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Hydrology and Earth System Sciences Discussions

Interactive comment on "Uncertainty in river discharge observations: a quantitative analysis" *by* G. Di Baldassarre and A. Montanari

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This paper presents concerns about the lack of uncertainty quantification of measured river discharge series, as common practice in hydrological applications. In the introduction, it appears that the authors have in mind, that these series are to be used for the development of calibrated hydrological models. This is in my opinion a fundamental issue for modellers and therefore the paper may become an important contribution. There are however a number of quite fundamental issues that I would like to raise and the paper needs in my opinion quite a bit of work before it is ready for publication.

One of the most important is the assumption that the stage-discharge relation may be modeled as a polynomial function. This is in my opinion a very poor representation of physics as we know that this relation, given that momentum plays a minor role should

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Interactive Discussion

Discussion Paper

be of the form

$$Q(t) = a(h(t) - h_0)^b \quad (1)$$

where h_0 is a no-flow reference water level [L], $h(t)$ is the measured water level at time t [L] and a and b [-] are model parameters. Physically, we may expect that b should have a value around 5/3. In fact, acceptability limits on this value of b could already result in a larger constraint of the rating curve (under the assumption of stationarity and steady flow) than presented here in this study. So the experiment should at least be repeated with a more physically based rating curve equation to see if this further marginalizes the uncertainty in discharge.

A second issue is the assumption of indepence of the errors. I would like to agree with the authors that many errors may be more or less independent from each other, but there may be a great deal of correlation in time in the errors, made in each time step. This may cause a previously made error to be compensated for in the near future. I refer in particular to the error due to unsteady flow conditions. In the Po river, this may amount to a considerable 15% error, but if you would accumulate discharge over time, this error may prove negligible. I do realize the problem when a standard least squares optimization is used for model calibration, but if the flows are to be used for instance to calculate the dimensions of a dam, with the purpose of storing water for agricultural use or power production, this error is irrelevant. Such issues should at least be discussed.

In short, I recommend the following: although numbers should give proof, I have the feeling that results may greatly depend on the assumptions made and the application foreseen. The authors should therefore put more effort in showing how their assumptions influence the results, for instance by performing additional computations with other assumptions. Also they should discuss the assumptions in a discussion section which is now completely lacking. The authors should at least consider in what situations the errors presented are important, because, as I described before, it really depends on the application, or time scale on which the discharge observations are used. A suggestion: besides the points mentioned before, you may furthermore

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consider in the discussion how the results from this study should be used by hydrological modellers or even come up with a standard for uncertainty estimation in real-life cases (in gauged and ungauged conditions). This would really help the hydrological community and make the paper much more than just another uncertainty assessment.

Hereby I present some further, minor comments.

It may be worthwhile to give the manuscript to a native English speaking person.

p. 40. The authors link their work to the PUB initiative. However, in ungauged situations, one may expect that a rating curve is seldom available to the user. If you want to make a link with PUB, please do this throughout the whole paper, by adding the assumption that a rating curve is available, and perhaps discuss what a user should do if a rating curve is not available.

p. 40, l. 24-25 “observable uncertainty... marginal role”. It may be a good idea to mention that Keith Beven is promoting the use of uncertainty in output data to assess model uncertainty by means of limits of acceptability. These could for instance be based on uncertainties in the rating curve, which would make this paper much more valuable to many readers. I’m quite sure the authors are aware of all the relevant literature.

p. 41, l. 23 rephrase “being the ... used” the English is incorrect. Furthermore, this sentence suggests that the rating curve is a completely independent method, with respect to the $v - A$ method, while the first is usually dependent on the second.

p. 41, l. 25 “accordingly” should be “according”.

p. 42, l. 4, I would add “the methodology used to estimate river discharge *during rating*”. And what about the instrumentation, used to measure?

p. 42, l. 13. add an ‘s’ to ‘measurement’.

p. 42, l. 24. replace “plugged in” for “substituted in”.

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p. 43, add a schematic of a cross-section with all the dimensions, used in the text.

p.43, l. 11. I believe you mean “integrated over” instead of “plotted against”.

p. 43, l. 23. remove the word “here”.

p. 44, l. 2. remove “actually”.

p. 44, l. 19. “not precise” replace “imprecise”

p. 45. What about bias (for instance due to poor maintained instrumentation)?? And the uncertainty due to poor rating-curve model selection, as I mentioned before?

p. 47. Why did you not use the downstream water level in the Adriatic see as a boundary condition? Surely these water levels are easy to obtain and on average easy to estimate? This would yield more calibration points.

p. 47, l. 15. The symbol X was already reserved for being an error. Here it is used as distance, please consider another symbol.

p. 48, l. 19. Remove the brackets around $Q'(t), h(t)$. Add a thorough discussion section. Besides the points mentioned before, you may consider how the results from this study should be used by hydrological modellers or even come up with a standard for uncertainty estimation in real-life cases (in gauged and ungauged conditions). This would really help the hydrological community and make the paper much more than just another uncertainty assessment.

Fig. 1. Add places “Casalmaggiore” and “Boretto” in this figure.

Figs. 4, 5, 6 and 7. Please change colors or line types for black-and-white printers.

Fig. 6. There is a font problems, the letter ‘e’ comes out as ‘R’.

Fig. 8. Here the problem with unsteady flow errors becomes apparent. The authors suggest a percentage error which is always positive, while it actually should be allowed to be negative as well.

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Interactive Comment

I hope this review is valuable to you and would be interested to read replies and/or see a second version of the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 39, 2009.