Reviewer#2

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

General

This paper aims at assessing the contribution of different sources of uncertainties associated with the low-flow projections by 2050 in 262 basins in Austria. The different sources of uncertainties explored are related to the calibration of one hydrological model, TUWmodel, and four future climate scenarios. The impact of the objective function selected on the calibrated parameters as well as their temporal stability are investigated by using 11 objective functions and 3 contrasted periods.

This paper is interesting as it addresses a few aspects that are often missing in studies about impacts of climate change on hydrology: - Low flow projections are of great importance for water management and I agree with the authors that analysis should not only focused on mean flow or hydrological regime as it is often done. - Role of calibration strongly questions the relevance of extrapolating hydrological models in climate change and it is often neglected.

For at least these two reasons, this article is worth to be published. Moreover, the important number of basins considered with contrasted hydrometeorological conditions allows to carry out robust statistical analysis. Eventually, this paper show great technical skills: figures and legends are well designed, and illustrate well the authors' statements.

However, I think that some corrections should be made before publication. I have some concerns about some points of the method, especially concerning the concept of uncertainty, and I think that the readability of the text could be improved.

Response: Thank you for these positive comments.

To my point of view, the main difficulty of this paper is to understand what the authors mean by uncertainty. First of all, a few definitions of uncertainty is used through the text which do not help the reader to follow the authors' statement. It sometimes refers to the range of relative differences between simulated and observed Q95 or SI (e.g. figures 6 or 7), but other times it refers to the range of relative differences (%) between simulated Q95 or SI between the future and reference period (e.g. figures 11 or 12). As the main goal of this article is to assess the uncertainty contribution to low flow projections, I think that uncertainty should only be used for the former definition. In this paper, a little discussion about the concept of uncertainty could also be added a: only a limited part of uncertainty is here explored as only one hydrological model, one method of climate downscaling (especially because other methods of downscaling are now more precise and more widely used than the delta approach, e.g. statistical downscaling methods, Boé et al., 2006 ; Mezghani and Hingray, 2009 ; Chauveau et al., 2013) and four future climate scenarios are used.

Response: The idea of the paper is to compare the ranges of low-flow indices obtained for two cases. One is the model calibration in the reference period, where we tried to assess the variability (range) obtained from different calibration settings (i.e. objective function and decade). The second is the range obtained for future low flow projections, which is affected by different calibration setting as well as by selected climate scenario. But we agree with the reviewer that using "uncertainty " for both cases might be confusing for the readers, so we have tried to revise the formulations and to

use the uncertainty only in connection with future low flow projections (i.e. simulations for the future period). In response to this comment we have also made numerous revisions (including the new ANOVA analysis and corresponding extensions in the Introduction, Methods, Results and Discussion sections), which – we believe – improve the readability of the manuscript.

Eventually, I am not comfortable with the method used to assign uncertainty contributions, the ratios method neglects the interactions between the different sources of uncertainty and is not very rigorous. At least, classical ANOVA (von Storch and Zwiers, 1999, chap. 9 ; Yip et al., 2011 ; Sansom et al., 2013) could be used, but to be more rigorous, adapted ANOVA designed especially for this kind of studies are highly recommended (Hingray and Said, 2014 ; Lafaysse et al., 2014 ; Vidal et al, 2015).

Response: In response to this comment, we have extended the manuscript and quantitatively assessed the relative contribution of the three components: climate scenario, calibration decade and calibration objective function to the overall uncertainty of low-flow projections. We have revised/extended the introduction, methods, results and discussion sections accordingly.

The selection of future climate scenarios, although this selection is justified by literature references, is small and unbalanced and this should be added in the conclusion as a limit of this study. In general, I think that conclusions should be moderated or limits of the study should be more explicit. I am not sure the conclusions can all be generalized as it is done presently.

Response: We agree with the reviewer. In order to more clearly moderate the limits of our results, we have revised the discussion section.

The readability of the paper could be improved by deleting some heavy formulations or some redundant parts (parts of the legend are often included in the text). The use of percentage points when differences of percentages are discussed could also really help the reader to get more easily into the results. The use of comparative formulation (less or more) should always be associated with a reference (than sthg).

Response: We have tried to improve the readability of the manuscript. For more details, please see the responses to minor remarks and response to the other reviewer.

Minor remarks

Abstract:

Page 12396, Line 7: "which allows disentangling the effect of model uncertainty and temporal stability of model parameters" I would not say the model uncertainty as different structures of model are not used and thus structural uncertainty is not investigated. However, all aspects of uncertainty related to calibration are explored a: the impact of the criteria selected and the temporal stability of parameters. Therefore, I would suggest something like: "which allows exploring all aspects of uncertainty related to calibration: choice of objective function and temporal stability of model parameters." or "which allows disentangling the impact of simulation scoring and temporal stability of model parameters." Or taking again the sentence page 12402, line 19 : "which allows exploring

the relative contribution of the impact of model calibration (i.e. time stability and objective function selection) and of future climate scenarios."

Response: In response to this comment and also comment of reviewer #1, we have revised the sentence as follows: “which allows disentangling the effect of the objective function-related uncertainty and temporal stability of model parameters.”

P. 12396, L. 25: I think that "and" has been added by mistake. "In basins with summer low flows and, the total uncertainty is mostly less than 20 %"

Response: Corrected.

P. 12396, L. 25 "While the calibration uncertainty dominates over climate projection uncertainty in terms of low flow magnitudes, the opposite is the case for low flow seasonality." This refers to Fig. 13, I would moderate this statement as the method used is not very robust or I would use one of the method mentioned above.

Response: In response to this comment, we have rephrased the sentence as follows: “While the objective function-related uncertainty dominates over climate projection uncertainty in terms of low-flow magnitudes, the opposite is the case for low-flow seasonality.”

Introduction :

The last paragraph is very well written and clearly defines the objectives and methods of this paper.

Response: Thank you.

Methodology :

2.1 Low flow projections In this paragraph, I would start with the general descriptors of low flow as it is used all along the text, and end with the projections (from the general to the specific).

Response: We agree with the reviewer that there are always different options how to structure the text, however, in this case we like the current structure and prefer to retain the low flow projection part as it is.

P.12398, L.23 Delta change approach: I think that this approach should be criticized in the discussion especially regarding internal variability of climate. I would also delete "typically" as this approach has been criticized and new downscaling technics are now more widely used.

Response: In response to this comment (and comment of reviewer #1), we have rephrased 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. 12399, L.5. "The differences between simulations of a hydrological model in the reference and future periods are the used ..."

Response: Corrected.

P. 12399, L.8 could be deleted as it is already said, and the paragraph could directly start from "The future low flow changes...".

Response: Corrected as suggested by the reviewer.

P. 12400, L. 11 : The formulation is a bit heavy, it is obvious that an hydrological model is used, so it could be reduced as follow : "The SI index is estimated for observed and simulated low flows"

Response: Corrected as suggested by the reviewer.

P. 12400, L. 12 : agreement between singular and plural "The differences between model simulations (i.e. Q95 and SI estimates) in the reference and future periods are then used to quantify potential impacts of climate change on low flows."

Response: Corrected.

2.2 Hydrological model

P. 12401, L. 1 : ": : : potential evapotranspiration data: : :"

Response: Corrected.

P.12401 L.3 : I would suggest to add how many parameters are calibrated to have an idea of the degrees of freedom, and especially because some parameters are further mentioned (P.12406 L.3). This very brief description of parameters could be added as a table or a scheme of the TUWmodel, without having to read the reference papers mentioned.

Response: In response to this comment, we have added requested table with brief description of model parameters.

2.3 Uncertainty estimation

P12402, L.3 : comas missing "The uncertainty, defined as the range of low flow projections, is evaluated for two contributions."

Response: Corrected.

P12402, L.7 : "The effect of objective functions ..."

Response: Corrected.

Data

P. 12402, L.24 : conditions that are reflected in different hydrological regimes. "Austria represents diverse climate and physiographic conditions of Central Europe, which are reflected in different hydrological regimes..."

Response: Corrected.

P. 12403, L.14 : As Austria is a land surface, I would rather talk about evapotranspiration than evaporation. ": : : when evapotranspiration exceeds precipitation..."

Response: Corrected.

P. 12403, L. 18 : I am not confortable with the units. I would rather described precipitation in mm/day or kg/m2/s or even mm/yr as it is done in Figure 2.

Response: This unit represents a specific discharge (a typical hydrologic characteristics), which allows to compare basins with different sizes. It is estimated directly from measured discharge (m3/s). We thus preferred not to change the units.

P. 12403, L. 27-29 : The two sentences "The thin lines...winter low flows. The thick lines: : :. selected decades." should be part of the legend and not in the text, and thus should be deleted. When legends are put into the text, it makes the text heavy and the reader confuse. I think that messages are thus not clear enough.

Response: We have removed the two sentences from the text, as suggested by the reviewer.

P. 12403, L. 29 : I would add in brackets, and adverb before verb "The two groups of basins (winter vs. summer low flow regimes) clearly differ..."

Response: We have modified the sentence as suggested by the reviewer.

P. 12404, L. 10-17 : I am not sure it is worthy to describe the different GHG scenarios especially because the results are here examined by 2050, and A2 and A1B do not differ before 2050. Moreover, I am not quite confortable with the justification of the "best performing ones". First, how are the performances assessed ? Second, how do you know that the best performing ones in present would perform the best in future? It can also mean that they are similar so that it reduces the range of possible future climates: : :.? Anyway, I understand that the authors had to make a choice and the justification given in the discussion. P. 12412 from line 7 to line 13 seems to me a better one, and should either be added here or just kept in the conclusion.

Response: We agree with the reviewer that the formulation was not very clear. In response to this comment, we have thus removed the following sentence: “The decision on the two driving GCMs is justified by an analysis of Prein et al. (2008) who investigated the skill of the CMIP3 GCM ensemble over Central Europe and show that these two models are among the best performing ones.” We prefer to retain both parts (a brief description of the scenarios in the Data section, as well as in the Discussion).

Results :

4.1 Low flow simulations in the reference period

This paragraph deals with "uncertainties" related to calibration.

Response: In response to this comment we have extended the title of section(s) 4.1. (and 4.2) to:

4.1 Low-flow simulations and uncertainty in the reference period (4.2 Low-flow projections and uncertainty in the future period).

P. 12405, L.8 : "Such a regime has stronger runoff seasonality (see e.g. Fig. 5 in Laaha et al., this issue) and less difference in rainfall regime, which allows modelling of rainfall-runoff process than in basins with rainfall dominated runoff regime.". An adjective is missing in the last part of the sentence, starting from which, and could you be more explicite, I do not understand why would it be easier? Please rephrase.

Response: The typical snow regime has a relatively simple runoff regime – i.e. minimum runoff during snow accumulation period (which typically lasts from October/November to March) followed by a snowmelt period with large runoff volumes. The runoff generation processes are more linear, the effects of evapotranspiration and soil processes are less important, so easier to model than in rainfall dominated basins. We prefer to retain this part as it is.

P.12405, L.11 : zQ = wQ ME+ (1-wQ) Melog "ZQ increases with decreasing weight wQ, which indicates that the runoff model performance tends to be better for low and high flows (i.e. model has larger runoff efficiency if it is calibrated to logarithmic transformed flows than to non-transformed flows only." I do not agree with this sentence, it seems right but it implies that you can directly compare both efficiencies ME and MElog which is not true. Same values of ME and MElog do not mean the same. You should add a likely somewhere in the sentence because it could only be a mathematical artifact of ZQ.

Response: In response to this comment we have added “likely” to the sentence as suggested by the reviewer: “… runoff model performance likely tends to be better…”.

P. 12405, L.23-25 : To be deleted, part of the legend "The top panels show the Q95 difference estimated from simulated and observed daily flows in the period 1976-2008. This means that t The model calibrated for 11 year period: : :."

Response: We have revised the section as suggested by the reviewer.

P. 12406, L.10 : fit instead of fits "The simulated Q95 in basins with winter low flows fits closer to the observed estimates."

Response: Corrected.

P. 12406, L. 15 : "Overall, the results are similar for large range of wQ." I would delete it, (heavy formulation), with the previous sentence and the following one, the reader understands.

Response: Deleted as suggested by the reviewer.

P. 12406, L. 20 : an article is missing, "this hydrological model tends to..."

Response: Corrected.

P. 12407, L. 1-5 : "In some cases, there is also a difference in the length of the low flow period, when the model parameterization does not allow to fit well some small rainfall-runoff events in the summer or autumn, which interrupt the observed low flow period but not the simulated one the flows simulated by the hydrologic model (i.e. the precipitation event is completely absorbed by the soil storage of the model and does not contribute to the runoff generation)." Besides the heavy formulation of this sentence, I don't understand this sentence : the observed low-flow period is interrupted but in brackets, I understand that this is the simulated low-flow period that is interrupted because the soil storage absorbes the precipitation event: : :.?

Response: In response to this comment we have rephrased the sentence as follows: "In some cases there is also a difference in the length of observed and simulated low-flow periods. Some small rainfall-runoff events in the summer or autumn cause an interruption of the observed low-flow periods, but the model simulates a complete absorption of the precipitation event by the soil storage and hence a longer low-flow period."

P.12407, L.8-13 : Part of the legend that should be deleted from the text "Left panels show: : :-1998-2008)."

Response: For clarity we prefer to leave the more detailed description of the figure layout as it is.

P.12407, L.17 : ": : : the differences are larger in basins with the summer low flows..."

Response: Corrected.

P.12407, L.18 : "For particular basins, ..."

Response: Corrected

P. 12407, L.24 : Part of the legend that should be deleted from the text "Figure 7 shows, similarly as Fig. 6, ...calibration variants."

Response: Deleted as suggested by the reviewer.

P. 12407, L.26 : ": : : basins with the winter low-flow regime...than the basins with the summer low-flow regime."

Response: Corrected.

P. 12408, L.1 : The sentence is complicated and, if my understanding is right, I would replace it by : "The comparison of SI and Q95 uncertainties indicate that large SI variability does not systematically mean large variability in terms of Q95."

Response: Thank you. We have rephrased the sentence as suggested by the reviewer.

P. 12408, L.9-11 : Part of the legend that should be deleted from the text. "The line (median) and : : : low-flow regime."

Response: Deleted as suggested by the reviewer.

P. 12408, L. 21 : I am not completely convinced as this is not true for ECHAM5-A1B2 and A2 for wQ=1.

Response: This sentence refer to Fig.8, which shows that selected scenarios for basins with winter low-flow regime (blue colour) project an increase of Q95 (for all weights) and for basins with summer low-flow regime tend to project no change or small decrease in Q95. In response to this comment we have completed the sentence as follows: “The comparison of different scenarios indicates that they are similar in terms of projecting an increase of winter low flows and a tendency for no change or decreasing Q95 ...”

P. 12408, L. 26 : repetition : last part of the sentence is already written in the previous sentence. "The change in low-flow seasonality (Fig. 8, bottom panel) is less pronounced. And i not sensitive to wQ." And the change in low-flow seasonality is less pronounced than what?

Response: Changed as suggested by the reviewer.

P.12409, L. 1 : This point is quite interesting. Do you have notice anything on the parameters ? With the increase of temperature, one process could be not dominant anymore, such as snow processes. The model would thus be less sensitive to the change of one or a few parameters than in the reference period. Do you think it could be a possible explanation?

Response: Yes, we agree with the reviewer that it might be a possible explanation.

P. 12409, L. 6 : "mostly"?

Response: Yes, in some basins the change is larger, but most of the basins have this category (class) of change.

P. 12409, L. 7 : "AIT HADCM3 A1B", AIT has never been mentioned before.

Response: Deleted.

P. 12409, L. 8 : ": : : an increase of Q95..."

Response: Corrected as suggested by the reviewer.

P. 12409, L. 16 : "These Ffigures..."

Response: Corrected.

P. 12409, L.27-29 : Part of the legend.

Response: For the clarity, we would prefer not to remove this description from the text.

P. 12410, L.10 : It sounds in good agreement with P. 12409, L.18, the responses of Q95 to climate scenarios are larger for basin with winter low flows.

Response: Thank you.

Discussion and conclusions

The first paragraph (L. 18-24) of this conclusion is very well written.

Response: Thank you.

P. 12411, L.8-10: "Our results indicate that, although the uncertainty from different emission scenarios is larger than 40% in many basins, the uncertainty from model calibration can exceed 60%." I think that this result is important and I am convinced that uncertainty due to hydrological modeling is very often underestimated but I don't think this conclusion can be generalized in this article considering the selection of future climate scenarios, the use of only one downscaling method and the use of only one hydrological model. For instance, snow processes may be more related to the structure of the model and since only one model is used here, this uncertainty may be under estimated. Using two snow model schemes such as one using a degree-day scheme and another more physically-based would probably change the results. Eventually, I would use "from different climate scenarios" instead of "different emission scenarios", as the results are analyzed by 2050.

Response: Yes, we agree with the reviewer. In response to this comment, we have changed the “emission scenarios “ with “climate scenarios” as suggested by the reviewer.

P. 12412, L. 2: Because the word uncertainty is sometimes not properly used, the following sentence seems to be in contradiction with P. 12411, L.17 "Our results show that impact of the objective function is larger for the estimation of low-flow quantiles in basins with winter low-flow regime, and is particularly large for the estimation of seasonality changes." ("Our results indicate that the calibration runoff efficiency is larger (than what?), and the uncertainty lower in basins with winter low-flow regime.")

Response: In response to this comment we have rephrased the sentences as follows: “Our results indicate that the calibration runoff efficiency in basins with winter low-flow regime is larger, and varies between basins less than in basins with summer low-flow regime.”…” Our results show that the impact of the objective function is larger for the future projections of low-flow quantiles in basins with winter low-flow regime and for the reference simulations of low-flow seasonality in basins with summer low-flow regime.”

P. 12412, L. 20-23: "The comparison of climate scenario and model calibration uncertainties indicates that the model calibration uncertainty dominates in the estimation of low flow magnitude (in the reference period), and the uncertainty in low-flow seasonality is larger in future climate scenarios...". This formulation is a bit clumsy, I would replace it by : "The comparison of climate scenario and model calibration uncertainties indicates that model calibration uncertainties dominate in the estimation of low flow magnitude, while the future climate scenarios dominates in low-flow seasonality."

Response: Thank you, we have replaced the sentence as suggested by the reviewer.

Table

What are WEGC, ZAMG, AIT, ZAMG? I am sure it is not very important, but as it is written, it should be mentioned. What is the difference between A1B2 and A1B?

Response: We have added the explanation of the abbreviations to the Table 2.

Figures

They are all well designed. I especially appreciate backgrounds of Fig. 6, 7, 9, 10, 11, 12, 13. It is clever and it enables the reader to identify very quickly if there is spatial patterns or not.

Response: Thank you.

Figure 2. I am wondering if summer and winter low flows would not be interesting to show, such as annual minimal runoff or maybe a descriptor of low flow more relevant, because everything is explained in terms of percentage in this article. I am curious about the absolute value of these minima.

Response: We prefer not to extend the figure, as the absolute values of the low flow quantiles are already published in some previous papers (see e.g. Laaha, Bloschl, 2006 or 2007).

Figure 5. Could you enlarge this figure?

Response: We believe that the size of the figure will be changed during the typesetting of the manuscript.