Hydrology and
Earth System
Sciences

Open Access

Discussions

Interactive comment on "Importance of hydrological uncertainty assessment methods in climate change impact studies" by M. Honti et al.

Anonymous Referee #1

Received and published: 16 March 2014

Using a Bayesian framework with two different likelihood functions, this paper estimates uncertainty in simulated discharge quantiles for two Swiss catchments based on a climate change scenario. In the first case, the calibration is aimed at predicting the discharge time series – the quantiles are derived afterwards. In the second case, the calibration is aimed at predicting the quantiles directly. The authors conclude that the estimated uncertainty depends strongly on the uncertainty assessment method.

This is quite an expected conclusion. We are talking about 'an estimate' of uncertainty, therefore the estimation method does influence. However, it can still be useful to show how much the method influences the result and, perhaps more importantly, to investigate if the difference is such that it may result in opposite conclusions.

My main concern however, is that the manuscript is too long. I do not think that it needs 53 pages of HESSD to illustrate the methods/results and convey the message. In the present form, it is difficult to get the essence of the paper quickly and in some places it may confuse the readers. Therefore, I recommend to significantly reduce the manuscript length. Some parts may be presented as supplemental materials if the authors find it necessary. I have a number of specific comments and suggestions for reducing the manuscript length:

P503, L22 - P504, L3: No need to explain this; should be deleted.

P505, L29: full name for i.i.d. should be given.

P506, L12: "complicated statistical properties" – e.g.?

P509, L20: In Fig. 2 caption, you have "logSPM". In Fig. 3, you have "CRRM". But the section heading 2.3 is "Hydrological model". This is somewhat confusing.

P513, L6-9: If 1, 2, . . . here refer to the stages (Fig. 3), say Stage 1, Stage 2, etc here.

P513, L21-24: Not clear, what do you want to suggest here?

P514, L21-22: Why do you merge this only for the quantile approach? If I understood correctly, you could do the same for the time series approach too. I think the important thing is whether the generated (by the weather generator) precipitation (P) is a better representation for the catchment and/or if the rainfall-runoff model simulates catchment runoff better with this P. If yes, I would use only this (i.e. Stage 2 and 3) in both cases, otherwise only the observed P (i.e. Stage 1 and 3). Or use both in both approaches: in this case the results should be presented/discussed in pairs, otherwise it becomes more confusing.

P522, L24 - P523, L5: The assumptions should be part of the method section.

P523, L6-24: These are known, no need to repeat this way, but it may be useful to discuss if these known limitations would have influenced the conclusion drawn on the

hypotheses.

P524, L14: "...typical"?

Tables 1-4: I do not think that these tables need to be in the main body of the manuscript. I suppose the model is not used for the first time. So the previous publications can be referred to for the details of the parameters and distributions. The relations of these parameters or parameter values were not particularly discussed in the results and discussions, therefore these details do not necessarily help the readers. Some of these details may be presented as supplemental materials.

Table 5: Separate table is not needed. This can be easily described within the text.

Table 7: Are these numbers same as in the lower panel of Figs. 6-9? If yes, the lower panel of these figures should be removed. Then these figures (6-9) can be combined into one with four panels.

Fig. 3: What is "climate" (second column) here? Temperature, Radiation ...? Better to specify them directly.

Figs. 6-9: Please see my comment on Table 7 above.

———————————————