Response to technical corrections

We would like to thank the editor and reviewer #3 for their very helpful and constructive comments, which improved the manuscript a lot. Please find enclosed our response to their comments below (listed in the sequence as provided by the editor)

Dear authors,

Referee #2 has provided a further review and found the manuscript substantially improved. I fully agree with this conclusion and I would like to thank you for your efforts on revising the manuscript.

I can now accept the manuscript for publication in HESS, provided that you address the technical corrections suggested by Referee #2 as well as my own ones listed below:

- I found several typos/mistakes in the manuscript (on top of the ones spotted by Referee #2), plus some trimmed (and unclear) expressions like “summer and winter dominated basins”. Please have a careful read of the manuscript.

In response to this comment, we have corrected numerous trimmed and unclear expressions.

- P2L1: the figure “60%” is not understandable as is in the abstract (60% of what?). Idem for L9 and L10. It has to be clear that these figures denotes a spread in relative changes, expressed in percents (this is not straightforward from the abstract alone, which should be self-contained).

In response to this comment we have rephrased the sentences as follows:

(P2L1): “The accuracy of simulated Q95 may result in a range of up to 60% depending on the decade used for calibration.”

(P2L9-10): “The total uncertainty of Q95 projections is the largest in basins with winter low-flow regime and, in some basins the range of Q95 projections exceeds 60%.”

- P1L15: I would add “respectively”.

Corrected.

- P1L15-17: I doubt this sentence is consistent with the actual findings reported...

We agree, and thus we have removed the sentence.

- P6L5: I believe you meant “low flows” instead of “floods”.

Yes, thank you. It has been corrected.

- P7L25: “seasonally at the regional scale”. Please rephrase: I believe you meant “for two low flow regimes and for individual stations over Austria”.

Corrected as suggested by the editor.

- P8L18: I would be much more comfortable in reading “variance” instead of “variability” here.

Corrected.

- P9, equation 17: It has to be made explicit that similar equations may be written for factors B and C.

In response to this comment we have extended the paragraph with following sentence: “Similar equations to Eq. 17 may be written for factors B and C.”

- P14L30-31: There are also counterexamples of high Q95 variability in this large-SI cluster. Please rephrase.

Yes, we agree, but the meaning of this part is to show that large SI/Q95 does not automatically/systematically mean large Q95/SI difference. In order to clarify this we revised the sentence as follows: “For example, a cluster of basins situated in the south-eastern part of Austria (Styria) has a large SI range of difference (i.e. more than 90 days) for 11 calibration variants in the period 1976-1986, but the variability in Q95 is in many basins less than 20% for this case.”

- P16L27 and Figure 14: Aridity should be defined somewhere in the manuscript.

In order to clarify the meaning of aridity, we have expanded the sentence as follows “… , which are characterized by lower mean basin elevation and larger aridity (i.e. ratio of mean annual potential evaporation to mean annual precipitation).”

- P18L24 and following. As for the limitation of this study due to the use of a single hydrological model structure, it should be mentioned in the manuscript that only one RCM is considered here (and both ENSEMBLES and CORDEX studies found that RCM uncertainty is far from being negligible for hydrology-related variables).

In response to this comment, we have extended the discussion with following part: “Previous ENSEMBLES and CORDEX studies showed that RCM uncertainty is far from being negligible for hydrology-related variables. Even if only one RCM is tested here and the variability and uncertainty of GCM and emission scenarios can be large, the results clearly indicate the importance of selecting objective functions in hydrologic model calibration for simulating low-flow projections.”

Response to Reviewer #2

I thank the authors for having taken into account most of my remarks, and for their intensive work to make their paper a lot better.

It is interesting to see the different behaviours and sensitivities between the two types of low-flow regimes (summer and winter) and it highlights the importance of making the distinction between these regimes in this kind of study. I also think that one of the most interesting result is that the different calibration variants do not impact the same features of the low-flows in the reference period as in the future period.

At last, do you think you could explain the relative contribution from hydrological modelling higher for winter low-flows than for summer low-flows by a transfer of hydrological regime in these catchments, from snow-dominated to rain-dominated regimes ? Snow processes parameters that control the discharge and that are calibrated in the reference period may not play such an important role in the future, whereas soil storages parameters would be of greater importance in a regime where evapotranspiration increase. It would be interesting to see if the range of these soil storages parameters obtained from different calibration variants are bigger in these winter low-flows catchments or not . Even if their study has been carried out focusing on mean hydrological regime, Magand et al., 2015 suggest that « the differences in parameters related to the soil are stifled in present time by the dominant snow process. ». I think that the results of this paper support this suggestion.

Yes we agree with the reviewer. This point is very interesting and our preliminary results indicate that the soil moisture parameters are the most sensitive in changing climate conditions. In some alpine basins, also the melt factor is important or the combination of both. We are currently finalising a paper about the sensitivity and effects of changed climate conditions on the estimation of water balance in Austria, so prefer not to elaborate on this point in this manuscript.

To sum up about this paper, the scientific content has been improved ; the methods used are more rigorous ; the bibliography that was already quite complete, has been expanded ; the readibility has been a lot improved ; the figures were already well built and telling a lot, they still are. The legends in the paper I downloaded were missing, but once they will be added, I definitely recommend the publication of this excellent paper.

Thank you.

Minor :

The few misprints I found are listed below :

Page 2, line 5 : the future period 2021-2050

Corrected.

Page 8, line 4 : I would not refer here to Hingray and Said, Lafaysse et al., 2014 or Vidal et al., 2015 in this part as their method are a bit different, but I would only refer to them in the discussion as it is already done.

We have removed the references as suggested by the reviewer.

Page 8, 9 : The authors should be consistent in their notations and chose between SSE and SSe

We have corrected the notation to SSE.

Page 11, line 6 : « While yellow bullets/dots/circles indicate 130 stations with dominant summer (June-November) low-flow occurrence, blue bullets/dots/circles indicate 132 gauges… »

Corrected. We have replaced colours by circles.

Page 12, line 25 : There is an extra dot.

Corrected.

Page 12, line 29-30 : I think that ZQ should not be preceded by « the ».

Corrected.

Page 14, I would delete from line 8 to line 13 because it should be in the legend of figure 6.

We have deleted this part, as suggested by the reviewer.

Page 16, line 2 : « …, but only one set of hydrological model parameters. »

Corrected.