Reply to Prof. John Fenton

Reply First of all we would like to thank Professor John Fenton for his thorough review and appreciative words and for the several comments and references that will help us to improve the paper.

General comments

Reviewer

It has been around for a while, and the authors really should refer to Herschy (1985, Chapter 8), who calls it the Stage-Fall-Discharge method.

Authors

We will certainly make a reference to the work of Herschy as requested by the reviewer, but we would like to point out that the Herschy Stage-Fall-Discharge method is rather empirical and hardly comparable to the one proposed in our manuscript. The Herschy approach assumes that

$$Q = Q_r \sqrt{\frac{F}{F_c}}$$

while correctly starting from

$$Q = K\sqrt{-\frac{\partial \eta}{\partial x}}$$. *(F&K, Eqn 2.4)*

one can only obtain:

$$Q = Q_r \frac{K(\eta)}{K(\eta_r)}\sqrt{\frac{\Delta\eta}{\Delta\eta_r}}$$

Which implies that the relation between Q and Q_r is a more complex function of η, η_r as well as of $\Delta\eta$ and $\Delta\eta_r$ and reasonable results can only be obtained when the ratio

$$\frac{K(\eta)}{K(\eta_r)}$$

tends to 1.

Reviewer

For my own taste the article was a little bit unscientific. The variety of methods were presented with not much explanation or scientific justification, and I felt that I had not been given much insight into them. Maybe that is my problem. Having said that, I am sure that the method they advocate, the Stage-Fall-Discharge method really is much better than any of the others they tested.

Authors

We can only agree with the reviewer when he says that "the variety of methods were presented with not much explanation or scientific justification"; however we were confronted with a choice of adding the derivations of the various formulae or to refer the reader to the original papers. We chose this second approach because even adding short explanations to each formula (10 formulae plus the two newly proposed) would require lengthening the paper, making it less focussed to the basic aim of comparing all these methods, which are already available and well documented in the literature. That's why we referred the reader to the original works, most of which are also freely available online for a detailed description and derivation. Please note that we also made an effort, for the sake of clarity, to maintain as much as possible the same symbols in the different equations, by slightly modifying the original ones.

Reviewer

I think that the method that the authors suggest could be placed on a stronger and simpler theoretical
footing. For example consider the momentum equation in the form

$$\frac{\partial Q}{\partial t}+\left(gA-\beta\frac{Q^2B}{A^2}\right)\frac{\partial y}{\partial x}+2\beta\frac{Q}{A}\frac{\partial Q}{\partial x}=\beta\frac{Q^2B}{A^2}\bar{S}-gA\frac{Q^2}{K^2} \qquad (1)$$

where this follows the notation of Fenton & Keller (2001, equation B.7). In Appendix B.2 of that report it is shown that almost all terms of the equation are of the order of the square of the Froude number
$F^2=Q^2B/gA^3$*, leaving only two terms and giving the equation*

$$Q=K\sqrt{-\frac{\partial\eta}{\partial x}} \qquad \text{. (F\&K, Eqn 2.4)}$$

where the only approximation made is that $F^2<<1$*. Now consider equation (23) of the paper under review. If the dynamic terms are ignored, it is clearly an approximation to equation (F&K, Eqn 2.4). It seems that there might be some room for simplification in the authors' approach to the problem. In particular, it is inconsistent to include terms like the dynamic terms in* β*, when terms of similar magnitude have already been omitted. A legitimate approximation to (F&K, Eqn 2.4) is something like*

$$Q=K\sqrt{-\frac{\eta_2-\eta_1}{x_2-x_1}}$$

Of course this does not require iteration. This would make the paper easier to read and understand, which are both desirable virtues. Complication is not.

Authors

We fully agree with the reviewer that complication is not desirable and we thought that directly starting from the basic momentum conservation equation written as in equation (18a) was the simplest way of deriving the approach in finite difference terms.
Let us first show that our starting equation (18a)

$$\frac{\partial\left(z_0+y+\beta\frac{Q^2}{2gA^2}\right)}{\partial x}+\frac{1}{g}\frac{\partial\left(\frac{Q}{A}\right)}{\partial t}=-\frac{Q^2}{K^2}$$

fully corresponds to equation (1) given in the review.
Equation (18a) can be initially expanded as

$$\frac{\partial z_0}{\partial x}+\frac{\partial y}{\partial x}+\frac{\partial\left(\beta\frac{Q^2}{2gA^2}\right)}{\partial x}+\frac{1}{g}\frac{\partial\left(\frac{Q}{A}\right)}{\partial t}=-\frac{Q^2}{K^2}$$

Substituting for

$$\frac{\partial z_0}{\partial x}=-S_0$$

and further expanding the derivatives:

$$-S_0+\frac{\partial y}{\partial x}+\beta\frac{Q}{gA^2}\frac{\partial Q}{\partial x}-\beta\frac{Q^2}{gA^3}\frac{\partial A}{\partial x}+\frac{1}{gA}\frac{\partial Q}{\partial t}-\frac{Q}{gA^2}\frac{\partial A}{\partial t}=-\frac{Q^2}{K^2}$$

after rearranging the terms and substituting for

$$\frac{\partial A}{\partial t} = -\frac{\partial Q}{\partial x}$$

from the continuity of mass equation with null lateral inflow or outflow, one finally obtains:

$$\frac{\partial Q}{\partial t} + \left(gA - \beta \frac{Q^2 B}{A^2} \right) \frac{\partial y}{\partial x} + (1+\beta) \frac{Q}{A} \frac{\partial Q}{\partial x} = gAS_0 - gA\frac{Q^2}{K^2}$$

It is not difficult to show that equation (1) is identical to the obtained one when the average flow slope is defined as:

$$\bar{S} = \frac{A}{\beta QB} \left[\frac{gA^2}{Q^2} S_0 + (\beta - 1) \frac{\partial Q}{\partial x} \right]$$

Therefore, the two equations are the same, but there is a reason why we did not approach the problem as suggested by the reviewer, namely by starting from and simplifying equation (1). This is due to the findings described in a recent paper by Gasiorowski and Szymkiewicz, (2007), where these authors show that due to the nonlinearity of the terms under the spatial derivative, while the direct integration of the momentum conservation (corresponding to equation (18a) of our manuscript) leads to a conservative scheme, its expansion does not.
We would finally make the reviewer aware that, with respect to β we did not disregard some other term: equation (23) of our manuscript includes all the terms. We essentially showed the results of equation (21) only because the two equations lead to practically identical results.

Reviewer

There is one method they did not test, although it is in the paper they referred to (Fenton 1999), and is a more sophisticated version is presented where a differential equation for the discharge is obtained.
Although Fenton found that it gave very little better results then an explicit approximation for the one example he presented, it should probably be tested in a paper that purports to examine all.

Authors

We agree. We did not test it because the expression involves higher order time derivatives and we thought that these derivatives could lead to a higher degree of instability. Nonetheless, we will certainly take into account the expression in the revised manuscript.

Reviewer

Having said that, I think the authors are in a position to come up with a method that might solve most of the problems. Rather than considering the presence of the time derivative a problem, I think they should turn it to their advantage. That is, turn equation (1) into an ordinary differential equation and solve it:

$$\frac{\partial Q}{\partial t} = \beta \frac{Q^2 B}{A^2} \bar{S} - gA\frac{Q^2}{K^2} - \left(gA - \beta \frac{Q^2 B}{A^2} \right) \frac{\eta_2(t) - \eta_1(t)}{x_2 - x_1} \qquad (2)$$

Now, there is no essential analytical approximation being made. All the dynamic terms are included, and they are calculating the spatial derivative accurately, which otherwise is the only real problem. All terms on the right side are either able to be calculated, $B(\eta(t))$ for example, or are part of the problem, namely $Q(t)$. I think this could work quite well.

Authors
We might, for the interest of the reviewer, test the results produced by equation (2), but we would not want to officially commit ourselves, since this alternative idea still starts from an expanded momentum equation, with the limitations in the conservation of momentum expressed above. The approach might probably work if the integration in time was made in terms of the volume stored in the reach (as in Muskingum) rather than in terms of discharge. In any case equation (23) of our manuscript includes all the terms with the only reasonable approximation that within the reach the variation of discharge is not significant (which is exactly the same approximation of the reviewer's equation (2)).

Reviewer
I think the authors have not considered the problem of calibration of the $K(\eta)$ *as well as they might.*

Authors
We dealt with the calibration problem in a previous paper, but we take the point and we will include the callibration issue as well.

Cited Literature
D. Gasiorowski and R. Szymkiewicz, (2007). Mass and momentum conservation in the simplified flood routing models, Journal of Hydrology, 346, 51– 58

Specific comments:

- *""Chow (1959)" – please include a page number. I think it should be compulsory for all book references in all papers to come with specific locations included. I can't believe that we don't all do it".* We will include the page number in the reference, although it is not required in HESS guidelines for manuscript preparation.
- *"DyRaC – I HATED the name. It is ugly and it is not necessary. Call it anything, like Stage-Fall- Discharge or something, but please not a name that means nothing. And, I will point out, the very characteristic of the authors' method and equation (F&K, Eqn 2.4) here is that it is not dynamic -it is based on an approximation where dynamic effects have been neglected."* We would like to remind the reviewer that equation (23) incorporates all the terms of the momentum equation, including the dynamic ones. We understand that someone may dislike it but we still think that the given name is appropriate.

- *“I don’t like the use of z for the free surface. I think of it as an independent variable. Maybe η, ζ...”* Given that the approach is mainly oriented to practical applications, we think that the symbols must be the ones that are more commonly used by practitioners rather than scientists. It is in fact a common practice in hydraulic modelling and in particular in practical hydraulic applications to use y as the water depth, while $z = y + z_0$ represents the free surface water level above a horizontal datum, and z_0 the cross section lowest point, still referred to the same horizontal datum.
- *“Reference to (21) - equation number seems wrong.”* We will correct this error.
- *“Reference to Eq. (14) – it was presented without much explanation.”* Agreed. We will improve the explanation
- *“Eq. (23) was always used instead of Eq. (25) The incorrect equation numbering is frustrating.”* We will correct this error.
- *“Also, I really do not like the style, favoured by the ASCE, of using "Eq. (123)" – writing it as a sentence to read is much nicer: "equation (123)".”* We just followed HESS guidelines for manuscript preparation.
- *“What hasn’t been explained or tested is how roughness would be estimated.”* As written in the general comments replies, we will add the description of an operational procedure for estimating the roughness coefficient.
- “*Is equation (25) not just a complicated way of calculating the mean?”* The main scope of this paper is to show that the final resulting equation can be installed in a microprocessor. We have discussed this issue with a company producing instrumentation. As a matter of fact, equation (25) is nothing else than a recursive computation of the mean; the use of this version allows estimation of the mean on a microprocessor only using the previous mean and the new observation, without the need of keeping track in memory of all the previous observations.