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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

Anonymous Referee #2

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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 Forecasts 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_00d41c4c01bd4db

7a25f15faacf9705d.pdf, ÑĄŃĆÑĂ. 24 – 28). 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.

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.

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).

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2018-108/gmd-2018-108-RC2-supplement.pdf

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