The referee's comments for the Editor of *Hydrology and Earth System Sciences*

on the paper entitled

FRAMEWORK FOR ASSESSING LATERAL FLOWS DURING FLOODS IN A CONDUIT-FLOW DOMINATED KARST SYSTEM USING AN INVERSE DIFFUSIVE MODEL

by Cybèle Cholet, Jean-Baptiste Charlier, Roger Mussa, Marc Steinmann and Sophie Denimal

General comments

In the submitted manuscript the time and space variability of lateral exchanges for flow and dissolved matter in karst conduit network is considered. According to the Authors information "a framework giving new keys ..." is proposed. To my mind the new keys are dealing with application of the advection - diffusion equation for both 1D unsteady flow with free surface and solute transport. However, the Authors assumed a priori linearity of both considered processes. This causes that a superposition is valid (separation of the base flow and the base solute transport can be done) as well as the convolution approach can be applied. In the case of flow such assumption is not obvious as the kinematic wave celerity involved in the diffusive wave equation depends on the unknown discharge. Maybe this requires additional Authors' comment. Consequently, because of the assumed linearity both problems unsteady flow and unsteady solute transport are analyzed using a uniform approach because both problems are described using the same type of equation. When the observations at the upstream end and at the downstream end are known then determination of the lateral inflow/outflow constitutes some kind of inverse problem. The problem is solved using analytical techniques applied to the advection-diffusion transport equation describing both flow and transport.

My general conclusion is as follows: the manuscript is interesting contribution dealing with application of the similar mathematical description in the form of advection – diffusion equations for two different processes for which some kind of inverse problem is solved. However, before the final decision of the Editor, minor revision of the manuscript should be carried out.

Specific comments:

Page 2, line 35 and page 3, line 1:

“... a simplification of the full SVE, and is even a higher order approximation than the uniform formulae (i.e. Manning’s formula.”

To my mind it is impossible to compare both mentioned cases of flow as they are incomparable. The diffusive wave is dealing with unsteady flow while the Manning formula describes steady uniform flow.

Page 3, lines 7-9

“Combined with Manning’s equation or Chézy equation the DW can be simplified to one single equation (Mussa, 1996; ...)”.

The diffusive wave equation in the form of advection-diffusion transport equation is derived using original differential continuity equation and the simplified momentum equation only. If we use the continuity equation and the Manning’s equation, i.e. the simplified dynamic equation in which only the gravitational and friction forces are taken into account, then one obtains another type of simplified flow equation, namely the kinematic wave equation.

Page 3, lines 10-12

In the sentence presented in these lines is stated that “..., an analytical solution unconditionally stable of the Hayami model exists Mussa (1996).”

This is incorrect because the question of stability or instability of solution of the differential equations is related to the numerical methods applied for their solution but it has nothing in common with the analytical solution.

Page 3, line 20

The Authors use the term “the advection-dispersion equation”. It seems to me that it would be better if they used rather the term “advection-diffusion equation” as it is commonly applied in mathematical physics. Note that the term “dispersion“ has triple meaning in hydromechanics. One of them is related to the groundwater flow. Regardless on the roots of diffusive term in the transport equation and its physical interpretation, from the mathematical point of view it is the diffusive term.

Page3, lines 24-27

To my mind the presented comment is written imprecisely. Although the diffusive wave equation and the advection-diffusion transport equation are very similar being of the same type, it is worth to remember that they were obtained in completely different ways. The advection-diffusion transport equation was derived starting from the mass conservation principle applied for matter dissolved in the water and taking into account two basic processes of transport: advection and diffusion in which the Fick's law leading to the diffusive term was applied. As far as the diffusive wave equation is considered, the continuity differential equation and the simplified dynamic equation were combined. The diffusive term appeared as a result of mathematical transformations, not as a flux representing typical physical diffusion. Summarizing, in such a situation it is hard to tell that the diffusive wave equation is applied for solute transport. Both phenomena are treated using the same mathematical approach as the governing equations represent the same type.

It seems to me that the process of "mass propagation" does not exist. Rather the propagating wave causes transport of dissolved matter.

Page 4, lines 14-16

The explanations given in these lines are incorrect. A unique solution of Eq. (1), which is of parabolic type, requires appropriate additional conditions imposed at the limit of the solution domain ($0 \le x \le L$ and $t \ge 0$). Solution of Eq. (1) with only one boundary condition, as stated by the Authors, is impossible. Of course, Hayami respected the required conditions. He assumed the following domain of solution: $0 \le x < \infty$ and $t \ge 0$. The initial condition was: for $t=0$ $Q(x,t)=0$ for $x \in \langle 0, \infty)$ whereas, two boundary conditions are as follows: for $x=0$ $Q(x,t)=\delta(t)$ and for $x=X\to\infty$ $Q(x, t)=0$ is the Dirac delta function. Consequently, he obtained the following solution:

$$Q(x,t) = \frac{1}{2\sqrt{\pi \cdot D}} \frac{x}{t^{3/2}} \exp\left(- \frac{(C \cdot t - x)^2}{4D \cdot t} \right) \tag{R.1}$$

Since the initial and boundary conditions assumed by Hayami correspond to definition of the impulse response function, then with any open channel reach of length $x=L$ can be related the following impulse response function:

$$K(t) = \frac{1}{2\sqrt{\pi \cdot D}} \frac{L}{t^{3/2}} \exp\left(- \frac{(C \cdot t - L)^2}{4D \cdot t} \right) \tag{R.2}$$

Note that this equation corresponds to Eq. (5).

On the other hand, each linear dynamic system described by a differential equation can be described alternatively, using the convolution:

$$O(t) = \int_0^t I(t-\tau) \cdot K(\tau) \cdot d\tau \tag{R.3}$$

where I(t) is the input function, O(t) is the output function whereas τ is dummy parameter. Summarizing, the linear dynamic system can be described either by the differential equation or by the convolution. Both representations are equivalent, what means that the downstream hydrograph can be obtained via numerical solution of appropriate differential equation (Eq.1)) or by computation of the convolution integral (Eq. (4) for known kernel function K(t).

My question is following: which reasons decided that instead of direct solution of the diffusive wave equation the Authors preferred using of the convolution approach? It is well known that numerical solution of the linear advection-diffusion equation, particularly when the diffusion is sufficiently strong, is not a problem.

Another question - when the time interval in which the flow is considered is very large, i.e. when time t, being the upper limit of the convolution interval, is increasing while computation then the problem of the system's memory occurs. It is well known, that the memory of real dynamic system is limited and finite so that an input from distant past does not influence the output at the moment. Another speaking, the flow memory corresponds to time elapse of the kernel function. In such a case the convolution (R.3) should be written rather as follows:

$$O(t) = \int_0^p I(t-\tau) \cdot K(\tau) \cdot d\tau \qquad \text{(R.4)}$$

where p is a memory of considered system. Certainly the Authors had to face this problem during computations and they had to solve it. It seems to me that a short comment on this question would be interesting for the readers.

Page 5, line 7

It seems to me that Eq. (9) is written incorrectly. If time t is the upper limit of integral then integration of the function q(x, t) cannot be carried out with regard to space x.

Page 6, lines 23-24

If the coefficients C_Q and D_Q corresponds to water flow then the coefficients C_M and D_M should correspond rather to solute transport than to mass fluxes. Similar improper terms are used in other places of the manuscript as well.

Moreover, because C_Q represents the kinematic wave celerity then it can be related to the advection velocity (flow velocity) occurring it the advection – diffusion transport equation. As it is well known, for the Manning formula one has

$$C_Q = \frac{5}{3} C_M .$$

Some results presented in Fig. 8 seem to confirm this relation. This fact allows us reducing of the total number of optimized parameters.

Page 12, lines 29-30

The presented sentence contains the same mistake as discussed above (see Page 3, lines 10-12). The problem of solution stability or instability has nothing in common with the Hayami solution, which is an exact solution of the diffusive wave equation. Moreover, numerical methods introducing numerical instabilities are rather not interesting.

Technical corrections

Page 3, line 12

It seems to me that in this case instead of “Mussa (1996)” it should be rather (Mussa, 1996).

Page3, lines 26

Instead of “by (Mussa, 1996)” it should be rather “by Mussa (1996)”.