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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.

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The reviewer complains about poor quality translation of the article from Russian to English. In his opinion, this makes correct understanding of the article more difficult. At the same time the reviewer provides specific comments:

1. It is necessary to reference foreign (non-Russian) authors, involved in the use of stochastic differential equations in hydrology. This "would put the paper in the proper international context."

2. Since there are several types of white noises, then an indication should be made that the Gaussian white noise is meant in the article.

3. Since the connection between the Langevin (2) and the Fokker-Planck-Kolmogorov (3) equations is ambiguously defined by white noise, then it should be specified in what sense the last equation (either Ito's or Stratonovich's) is interpreted. Since (in the reviewer's opinion) the article implies the use of Ito's formula, it is necessary to justify the fact that such an interpretation was criticized for the lack of physical meaning. In the review, reference is made to the work of Van Kampen ÂńStochastic Processes in Physics and ChemistryÂż, Elsevier, 2007.

4. The reviewer gives his answer to the question raised in the article (p. 13 640, lines 4-5 "Why in engineering hydrology no one has ever raised the issue of instability?"). He believes that the instability of the solutions of the model reveals shortcomings of the model (its linearity, the Gaussian nature of the noise, and so on). 5. The reviewer points out that he does not understand how kurtosis (excess) coefficient was estimated in Table 2 and what is the authors' practical proposal. He also points out that the authors try to derive the value of the kurtosis coefficient from the intensity of the multiplicative noise. But what is this intensity, roughly? Authors (allegedly) claim that this method of estimation is more effective than the standard, based on the processing of series of observations. This part of the article is not clear to all (the reviewer generalises his own opinion).

Author's responses are as follows. As for the quality of the translation, it is accepted that translation makes understanding the article more difficult. But after re-reading our responses to the reviewers' comments, the panellists might agree that the blame might lay not only on the objective factors related to the quality of the translation, but also on reviewer's subjective perception of the content. The answers that follow numbered are the same as stated above.

1. Of course, the absence of such reference is our omission. However, these references would not be of direct relevance to the article topics, but instead be a formal sign of respect to foreign hydrologists, involved in applications of the stochastic models. But even without this strictly formal attribute, the role of foreign scientists in the article is

shown in the most striking manner in the name of its main model ("Fokker–Planck–Kolmogorov equation"). The names of prominent international scientists are put first, when Kolmogorov appears only in the third. In the review Kolmogorov is, for some reason, placed first. This is unfair in the "international context."

When deciding to publish the article, editors of HESS magazine advised us to refer to foreign publications of Efraín Antonio Domínguez Calle (Colombia). But when they have learned that this researcher was a former postgraduate student of one of the authors (V.V. Kovalenko), decided not to insist on the suggestion.

The creation of the article was not motivated by the works of foreign hydrologists, but was discovered in Russia because of the fact of instability of the solution equations for moments (in the article - the system (4)). It is to this fact reference is made ("self-citations").

2. Of course, this article talks about Gaussian mutually correlated white noises. But it is such a clear "default". Our response to paragraph 3 shows the reasons for this. 3. The reviewer is certainly right noting that the relationship between the Langevin and Fokker-Planck-Kolmogorov equation is ambiguously defined. But this ambiguity does not affect the form of equation (3) itself, but only the form of expressions of drift and diffusion coefficients (to be exact – just the drift coefficient). However, in the article these expressions are not presented for the reviewer to conclude that the article means the use of the Ito's formula. The equation (2) also does not give the opportunity to make such conclusion. In the equation (2) the noise is not only additive (N), but is also multiplicative (member (c + c)Q, which, although, has a misprint (c – c), but has absolutely no effect). Regardless of the previous points, the record of system (4) clearly points to the fact that:

a) it is a question of correlated white Gaussian noises;

b) the model clearly has not only additive, but also multiplicative noises present as a product i(c –0.5iGc)mi, where i – the order of the moment (the second equation of

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system (4) also has a typo –2(c –0.5Gc)m2 instead of –2(c –Gc)m2, but is not of crucial importance in the present context).

It is the presence of multiplicative noises that indicates that the whole situation is interpreted in the sense of Stratanovich and not of Ito. In this paper, these details are not presented as they have become classics in Russia (debates were started nearly 50 years ago by a foreign publication (Gray A.H., Caughey T. K. “A controversy in problems involving random parametric excitation”, Jour. Math. And Phys., 1965) and the long ago have ended in favour of Stratonovich. Therefore, the reviewer’s proposal to refer to work of Van Kampen in 2007 is somewhat puzzling...

4. Authors fully agree with the reviewer’s conclusion: the instability is associated with the model itself, namely the solution is unstable. But the question in the article is of entirely different aspect: why no one yet has paid attention to the instability of the solutions of the model, which leads to the family of Pearson distributions, used in hydrology. The question is rhetorical for the authors. The answer is implied in the article because the model (4) has not been considered in hydrology (either Russian or foreign), although, the stochastic theory has been used it for a long time. In hydrology typical approach was to aggregate series of observations. The need for such a model appeared, when it was necessary to assess the probabilistic characteristics of the flow in a changing climate. As far as the authors are aware, this issue of instability was first discovered in the Russian State Hydrometeorological University.

5. The author in his article did not give a specific formula, which would relate the numerical value of the coefficient of kurtosis (excess) with the intensity of multiplicative noise. But the effect of the intensity of the noise on the "tails" of the distribution is shown: they rise. Since Gc is part of the expression for the coefficient b2 in the Pearson equation (5), as it determines the fourth initial moment and hence the kurtosis (excess) coefficient. It is natural that between the excess and the intensity of the multiplicative noise (or rather its relative intensity Gc/c) there is a very direct relationship as shown in Fig. 1, based on the Pearson equation solution (5). The value of this relative intensity

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(in the article, it is denoted by betta = Gc/c) lies in the open interval (0, 2) and when betta >0.5, river flow (within the boundaries of the simple Markov processes theory) is formed unstably by the fourth initial moment, and hence by the coefficient of excess (this is present in almost half or may be more of river basins in the world.)

Now to the answer the question "what is the practical suggestion of authors?" The suggestion lies in the fact that the problem of instability, although discovered when modelling long-term hydrological impacts of climate change, also exists in hydrological regime of long-term flows. It was simply unnoticed, because, firstly, actual series of observations were used instead of the model of density forming of probability (for example in the form of system (4)). Secondly, the fourth moment (and hence the excess coefficient) was ignored, because of the horrendous accuracy of its determination, when calculated with standard formulae, shown in the paper. However, the authors noticed (empirically) that the calculated values of hydrological characteristics hardly change after a certain number of years of observation. For the kurtosis coefficient this happens after about 50–60 years of observation. Further increases of this variable, in error range of 8%, do not change coefficient's numerical values (in the article an example is given in Fig. 2). This opens the possibility (at least for basins with such long sequence of observations) to introduce this attribute into the practice of engineering hydrology. What does it lead to? It leads to potential increase of secured value costs (based on which hydro facilities are projected) for some river basins. Thus the cost of facilities, unfortunately, increases, but so does their reliability.

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