We noticed that it was not easy to find Referees for our manuscript. The discussion of the manuscript is started 18 April, 2018 and, since then, many Referee's nominations were rejected. Finally, the comments of two Anonymous Referees and A. Frolov result to the revised text of the manuscript.

Answers to the potential review of the Anonymous Referee #1

General comments:

Comment 1: At a first glance, the paper show how competent the authors are in probabilistic hydrological models. Reviewer thinks that key aspects of this research are to provide the theoretical background of Markov Chain System. The manuscript is well written and logically structured. The extensive literature review is much appreciated as well.

Answer: *One formulates more precisely, the manuscript provides the basics of the statistical theory of automatic system (Pugachev et al., 1974), the simplifications behind to the Advance of Frequency Analysis (Kovalenko, 1993) as well as the equations used on the core version 0.2 of the probabilistic hydrological model MARCS (MARkov Chain System). In this manuscript, the authors not only translate the parts of two books with theoretical basis from Russian, but also try to formulate material logically and to provide the equations for the new core of the hydrological model. The theory of Markov Chain System is outside of the manuscript content, even it gives the name for the hydrological model with simple Markov Chain core (see the Eq. 2 in the manuscript). Only a couple of months ago, the authors realized that the model with name MARCS is already exists (http://marcs.astro.uu.se/index.php), however the official name is the MARCS – atmospheres. We would need to change the name of our probabilistic hydrological model (MARCS) or becomes to be involved to the MARCS model community with the probabilistic hydrological model MARCS – hydrosphere. It would probably needs to change the version of the core, status and content of the code during revision process.*

Comment: Even though the goal of the paper relies on the scope of GMD, the intuition of the approach is not clearly stated. Since the approach uses Markov chain system, for the recent scientific community, it may not be new. So, the reviewer suggests laying the objective of the paper in different way. Indeed, the authors showed much effort on the topic but there is not much about the model use and its description. The manuscript mentions the version of model is 2. Reviewer does not see properly how they are different. The assumptions of the model are not clearly stated. The paper is mathematically enriched. Sometimes, reader may lose the concentration due to inappropriate description of the technical jargons.

Answer: *We agree that the Markov Chain System approach is known by the recent scientific community and it is not a new. However, in this manuscript we attempted to explain the method used in the math "language", not on the intuition "language". On both "languages" it is not easy if the topic is on a boundary of two scientific disciplines (Hydrology and Statistical Radiophysics in our case). On the boundary, the therms may come from both sides to add or to complement each others, and it results to a specific jargon, which is noticed by the Referee 1. The back ground of the authors comes from the Hydrology (the frequency analysis and physical modeling) and the Radiophysics (the statistical theory of automatic system), and the explanations in the manuscript were given on the "language" in common. In our manuscript, we try to use the math equations as much as possible to prevent non-correct description of the method due to the difference in the therms. It results to the "mathematically enriched text". In this manuscript, the core version 0.2 was presented in details. The previous model version 0.1 is shortly described in the Annex to Shevnina et al. (2017) without any theoretical details, which we have promised to present in our next manuscript. To follow our promises, this manuscript fills the gap and provides the theoretical basis of the probabilistic hydrological model MARCS. In the revised manuscript we stressed these two circumstances.*

Shevnina, E., Kourzeneva, E., Kovalenko, V., and Vihma, T., 2017: Assessment of extreme flood events in a changing climate for a long-term planning of socio-economic infrastructure in the Russian Arctic, Hydrol. Earth Syst. Sci., 21, 2559-2578, doi:10.5194/hess-21-2559-2017.

Comment: The conclusion made in this manuscript seems to be the summary of the whole content. It may need revision posing future research and recommendation of this research. Right now the direction of this research is not clear. The reviewer suggests including some potential application beyond the water engineering even though the method is similar to Pearson type distribution. The extension of the paper will be better if the idea of posing such approach in space. Such statement shall be made clearly.

Answer: *We agree, that it is important to place the probabilistic approach amount others hydrological modeling approaches. The general view on this place is done in Shevnina et al., 2017 (Fig. 1), and the details are provided in Shevnina et al. (2018). In the revised version of the manuscript we add one figure to show how different scientific disciplines are overlap in the AFA approach. It should be noted, that the statistical theory of automatic system is adopted to be used for a seasonal prediction of water inflow to hydropower reservoirs by Domínguez and Rivera (2010) and Shevnina (2001). There are also more studies published in Russian whose not included to the list of References since it is already long. It does not include a number of oral and poster presentations and lectures. However, in revised version of the manuscript we extend the section of discussion. It helps to clarify the place of the approach among others as well as to suggest the direction of the MARCS model development.*

Domínguez, E., and Rivera, H.: A Fokker–Planck–Kolmogorov equation approach for the monthly affluence forecast of Betania hydropower reservoir, J. Hydroinform., 12(4), 486–501, doi: 10.2166/hydro.2010.083, 2010.

Shevnina, E.: Deterministic and stochastic models for seasonal forecasting of inflow to reservoirs of hydropower stations, PhD thesis, Russian State Hydrometeorological University, Russia, 188 pp., 2001. (in Russian).

Constructive suggestions

• Author mentioned three statistical moments in line 79. But these are not listed here. For general audience, reviewer suggests to list them.

Answer: we added the list of the moments in the revised text.

• Section 1.1 is very rich in mathematical expression. Only audience or practitioner with sound mathematical background easily understands. But for general audience, this section shall be revised in a simpler way…

Answer: We would like to keep the math "language" of the section, however we arranged the equations on other way: the revised text of the section 1.1 now included only the equations behind the model core, and the Annex provides the theoretical basis for the readers wanted to the details.

•Please briefly mention what kind of parameters are lumped one and why such is called.

Answer: we clarify the situation in the revised version of the manuscript.

• The reviewer wants to have implicit explanation of the secondary parameters like a, b, c and c, Gs. It is not clear how such empirical equations are related with either data or physics.

Answer: we try to clarify the situation in the revised version of the manuscript.

• Are the time-series data are daily or monthly or yearly as mentioned in line 227? It would be better to define the time scale.

Answer: the time series of runoff consists of yearly discharges, thus the time scale of the process considered is multi-year, long term. It was was stressed in the revised version.

• In order to make the paper strong, reviewer suggests having some key statistics pictorially. This means how the observed set and models are correlated. What is the degree of performance?

Answer: In this manuscript we presented only the core, not the validation procedure for the probabilistic hydrological model. The validation procedure is described in Shevnina et al. (2017)

and includes also figures and tables to show the degree of the model performance under two characterization schemes. Since the text is already long we refrained to add discussion of the model validation block (Shevnina and Gaidukova, 2017).

•Reviewer feels the paper is somewhat incomplete as in the several statements; the authors did not mention how future works will be proceed. They just envisioned about the future paper.

Answer: We agree, that steps of the future work were not described in the manuscript, and only the main directions were mentioned. However, now it is still difficult to outline a circle of potential stockholders for a probabilistic form of forecasts of river runoff. This form of forecast allows evaluation of extremes, which is important for risks assessment, in particularly in a design of building construction (Shevnina et al. 2017). In our opinion, it needs to find a common "language" with an Economic, and we have tried to do it in Shevnina et al., 2018. Recently, the direction of the development for the probabilistic hydrological model depends on the Academy of Finland.

Specific comments

• In line 90, comma is needed between features and which.

Answer: We revised the text.

• The authors mentioned in parenthesis ("the reference"). What does it mean? It seems the authors forgot to have proper citation. In line 158.

Answer: We added the explanation.

• There are three graphs in the paper however, they are not proper captions. In line 273 has Figure 2, but where is Figure 2?

Answer: We corrected the numbering of the figure.

The Referee #1 concluded that the manuscript "still needs further improvement as described below prior to acceptance".

Authors' comments to Anonymous Referee #2

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 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_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 text-book 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, b_0, b_1 and b_2 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:* $m_k \to \infty$ *for 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.*

Anonymous Referee #2 suggests how to improve the manuscript.

Authors' changes were implemented in the revised text of the manuscript:

Title: In the new test, the name of the model is the MARCS^{HYDRO} instead of MARCS. We realized that the model MARCS is already exists (http://marcs.astro.uu.se/index.php) with the official name the MARCS – atmospheres. Then, the abstract was rewrote to stress that our paper introduces a new version of the model. In the revised Introduction, the last paragraph explains our motivations to include the theoretical basis of the model core 0.2 into the model description paper.

Structure: We moved the theoretical basis of the AFA to the Annex 1. On our opinion it allows to introduce the Section 1.1 to "a general audience" (see the Constructive suggestions by Anonymous Referee #1) and, at the same time, to keep the math "language" in description of the AFA method behind the new model MARCS^{HYDRO} model. We supposed, that "general audience" are people working on development and evaluation of numerical models of hydrological system and its components.

Sections' content: To place better the MARCS^{HYDRO} model core version 0.2 into the structure of the model we added the Fig. 1 into the Section 1. Then, we discussed the features of the current version of the model such as the prediction on a climate scale, the low computational costs and the direction toward socio-economic applications in long-term risks assessment. Six blocks of the MARCS^{HYDRO} model were breathy introduced in the revised Section 1. The last paragraph of this section is now discussed the limitations of the current core version of the model according to the comments of two Anonymous Referees and A. Frolov. The specific comments by Anonymous Referee #1 in lines 90, 158, 273 were accounted in the text revision. Section 2 hasn't much changes, it describes details of the model set up, forcing and output for the case of the Iijoki River basin.

Discussion: The section of Discussions was extended, and now we have been trying to specify the model ability for an "express analysis" of water extremes in changing climate due to low computational costs and direct connection to social-economical applications. We stressed that the method behind the model is not a new (see the comment 1 the Anonymous Referee #2), but not well known outside the Russian hydrological community due to the lack of publications in international journals. In the revised Discussions we also focused on limitations of the method used as well as on the further development of the MARCS^{HYDRO} model (according the comment 3 of the Anonymous Referee #1).

Conclusion: We stressed the novelty of the core version 0.2 and gave final remarks about results obtained for the Ilijoki River.

Annexes: Recently, the Annex 1 provides the theory and limitations of the AFA for readers wanted for the details. Two tables with the notations used for the secondary parameters were also added into the Annex 1 according suggestion given by the Anonymous Referee #1

References: We have been trying to incorporate as much references as possible in the revised manuscript. However, some of them were not finally included to the revised manuscript to keep the section of References balanced 50/50 for Russian and English publications. In particular, we do not rise the discussion the comment 1 by A. Frolov (in the RC2) or the suggestions by the Anonymous Referee #2 since it needs to additional references to the regional studies or specific technical papers in Russian. However, three important references in Russian were added to the list of references to discuss the comment 3 of the Anonymous Referee #2.

The following references ***were added to the list***:

1. Veijalainen, N., Korhonen, J., Vehviläinen, B. and Koivusalo, H.L Modelling and statistical analysis of catchment water balance and discharge in Finland in 1951–2099 using transient climate scenarios, Journal of Water and Climate Change, Vol. 3, 55–78, 2012.

2. Willmott, C. J. and Robeson S. M.: Climatologically aided interpolation (CAI) of terrestrial air temperature. International Journal of Climatology, 15(2), 221-229, 1995.

3. Yip, Q. K. Y., Burn, D. H., Seglenieks, F., Pietroniro, A. and Soulis, E. D.: Climate impacts on hydrological variables in the Mackenzie River basin, Canadian Water Resource Journal, 37(3), 209–230, 2012.

4. Kovalenko, V. V.: Partial infinite modelling and forecasting of the process of river-runoff formation. St. Petersburg, RSHU Publishers, 2004. Available on-line: http://elib.rshu.ru/files_books/pdf/img-417153826.pdf

5. Sokolovskiy D.L.: River runoff (bases on a theory and methods of calculations). Leningrad, Hydrometeoidat, 540 p. 1968. (in Russian)

6. Shevnina, E.: Long-term assessment of the multi-year statistical characteristics of the maximal runoff under the climate change over the Russian Arctic, doctor of science thesis, Russian State Hydrometeorological University, Russia, 355 pp., 2015. (in Russian).

The following references ***were excluded from the list***:

1. Serinaldi, F. and Kilsby, C. G.: Stationary is undead: Uncertainty dominates the distribution of extremes, Adv. Water Res., 77, 17–36, doi:10.1016/j.advwatres.2014.12.013, 2015.

2. Hamududu, B. and Killingtveit, A.: Assessing of Climate Change Impacts on Global Hydropower, Energies, 5(2), 305–322, doi:10.3390/en5020305, 2012.

3. Obrezkov, V.I. (Eds.): Hydroenergy: a handbook for engineers, Energoizdat, Moscow, 1988. (In Russian).

4. Salvosa, L. R.: Tables of Pearson's Type III Function. Ann. Math. Statist., 1, 191–198, 1930.

5. Shevnina, E.: Changes of maximal flow regime in Arctic, Construction of Unique Buildings and Structures, 7(22), 128–1412014, 2014. (in Russian).

6. Shevnina, E. and Krasikov, A.: The probabilistic hydrological model MARCS (MARkov Chain System): the core code (Version 1.0), doi:10.5281/zenodo.1220096, 2018.

We thank to two Anonymous Referees and A. Frolov for their comments to our manuscript. We hope, that the new text allows better understanding of the MARCShydro model specifics as well as our motivations behind the submission this model description paper to GMD.

with the best regards,
Elena Shevnina and Andrey Silaev