

## ***Interactive comment on “Impacts of climate change on the seasonality of low flows in 134 catchments in the River Rhine basin using an ensemble of bias-corrected regional climate simulations” by M. C. Demirel et al.***

### **Anonymous Referee #2**

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The article presents an evaluation of the impacts of climate change on the seasonality of low flows on the Rhine basin. The authors use the HBV model and a number of downscaled and bias-corrected climate scenarios to evaluate the changes in three seasonality indices. The article is quite clear and well written although some improvements could be made (see detailed comments below).

My main concern is the link of this work with existing studies, especially the RheinBlick project that led a very similar study (though not specifically on low flows). The authors

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seem to use the same model and inputs, and therefore it is unclear whether their study is a specific analysis of the outputs of this project, and if it not the case, which additional insights is brought by this study. I think this should be clarified in the introduction to avoid confusion.

Minor revision is requested.

Specific comments:

1. Abstract: As mentioned above, the possible link with existing studies could be shortly mentioned in the abstract.
2. Introduction: I found that the introduction could be better structured and more focused. The authors tackle various (maybe too many) aspects of climate change impact studies and do not give much details of what can be learnt from existing work. Therefore I think the authors should better explain why the impacts of climate change on the seasonality of low flows on the Rhine basin deserve more attention. Besides, I think it would be useful to clarify at this stage of the article the links or differences with existing works, especially the recent work made by the Commission for the Hydrology of the Rhine basin (RheinBlick project).
3. 6809 - 13: “affecting a much larger area”: do the authors refer to low flows or droughts?
4. 6812 - 11: It is unclear at this stage why there is this difference in the number of catchments (101 and 134).
5. 6812: I think the Study area section (section 3) could be moved before section 2, since section 2 heavily uses the catchment description (especially the 134-catchment division).
6. 6812 - 25-26: I found this sentence unclear. Do the authors mean the evaluation of model errors on low-flow descriptors?

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7. 6816 - 9-10: Why the number of catchments used for calibration is again different (95 instead 101)? It is unclear whether the calibration was made by the authors or in past studies.
8. 6816 – 11-13: The authors could shortly explain why these periods were chosen. Why not using the longer period 1961-2007 for model evaluation, which is then used as the reference period?
9. 6817 - 10-12: What is the average length of available observed flow series over the 101 sub-catchments?
10. Section 4: In this section, the authors heavily use acronyms (for low-flow indices and sub-basins). Although it can be useful to avoid repetitions, I felt sometimes a bit lost in the meaning of each of them (especially for sub-basins). Maybe the authors could use full names sometimes in the discussion to remind the meaning of acronyms.
11. Section 4: I found that the use of “effect” in the titles of sub-sections not very clear and too vague. What the authors wish to quantify? The sources of uncertainty? The sensitivity?
12. 6821 - 8: What “significant” means here? Did the authors apply some tests? Is this comment for observed or simulated values?
13. 6822 - 7: Was only the station at the outlet of each of the seven sub-basins considered, or did the authors made some averaging on all the stations within each sub-basin?
14. 6822 - 27: Why this period was chosen as the reference period. 1961-1990 is often considered as a reference period in the climate community.
15. 6823 - 6-9: I found this comment a bit strange. How can the author say that the low flows are “well simulated” since they only compare simulations with other simulations here? Maybe they should more clearly say that the simulations obtained with simulated inputs are close to those obtained with observed inputs. Then, only if the model

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simulations using observed inputs were shown to be accurate when evaluated against flow observations, their conclusion would be valid.

16. 6826 - 19: “Representative” of what?
17. Discussion and conclusion: I found that the authors could push a bit further the information they give on the comparisons of their various model runs. They show that there is some level of error induced by the hydrological model on the reference period, and then they show that some trends on low flow indices between reference and future periods are found. However, they do not discuss the relative importance of the noise induced by the model on the reference period (delta between observed and simulated) and the delta change they show between simulations in present and future conditions. Can the delta change between present and future be considered significant compared to the noise in present conditions? If the errors in current conditions are much larger than the change between present and future, can this change be considered significant? Does this depend on the sub-basin and/or the low-flow index? Although this question goes probably a bit beyond the objectives of this article, I think it would be interesting to discuss this question to make a better link between the results shown by the authors.
18. 6827 - 18: What the “sixteen experiments” refer to? Model runs?
19. Table 1: Indicate the target periods for the first two lines.
20. Tables 3 and 4: These tables could be grouped to ease comparison. Or at least sub-basins could be put in columns in the two tables to ease comparison.
21. Fig. 3: The legend on the maps is not legible (too small).

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