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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Uncertainty contributions to low flow projections in Austria" *by* J. Parajka et al.

Anonymous Referee #1

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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) Âż.

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 hy-

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drological 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.

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.

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.

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).

Minor remarks:

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”.

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 uncer-

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tainty and nothing more. Modelling uncertainty would have considered using different modelling approaches.

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

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. . .

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.

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

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

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.

p. 12407, l. 18: basinS

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

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

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

p. 12416, l. 18: November is misspelled

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.

Figure 1 (and all other maps): what is this point outside of Austria south of Tyrol? In

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the caption: “Colour and symbol size (. . .) represent. . .” and “The SI and its strength ARE estimated”.

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

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

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

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

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.

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