Hydrol. Earth Syst. Sci. Discuss., 12, C6343–C6349, 2016
www.hydrol-earth-syst-sci-discuss.net/12/C6343/2016/

Hydrology and Earth System Sciences
Discussions

Interactive comment on "Hierarchy of climate and hydrological uncertainties in transient low flow projections" *by* J.-P. Vidal et al.

Anonymous Referee #2

Received and published: 25 January 2016

Summary

The article presents a transient decomposition of uncertainties in low flow changes in two Alpine catchments. The decomposition is done for 30-year as well as for yearly statistics. As a method for the decomposition, the quasi-ergodic ANOVA method proposed by Hingray and Saïd (2014) is used. It is shown that in the ensemble mean, the low flows generally decrease. The largest fraction of uncertainty comes from internal variability. Hydrological models also contribute substantially to the total uncertainty, which is discussed to be due to differences in snow and evapotranspiration routines between the different hydrological models. Also, a comparison to a standard ANOVA method is presented. It shows that the quasi-ergodic ANOVA method results

in a smoother transient uncertainty decomposition than the standard ANOVA.

General comments

The discussion paper studies a relevant topic and applies state-of-the art methods to look at low-flows in a climate-impact study. It is well written and has a high scientific quality including a sound literature discussion. Also, the mathematical details of the applied method are given in a conclusive way. I recommend publication after my comments below have been taken into account.

Major comments

The paper very much relies on the statistical method of the quasi-ergodic ANOVA introduced by Hingray and Saïd (2014) and consequently, other aspects in the impact modeling chain are less well detailed. I do not mind that and in fact would like to see even more discussion of the QE-ANOVA. There are many assumptions made in the QE-ANOVA and some of them could be verified. For example, the stationarity of the variance has not been proved, something which is even more relevant since the authors use yearly anomalies with a higher degree of variability. If there is a considerable degree of non-stationarity in the variability, I would like this to be included in the discussion of the QE-ANOVA results. Over all, I would also like to see a more critical discussion of the results, not only highlighting the advantages but also the limitations of the QE-ANOVA results. It has to be clear for a Non-ANOVA specialist what they can expect from the method, since many impact modelers would probably like to use the QE-ANOVA approach. For example, the QE-ANOVA approach would not be suitable to study changes of extreme precipitation for which other studies have shown that the variability can increase even in case of decreasing mean. Also, although the literature review is generally good, it would be good to include a part about other ANOVA methods. In particular, the study by Northrop and Chandler (2014) could be cited to refer to another method that is able to deal with an unbalanced design. I have tried to give as detailed comments as possible below. I am looking forward to the author's response and would also be happy to discuss certain aspects if necessary.

Detailed comments

Title: The term "Hierarchy" is a bit misleading, as there is no dominating hierarchy but the contributions of the different uncertainty sources are changing over time. To me, hierarchy is something structurally inherent. Also, the term might lead to confusions with the use of hierarchical ANOVA models, which are not used in this study.

Section 2.2.1: Which variables were used from the GCMs? And what do the different runs in Tab. 1 stand for? Of course, the introduction gives some hints, but it should be clearly stated in this section, too. Also, has the data since the end of ENSEMBLES been published publicly? If so, please indicate the data source.

Page 12656, lines 18-29: It is unclear which parameters that are used for the subsampling. Was it changes in mean annual temperature and precipitation or anything else? Please specify. Also, what are the properties of LHS regarding the joint properties of the subsampled distributions?

Section 2.2.3 and Table 3: A list of required input variables for each hydrological model should be given. Furthermore, since the evapotranspiration process description is mentioned later on to be a potential reason for differences in low flow projections, a short description of the evapotranspiration routines should be included in Table 3 in a similar manner as the snow routines have been listed.

Section 3.1: Please indicate in the text and caption of Fig. 2 that the regimes were

estimated based on reconstructed streamflows and not observations.

Page 12660, lines 19-20: This is only true if the trend model is correctly separating the LSIV from the NFS for any given lead time. In general and given the linear trend model, it is likely that LSIV and the SSIV are overestimated (see also discussion in Raisänen 2001 and Hingray and Saïd (2014). Please discuss this limitation here and at other text passages where the partitioning between NFS and variability is presented.

Page 12660, line 20-22: I understand that the SSIV is generated using the stochastic SDM realizations which in turn use the GCM data as input. Thus, there might be some sort of interaction between the LSIV and SSIV. For e.g., the SDM might generate a different variability for a GCM that is at the high end of the range with respect to one that is at the low end of the range of projected changes. It would be good if the authors could comment on that and discuss this either here or later in the article. Is there a reason why not to construct a 2-way-ANOVA for the variability part of the data? Such an ANOVA could take interactions into account. The design is unbalanced, but this should not affect your sum of squares estimation in a more severe way as what you do in Eq. A3 and A4 where all available runs of a particular GCM are taken thereby giving more weight to the GCMs with more runs.

Page 12662, line 6: It is unclear how the time slice averages are calculated. Do you use some fixed time slices or a moving 30 year approach? Please clarify at some stage in the manuscript. I have noticed that this comes later in 4.2 but I would have expected it to be defined earlier.

Section 4.2, subsection title: Same comment as for the title. Hierarchy is arguably not the best term here.

Page 12663, line 21: Although I agree that internal variability often is larger than other sources of uncertainty, the manuscript has up to this point not given a reason why this had to be expected.

Page 12663, line 27: "previous studies". References are needed to point the reader to the previous studies.

Page 12664, lines 2-4: Interesting to see that the same set of SDMs leads to different degrees of uncertainty distributions when mean streamflow or low flows are analysed. I would ask the authors to also include a short discussion on the relation to other uncertainty sources. Without knowing the details about the employed SDMs, it seems to me that all make use of a similar concept (analogues) and represent only a small part of all available SDMs. If more diverse SDMs would have been used, the SDMs might have contributed more to the total uncertainty.

Page 12664, lines 25-27: The authors use a lognormal distribution to transfer the estimated variances into confidence bounds. I think this cannot be done straight-forwardly since the variance parameter in QE-ANOVA is estimated based on non-logarithmized data. In other words, from the QE-ANOVA you get an effect with is normally distributed with zero mean and some variance, but those parameters are not directly portable to a lognormal distribution, which can be seen by, for e.g., the fact that a lognormal distribution never has zero mean. Anyway, judging from the results in Fig. 10 that look fairly ok, I assume that the authors have taken this into account and there is just a need for more clarification in the text on how the estimated variance and mean parameters are transferred so that a lognormal distribution can be used.

Page 12665, lines 9: The authors should state that also here, the decrease in internal variability for the 30-year time averages is due to the decrease in the ensemble mean.

Page 12665, lines 10-14: A discussion of the decrease in the internal variability is necessary. A link to the relevant equations in the appendix might be helpful for the interested reader. It should also be stated that this decrease is a direct consequence of the quasi-ergodic assumption and could be an artefact.

Page 12665, line 21: "...in 2033-2039, that is for 30 year time-slices starting before 2015." Unclear as both 2033-15 and 2039-15 are not less or equal than 2015.

Page 12666, lines 13-17: Unclear sentence. I understand it in a way that you are discussing the time of emergence for the results based on yearly anomalies, however, I cannot see that the lines in Fig. 11 exceed the 95

Section 5.1: I would suggest including a discussion of the relation between hydrological model uncertainty and the performance of the hydrological models with respect to the analyzed variable - here low flows. The two LSMs (ORCHIDEE and CLSM) used are behaving quite differently from the rest of the hydrological models. If those two were excluded from the analysis, the hydrological model uncertainty would probably be quite a bit smaller. And I would also expect those two HMs to have a worse performance in the reference period than the remaining ensemble - of course due to their main goal to be a LSM rather than a catchment model.

Section 5.1, first paragraph: I would argue that also the common way how HMs are calibrated leads to larger uncertainties for low flows. If, for e.g., NSE is maximized, the model is fitted better to high values than low values as the squared deviations give more weight to high values.

Page 12667, lines 17-19: Isn't this to be expected since HM's fraction of variance is estimated based on the linear trend fit as well as the internal variability is very much smoothed due to the quasi-ergodic assumption, therefore removing a large part of the variability in time? The authors should discuss that the smoothness comes at the cost that one relies on the assumptions made.

Figure 1: The coordinate system is not defined here. Preferably, the coordinates should be converted to Lat/Lon or at least the projection specifications for the lambert projection should be indicated.

Technical comments

Page 12653, line 3: Should be "water manager's"
Page 12666, line 9: "an unchanged . . ." instead of "a unchanged . . ."

References

Hingray, B., Saïd, M. (2014). Partitioning internal variability and model uncertainty components in a multimember multimodel ensemble of climate projections. Journal of Climate, (Madden 1976), 6779–6797. doi:10.1175/JCLI-D-13-00629.1

Northrop, P. J., Chandler, R. E. (2014). Quantifying Sources of Uncertainty in Projections of Future Climate. Journal of Climate, 27(23), 8793–8808. doi:10.1175/JCLI-D-14-00265.1

Räisänen,J., 2001: CO2-induced climate change in CMIP2 experiments: Quantification of agreement and role of internal variability. J. Climate, 14, 2088–2104, doi:10.1175/1520-0442(2001)014,2088:CICCIC.2.0.CO;2

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 12649, 2015.