Response to Referee #3

We thank the referee, for the constructive 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.

The article gives a huge amount of new data about the Arctic runoff and climate, can help to understand some conformity to natural processes and statistical regularities. The long data grids are used for the work. It allows to support the article for the publication. But the article should have a major revision. Restructuring of some chapters are also necessary.

Main remarks for the paper

1. The period of modeling in the paper is 2010-2039. 2016th is nowadays. Why the model has not been checked for 2010-2015 (2014)? It can be helpful for estimation the method adequacy.
Our response: The model simulates the multi-year statistical values (the mean and coefficient of variation). The 5-year period is completely insufficient to estimate these values to verify the model (2).

2. If the mean values increase (17-23%) and the Cv decrease (5-16 %) simultaneously it can mean the runoff magnitude uprising. Thus, it should be analyses in the article (in discussion chapter, for example).
Our response: The changes in the runoff multi-year statistics lead the alterations in the tailed values of the PDF: escalating (usually due to the increase in the mean and/or Cv), diminishing (usually due to the decrease in the mean and/or Cv) or neglecting (usually in case if the increase/decrease in the mean values is accompanied by the decrease/increase in Cv). The evaluation of the thresholds for the simultaneous changes in the mean and Cv, which lead these three types of the alteration in the tailed values of the PDF Pearson Type III is the issue have to be considered in the special statistical (not hydrological) study. In our study we rely on the simple assumption (P. 11 L. 10–15).

3. The Nadym River is one of the Russian Arctic Rivers, and not the lager one. There are no socio-economic aspects in the article results that had been analyzed according to the examined modeling evaluation. In the second part of the paper the Yana River is considering. These incompatibilities could be solved by changing of the paper title or addition river examples examining from the other Russian Arctic regions. It is actual in the case of examination of the aim of the paper study (Page 3, Line 23) – "…to perform a regional-scale assessment…"
Our response: The paper discusses the issues of the regional scale assessment the expected changes in the extreme floods for the Russian Arctic. The main result of the study is shown in Fig. 7, where the warning regions are outlined. However, the paper also provides two examples for the particular catchments. The first example illustrates the model cross-validation procedure (the Yana River), and the second example shows the practical application of the maps with warning regions to calculate the maximal discharges of low probability of exceedance for the Nadym River. The economic issues itself were not include into this study, which was mostly dedicated to the hydrological aspect.

4. The methods \ materials chapter has to be restructured and clarified.
Our response: This chapter was improved in the revised version of the manuscript according to the suggestions provided by all Reviewers. In particular, we (i) discussed the opposing views on the significance of affecting the climate change to the hydrological regime (P. 2, L. 3–13); (ii) provide the background, why there needed to be a statistically significant shift in the observed time series to perform the model cross-validation (P. 6, L. 22–29); (iii) discussed the dataset used in the study and references connected with data (P. 8, L. 10–13, P. 13, L. 3–8); (iv) explained the case of "no model" represents in the cross-validation section (P. 8 L. 28–31); (v) added description of the data sets used in the study (Fig. 4, P. 9, L. 5–13).

5. The link to the table 6 is earlier than to the tables 3-5 in the text. The table numeration has to be done sequentially.
Our response: We corrected the numeration of the tables and figures to be sequentially.

6. Some results of the paper are presented in the methods chapter and in the Introduction but not in the result chapter. There are some results without discussion or without explanation of methodic that leads to carried out it. Thus, the Figure 5 shows regions of the spring flood depth of runoff according two models calculation without comparison or other explanations.
Our response: We have revised the manuscript in conforming of the name of the sections and their content and improved the text. Also, the discussion of the Fig. 5/(Fig. 7 in new version) is now presented in the text (P. 11, L. 36–38, P. 12, L. 1–5).

7. Widening of the Discussion is necessary.
Our response: We have expanded the Discussions/Conclusions sections as follows: (i) the other data sets, which could be used in evaluating of the regional scale hydrological response to the expected climate changes are presented, and the corresponding references are provided (P. 13, L. 3–8), (ii) the steps of the model application for the other regions/data sets are described (P. 13, L. 9–20).

8. An improvement of the reference list is insufficient. The current foreign publications on statistics methods are suggested to be added. The reference on the archive of Bryazgin (2008) – "…personal communication…" (Page 7, Line 14) is impossible. Following further Arctic and Antarctic Research Institute is an owner of data. Check and correct this reference, please. For the Page 6, Line 9-10 is the same. The old references for the model verification \ validation (Figure 6, for example, - Ivanov and Yankina (1993)) have to be improved and current publications should be added.
Our response: The reference list was substantially improved in the revised version of the manuscript.
(i) In performing the historical context of the study, the following references were added:
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.
(ii) In providing the reference to the source of the meteorological data used in the study:
1. 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).
This data set was originally created basing on the data set of the Arctic and Antarctic Research Institute (St. Petersburg). The personal communication with N. Bryazgin (2008) expands the collection of the data in space and time.
(iii) In adding the recent publication connected with delineating the hydrological boundary of the Russian Arctic, the following reference was included into the list:
Nikanorov, A.M., Ivanov, V.V., and Bryzgalo V.A.: The rivers of the Russian Arctic, the current conditions under the human impact, NOC, Rostov-on-Don, 2007. (In Russian).
(iv) In performing of the recent studies, which are dedicated to the hydrological statistical and stochastic modelling issues, the following references were included:
1. Kuchment, L.S. and Gelfan, A.N.: Assessment of extreme flood characteristics based on a dynamic-stochastic model of runoff generation and the probable maximum discharge. Journal of Flood Risk Management, 4, 115–127, 2011.
2. Montanari, A. and Koutsoyiannis, D.: Modeling and mitigating natural hazards: Stationarity is immortal! Water Resour Res, 50 (12), 9748–9756, 2014.
3. Serinaldi, F. and Kilsby, C. G.: Stationarity is undead: Uncertainty dominates the distribution of extremes, Adv. Water Res., 77, 17, 2015.

9. There are a lot of abbreviations in the text without decoding or explanation. So, all names of models have to be named or the list of the used models with references can be added.
Our response: the models' abbreviations decoding and references were added to the text of the revised manuscript:
1. Roeckner, E., Bäuml, G., Bonaventura, L., Brokopf, R., Esch, M., Giorgetta M., Hagemann, S.,Kirchner, I., Kornblueh, L., Manzini, E., Rhodin, A., Schlese U., Schulzweida, U., and Tompkins, A.: The atmospheric general circulation model ECHAM5. Part I: Model description. Max Planck Institute for Meteorology Rep. 349, 2003.
2. Giorgetta, M., Jungclaus, J., Reick, C., Legutke, S., Bader, J., Böttinger, M., Brovkin, V., Crueger, T., Esch, M., Fieg, K., Glushak, K., Gayler, V., Haak, H., Hollweg, H.-D., Ilyina, T., Kinne, S., Kornblueh, L., Matei, D., Mauritsen, T., Mikolajewicz, U., Mueller, W., Notz, D., Pithan, F., Raddatz, T., Rast, S., Redler, R., Roeckner, E., Schmidt, H., Schnur, R., Segschneider, J., Six, K., Stockhause, M., Timmreck, C., Wegner, J., Widmann, H., Wieners, K.-H., Claussen, M., Marotzke, J. and Stevens, B.: Climate and carbon cycle changes from 1850 to 2100 in MPI-ESM simulations for the coupled model intercomparison project phase 5. J Adv Model Earth Sy, 5, 572-597, doi:10.1002/jame.20038, 2013.
3. Delworth, T. L., Broccoli, A. J., Rosati, A., Stouffer, R. J., Balaji, V., Beesley, J.

A., Cooke, W. F., Dixon, K. W., Dunne, J., Dunne, K. A., Durachta J. W., Findell K. L., Ginoux P., Gnanadesikan, A., Gordon, C. T., Griffies S. M., Gudgel R., Harrison M. J., Held I. M., Hemler R. S., Horowitz L. W., Klein S. A., Knutson T. R., Kushner P. J., Langenhorst A. R., Lee, H.-C., Lin S.-J., Lu J., Malyshev, S. L., Milly, P. C. D., Ramaswamy, V., Russell J., M. Schwarzkopf D., Shevliakova, E., Sirutis, J. J., Spelman, M. J., Stern W. F., Winton M., Wittenberg A. T., Wyman B., Zeng F., and Zhang R. GFDL's CM2 global coupled climate models. Part 1: Formulation and simulation characteristics, J Clim , 19 (5), 643–674, 2006.

4. Chylek, P., Li, J., Dubey, M. K., Wang, M. and Lesins, G.: Observed and model simulated 20th century Arctic temperature variability: Canadian Earth System Model CanESM2. Atmos. Chem. Phys. Discuss., 11, 22 893–22 907. 2011

5. Johns T.C., J. M. Gregory, W. J. Ingram, C. E. Johnson, A. Jones, J. A. Lowe, J. F. B. Mitchell, D. L. Roberts, B. M. H. Sexton, D. S. Stevenson, S. F. B. Tett and Woodage, M. J.: Anthropogenic climate change for 1860 to 2100 simulated with the HadCM3 model under updated emissions scenarios, Clim. Dyn 20: 583-612, 2003.

6. Collins, W.J., Bellouin N., Doutriaux-Boucher M., Gedney N., Hinton, T., Jones, C. D., Liddicoat, S., Martin G., O'Connor, F., Rae, J., Senior, C., Totterdell, I., Woodward, S., Reichler, T. and Kim J.: Evaluation of the HadGEM2 model. Met Office Hadley Centre Technical Note no. HCTN 74, 2008.

Comments to the Abstracts

Page 1, Line 10. "…major challenges for adaptation…". Adaptation for what? Is it regeneration or adaptation?

Our response: The challenges connected with the economic activity (the long-term development of the infrastructure) in the region are mentioned. The text was accordingly corrected (P. 1, L. 9–11).

Page 1, Line 18. Extreme flood events in the Russian Arctic are connected only with spring snow melting seldom. The most hazard events are occurred during the multiplying of a river flood, tides and surges on the Arctic coast (marsh areas). It can effect on a river discharge more than 50-70 km upstream of rivers. There is no explanation of such event in the article as well as in the results of modeling.

Our response: In this study, the regional scale assessment of the extreme flood events was performed based on the observations for the catchments of medium size (from 1,000 to 50,000 km^2), which are located in single climate zone. We do not consider the features of the runoff processes in the local scale (appeared on the small watersheds) and in the global scale (revealed on the huge watersheds located within several climate zones). The flooding due to ice jams and tides/surges were not elaborated. This explanation now is added to the revised text (P. 9, L. 29–34).

Page 1, Line 20. Abbreviations in abstract should be interpreted before, not in the text.

Our response: The text of abstract was corrected and the abbreviations were excluded.

Comments to the Introduction.

Page 2, Line 13-14. The models can be global or regional but stochastic of

physically-based etc. What exactly models had been used in the paper?
Our response: The "in-house" developed probabilistic model described in Kovalenko (1993, 2014) were used in performing the regional scale assessment of the parameters of PDFs of the spring flood flow depth of runoff. The basic principles and hypotheses behind this modelling approach are shortly presented in the Annex of the revised manuscript.

Why the Markov randomisation had been used if the authors have a huge amount of observed data?
Our response: The simple Markov chain is the basic paradigm behind the traditional flood frequency analysis, which is used in the engineering hydrological applications (see e.g. Bulletin 17-B or Rogdestvenskiy, 1988). This model was proved by the statistical analysis of the autocorrelation functions, which were obtained based on the numerous time series of annual and spring flood runoff (Rogdestvenskiy, 1988). We add this comment to the revised text (P. 3, L. 22–25).

Authors said about the cheapest (Page 3, Line 2) stochastic approaches in the comparison of physically-based. But using only 3 parameters of PDF (Page 3, Line 3-4) for meteorological variability is also insufficient for the whole Arctic climate prediction.
Our response: The benefit of the method used this study is in skipping the simulation of the future hydrological time series (Fig. 1 in the revised manuscript). Within this study we do not predict the meteorological variability and climate for the whole Arctic.

Page 3, Line 5. Kovalenko is not the first scientist who suggests the stochastic approach for hydrological engineering.
Our response: The historical aspect of the method is not presented in the text with corresponding references (P. 3, L. 20–23).

Page 3. There is no explanation of stationary and quasi-stationary regimes.
Our response: The stationary regime means that the statistical parameters of runoff PDF are not changed for past and future time periods (as considered in classical engineering application). The quasi-stationary regime means that these parameters of PDF are differ for past and future time periods. The explanation is now given in the text (P. 3, L. 31–36).

Page 3, Line 14. I am interested how the authors explain of using the same approach for drought extremes in European part of the Arctic as well as for extreme flood events in the West and East Siberia, - such climatically and physic-geographical different regions.
Our response: The approach proposed based on the theory of Markov processes, which can be apply to evaluate the extremes from the PDF. In engineering hydrology the extremes (floods and droughts) are defined as the PDF tailed values, which conform to given probability of exceedance (0.1, 1, 5, 10 % for floods and 90, 95, 99, 99.9 % for the droughts). Then, the issue of the floods and droughts prediction is only the estimation of the PDF parameters, which are expected under a new climate. The geographical peculiarities of the regions are accounted by the parameters of the model (2), thus the regional-oriented parameterization scheme is usually required. We add this explanation into the text of revised manuscript.

Page 3, Line 27. What “domains” does the authors mean? They had not been explained before.
Our response: The "domain" means geographical region, the territories with specific climate conditions, land cover and runoff regime. We replace this word with the word "territories" in the revised manuscript.

Comments to the Methods and data

The chapter should be restricted totally. The methods are not clearly outlined. Some comments should be added to the chapter.
Our response: This chapter was improved in the revised version, in particular, we (i) discussed the opposing views on the significance of affecting the climate change to the hydrological regime (P. 2, L. 3–13); (ii) provide the background, why there needed to be a statistically significant shift in the observed time series to perform the model cross-validation (P. 6, L. 22–29); (iii) discussed the dataset used in the study and references connected with data (P. 8, L. 10–13, P. 13, L. 3–8); (iv) explained the case of “no model” represents in the cross-validation section (P. 8 L. 28–31); (v) added description of the data sets used in the study (Fig. 4, P. 9, L. 5–13); (vii) presented the Fig. 4, which shows the data sets used; (viii) improve the list of references.

There are results (for example, for the Nadym River) that could be removed to the result and discussion chapter.
Our response: There are no results for the Nadym River, which are shown in the method and data section. The result of the cross-validation for the Yana River shows the successful / non-successful the cases of the "nominal" prediction of the PDFs. It is necessary in this section from our point of view.

The list of equations and their conventional signs is recommended to be done. Some equations are not used in following text but another are written twice (Cv, for example), the equation GN (Page 4, Line 30) does not have a number, etc.
Our response: Now the duplicated equation was removed, and the Annex with basis of the approach was add to the revised manuscript. Only the equations, which have references in the text were numbered.

Are there differences between Cv and Cvf, Cs and Csf equations?
Our response: The index "f" (Cvf) indicates that the coefficient of variation is calculated for the future time period based on the modeled two statistical moments. The equation used to calculate the CVf is similar as for Cv and it is provided now in the annex of the manuscript. Thus we exclude this equation from this section and include it to the Annex.

Are the authors sure that Cv\Cs will be constant (Page 5, Line 8)?
Our response: In present study we assume, that the ration of Cv\Cs is constant for the past and future. However, the assumption of the constant ratio of Cv\Cs for the past and future time periods can be refused in the future study. Then, the system of the equations for three statistical moments (A.5) should be used (see Annex Eq. A.5) and Cs can be evaluated from three statistical moments.

The list of all parameters from the calculated according to SP equation (Page

3, Line 45) for the estimating rivers is recommended to be added to the text.
Our response: In performing of cross-validation of the model (2) we did not used the extremal discharges with low probability of exceedance and the nominally predicted and empirical PDF are compared integrally by the goodness-of-fit statistical tests. Thus, there is no needs to calculate the maximal discharges using Eq. (1), and they are not shown in the text. However, the values of the parameters in Eq. (1) for the Nadym River is now presented in the text (P. 12, L. 23–25).

The reason of a runoff reduction (Page 4, Line 5) and using b and n factors and degree have to be also explained.
Our response: Eq. (1) includes parameters *b*, which is the additional area which adjusts the reduction of the runoff (km^2) and *n*, which is degree of a runoff reduction. The numerical values of these parameters are presented in the look-up tables e.g. Guidelines SP33-101-33 (2004) or (1984).

The "…flood flow depth of runoff…" (Page 4, Line 8) and "…the spring flood depth…" (Page 4, Line 23 and following the text) is misunderstanding. Are these same things or differences? If it is the same it can be unified in the text.
Our response: In this study, these two terms mean the same, and we use "spring flood flow depth of runoff" in the text of revised manuscript.

The authors supposed "…the future time period 2010-2039…" (Page 4, Line 12) in spite of that a current time is 2016. Have the authors done the verification of their model for 2010-2015? What the result have they received? May be it can help in understanding of the model availability for a runoff prediction in the Arctic.
Our response: The model (2) allows estimating the multi-year mean value and coefficient of variation of the spring flood flow depth of runoff. In estimation of these values, the 5-year period is not enough. This is the reason why the model (2) can not be verified using the observations for 2010-2015.

How the authors estimate the reference periods (Page 4, Line 38)? Why it is necessary for the methods and modeling? It is unclear in the chapter.
Our response: The reference period is the time slice with (i) the observed data available and (ii) steady climate and runoff regime. The "steady" means that there are no statistically significant trends and changes in the mean values of meteorological and hydrological characteristics. The reference period is necessary for the modeling since (i) the parameters of the model (2) are evaluated basing on the climatology and runoff statistics of this period, and (2) the warning regions are delineate basing on the differences in the mean values and coefficients of variation for the reference and projected periods. We add the explanations to the text of revised manuscript (P. 5, L. 23–30).

Is the sub-periods in the table 1 and 2 (and Page 6, Line 7 and Line 39, for example) are the same as "reference periods" or not?
Our response: In the cross-validation section (and Tables 1 and 2) we used terms "training" and "control" periods, the text of the revised manuscript was corrected.

What the differences \ similarities between "reference periods" and "training" and "control" (Page 6, Line 28, 30, 38) or "…reference and future …" (Page 5,

Line 8) periods.
Our response: The model (2) operates within two time periods with steady climate and runoff regime (idea of quasi-stationarity). One time period is used to evaluate the model parameters, and it is noticed as "training" period in the verification section and "reference" in the section of "data and method". Other time period is the period of the prediction (or the nominal prediction), and it is noticed as "control" period in the verification section and "projected/future" in the section of "data and method".

Later, in the table 6, the "Historical period" is. There are misunderstanding of periods definitions in the text.
Our response: The "Historical period" was replaced by the "Period 1950–1980" in the text of the revised manuscript.

The sub-periods were selected according to the statistically significant differences in the first statistical moments (Page 5, Line8-9) but authors have not explained what does "...subsampled mean values..." \ "..subsample equals..." means.
Our response: The "subsample" is the observed time series within the selected sub-period, it is used to evaluate the mean values and coefficient of variation (or the first and the second statistical moments).

Dimensions of the first (m_1 (mm)) and the second (m_2 (mm^2)) statistical moments of the spring depth of runoff (Page 4, Line 25-26) could be explained too as well as the parameter GN (mm^2). How these two statistical moments have been received (see the table 1 and table 2)?
Our response: The the statistical moments of the spring flood flow depths of runoff are estimated from the observed time series using the method of moments (Bowman and Shenton, 1998). The dimension of the first statistical moment (m_1 or the mean values) is equal to the dimension of the value (for the spring flood flow depth of runoff the dimension is mm, since this value is calculated as the volume of spring flood flow (m^3) from the drainage basin divided by its area (m^2)). The dimension of the second statistical moment (m_2 or dispersion) is equal to dimension of random variable square. The parameter GN reflects the dispersion of the precipitation, and the dimension of this parameter is mm^2.

The reference for the "...Pearson chi-squared and Kolmogorov-Smirnov one-sample tests..." is necessary as well as the explanation for the used methods.
Our response: The following reference is add to the list: Hollander, M., Wolfe, D.A. and Chicken, E.: Nonparametric statistical methods, 3d edition, Wiley, 848 p., 2014.

What does "...cross-validation..." mean (Page 6, Line 16)?
Our response: The cross-validation is a model evaluation method, which allows performing the model ability to reproduce the measurements. In simplest case, the dataset of the measurements (observations) is separated into two sub-sets, called the training set and the testing/control set. Then, the training set is used to evaluate the model parameters, which are further used to calculate the modelling (or nominally predicted) dataset to compare with the testing/control set using chosen measure (the statistical goodness-of-fit tests in our case). The procedure of defining the training set and testing/control set

are described in the section 2.2 together with the results of the ability of the model suggested to represent the empirical PDFs (P. 6, L. 22–27).

There are no data in the chapter 2.3. Rename it or add explanation which exactly data had been used. Is it measured meteodata or received data from the climate models? One part of the paper is about analyses of measured multi-year data from Russian meteostations (Water Cadastr (Page 6, Line 41-44)), the second part is about modeled climatic data. The connection between these two parts is incoherent shown in the methods chapter.
Our response: The explanation about the data sets used in the study was added to the revised text (Fig. 4 , P. 9, L. 2–7).

According to the reference on Page 6, Line 5-6 the "…node the mean values and the coefficients of variation of the spring flood depth of runoff were extracted from the maps…" that had consequently been built before 1986. The 80th of last century is a time of the beginning of a huge climatic change in the Arctic. Do the authors suppose that all coefficients are the same and can be used for modeling and prediction?
Our response: The changes in the observed meteorological and runoff characteristics after 1980s are important in choosing of the reference period. In this study, the assumption of the quasi-stationarity of the changes in the climate and runoff regime was used. It means, that there are two periods with steady climate (defined by mean values of meteorological characteristics) and runoff regime (defined by mean values, coefficient of variation and coefficient of skewness for runoff characteristics). But, the statistical values are different for these two periods. Then, the reference period have to be defined as time slice without any statistically significant trends in climatology and runoff characteristics. This is the reason why we do not consider the runoff data on the prediction stage for the regional scale hydrological projections.
However, on the model verification stage (cross-validation) we used the observations until 2006 for the catchments, where two quasi-stationary periods were found.

On the base of a climate change the Cv\Cs ratio has to be also changed (Page 8, Line 16) and cannot be used for modeling as a fixed coefficient. If the Cv\Cs ratio is equal it means that Cv or Cs coefficients have simultaneous trends that need to be explained additionally.
Our response: In this study we used the assumption of constant ratio of Cs/Cv, however, this assumption can be avoided in future studies. The system of equations for three statistical moments (A5) have to be used in this case.

Comments to the Result and discussion

There are some examples from the global models calculations and predictions in the chapters. Authors' received results presented in the tables 1-6 are without properly clarification and interpretation in the Result and Discussion chapter.
Our response: In revised manuscript the discussions were expanded for the results, which are presented in the tables and figures. Also, the historical context and special questions connected with the method used are clarified.

How do the authors understand "…alarm regions…" (Page 11, Line 31-32)? Discussion on the Arctic alarm-regions analyze could fulfill the text and

conclusion chapters.
Our response: The term of "alarm" was replaced by "warning" regions. The regions, where the PDF tailed values with low probability of exceedance are going to change substantially are defined as "warning regions".

Unfortunately, authors did not compare their results with other publication sufficiently.
Our response: It is very difficult to compare our results with other studies, since the different hydrological characteristics are projected. In revised text we expand the discussion of this issue.

Small marks to the text

Page 3, Line 45 and Page 4, Line 6. The abbreviation should be clarified.
Our response: The “SP” is not abbreviation, this is the index of the state guidelines.
Page 4, Line 1. How the authors “…a flood coincidence factor…” had determined?
Our response: The a flood coincidence factor reflects the water income to the catchment (due to melting), which affect to the shape of hydrograph. It is usually depend on the geographical regions and obtained from the look-up tables (P. 4 L. 36–37).
Page 4, Line 2. The dimension of “probability” have to be marked and type of “… an exceedance probability curve…” should be also noticed.
Our response: It was corrected.
Page 2, Line 34. The name of “…Lehner...” – check the format, please.
Our response: It was corrected.
Page 2, Line 36. GCMs – decode the abbreviation in advance, please.
Our response: It was corrected.
Page7, Line 1. Not “…Arctic…” but “the Arctic”.
Our response: It was corrected.

Comments to the Supplementary materials

For all tables: in the title of a table all denotations have to be explained.
Our response: The notations are now presented in the revised manuscript.
Table 1. Rives can be divided to a large \ middle \ small size of their catchments.
Our response: All rivers in Table 1 have the catchments of middle size (according to definition given in the Guideline: Hydrology of land,Terms and definitions, Moscow, 1988).
Table 2. Why are there two periods for each river? Could be periods marked as “training” and “control”? The differences of the period lengths for each river are not clear explained in the text.
Our response: Both periods were used in cross-validation, then they are both “training” and “control” (since the validation was done forward and backward). The periods lengths depend on the year having the value of *t*-test exceeding the critical value 0.05 level of statistical significance.
Table 4. “…Reference climatology…” is misunderstanding.
Our response: The climatology for the period since early 1930s till 1980 which was considered as a reference in our study (P. L.).
Table 6. What does the “…Historical period…” mean?
Our response: The period was specified in the revised text.
Figures 3. Are the observed data in the figures for all period of measurements? Did the mean value mark on a screen?

Our response: The mean values/coefficients of variation are given for the reference period (1930–1980). The figure was corrected.

Figure 5. The resolution of the pictures are not enough for good understanding and comparison.

Our response: The figure was corrected.

Figure 6. The data of discharge for the presented models are very variable. It is recommended to be discussed in the text.

Our response: The discussion about results was extended.

We thank the Reviewer for the questions, useful comments and suggestions, which have allowed to clarify and improve the text of the manuscript.