuaries, **rivers**, lakes, and land masses), including water stocks, residence times, interfacial fluxes, and the pathways between various compartments;

b) among the others, the journal covers the following subject areas and techniques/approaches, which are used to categorize papers:

Subject areas:

- **rivers** and lakes;
- **engineering hydrology**;
- **water resources management**.

c) among the others, the following techniques and approaches:

- **modelling approaches;**
- instruments and **observation techniques;**
- **uncertainty analysis.**

Point 2: *the paper is a pure hydraulics/fluid mechanics paper*

Reply: we don't agree with the reviewer because both formulas are not based on advanced integration of the Reynolds equations, but are founded on the historical experience of the Manning formula and on three different validation tests carried out on the basis of laboratory and field data, as well as of the results of 3D numerical analysis (carried out with a commercial code). In the introduction, we have shown that integration of the Reynolds equations leads to formulas that depend on several empirical coefficients. On the basis of the previous observation, we go on with a different approach, strongly based on experimental evidence.

Point 3: *is it a bad idea….?*

We agree with the reviewer about the possibility of using the developed approach to simply estimate the vertically averaged velocity in single sub-sections and use this velocity value for the 1D transport equation in the same sub-section. On the other hand, we think that a preliminary requirement is to have some evidence that the proposed approach provides reliable results before using it for this and other possible applications.
The focus of our paper is about this evidence.

Questions:

Question 1): *When they are used in 1D shallow water simulations, the purpose of this kind of head loss formulas is to take into account the lateral variation of the roughness / flow resistance, that is, in fact, the Manning coefficient. In the paper only a constant Manning coefficient is considered. Even when the formulas are validated against a real case, the Manning coefficient is taken constant along a river reach 13 km long. I am pretty sure that it is possible to obtain the same agreement with standard calibration of Manning-Strickler formula and a Manning coefficient different in several sub-reaches.*

Reply: We first want to say that we don't fully agree with the remark in the premises of the question. This type of formulas is not motivated by the lateral variation of the roughness, but by the effect of the geometrical irregularity of the section. All the empirical formulas, like the Manning formula, are based on experiments carried out on sections with convex shapes (triangular, rectangular, trapezoidal or circular) and do not take into account the lateral variability of the vertically averaged velocity.
The lateral variability of the Manning coefficient is another source of uncertainty. With the proposed approach, we can easily differentiate the Manning coefficient along the section. Results will be reliable assuming that the secondary effect of the Manning coefficient lateral variation does not affect the relative dependency of the velocities, as structured in the homogeneous case.
The comment relative to the validation test number 2) does not take into account that the Manning coefficient has, in the context of the indirect method for discharge estimation, the meaning of a

calibration parameter, that we use to match the water levels measured in the downstream section with the level computed by the 1D hydraulic model.

It is well known from the parameter estimation theory (*Aster et al., 2012*) that in any numerical model the uncertainty of the estimated parameters, obtained by matching the computed and the measured model output, grows very quickly with the parameter number. This implies the need of minimizing the number of parameters, also by neglecting the effect of irregularities and heterogeneities. Of course, it would be very easy to fit very well the measured and the computed water level hydrographs (that are the output of the hydraulic model) also by differentiating the Manning coefficient along the reach or along the section, but the corresponding parameter error would become very large even with a small error in the topography or in the measured water levels. These errors strongly affect the discharge computed by the hydraulic model. Moreover, if the computed Manning coefficients or the parameters of other formulas were used for prediction purpose (which is a major benefit of the indirect measurement approach) the predicted discharges would be affected by an even stronger error. All this implies, of course, that test n.2 can be thought as the validation of the proposed formulas for their use in the specific method for the discharge estimation.

On the other hand, as explained in the paper, it is very difficult to carry on field tests for the validation of these formulas, because roughness coefficients are unknown and the results of all formulas can be scaled according to the selected Manning coefficient or to other parameters. The indirect method provides a unique possibility of carrying on a simultaneous estimation of both the Manning coefficient and the discharge hydrograph.

In the revised paper we expanded the introduction to the indirect discharge measurement method by adding:

" It is well-known in the parameter estimation theory (Aster et al., 2012) that the uncertainty of the estimated parameters (in our case the roughness coefficient) grows quickly with the number of parameters, even if the matching between the measured and the estimated model variables (in our case the water stages in the downstream section) improves. The use of only one single parameter over all the computational domain is motivated by the need of getting a robust estimation of the Manning's coefficient and of the corresponding discharge hydrograph."

Question 2): *It would have been interesting to compare the formulas against a standard 2D shallow water simulation, in both cases laboratory flume and real river.*

Reply: It is easy to show that to solve on the river domain a standard 2D finite volume model where turbulence inter-element strains are neglected is equivalent to implement the DCM method in a 1D model where the distance between two sections is similar to the longitudinal extension of the 2D elements and the width of each subsection is similar to the lateral extension of the 2D elements. It would be possible to apply the same strategy implemented in the 1D case also in the 2D case, in order to approximate the inter-element strains without the need of solving complex 2D turbulence models. Once again (see reply do points 2 and 3 of general comments) we preferred to maintain the focus of the paper on the validation of the proposed formulas, even if several application and extensions can be easily foreseen (which we believe is a positive indicator).

<u>Question 3):</u> *equation 1: if I understand the paragraph following this equation well, the energy slope is assumed to be identical in each sub-section, so the subscript for* S_f *is erroneous*

Reply: We corrected the mistake

<u>Question 4):</u> *page 2618, line 10: "If β is close to zero, …". This point is not clear for me; LRHM appears, at this point, like a continuous formula when DCM is a discrete formula.*

Reply: In Eq. (16a) the limit of the local radius at the l.h.s. for $\beta \rightarrow 0$ is equal to *h*. This implies that, in this case, velocity *U* computed in Eq. (15) is independent from the other velocities, as in the DCM approach. DCM can be thought as a spatial discretization of Eq. (15), where the local radius is replaced by *h*, in the same way the practical solution of the LRHM method is obtained by numerical integration of its continuous form given by Eqs. (14)-(16), with β different from zero. We changed the paragraph according to:
"After numerical discretization, Eqs (14)-(17) can be solved to get the unknown q, as well as the vertically averaged velocities in each subsection. If β is close to zero and the size of each subsection is common for both formulas, LHRM is equivalent to DCM; if β is very large LHRM is equivalent to the traditional Manning formula."

<u>Question 5):</u> *page 2619, last line: the numbering scheme of the validations look like a typo.*

Reply: We corrected the mistake

<u>Question 6):</u> *page 2620, §3.4: How is* Δq *computed?*

Reply: ΔQ is the difference between the discharges computed using two different β values. In the revised paper we specified the meaning of the symbols and also gave the size of the perturbation $\Delta\beta$ used in the sensitivity analysis ($\Delta\beta = 0.001\ \beta$).

Question 7): *page 2622, point 2 (lines 4-5): this (widely used) statement needs a discussion. It is surprising that it is said that the Manning coefficient is varying with the water depth, and not that it is the exponent in the relationship expressing Chezy coefficient in terms of Manning coefficient and hydraulic radius, or that the whole relationship should be revisited. Why the Manning coefficient and not the other terms of the formula?*

Reply: We partially agree with the reviewer. The historical lab experiences that are the basis of the Manning coefficient (set back in time much further than the referenced paper of Herschel of 1897) have been carried out with sections of simple geometry and, of course, with very simple measurement devices. On the other hand, it is common opinion that the roughness of the river bed is strictly related to the size of its irregularities, relative to the actual water depth. If the water depth is small even small grains have a significant relative size, but during flood only the main vegetation has a significant relative size. In parts of the river bed that are commonly submerged by water the same water provides a smoothing of the natural irregularities, but in the other parts submerged only during floods the water flow is likely to get a larger resistance.

Question 8) *page 2623, lines 21-22: it seems that there is a missing word (verb) in this statement.*

Reply: Right. It should be: 'The error obtained between measured and computed discharges, with all methods, **is** of the same order of the discharge measurement error."

Question 9) *page 2627, lines 8-10: that does not prove that an uniform flow has been reached. For that, the same condition must be verified in each cross section along the reach, so that the energy slope equals the bottom slope.*

Reply: Uniform flow conditions do not exist in nature, because secondary currents always show up, even in straight prismatic channels. This is one reason to perform 3D simulations, which are able to reconstruct the 3D velocity field and to test the robustness of the formula with respect to the (conceptual) model error. In the validation test n. 3 we compare the vertically averaged velocity components along the river bed direction computed by the 3D model with the values obtained by our formulas. The 3D configuration more close to the uniform flow approximation is the one envisaged by setting the average water depth in the most upstream water section equal to the downstream one, which is given as boundary condition.

Question 10) page 2628, point 2 line 11: I didn't see any proof that the estimation of β (to ~ 9) is still valid for real rivers.

Reply: Our strategy, in this and other challenges for the prediction and the observation of natural variables, is to perceive a rigorous scientific approach, even in the consciousness that heterogeneity and uncertainty in nature do not allow a precise estimation of the predicted/monitored variables. We agree with the reviewer that the accuracy attained in the results of lab experiments, carried out in small channels at least one order of magnitude smaller than the real rivers, with geometry and roughness very well known, cannot be assumed to be the same holding for real rivers. On the other hand, I would personally never apply for discharge estimation a formula that doesn't work at least in the lab. The other two tests are aimed to partially overcome the previously mentioned limit by a) validating the formula in the context of the indirect discharge estimation methodology, b) validating the formula by comparison of the estimated vertically averaged velocities with the results computed by a fully 3D model.

Question 11) *table 5: it is strange to want a Manning coefficient which can vary with the water level but must remain constant along a river reach 13 km long. How to be sure that the roughness is constant throughout 13 km?*

Reply: See the second part of the reply to Question 1.

Question 12) *table 6: this table does not clearly prove that IDCM, INCM and LHRM does better than DCM, the differences are too small to be convincing.*

Reply: We agree with the reviewer that the results of Validation test 2 are good for all the compared formulas, even if LHRM results match the measured values a little bit better. A possible explanation is that the indirect discharge estimation is based on the conservation of the wave volume and this depends on both the estimated wave celerity and on the storage occurring inside the reach along the observation time. The Manning coefficient and the water depth – discharge relationship affect only the celerity estimation. The Alzette river has a relatively small bed slope in the investigated reach (~0.08%) and this provides a relatively small sensitivity of the results with respect to the selected formula.

On the other hand we can see in tables 3 and 8 that LHRM provides results that are better than results of traditional methods DCM and IDCM in all the validation tests, not only in test 2, and that the error in tests 1 and 3 is much smaller than the error obtained by the other formulas.

Question 13) *Figures 5 to 10 (graphs) would be more readable if they were in colors.*

Reply: We agree with the reviewer (and hope it won't cost too much…).