

Interactive comment on “Ensemble modeling of stochastic unsteady open-channel flow in terms of its time-space evolutionary probability distribution: numerical application” by Alain Dib and M. Levent Kavvas

Anonymous Referee #1

Received and published: 31 August 2017

General comments:

The paper compares the numerical results of PDFs of the state variables of an unsteady open-channel flow to Monte Carlo reference simulations. The PDF equations are derived in a companion paper. The mean discharge, the mean flow velocity, the mean flow depth, and the PDFs of the discharge are compared in detail.

The comparison of the numerical results of the method derived in the companion paper is of interest, but some important points are missing, which will be addressed in the

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comments below.

Specific comments:

Please shorten your manuscript. It has too many repetitions and some statements are obvious.

In the abstract, on l. 1, you state that you use a "newly proposed Fokker-Planck Equation (FPE) methodology", whereas on p. 2, l. 32, you state that such a methodology has been applied many times. Please be more specific about what exactly is new about your methodology.

Please provide more information about the numerical setup of the MC simulations, like details about the finite-difference scheme, time step, grid size, parallelisation, ...

The paper will benefit from a plot of the standard deviation of the discharge dependent on the position and the time, analogue to figure 1. Please add such a plot.

Your choice for 1000 Monte Carlo realisations seems arbitrary. This is especially problematic when comparing the computational times of the MC simulations and the FPE simulations. Please add a comparison of MC simulations with fewer realisations, resulting in about 7 hours of computational time, like the FPE simulations, to the MC results with 1000 realisations. If one is only interested in the ensemble averages, it is very likely that fewer ensemble members will be sufficient for accurate results. This implies that you compare the results for the ensemble averages and also for the standard deviations with 1000 and fewer realisations. Maybe also add the results for 500 realisations. Furthermore, MC simulations are predestined for parallelisation. Neither did you write if the MC simulations were computed in parallel, nor did you incorporate this in your comparison of the computational times between the two different approaches. How difficult would it be to parallelise the numerical scheme of the FPE method?

On p. 11, l. 20 you write that the numerical errors caused by the spatial and temporal discretisation could lead to discrepancies in comparison to the MC results. Please

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check this by performing simulations with higher and lower resolutions of the spatial and temporal discretisation.

Please comment on the implications for applications, like flood forecasting, of the errors made by the FPE approach.

On p. 13, l. 12-13 you state that it is an advantage that the simulation can be performed in only one run, but you do not motivate why this is an advantage. For parallelisation, it is even a disadvantage.

The discussion of the results lacks some points or is partly in contrast to what the figures show. Figures 3c,d show a slight decrease of discharge at early times with the FPE method, how do you explain that behaviour? Please explain the offset of the standard deviation at time $t=0$ for the FPE results. In figure 8c, the FPE result does not reproduce the decrease of the standard deviation. How do you explain the variation of the standard deviation at early times from the MC simulations in figure 8d? Figures 9 and 10 show more of a qualitative match of the results, than a quantitative match.

Your outlook on p. 13, l. 16-20 rather belongs to the companion paper. What about faster or more accurate numerical schemes? How could the discrepancies be reduced?

Technical corrections:

The tense of your abstract makes it read like a summary, please change the tense accordingly.

In the abstract you write that the total simulation period of the FPE method is smaller than that of the MC approach. You certainly mean computational time.

Check the indentations at the beginning of chapters and after equations, please delete them.

On p. 2, l. 31 you write that you do not limit the working space of the parameter

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space. What about parameter combinations, where the neglected cross-covariance terms become large or the system shows a memory?

You write about the conservation of particles, although no particles were introduced in your paper, please reformulate.

On p. 2, l. 6 you write that parameters become random through uncertainties. You should write that the parameters are formulated as random functions in order to capture the uncertainties.

How did you ensure that Manning's coefficient never fell below 0.01, as described on p.6, l. 27?

The arguments of a PDF are usually separated by a semi-colon into arguments for which the PDF is a density and normal arguments, e.g. Pope (1985).

The notation of the m-dimensional delta function is confusing, drop the exponent m.

P. 9, l. 11: What other form of energy, besides kinetic energy is dissipated due to shear stresses?

P. 9, l. 18: change "very minimal"

P. 13, l. 9: I do not think that the results for the PDFs are satisfactory in general. In some cases they are, in others they are not.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-394>, 2017.

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