Review of HESS-2018-473

The paper deals with a method to estimate hydropower potential for six Arctic countries. This is interesting in the current debate on renewable energy, energy storage and balancing non-storable sources. And as such this could be a relevant paper, but I do think it needs significant clarifications and improvements in the description of the methods, the results and not at least in the discussion of the findings. Some major issues:

- 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 particularly easy to follow either.

- 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.

- 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?

- 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?

- 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.

- 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.

- 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.

- 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 region 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.

- 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).

- 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.

- P2-l61: Is the discussion on water-stress indicators at all relevant to this study?