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Hydrology and Earth System Sciences Discussions

Interactive comment on "An opportunity of application of excess factor in hydrology" *by* V. Kovalenko et al.

Anonymous Referee #1

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In my opinion the manuscript cannot be accepted for publication in HESS in its present version, and a profound rethinking is needed to meet the standard of publication in international journals. The main points which detract from the quality of the manuscript are listed below.

1) The quality of the English usage is so bad to make many of the concepts almost incomprehensible for the reader. As a non-native English speaker I fully understand the difficulties of the Authors to convey complex concepts in a language which is not theirs, but still communication is an essential step of science, and people will not be able to understand without efforts aimed at improving the readability of the paper. Professional translation services are available for scientists of any nationality, and these services

should be mandatorily used in this case. Due to the bad quality of presentation, some of my comments below may therefore be due to a lack of comprehension of the basic concepts, despite several reading and re-reading of the manuscript for trying to follow the reasoning by the Authors.

2) The manuscript opens a window on the scientific research carried out in Russia in the last two decades, which of course could be very interesting. However, if the Authors want to have their manuscript published in an international journal, they should put their methods and results in the context of international research: it is not acceptable that all references are self-citations or Russian documents, as if researchers from other countries had never approached this topic. Actually, there is a vast literature about the application of continuous-time or discrete-time differential stochastic equations to hydrology, see eq. (1) and (2) in the manuscript. To cite only some books, one can mention: Singh, V.P., Hydrologic Systems: Vol. I: Rainfall-Runoff Modeling. 480 pp., Prentice-Hall, Englewood Cliffs, New Jersey, l988; J.D alas et al, Applied modeling of hydrologic time series, Water Resources publication, 1980. These are just examples, but many other books and journal papers have been published on this topic: a quick search on ISI Web of Science or Scopus will allow to significantly strengthen the manuscript by putting it in a correct international context.

3) In passing from equation (1) to (2), the Authors introduce white noise in the coefficients: several kind of white noise exist, including Gaussian white noise, white shot noise, etc. The Authors should specify that they concentrate on Gaussian noise only.

4) The relation between the Langevin equation (2) and the Kolmogorov-Fokker-Plank equation (3) is not univocal under white noise forcing: are the Authors using Ito or Stratonovich stochastic calculus to interpret the white noise term? The mathematical form of equation (3) apparently implies the use of Ito formula, but this may should be better supported, because Ito interpretation has been criticized for lack of physical meaning (see for example Van Kampen, Stochastic processes in physics and chemistry, Elsevier, 2007).

5) My personal answer to question (1) raised by the Authors at page 13640, line 4-5 (“Why in engineering hydrology no one never lifted a question on instability?”) is the following: models can be very useful but we cannot confuse reality with model outcomes: the instability of higher-order moments may be a clue of the fact that the model (2) is not correct in some situations, due for example to a lack of linearity in basin response, or non-gaussianity of the noise terms. Is this what the Authors want to convey with their equation (12)? Sorry but I was really unable to follow the reasoning by the Authors here.

6) I was not able to understand how the excess factor has been estimated in Table 2: what is the practical proposal by the Authors? Do they attempt to infer the value of the excess factor from the intensity of the multiplicative noise? But how is this intensity estimated? Do the Authors claim that this estimation method is more efficient than the standard one (page 13643, lines 20-23)? This part of the paper in particular requires a complete revision, in the present version of the manuscript it is not comprehensible at all.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 13635, 2012.