Review of
“Discharge estimation in a backwater affected meandering river”
of H. Hidayat, B. Vermeulen, M. G. Sassi, P. J. J. F. Torfs and A. J. F. Hoitink

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1. Brief description

The paper concerns the estimation of river discharge through direct measurement of the velocity field with a horizontal acoustic Doppler current profiler (H-ADCP), carried out in a meandering stream affected by backwater (and perhaps other effects outside the framework of one-dimensional hydraulics – see comment below). The authors demonstrate that better discharge estimates can be obtained from the H-ADCP data by evaluating them with the semi-Deterministic semi-Stochastic Model (DSM) method, developed by members of the authors’ team, than with the simpler Velocity Index method. In addition, the authors infer discharge estimates from stage measurements, via a steady-flow rating curve corrected by application of the so-called Jones formula, and show that the traditional stage-discharge conversion with the Jones-formula correction is unable to explain the wide loop(s) of the measured stage–discharge curve.

2. Assessment

This well organized, properly documented and succinctly written paper addresses an interesting and difficult field problem. It is apparent that the authors have command of the subject, which encompasses measurements using H-ADCP technology and treatment of the collected data by application of fluid mechanics/hydraulics (velocity distribution, roughness etc.), in order to extract high-quality information from the raw data. The comparison of the methods of the semi-Deterministic semi-Stochastic Model and of the Velocity Index for the evaluation of H-ADCP data should be useful to anyone interested in estimating discharge from measurements of the velocity field in an open channel.

I would suggest to the authors a minor modification intended to improve the presentation of their very good work in the discharge estimation via H-ADCP measurements. But first a comment: From the authors’ presentation, the characterisation of their method (DSM) as stochastic does not appear plausible; perhaps, a little additional information would clarify this point. A more detailed sketch than that given in the lower Fig. 2 should be provided to explain symbols used in the text and equations in sections 2 and 3: d, z, H, η.

The additional and traditional component of the paper, concerning flood dynamics treated via 1-D hydraulic equations, is also useful, but should be improved. The improvement concerns mainly the correction of an error in the Jones formula related to the celerity $\boldsymbol{c}$. The authors write (p. 2678, lines 6-8) “The celerity $\boldsymbol{c}$ was estimated from $\boldsymbol{c} = \sqrt{\boldsymbol{g}d}$ (Ligget and Langley, 1998), where $\boldsymbol{g}$ is gravitational acceleration and d is hydraulic mean depth, according to $d = A/b$.” The authors use incorrectly the celerity of dynamic waves in the Jones formula; they should use the celerity of kinematic waves, $\boldsymbol{c} = dQ/dA|_{x\,=\,const.}$, which is typically evaluated from steady-state flow rating curves [closer to the physics, but more involved computationally, is an iterative evaluation based on the looped rating curve, as suggested by Koussis (2010)]. The value of the kinematic wave celerity is less than that of dynamic waves; therefore, the width of the loop(s) of the rating curve would increase. However, I do not expect this correction to change the conclusions reached by the authors. The authors should also provide the value of the bed slope. Additional, secondderivative corrections of the Jones formula appear less plausible in the light of such poor agreement of hydraulic estimates with field data (Koussis, 2010).

The authors write (p. 2680, line 29) that "there is no theoretical justification" for using the kinematic wave equation in the Jones formula to replace the surface gradient term $\partial h/\partial x$ by $(1/c)\partial h/\partial t$. It would be better to say that the replacement of the surface gradient term by the time derivative of the depth divided by the kinematic wave celerity is an approximation, the accuracy of which depends on the closeness of the actual flood wave to the kinematic wave. This position is in line with Henderson's (1966): "The logical basis of the [Jones] formula is not strictly correct,…". Use of the kinematic wave model to convert $\partial h/\partial x$ to $\partial h/\partial t$ does not imply its adoption in general. To the contrary, this first-order approximation is used to make the second-order approximation (diffusive wave) more readily useable, with little loss in accuracy. See also section 3 in Koussis (2010). In the particular case studied, I concur with the authors that the Froude number value $\boldsymbol{F} = 0.01$ indicates likely negligible inertial terms, although it must be said that Henderson's assessment of the magnitude of the inertial terms relative to the free-surface slope as $O(\boldsymbol{F}^2)$ rests on the assumption of a quasi-kinematic flood wave behaviour.

Minor comments

It is not entirely correct to refer to Eq. 10 as the Jones formula. Jones proposed the zero-inertia approximation, but it was Thomas who replaced the spatial derivative term by a temporal derivative term, in order to enable estimating the discharge from at-a-station stage measurements [see Henderson (1966)]. Better term: Jones-Thomas formula.

The authors' statement about lateral water level gradients having been ignored in previous work overlooks the fact that 1-D hydraulics inherently cannot take lateral water level gradients into account. Out-of-bank spills and return flows from flood plains should be also included among the possible reasons for the failure of the Jones-Thomas formula to adequately predict flood dynamics; such phenomena more than test the limits of 1-D hydraulics.

What is the purpose of the statement in the Introduction (p. 2670, lines 8-11) "Backwater from one or several downstream elements …, causing curved longitudinal surface level profiles for a constant and uniform river discharge."? If it means that, depending on the boundary condition, different (curved) water surface profiles result for a given flow rate, this is correct but well known. What am I missing here? Rephrasing might help.

Recommendation

This useful and well-presented contribution to the important problem of river discharge estimation under conditions of strong backwater effects should be published, taking into consideration the comments above, especially those regarding the Jones-Thomas formula.

3. References

Henderson, F.M. 1966. Open Channel Flow (pp. 365-394), Macmillan, New York, USA.

Koussis, A. D.: Comment on "A praxis-oriented perspective of streamflow inference from stage observations - the method of Dottori et al. (2009) and the alternative of the Jones Formula, with the kinematic wave celerity computed on the looped rating curve" by Koussis (2009), Hydrol. Earth Syst. Sci., 14, 1093-1097, doi:10.5194/hess-14-1093-2010, 2010.