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Hydrology and Earth System Sciences Discussions

Interactive comment on "Comment on "A dynamic rating curve approach to indirect discharge measurement" by Dottori et al. (2009)" *by* A. D. Koussis

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Introduction: On "missing the point" and on "arguments versus assertions"

Anonymous Referee #3 (2010), using unusually aggressive language, writes that my comments to Dottori et al. (2009) (DMT) work "miss the point of the original article" and that my critique is "by assertion rather than by argument". He (used as common gender) also disagrees with my position regarding the inconvenience of measuring at two cross-sections. Fortunately, the responsible Editor (and Referees #1 and #2) does not seem to think that I miss the point of the original article of DMT, otherwise he would

have rejected my Comment. Science, however, is not majority-ruled, it is based on arguments, and Referee #3 could still be right. So, let us examine the arguments and let the readers ascertain whether my critique is "by assertion rather than by argument".

On measuring at two cross-sections

My Comment places emphasis also on practical aspects of the approach advocated by DMT, the concern being that it may be difficult to apply in the field. The reason for this difficulty is that, unless the two stations give a good estimate of the surface slope, the mathematical calculations will miss the target, and, as is stated in my Comment more fully, such a requirement is not trivial, because depth and stage are influenced strongly by the local geometry. I was amazed to read the smart remark of Referee #3 "if measuring at one cross-section can be done, then measuring at two can be also", since, by induction, measuring at three, four,. . . to infinity can be also! Referee #3 then claims, by simply stating the complete 1-D linear momentum balance, that the surface slope may be smooth while the geometry may be varying substantially. Now, that is an assertion, not an argument! The fact of the matter is different. We can see this clearly by considering steady, gradually varied, subcritical flow, at constant rate in an open channel of rectangular cross-sections of variable width. By the laws of flow, the water surface profile responds locally, by rising or falling when the width decreases or increases, respectively; in the extreme case of a choke, the flow becomes critical in the constriction and rises sharply upstream of it. It follows, then, that, over a reach of variable geometry, good or poor estimates of the mean surface slope are obtained depending on the locations of the gauging points. In a flood, the flow rate varies spatially at any fixed time, which adds to the variability of the surface slope relative to the case of constant discharge. Referee #3 is advised to pay attention to what DTM themselves write about the selection of the locations of the two gauging stations, namely, "Please note that the distance between the two adjacent sections must be sufficiently small to allow for the constant flow rate assumption to be realistic, but at the same time it must be sufficiently large to allow the difference in water stage to be greater than the measurement instrument sensitivity and the water elevation fluctuations."

Then, Referee #3, while admitting that gauging stations may not be at hand where needed, asserts (again), that this "is not a valid technical criticism of the Stage-Slope-Discharge method". From this statement, I conclude that, in the opinion of Referee #3, one should not be concerned whether the essential prerequisites for the application of a method are secured, or not, because, presumably, a method exists in its own theoretical realm. This escapes my logic.

On the great differences (as alleged by Referee #3) of the DyRaC method and the Jones-Henderson formula with c computed on the looped rating curve

Subsequently, Referee #3, dismisses the body of my Comment with the simple, but sweeping statement "Koussis goes into a number of detailed remarks about the use of Jonestype methods. These, however, are not what Dottori et al. were concerned with. The Stage-Slope-Discharge method solves the problem differently." In this self-contradicting statement, Referee #3 claims that, because DMT solve the problem differently from the Jones-formula based approach that I described, which computes the kinematic wave celerity on the looped rating curve, that second approach is irrelevant, overlooking that both methods use a flow formula and stage observations for the same purpose and are thus closely related. It seems to me that, to paraphrase the colloquial expression, Referee #3 and I have been "reading and writing passed each other". This, however, does not change the fact that the DMT method and my analysis treat the same problem, estimation of an unsteady flow rate from stage measurements.

On the misconceptions of Referee #3 about flow rating formulas with second time derivative and the Jones formula amended with Henderson's correction

Finally, Referee #3 thinks that my "understanding seemed faulty and unnecessarily misrepresentational" also regarding introducing higher-order derivatives in the flow rating relationships. He explains that a formula such as Fenton's "was an attempt to incorporate analytically the sort of diffusion that Koussis' Jones-type methods do not

have, namely rational treatment of diffusion." Referee #3 errs for the following reasons: (1) neither the complete, physically-based rating relationship (the linear differential momentum balance inverted through the use of a Manning-Strickler type flow formula) nor the one truncated by omission of the acceleration terms include a diffusion term; (2) a wave-diffusion term derives from the surface slope term, but only upon insertion of a flow formula in the spatial derivative of the discharge in the differential mass (storage) balance equation (and usually upon linearisation), and (3) wave diffusion is accounted for through the routing procedure.

Henderson (1963, 1966) wanted to improve the not strictly correct basis of the Jones formula, yet maintain that formula's basic practical format (the shortcoming of the Jones formula derive from using the KW approximation in converting the spatial to the temporal depth derivative, thus ignoring attenuation of the wave). Displaying a magnificent understanding of flood hydraulics, Henderson corrected the rating formula of Jones for wave subsidence (wave crest region) by adding the fixed term 2/3rˆ2, i.e., Q/Qo = [1 + (1/cSo)dy/dt + 2/3rˆ2]ˆ1/2, where r the ratio of the bed slope to the "wave slope" Sw = 2y_crest/L, with L the wave length, r = So/Sw = So/(2y_crest/L) [note that dy/dt is to be understood as the partial temporal derivative of the depth y]. A judicious estimation of Sw is neither difficult [e.g., L can be estimated as (c)x(period of wave rise)] nor has strong implications, since typically r > 10 and thus 2/3rˆ2 is small. Indeed, Henderson had already derived the simplified formula of Fenton and Keller (2001), with the second temporal derivative of stage (Eq. 9-57, p. 379), also including approximations for the inertial terms at Froude numbers < 0.7 (Eqs. 9-64 and 9-65, p. 381), but insisted on applying the fixed-term correction only to the crest region (see Eqs. 9-92 and 9-94, p.393). Note also that Henderson was careful not to adopt generally the form with the second derivative, despite considering prismatic channels. Given that the routing scheme ensures wave attenuation, it is argued here that attempting to correct the Jones formula, by introducing higher-order derivatives (e.g., formulae of Fenton and Perumal 2) while incurring numerical oscillations, does not seem advisable, especially when considering the morphologic variability of natural streams. In contrast, incorporating in the Jones formula Henderson's fixed correction 2/3rˆ2 improves the estimate of the flood peak without oscillations.

On academic style, or on being eponymous when expressing personal criticism

In closing this reply to Referee #3, I would like to comment on style. It is my conviction that when a reviewer expresses an opinion in an unusually aggressive manner, e.g., with characterisations such as "I also thought that Koussis understanding seemed faulty and unnecessarily misrepresentational…", then that Referee ought to do this eponymously, not anonymously. Otherwise, it is like shooting at somebody from behind a fence. The expression "misrepresentational" indicates mal-intension, an unfair accusation that I fully reject. In our legal system, the accused has the right to face his accuser. In the academic world, the person being criticised severely should be shown the courtesy of knowing the identity of the person levelling the criticism. A Referee who unleashes an ad hominem attack (i.e., attacking not the views of the writer, but the writer-person) should not seek refuge behind the anonymity granted to reviewers by journals, but have the decency to sign the review, taking responsibility for its content.

References

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