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Interactive comment on "Climate change will increase potential hydropower production in six Arctic Council member countries based on probabilistic hydrological projections" *by* Elena Shevnina et al.

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The Referee #2 recommends to do "significant clarifications and improvements in the description of the methods, the results and not at least in the discussion of the findings." The following major issues were mentioned:

"The method proposed lack a proper demonstration of its applicability to the current conditions. There are no data that shows that the hydrology or production under current conditions are properly reproduced. I do not think the description of the model was

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particularly easy to follow either."

Answer: We agree, that only proper comparison to observations proves modeling results. The method used in this study, also offers the comparison of the observed and modeled probability distribution functions (see page 8 in the Supplement). The specific cross-validation procedure includes several steps: to define the sub-periods in the observed yearly time series of river discharges; to set-up the model for the first sub-period; to simulate the exceedance probability curve (EPC) for the second sub-period and to compare with the EPC constructed from observations in the second sub-period (Kovalenko, 1993). The results on the cross-validation procedure for the annual runoff are already published by Kovalenko (1993) based on the historical observations on the catchments located in the North of Russia. In this study, we relay on these results and this circumstance was mentioned on the page 7 lines 210-211. It should be noted, that in this particular manuscript we did not pay much attention to the method itself since there are more papers to describe the details (Shevnina and Silaev, 2018; Shevnina et al., 2017; Kovalenko, 2014; Viktorova and Gromova, 2008; Kovalenko, 1993). This method developed more then 20 years ago, and even there are plenty papers published in Russian, but only few manuscripts are available in English. Kovalenko, 2014: Russ. Meteorol. Hydrol. 39:115. doi:10.3103/S1068373914020071. Shevnina and Silaev, 2018: GMD. doi: 10.5194/gmd-2018-108. Viktorova and Gromova, 2008: Russ. Meteorol. Hydrol. 33:6. doi: 10.3103/S1068373908060071.

"In the computation of the hydropower production, how is the head estimated? Particularly for countries with large high head systems this would be important to know." Answer: We agree, that the head system is needed to evaluate the potential hydropower production (PHP) since the Eq. (1) was applied in this study. However, the PHP was not evaluated on absolute values even on the country level, which was finally done in this study. In this study, we have shown the relative changes in the water resource in terms of probability, i.e for the annual runoff of 10/90 % of exceedance probability and it was assumed that these changes are linearly related to the the PHP in all range of

the EPC (see p. 12, the Supplement). To estimate the PHP in absolute values, the information on the site-specific head system is needed. It could be the topic for "a case study" manuscript in the future.

"To what extent do current regulations influence output from the model? It seems that e.g. the Norwegian data used are heavily influenced by current regulations. What bias can this lead to and is this taken care of in the analysis?" Answer: In fact, the data on the observed yearly time series of river discharges was filtered only formally by applying the statistical tests to reveal non-homogeneity/trends in the time series and to calculate the length of the reference period. In this case, the current regulation rules can affect on the projected statistical moments of the annual runoff for the catchments with present hydropower network. To answer to the question: how?, a new study would be required. However, to revise this study we added the information on the current regulations for the catchments (e.g. Norway), which were chosen to model set-up, and we discussed the possible effects as well.

"How is the baseline for the production used in generating the results presented e.g. in figure 6 estimated? How well does this baseline values correspond with known production? Data are available from the energy agency and from literature (e.g. Hoes et al. (2017) PLOS One). Were there any corrections done to get this right in the current analysis?" Answer: In this study, the only relative changes on the potential hydropower generation were suggested based on an assumption that they are simply related to the changes in the annual runoff in all range of the exceedance probability. Thus the only estimations for the annual runoff for the baseline were estimated from the river runoff observations to compare with the projections of the annual runoff. This circumstance was mentioned in the section 2.1.3, but may not clearly. It means, that the relative changes is the hydropower production in the Fig. 6 actually show the relative changes in the annual runoff of low and high exeedance probability. Recently, we used another way to present the results of our study (see section 2.1.3 and Fig. 6 in new version of the manuscript). It is also possible to evaluate the PHP in absolute values

based on the approach used in Hoes et al. (2017) can be done in the near future.

“The hydropower output is only presented as an aggregated value in figure 6. I do miss some more detail on the results leading up to this, particularly since this is the topic of the paper.” Answer: Recently, the results of our estimation were aggregated on the country level since the detailed analysis comes to be a topic of “a case study” on a country level (at least for Finland). In this case, the details on the changes on the hydropower production on “a river catchment” level will be provided. During the revising, we realized that the title of the paper actually does not fit the content of the manuscript, which is more about the probabilistic projections of the annual runoff.

“The discussion sections tend to rather discuss the MARCS output and discharge and precipitation data rather than hydro power and energy production which is the topic of the paper.” Answer: We agree, that recently the title of the manuscript does not correctly express the idea, thus it should be changed. Even though the title would change, we think more details would be provided in the revised version of the manuscript.

“There is a number of hydropower studies available in literature, and some is cited in the manuscript, and the authors state that their contribution is a better assessment of variability and uncertainty of the future predictions. This is interesting, but unfortunately not much discussed in the manuscript. How does your predictions with better assessment of variability compare to previous studies? Generally, I think the discussion section lack a proper discussion of the findings of this paper in relation to what is available in literature and how the results of this paper relate to previous findings.” Answer: We agree, the contribution of this study was poorly discussed and the additional comparisons with recent studies was done in the revised version.

“There is a body of literature on this topic available, but some important recent work is missing in the current manuscript: van Vliet et al. (2016) Nature Climate Change; van Vliet et al. (2016) Global Environmental Change; Flörke et al. (2012) J.Water Clim.Change. A number of regional and single system studies exists, also in the re-

gion studied in this manuscript I do think these should be discussed in relation to the method and findings in this manuscript, see also comment above. Based on this discussion, what is the major benefit of the proposed method and what new insight does it provide? As stated before, you say there is a benefit in your way of doing the assessment of hydropower potential, but you do not present a convincing argument that this is the case in the paper." Answer: We agree, the contribution of this study was poorly discussed and the additional connection with recent studies (including the listed above) will be done in the revised version.

"In the discussion it is stated that the results have the highest potential for use where there is new hydro power planned. I am not sure I agree, since altered inflow will greatly influence existing plants regarding operational changes, possible expansions and upgrading (which is important topics in the hydropower industry)." Answer: In our opinion, the methods of risk analysis are should to be applied to show how to utilize the result of the study. We think that it is only a way to show the practical effect of any probabilistic forecasts, not only hydrological or meteorological. Recently, we did not find any specialist in the risks assessment to clarify the situation with the potential of the probabilistic hydrological projections in hydropower planning, however we hope that it will once happen. In our study, the probabilistic projections of the annual river runoff were presented to show the relative changes in water resources in the North. The relative changes of the water resources were simply related the potential hydropower production. We agree, that this level of aggregation is not enough to give any recommendations in i.e. an optimal operation on a particular hydropower station. However, the "catchment scale" aggregation will be next step while the perspective of the probabilistic form of long term hydrological projections in risks assessment will be clarify.

"Looking at the results, not only volume is important but also seasonal distribution of water. The timing of the extra inflow might be as important as the percentage increase, and to increase the relevance of the paper this is a topic that should be addressed.

Answer: We agree, that the seasonal distribution of water inflow to hydropower plants is more important to plan the regulation rules. The method used in this study allows evaluation also seasonal distribution of water (see e.g. Domínguez and Rivers, 2004). However, the probabilistic hydrological model should be substantially improved and it require a separate study and new model core. Recently, the simplest version of the core was applied (Shevnina et al., 2017), but even this version the approach makes issues in understanding by hydrologists get used to deal with physically-based "rainfall-runoff" models.

"P2-l61: Is the discussion on water-stress indicators at all relevant to this study?" Answer: We agree, and the discussion on water-stress indicators was removed from the revised version of the manuscript.

Please also note the supplement to this comment:
https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-473/hess-2018-473-AC2-supplement.pdf

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