< REPLY TO REVIEWER #2 >

- **Title: Evaluating robustness of dynamic reservoir management under diverse climatic uncertainties: Application to the Boryeong Reservoir in South Korea**
- **Authors: Kuk-Hyun Ahn and Young-Il Moon**

The authors sincerely appreciate the reviewer for his/her attentive and constructive comments. We have read the comments carefully and have tried to address them in the following text. An updated version of the manuscript with the reviewer's suggestions will be submitted if we are offered a chance for the next stage.

We acknowledge that the paper, as originally submitted, is not easy to follow. We will take the advice of the reviewer. Accordingly, the ENSO results will be presented in the methodology section in order to motivate and describe the methodology of the synthetic forecast index.

Regarding the second comment, we agree that the methodology is somewhat limited to representing hydrologic feedback that changes the hydrologic response as the climate changes. However, many climate impact studies are still conducted using a parsimonious hydrologic model (e.g., Ahn and Kim (2019); Knighton et al. (2017); Rossi et al. (2015); Vaze et al. (2010)). While these studies utilize a conceptual model, the suitability of a simple hydrologic model is supported by previous studies (e.g., Steinschneider et al., 2015). Also, some studies argue that hydrologic uncertainty represents just a small portion (less than 5%) of integrated uncertainty in future simulations (Lee et al., 2017; Teng et al., 2012), supporting the utilization of a conceptual model in climate change analysis. We are aware of contrary findings that neglecting major processes or over-simplifying process representations (e.g., snow process modeling) may lead to unreliable portrayals of climate change impacts (Lofgren et al., 2013; Milly and Dunne, 2011). Although the goal of the analysis in our paper is not to settle the debate on the underlying modeling approach in climate change analysis, we agree that ideally a future avenue of this research would use a more processed-based hydrologic model to take into account realistic representations of vegetation, snow and land cover. In the revised manuscript, we will discuss this issue raised by the reviewer.

For the third comment, we respect the reviewer's concern. While the degradation robustness index (DRI) is designed to consider a plan under a sufficiently large range of long-term climate change scenarios, an arbitrary threshold for the degradation robustness index (DRI) is introduced in this study. Instead of determining a predefined threshold, we purposely intend that determining the threshold should in part of the decision process of local planners. As we mentioned in the introduction, forecast information can be unique to each reservoir system. Also, the success of risk management is often linked to stakeholder involvement in the planning process (White et al., 2010), trust in expert opinions (Wachinger et al., 2013), and the ease of understanding expert risk estimates (Pappenberger et al., 2013). These two circumstances may suggest that determining hazard-risk criteria would be optimum if linked with stakeholders with knowledge of local relevant hazards. In addition, as described by Merz et al. (2014), defining the consequence-relevant threshold may itself time consuming, but once completed, the analysis can then be tailored to stakeholder vulnerabilities. We will address this in the revised manuscript.

Other comments for revisions:

1) The examples of awkward sentences will be corrected. Also, the revised manuscript will be sent to technical editing service to fix any awkward sentences.
2) The term "real-option" will be replaced by the term "optional inter-basin transfers" in the revised manuscript.
3) We agree that most hydrologic models represent the soil into multiple zones. Accordingly, this sentence will be eliminated.
4) That was a typo. It should be 108 (12×9). This will be changed in the text.
5) The information can be found in the introduction (between lines 137 and 156).
6) Following previous studies, we use forecast information to determine water rationing to circumvent severe shortfalls by diminishing the normal supply. Therefore, the dynamic operation rule is designed with each own C^{*} and C^{**}. However, the reviewer is correct that the current critical level C^{***} was not changed to make a fair comparison between strategies.
7) We acknowledge that the sentence is confusing. We will modify the sentence in the revised manuscript.
8) This is a good idea. We will provide a table to summarize the data.
9) For teleconnection analysis, observed streamflow data is employed. We will explicitly declare this fact in the revised manuscript.
10) The analysis is conducted with the original dataset. A PCA framework is not utilized.

11) As we mentioned in the first comment, this suggested change will be made in the revised manuscript.
12) The reviewer is correct. We will modify the small picture to make the connection clearer.
13) The adjectives will be changed from "storage" to "level".
14) The scale for the y-axis will be modified.
15) In the revised manuscript, the KGE metric will be presented for the calibration and validation periods. In addition, a shorter time period will be used to highlight the difference in the lines.
16) Each color represents a different operation rule curve. This information will be added to the legend.

((Reference))

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