Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2015-504-AC1, 2016

Hydrology and Earth System Sciences
Open Access
Discussions

Interactive comment on "Assessment of extreme flood events in changing climate for a long-term planning of socio-economic infrastructure in the Russian Arctic" *by* E. Shevnina et al.

E. Shevnina et al.

elena.shevnina@fmi.fi

Received and published: 16 March 2016

We thank the referee for his/her questions, comments and suggestions. They were very useful in further improving of the manuscript. The Referee's comments are copied below, and our responses are written after each comment.

General comments This paper presents an approach to estimate spring flood depth of runoff in a future climate using a stochastic modeling framework. The main focus of the paper is an application of this method to evaluate changes for the Russian Arctic. The method builds on statistical properties of historical data in combination with estimates of expected changes that are derived from climate model simulations of several generations. The authors conclude that increases in spring flood depths but decreases in its

variability are expected for much of the Russian Arctic. They also conclude that a main advantage of their approach is that it requires relatively little data and can be used in areas where detailed observations are not available.

The problem of estimating changing flood depths (or similarly, changing return periods of floods of a given magnitude) is certainly a challenge to engineering in many parts of the world. Stochastic approaches are quite common in engineering so the approach taken here could be possible to apply to infrastructure planning decisions. In general, the authors have used appropriate data for their climate projections, but in terms of the statistical and hydrological parameters, the data are drawn from Russian textbooks and reference material, which make them difficult to evaluate. The authors acknowledge this shortcoming, but I think it is a fundamental problem with the paper that also limits its applicability.

Our response: The study presents long-term assessment of extreme flood events only for the Russian part of the Arctic. Thus, the runoff data were extracted from the official issues of State Water Cadastre by Roshydromet. These data passed the quality control allowing to use the runoff time series in calculation of extreme flood events for engineering purposes. The runoff data are available by the order to the State Hydrological Institute (www.hydrology.ru). The method presented in the paper can be applied in other regions as well. Then, to calculate multi-year time series of spring flood flow depth (or peak flow), the daily discharge time series are required. Global and regional runoff databases may be also used to evaluate the regional and basin scale assessment of the future floods with required probability of exceedance using the climate change projections. Examples of such datasets include the following: 1. The Global Runoff Data Centre, Koblenz, Germany 2. The Environmental Information System (HERTTA) database at the Finnish Environment Institute. 3. Vattenwebb dataset at the Swedish Meteorological and Hydrological Institute. We have improved the manuscript accordingly.

Furthermore, it is not clear to me the degree of advance this method provides, in comparison with other methods.

Our response: Physically-based hydrological models (even with stochastic components) generate the flow time series (or runoff signal) based on the time series of meteorological variables. Thus, to estimate the extreme hydrological events (floods or droughts) with required probability of exceedance for a single catchment, one should run the physically-based catchment-scale hydrological model for a particular climate scenario (or a set of scenarios) and simulate the runoff signal. In case of the hydrological model with stochastic components, the signal could be performed by Monte-Carlo simulations within a-priory defined random generators. In performing of regional scale flood (or drought) frequency analysis using climate projections, the runoff signal should be simulated for a set of watersheds. It makes the calculations computationally expensive, especially in case of the climate ensembles. The approach presented in this paper allows to skip the simulations of the runoff signal since only the parameters of pdf are directly simulated from the meteorological mean values (for projected periods of 20-30 years). These parameters are further used to model the pdf within a theoretical distribution, allowing to evaluate the flood events with required probability of exceedance in the future. Thus, it makes it easy to perform regional-scale assessments of the detrimental hydrological events (floods and droughts) in the future, presented by a particular scenario or by ensembles of the climate projections. We have corresponding additions and improvements in the revised manuscript.

Presently, the paper comes across as mainly a regional assessment of changing flood risks, which, although useful, I am not certain merits publication in HESS. The paper would be substantially improved (and more relevant to cite for other researchers) if the authors could show more clearly a) how their method compares with other stochastic approaches to evaluate change in flood risks, and b) how the method could be applied using other data sources than the reference tables available for the Russian basins they studied here.

Our response: (a) The basic profits of the method applied are: 1. a low number of forc-

ing and simulated variables (only statistical moments of climate and hydrological variables are needed); 2. a low number of parameters (physical processes described integrally by a lumped hydrological model); 3. a relative simplicity of a regionally-oriented parameterization. Thus, the stochastic model used is extremely cheap computationally and allows to provide regional-scale assessment of extreme floods events in the future. Then, the warning regions where the risks to damage the social infrastructure increase may be outlined. We added this information in the revised manuscript.

(b) For other regions, the steps of the modelling are following: 1. The multi-year time series of a yearly maximum runoff (discharges or spring flood flow depth of runoff) are calculated from the daily runoff time series. 2. The mean values, the coefficients of variation and skewness are estimated from the observed time series of a yearly maximum runoff. Also, the mean values are estimated for the annual precipitation amount and air temperature for the selected period (considered as reference). 3. The numerical values of the model parameters are evaluated from Eq. (3). 4. The mean values of the annual precipitation amount and air temperature for the future period are evaluated from the climate projections. 5. The future mean values, the coefficients of variation and skewness of an yearly maximum runoff are evaluated using the mean values of the annual precipitation amount and air temperature with Eq. (4). To perform the model cross-validation and to develop the regional-oriented parameterization scheme, the multi-year time series of a yearly maximum runoff with the periods of statistically significant shifts in the mean values and coefficient of variations are required. We added this information in the revised manuscript.

Specific comments

P5 L1-17 The authors state that “[t]he stochastic approach was first proposed by Kovalenko (1993) and Kovalenko et al. (2010) simplified the basic stochastic model for applications of hydrological engineering”. This is all the background we are given on stochastic approaches to river engineering under climate change. Surely there must be many other relevant contributions to research on stochastic hydrological modeling.

The authors must here put their method in the context of the field, the state of the art, and how their method contributes to advancing the research frontier (if it does).

Our response: The description of the general context of the stochastic approach used in the study was expanded, and the corresponding references are provided. We added the details in the revised text of the manuscript, the reference list was expanded with: 1. Kritsky, S.N. and Menkel, M.F.: 1946. On the methods of studying the random variations of river flow, Gidrometeoizdat, Leningrad. Kite, G.W. 1977: Frequency and risk analysis in hydrology. Water Resour. Publications. Colorado: Fort Collins, 224 pp. 3. Benson, M.A. 1968: Uniform flood frequency estimating methods for federal agencies. Water Resour. Res. 4, 891–908. 4. Elderton, Sir W.P, Johnson, N.L. 1969: Systems of Frequency Curves. Cambridge University Press, London, 224 pp.

P5 L18-19 The authors state that "[t]he aim of this study is to perform a regional-scale assessment of the future extreme flood events based on climate projections for the Russian Arctic". This is fine, but as mentioned above, I am not sure HESS is an ideal outlet for a study with such an aim.

Our response: The study aims to perform the spatial and temporal characteristics of the regional water resources with particular focus on the Russian part of Arctic. The region was chosen since its sustainable development in changing climate is important for the Russian Federation from the economical point of view (and the study was supported by the Ministry of Education and Science). However, the stochastic approach applied can be used also to perform global as well as catchment (see example in fig. 6) scales assessment of flood events with required probability of exceedance. The paper presents the simplified model, the method of its validation and the regional oriented parameterization scheme (Arctic). The general statements of the stochastic approach used have been already presented in the previous papers (unfortunately mostly in Russian, but there are at least two papers in English by Kovalenko, 2014 and Domínguez and Rivera, 2010). From our point of view, the paper fits to scopes 2 and 3 of the HESS journal, as defined at http://www.hydrology-and-earth-system-sciences.net/about/

P10 L8 It is not clear what the authors mean here. From the text it reads as if the model's prediction scores should be shown in Table 1, but I think the authors mean that the whole dataset is shown in Table 1. It must be clear why the authors refer the reader to look at Table 1 here.

Our response: Yes, the reference to Table 1 was removed in the revised version.

P11 L17 It is not explained why there needed to be a statistically significant shift in the historical time series in order to consider it.

Our response: The parameters of pdf of multi-year runoff the future time periods are simulated by the model using the parameters of pdf evaluated for the present period (the initial conditions of the model). The hypothesis of quasi-stationarity considers the present and future time periods as two steps with different (statistically significant) values of the pdf's parameters. Thus, to verify the model using historical data, two periods with different values of the pdf's parameters are required to perform the cross-validation procedure. For the Pearson III type distributions the pdf's parameters can be calculated using the mean value, coefficient of variation and skewness. Therefore, we analyzed observed time series to define two periods with statistically significant difference at least for the mean value (t-test). These time series were used to verify the model. We have corresponding additions and improvements in the revised manuscript.

P11 L25 I am not too fond of personal communication when it comes to data references. Could these data not be obtained elsewhere? It means then that this study quite difficult for others to reproduce. Also, what reference is meant by "the multi-year catalogues of climatology (e.g. 1989)"?

Our response: Yes, we agree that it is not a good idea to furnish the personal communication as the reference to the data source. Thus, the reference to the Meteorological Data from the Russian Arctic (1961-2000, Version 1. 2003. Edited by V. F. Radionov and F. Fetterer. National Snow and Ice Data Center. Boulder, Colorado USA. NSIDC: National Snow and Ice Data Center. http://dx.doi.org/10.7265/N56H4FB3) was added, which was originally created based on the dataset of the Arctic and Antarctic Research Institute (St. Petersburg). The personal communication (N. Bryazgin) just expands the collection in space and time. The multi-year catalogs of climatology are official editions by Roshydromet, and these data are usually used in engineering calculations. There are several issues covering the data from different regions. However, using these data sources does not restrict the application of the method, and the global dataset of the climatological data can be used (i.e. CRU dataset from http://www.cru.uea.ac.uk/data). We have corresponding additions and improvements in the revised manuscript.

P12 L28-29 It is not clear which statistical moments are meant here. The two moments shown in the table are explicitly not the same, so I am not sure what the authors mean by saying that they are assumed to be constant. Reading further, I assume this refers to a case or scenario where the authors used this assumption ("no model" case), but it is not clear from the text.

Our response: The term "statistical moments ..." used in the text because of the Eq. (2) is written for the initial statistical moments. These moments are further used to calculate the mean values and coefficients of variation. "No model" represents the case, when the climate change is not taken into account, and thus the parameters of pdf are not modified for the period of prediction. This case shows the situation considering in the guidelines for the engineering hydrology (Bulletin 17-B), which used only observed time series to evaluate the parameters of pdf. We have corresponding additions and improvements in the revised manuscript.

P12 L7-9 It is not correct to say that these are "5-10%" higher. Rather, the results are 5-10 percentage points higher. Our response: Yes, the text was corrected.

P12 L20 Was there any reason for selecting these models? Previous research shows that the choice of climate model greatly influences the results of any hydrological model simulation that uses the GCM results. I strongly suggest the authors motivate why these models were selected. Are they representative of the whole ensemble, or is the

sample perhaps biased in terms of key model aspects (climate sensitivity, hydrological response to temperature increase, etc)? To be clear, I do not think it necessary that the models be representative, but the reader should know why the models were selected and if there are any aspects of these particular models that could influence the result. For instance, if the models all are in the low end of the climate sensitivity range, the results obtained in this study could be overly conservative (or vice versa of course in the case of very sensitive models). This is important information for the interpretation of the results, not to say any practical application of them.

Our response: The long-term assessment of extreme flood events was performed for each scenario and model from the Fourth and Fifth IPCC Assessment Reports and published as the final reports for the Ministry of Education and Science of the Russian Federation (2013-2015). This paper presents the results of flood frequency analysis obtained for the models that produced typical scenarios and models that were close to the regionally averaged scenario recommended by Gaidukova (2012). In this paper, the author provides the estimates of ensemble average scenario for the projections from the Fourth Assessment Report of IPCC. The GCMs used represent the climate projection close to the typical, and show that the hydrological modelling results do not vary much under the the climate forcing with the small differences. We have corresponding additions and improvements in the revised manuscript.

P12 L25 What periods?

Our response: The reference period of climatology was considered as 1961-90 for climatology. We added the details in the revised text of the manuscript.

P13 L5 The procedure of extraction from maps should be described much clearer.

Our response: The technical details of this procedure were not included to the text of this paper, since the procedure is commonly used in GIS-applications without specific references except of user manuals for the particular software. Thus, the following description was added: "The procedure to obtain the mean values and coefficients of variation from the maps included scanning of paper maps, georeferencing of images, data digitizing, and interpolation into the grid nodes of the particular GCM."

P14 L5 It needs to be made clear to what degree these results were already published. If the figures used here are directly from some previously published report, they should be part of the methods and data, and not a result of the study. If the authors analysed spatial datasets for new geographical regions, it is ok to have them as new results here. But it is not clear what the authors mean.

Our response: Yes, it is important to separate the previous results from the new ones. The studies by Govorkova and Meleshko include the climate changes assessment for the territories of the Russian Federation as a whole, and do not provide the estimates within the geographical domain of the Russian Arctic, which was outlined in this study according to the hydrological principles as suggested by Ivanov and Yankina (1992). Then, the results presented further in the text of manuscript have not been published in previous papers (except the technical reports). We have corresponding additions and improvements in the revised manuscript.

P14 Results from Table 4 It is difficult to follow the discussion of the results when they are presented in absolute values in the table, but discussed in terms of percentage increases in the text. I would prefer the authors either discussed also the absolute changes in the text, or that the percentage changes are shown in the table, so that one can follow which figures in the table are discussed in the text.

Our response: We added the absolute values of the changes into the text of the discussion.

P14 L22-23 “The strongest increase (over 27 %) of the mean values with a lowest decrease of the coefficients of variation (over 17 %) is predicted by CaESM2 for the RCP2.6 scenario.” I can’t follow this from the table – which parameter increase of 27% are the authors talking about? Precipitation, temperature, or spring flood depth? Our response: This is for the spring flood flow depths. We corrected the text to provide

clear descriptions in this part of the manuscript.

P14 L25-27 Here the authors talk about the “European part of the Arctic”, and refer to figure 3, where there is one region referred to as “Northern European Arctica [sic]”. There is also another region termed “Kola peninsula and Karelia”. Do the authors by “European part of the Arctic” refer to only the “Northern European Arctica”, or to both these regions? If so, it would be good to state this, for instance by labeling the panels in Fig 3 as a, b, c, d, and then here refer to (Fig 3a-b) or similar. Not everyone knows where the Kola Peninsula and Karelia is. Furthermore, I assume this still only refers to territory within the Russian Federation, and it is therefore technically incorrect to refer to it as the “European part of the Arctic”. Such a region would include parts of Scandinavia as well. I suppose that calling this the “European part of the Russian Arctic” would be more correct. In general, it would be helpful if the authors referred to the sub-regions they define in a consistent way throughout the paper, both in text and figures, and also clearly outlined these on a map.

Our response: In corrected version we refer the panels of fig. 3 as a,b,c and d and correct the text in the discussion. Moreover, the fig. 6 presents the location of geographic domains, which were discussed in the fig. 3 and text.

P15 L16-18 Here it is a bit difficult to follow what Hirabayashi et al found. From the text I assume they found a decrease, but of what magnitude? How do the results really compare?

Our response: It is very difficult to compare our result with other studies because different flood characteristics are addressed. Only indirect and quantitative comparison is possible. For the comparison we assume that for Pearson Type III distributions, an increase of the mean values and the coefficients of variation leads to an increase of upper-tail values. Then, present 100-year floods occur more frequently (Fig. 4). Also, a decrease of the mean values and the coefficients of variation leads to a decrease of upper-tail values. In this case, we can expect 100-year floods decreased. For the eastern part of the Arctic, an increase of historical 100-year maximum discharges is predicted by Hirabayashi et al. (2008; 2013) under the SRES:A1B scenario for the period 2001–2030. This is in accordance with our results, we also expect an increase of upper-tail runoff values since the mean values and coefficients of variation were estimated to enlarge in average for this region. For the north-east European Arctic we expect a significant increase the frequency of present 100-year flood events. This is in contrast to Hirabayashi et al. (2013), which presents the global scale estimates of the projected change in flood frequency. The flood frequency is decrease in many regions of northern and eastern Europe according to Hirabayashi et al. (2013). The feasible reason of such disagreement is the spatial coarseness of the model used by Hirabayashi et al. (2013). The model is calibrated using the observations from the watersheds larger than 100,000 km2. We added the details to discuss the comparison of our results with previously obtained in the revised text of the manuscript.

P16 L17 What Strategy? Please explain.

Our response: The Strategy is the official document of the Government of the Russian Federation, it is more political (not research) issue. We exclude this reference in the revised version of the paper.

Table 3 It is not clear what the percentage refers to. All periods? All basins? How many value pairs are compared?

Our response: We provide the details about the percentage of the successful "nominal prediction" (used to perform the model cross-validation) in the text and in the header of the table 3.

Figure 1 The figure should indicate the critical value of the t-test for the chosen significance level. Is this the dotted line in the figure? Please label this line. Our response: The explanation was added on the text under Figure1

Figure 5 must be improved. It is very difficult to see the patterns, and what pattern that

corresponds to which value. I would suggest using grey shading instead for at least some of the categories, so that one does not have to use so many different patterns that are difficult to distinguish on the map. Figure 6 must also be improved for the same reasons. Also, the ordering of the figures should be the same as the order they are referred to in the text.

Our response: Figures 5 and 6 were improved with the regions presented by color patterns.

References P23 “Government development strategy. . .” I was not able to retrieve this file from the web link provided.

Our response: The reference to the Strategy was removed.

Language and other minor points Although the language is generally acceptable, there are quite a number of grammatical errors, and the paper needs editing before it can be accepted for publication. I have not noted all language points but list some issues that I noted here.

Our response: We corrected the revised text to exclude the grammatical mistakes mentioned in the list.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2015-504, 2016.