Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-108-AC3, 2019

Geoscientific Model Development Discussions

Interactive comment on "The probabilistic hydrological model MARCS (MARkov Chain System): the theoretical basis for the core version 0.2" *by* Elena Shevnina and Andrey Silaev

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Received and published: 1 March 2019

Anonymous Referee #2 formulates three issues inherent to the manuscript: it presents the method which is not a new (1), it describes the method well known by the international hydrological community (2) and it does not include discussion on the method's limitations (3). We are following the comments of the Anonymous Referee #2 to present our vision of the tasks of the manuscript.

Anonymous Referee #2: "The article is not original in terms of research methodology. The authors describe the method for assessing the hydrological repercussion of climate change developed at the Department of Hydrophysics and Hydrological Fore-

casts of the Russian State Hydrometeorological University (RSHU). The theses from the textbook of V.V. Kovalenko are the main content of the article (Modelling of hydrological processes, Gidrometeizdat, St. Petersburg, Russia, 1993). It should also be noted that the method from article is taught to students in the undergraduate program RSHU and the method is described in textbooks and methodological recommendations for students with a "step-by-step" algorithm for obtaining results (see for example Practical tasks on the discipline "Hydrological forecasts" http://elib.rshu.ru/files_books/pdf/rid_00d41c4c01bd4db7a25f15faacf9705d.pdf, 24 – 28)."

Authors' comments: The method is not a new (see line 69, p. 3). However, despite the fact that the method has long history in the Russian State Hydrometeorological University, it not yet known in a hydrological modeling community. The method needs to knowledge on the theory of automatic systems, which is not among traditional disciplines for hydrologist and water resources managers. The method still rises many questions from the hydrological modeling community (see the discussions to Shevnina et al., 2017: https://www.hydrol-earth-syst-sci.net/21/2559/2017/hess-21-2559-2017-discussion.html and to Shevnina et al., 2018: https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-473/. The discussion of this "model description paper" is too long because the "an unusual statistical approach" is applied, and the text of the manuscript is "mathematically enriched". In fact, our task is to present the formulas coded in the MARCS model version 0.2, not the AFA method itself. By now, the formulas of the model core version 0.1 is only published in the Annex to Shevnina et al., 2017.

Anonymous Referee #2: "The authors assert that this method is presented for the first time in English, contains no typing errors and the calculation formulas are obtained "step by step". These statements can be disproved. 1. There are publications that contain a description of the method under consideration, and in some sources the presentation of method is clearer than in this article. The methodological approach was developed more than 30 years ago. Since that time, it has been tested in many

world catchments. Its methodology is applied and developed in countries such as Russia, Colombia, Bolivia, Côte d'Ivoire, Mali, and others. The results are published in journals that are part of the world's scientific bases. The authors probably spent little time to get acquainted with published works. 2. The authors mainly refer to the textbook of V.V. Kovalenko, 1993 (Modelling of hydrological processes, Gidrometeizdat, St. Petersburg, Russia, 1993), but this textbook was complemented and reissued in 2006 (Kovalenko, V. V., Victorova, N. V., Gaydukova, E. V.: Modelling of hydrological processes, the Russian State Hydrometeorological University press, St. Petersburg, Russia, 2006). In the reissued version of the textbook, the typos contained in the 1993 textbook were found and corrected. 3. The algorithm given by the authors skipped some important steps. The main skip is the absence of the dynamic core, from which the stochastic equation is obtained. Probably, the authors have done this intentionally, since it is the dynamic core that causes the discussions.

Authors' comments: The main task of the manuscript is to present the formulas for the MARCS model core version 0.2. The FPK approach has much broader framework, and more details will be given in following publications in English. (1). The FPK equation approach is used on hydrological studies of river basins located in Russia, Colombia, Bolivia, Mali, etc. However, the majority of studies are published in Russian only, these studies result to PhD theses defended in the Russian State Hydrometeorological University. We do not include them to the list of references, which is already long. We have refereed to publications in international journals (Viktorova and Gromova, 2008; Domínguez and Rivera, 2010; Kovalenko, 2014; Rosmann and Domínguez, 2017; Shevnina et al., 2017, etc) as well as to original studies in Russian critically needed to formulate the MARCS model core version 0.2 (Kovalenko, 1993; Pugachev et al., 1974). (2). It should be noted, that the reprint of Kovalenko (1993) published in 2006 contains even more typos in the formulas was well as miss meaning statements than the original work. For example, the formulas 4.1 on p. 189 contain the notations for summands that used only once, they are not discussed in the following text. The same formulas are given in p. 245 without these summands. The third equation on p. 247

contains the typo, while this formula is given correctly on p. 191. The statement on p. 191 is that a, b0, b1 and b2 are parameters of the FPK equation, however they are the parameters of the Pearson equation (Andreev et al., 2005). We included some pages from Kovalenko et al. (2006) to see the cases of typo mentioned above a as the supplements to our answers to Anonymous Referee #2). (3). In our opinion, the text contains enough formulas and does not skip critical steps in the narration of the AFA method. In particular, the Eq. 3 and Eq. 4 (p. 4) are dynamic and stochastic equations behind the model MARCS. The notations in the Eq. 3 are differ from the original text in Kovalenko (1993) as well as Kovalenko et al. (2006). It is not clear what Anonymous Referee #2 means while mentioned the "absence of dynamical core"? Is it "core" of the algorithm, the method or the MARCS model? In any case, in the revised version of the manuscript we moved the description of the AFA method from the Section 2 to the Annex. We hope, that it would helps better present the MARCS model core version 0.2.

Anonymous Referee #2: "But the most important remark is that the article proposes to use calculation formulas that can result in unstable solutions, especially about the third moment (skewness coefficient). In 2010, recommendations were issued (Kovalenko, V. V., Victorova, N. V., Gaydukova, E. V., Gromova, M. A., Khaustov, V. A. and Shevnina, E. V.: Guideline to estimate a multi-year runoff regime under non-steady climate to design hydraulic contractions, Russian StateHydrometeorological University Press, St. Petersburg, 2010. http://elib.rshu.ru/files_books/pdf/img-504161958.pdf) in which a model was presented that allows one to obtain reliable solutions of the Fokker – Planck – Kolmogorov equation. From this model, multiplicative noise was removed, which results in reliable solutions. Also, the stability of solutions can be achieved by transferring the multiplicative noise to the additive component of the equation or another way – by increasing the number of phase variables taken into account by the model. The authors propose to apply calculation formulas that can give unstable solutions without checking whether it is possible to trust the obtained results. A method for checking the stability of solutions of the this prognostic approach has long been known. I hope that

in future studies, the authors will take this into account. In this form, as in the article of the authors, the formulas are dangerous to use because of the probability of obtaining unreliable results."

Authors' comments: In our opinion, the Anonymous Referee #2 claims to the unstable solutions of FPK equation: infinite of statistical moments of high orders. It limits application of the AFA method (Kovalenko, 1993), and it is discussed in Kovalenko (2004) http://elib.rshu.ru/files_books/pdf/img-417153826.pdf. The author suggests two ways resulted to the stable solutions of FPK. The first one is introduced in Kovalenko, 2004 and briefly described by Anonymous Referee #2. The second way is given in Kovalenko et al. (2010) and coded in the MARCS model version 0.1 (Shevnina et al., 2017). In the revised version of the manuscript we stressed the limitation of the model core version 0.2 and further direction to the model development.

Anonymous Referee #2: "I believe that the method of scenario assessment of the hydrological consequences of climate change considered in the article is relevant (since the fact of climate change is recognized by the world community and one should be able to assess the consequences of this change), credible if sustainable solutions are obtained (its approbation was carried out on numerous world catchments on retrospective material) and practically important (as it allows to obtain probabilistic characteristics of the hydrological regime)."

Authors' comments: We agree that the AFA method is relevant, however we believe that it needs to better formulations in English to be become well known tool for the international hydrological community.

In summary, three issues inherent to the manuscript should be stressed: it presents the new version (0.2) of the probabilistic MARCS model (1), it describes the formulas of the model version 0.2 together with the limitations inherent current version (2) and possible directions of the MARCS model development (3).

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-108/gmd-2018-108-AC3-supplement.pdf

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