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

Anonymous Referee #1

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The Comment by A. D. Koussis moves five different critiques to the 2009 published work "A dynamic rating curve approach to indirect discharge measurement" by Dottori, Martina and Todini (DMT).

In synthesis the five critiques are:

1) "DMT seem to miss its practical aspects because choosing the right location is not easy, measuring two cross sections and installing two level gauges instead of one is too expensive";

2) "there seems to be an oversight in the sign of Eq. (4) of DMT for the celer-

ity c of the kinematic wave (KW), which should be positive: c=dQ/dA|x=const.=B-1dQ/dy|x=const.";

3) In Jones formula they computed the celerity on the basis of the steady-state rating curve, while "when the flow departs markedly from the KW status, the Jones formula should be evaluated with c(Q) computed on the looped rating curve";

4) Dr. Koussis contends "that no procedure based on hydraulic equations, no matter how mathematically elaborate, can eliminate a judgment-based "required extrapolation of the rating curve beyond the range of actual measurements used for its derivation", as DMT state in their Abstract; and DMT's claim (Conclusions) for the DyRaC approach that "its calibration procedure only requires the evaluation of roughness coefficient, thus eliminating the extrapolation errors" seems overly optimistic.";

5) Dr. Koussis also remarks that "It thus follows that the idea of using stage observations at two cross-sections to estimate flood flows is a reversing of the BGS procedure. This standard procedure of BGS (Darmstadt, Germany) was adopted in the modeling of flood flows in the River/Canal Kiphissos, in Athens, Attica Region, Greece (Koussis et al., 2003; Mazi and Koussis, 2006).

First of all, I think that four of the critiques seem properly acceptable and valid for discussion. On the contrary I do not believe that the fifth is relevant for two reasons and I suggest Dr. Koussis to think if it is not worthwhile eliminating it. The first reason is that the idea of inverting an equation to estimate flow instead of the backwater profile is something new and does not correspond to the use of the "standard step" method to estimate backwater profiles. The second one is that it may somehow give the reader the negative impression that the author of the comment wants to step in and say "I did it first".

In the text I found one major error and few minor ones.

The major error is in Figure 1. The upper part of the figure where the inflow as well as

outflow computed discharges are plotted, shows a maximum value for the discharge computed using full SV equations and the corrected KW, of 770-780 m3 s-1, while the loop rating curve displayed in the lower part of the figure for the same approaches shows a maximum flow of 850-860 m3 s-1, which implies that the two figures were independently obtained and possibly relate to two different cases.

Minor errors Page 3, Line 24, "B ∂Q/∂t/c2So" is obviously wrong. The corrected one is: "B-1∂Q/∂t/c2So" Page 5, Line 20, as well as Page 7 Line 12, The reference "Wang and Laurenson" is misspelled, it should be "Wong and Laurenson" Page 5, Line 22 "judgement-based" should read "judgment-based"

Therefore I suggest Dr. Koussis to consequently amend the submitted comment prior to its final acceptance.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 7429, 2009.

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