Response to Referee #1

We would like to thank the reviewer for his/her positive and insightful comments on the manuscript. Below is our response to the issues raised in the review (printed in italics).

General comments:

1) *This paper presents an assessment of low flow projections in Austria, putting a strong emphasis on several sources of uncertainty, namely GCM uncertainty, calibration period uncertainty and objective function uncertainty for the hydrological model used. This paper is completely within the scope of HESS and it also responds pretty well to some of the topics of interest of the special issue "HYPER Droughts (HYdrological Precipitation – Evaporation – Runoff Droughts)".*

The topic of this paper also represents an important research field regarding hydrological climate change impact studies. Indeed, still too often authors who write papers presenting an assessment of flows (high or low flows) completely neglect the uncertainty of hydrological models: they use them as a trustful representation of the transformation of P and T into discharge, that will not change over time, meaning that only one hydrological model is used, with only one parameter set (see Alfieri et al., 2015; Thorne, 2011; Milano, 2015). While this kind of studies was justifiable years ago, it is no more defendable in my opinion, now that studies are repeatedly showing the lack of robustness of hydrological models when applied to contrasted climate conditions (Chiew et al., 2015; Coron et al., 2012; Thirel et al., 2015). So the present study is very interesting, but could be improved through several aspects listed below.

Response: We would like to thank the reviewer for this positive evaluation.

2) *The introduction, which serves at locating the paper into the field literature, is rather short. Some "good practice" and some "bad practice" examples of studies are given, but the authors fail to really show what novelty their study brings. I would suggest the authors to work on that.*

Response: In response to this comment, as well as to the comments related to new ANOVA analyses, we have extended the introduction and discussion section. The main idea was to introduce and refer to studies which are related to the uncertainty assessment of hydrologic (low flow) projections.

3) *My second major remark is about the use of a single hydrological model. While this article already presents more than many articles, I would say that the results may be to some extent model-dependent, and that it is worth discussing that somehow in the paper.*

Response: We agree that our results are to some degree model dependent. In response to this comment, we have thus extended the discussion section as follows: "The assessment in Austria enabled us to account for one conceptual hydrologic model and two different low-flow regimes. In the future we plan to extend such comparative assessment to more types of low flows (e.g. as classified in Van Loon and Van Lanen, 2012), their combinations linked with changes in land use and management at the wider, European scale, as well as to account for hydrologic models of different complexity, wider range of climate scenarios and different downscaling techniques. "

4) Some plots and analysis compare the relative uncertainty between 3 calibration periods and 11 objective functions. I wonder how the difference of the sample size (3 against 11) impacts the range of uncertainty and thus the comparison. I wonder if an ANOVA-type analysis could not be a useful tool for palliating this potential issue (see Vidal et al., 2015, this issue for example).

Response: Thank you for this suggestion. We agree that a variance decomposition as can be extracted from a statistical ANOVA framework may be useful to assess (quantify) the uncertainty contributions from different model/scenario components. In response to this comment we have thus extended the manuscript by introducing the ANOVA-type approach (in the Introduction and methodology sections) and estimating the relative contribution of three components (climate scenario, calibration decade and calibration objective function) to the overall uncertainty of low flow projections in Austria (in the Results and Discussion sections).

Minor remarks:

1) Throughout the whole document, please pay attention to the use of "low flow" -> when it is use as an adjective to a noun, it should be written "low-flow".

Response: Corrected.

2) Abstract: I am not sure that this article "allows disentangling the effect of modelling uncertainty and temporal stability of model parameters". While the second element is correct, I think that the first one is actually about the objective function-related uncertainty and nothing more. Modelling uncertainty would have considered using different modelling approaches.

Response: We agree with the reviewer and in response to this comment we have modified the sentence as follows: "… which allows disentangling the effect of the objective function-related uncertainty and temporal stability of model parameters."

3) p. 12396, l. 24-25: something is missing in this sentence.

Response: Corrected.

p. 12398, l. 18-20: I think that the authors are a bit too optimistic: the Austria climate is very humid, so I doubt that for example the results could be easily generalized for Australia:

Response: We agree with the reviewer and have revised the sentence as follows: "The assessment of uncertainties for winter and summer regimes allows to make generalisation for a similar spectrum of physiographic conditions around the world."

p. 12398, beginning of section 2.1: I am surprised that the authors state that low flow projections are typically performed by a delta change approach. Indeed, other downscaling approaches than the delta change can be used to provide future (or past) climate forcing to hydrological models. What is

truer is that usually the (low) flow projections are analysed by comparing future (low) flows to past (low) flows, as this article presents, and maybe the authors mean that.

Response: We agree with the reviewer and in response to this comment (and comment of reviewer #2) we have changed the sentence as follows: "In this study, low-flow projections of future climate scenarios are analysed by comparing future to past flows by using a delta change approach."

p. 12400, l. 2: please remove "(3)". L. 17: "rainfall-runoff"

Response: In response to this comment we have corrected the numbering of Equations, as well as the term "rainfall-runoff".

l. 12401, equations 7 and 8: the epsilon term is missing see Pushpalatha et al. (2012).

Response: In our study, no zero flows were observed/simulated, so it was not necessary to set the epsilon term to a small value. We would therefore prefer to leave the equations as they are.

p. 12402, l. 17: is it really 1987-2008? or 1976-2008? (see p. 12404, l. 20) If 1987-2008, please comment the impact of comparing 30-year indices to 20-year indices.

Response: Corrected. In this study, we have compared 30-years periods.

p. 12407, l. 18: basinS

Response: Corrected.

p. 12408, l. 2: "SI variability has A large variability..". l. 8: "weightS"

Response: We have rephrased the sentence as suggested by the reviewer #2. ("The comparison of SI and Q95 uncertainties indicates that large SI variability does not systematically mean large variability in terms of Q95.")

p. 12409, l. 8: "a Q95". L.9-10: please refer to figure 10 here.

Response: Corrected and added a reference to Fig.10.

p. 12409, l. 21-22 and p. 12410, l. 13-14: the verb is misplaced

Response: Corrected.

p. 12416, l. 18: November is misspelled

Response: Corrected.

Table 1: A1B instead of A1B2 (see also Fig. 8). Also, for positive values, sometimes a plus is used, sometimes not. I would suggest homogenising the table.

Response: Corrected.

Figure 1 (and all other maps): what is this point outside of Austria south of Tyrol? In the caption: “Colour and symbol size (: : :) represent: : :” and “The SI and its strength ARE estimated”.

Response: Corrected.

Figure 4: rather than the difference, this graph represents the relative difference between sim and obs.

Response: It shows the relative difference for low-flow quantiles, but difference in days for seasonality index. We thus prefer to retain the caption as it is.

Figure 5: am I right if I say that the Q95 value is different for both curves? That should be specified.

Response: The difference is somewhat less than 8%. We have added this information to the figure caption, as requested by the reviewer.

Figure 7: please use the same panel titles as in Fig. 6.

Response: Corrected.

Figure 8: “Line represents” and “scatter (: : :) showS”.

Response: Corrected.

More generally, although I am not a native English speaker, I feel that regularly articles are missing in the text before nouns. I would suggest checking that.

Response: The English will be checked.

References:

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