Review « Uncertainty contributions to low flow projections in Austria »

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 insteresting 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 pusblished. 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 readibility of the text could be improved.

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 Q_{95} or SI (e.g. figures 6 or 7), but other times it refers to the range of relative differences (%) between simulated Q_{95} 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 : 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. Eventually, I am not confortable 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, adpated ANOVA designed especially for this kind of studies are highly recommended (Hingray and Saïd, 2014 ; Lafaysse et al., 2014 ; Vidal et al, 2015). 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.

The readibility 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).

The following paragraphs list minor remarks.

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 : 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. »

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 % »

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 mentionned above.

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

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).

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.
P. 12399, L.5. « The differences between dimulatios of a hydrological model in the reference and future periods **are** the used ... »
P. 12399, L.8 could be deleted as it is already said, and the paragraph could directly start from « The future low flow changes... ».
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 »
P. 12400, L. 12 agreement between singular and plural « The differences between model simulations (*i.e.* Q_{95} and SI estimates) in the reference and future periods **are** then used to quantify potential impacts of climate change on low flows. »

2.2 Hydrological model
P. 12401, L. 1 : « … potential **evapotranspiration** data… »
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 mentionned (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 mentionned.

2.3 Uncertainty estimation

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

P12402, L.7 : « The effect of objective function**s** ... »

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 hydrologic**al** regimes... »

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

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.

P. 12403, I think that lines 27 to 29 could be deleted as it is already mentionned in the legend « The thin lines … over the three selected decades. »

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. 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.

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... »

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.

Results :

4.1 Low flow simulations in the reference period

This paragraph deals with « uncertainties » related to calibration.

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 modeling 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 it would be easier ? Please rephrase.

P. 12405, L.11 : $\mathbf{z_Q = w_Q\ ME + (1-w_Q)\ Me^{log}}$

« Z_Q increases with decreasing weight w_Q, 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 ME^{log} which is not true. Same values of ME and ME^{log} do not mean the same. You should add a likely somewhere in the sentence because it could only be a mathematical artifact

of Z_Q.

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…. »

P. 12406, L.10 : fit instead of fits « The simulated Q95 in basins with winter low flows fit~~s~~ closer to the observed estimates. »
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.
P. 12406, L. 20 : an article is missing, « **this** hydrological model tends to... »

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…. ?
P.12407, L.8-13 : Part of the legend that should be deleted from the text « Left panels show…-1998-2008). »
P.12407, L.17 : « … the differences are larger in basins with ~~the~~ summer low flows... »
P. 12407, L.18 : « For particular basin**s**, ... »
P. 12407, L.24 : Part of the legend that should be deleted from the text « Figure 7 shows, similarly as Fig. 6, ...calibration variants. »
P. 12407, L.26 : « … basins with ~~the~~ winter low-flow regime...than the basins with ~~the~~ summer low-flow regime. »

P. 12408, L.1 : The sentence is complicated and, if my understanding is right, I would replace it by : « The comparison of SI and Q_{95} uncertainties indicate that large SI variability does not systematically mean large variability in terms of Q_{95}. »
P. 12408, L.9-11 : Part of the legend that should be deleted from the text. « The line (median) and … low-flow regime. »
P. 12408, L. 21 : I am not completely convinced as this is not true for ECHAM5-A1B2 and A2 for w_Q=1.
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 ?

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 ?
P. 12409, L. 6 : « mostly » ?
P. 12409, L. 7 : « AIT HADCM3 A1B », AIT has never been mentioned before.
P. 12409, L. 8 : « … an increase of Q95... »
P. 12409, L. 16 : « These ~~F~~figures... »
P. 12409, L.27-29 : Part of the legend.

P. 12410, L.10 : It sounds in good agreement with P. 12409, L.18, the responses of Q_{95} to climate scenarios are larger for basin with winter low flows.

Discussion and conclusions

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

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.

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. »)

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.»

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 ?

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.

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.

Figure 5. Could you enlarge this figure ?

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