Review of the manuscript, HESSD 9, 12, 12649–12701, 2015,
Hierarchy of climate and hydrological uncertainties in transient low flow projections,
by J.-P. Vidal, B. Hingray, C. Magand, E.Sauquet, and A. Ducharne

Summary
The study analyses climate changes in low flows in Durance catchment in the French Alps. Using the quasi-ergodic ANOVA method, the uncertainty is decomposed into contributions from different components of the modeling chain. A particular focus is put on the contribution of internal variability and hydrological models. It is shown that the quasi-ergodic ANOVA method can deal well with the high degree of variability, producing smooth results in a transient manner. Also, the time of emergence is estimated and shown to depend on the aggregation time (either 30 year or yearly data). The uncertainty due to the hydrological models is traced back to differences in simulated evapotranspiration and maximum snowpack.

General comments
This is a revised version of the manuscript. Having also reviewed the first version of the manuscript, I would say that is has improved a lot and it is interesting to see the additional results that the authors have included in the supplementary material. The restructured results and discussion sections are much easier to follow now and I particularly like the newly introduced discussion of the quasi-ergodic ANOVA method. I think the authors have addressed the reviewer's comments in an excellent way.
Due to the substantial changes, I have still a few minor comments below. Overall, I suggest acceptance after those minor comments have been addressed. As a reviewer, I would like to thank the authors for an interesting manuscript. I have also learned a lot while reviewing it!

Major comments
No major comments

Detailed comments

Comment on the reply to reviewer 2's comment on section 5.1:
I partly disagree with the authors that HM's performance in today's climate is unrelated to the climate change signal, similarly as one would say for GCMs. I rather think that the nature of GCMs and HMs is rather different. The latter are very much calibrated to time series and should be able to represent the observed values quite well. The former are free running models that derive their own internal variability. A part of the difference to observed data can be explained by different prominent modes in the variability, even over longer aggregation times. Having written this, I do not think the authors need to address this issue in the manuscript. It is rather a general issue of climate impact research, and the study employs a commonly used and well accepted simulation setup.
Section 4.5: The two paragraphs are not really connected to each other. The first part relates to a method comparison of the variability in the contribution of HMs, whereas the second part addresses probable drivers of the contribution. I suggest a better linkage. In my point of view, it would be enough to motivate why the first part is important in the analysis of the HM contribution. The motivation of the second part seems more intuitive to me.
Page 21, lines 13-14: I agree that the results mostly support the QE-ANOVA assumption. Thanks

a lot for this additional analysis! Looking at the figure 1 in the supplementary material, it seems that for Durance in winter the trends in SSIV are different. The QE-ANOVA estimate shows a decrease, while I would say that non-QE-ANOVA result shows a slight increase with time. The difference as such is not very large. However, I would still like to authors to introduce a statement in the manuscript about this, since this might impact on, for e.g. the ToE estimate. In my point of view, this discrepancy leads to the conclusion that the ToE estimate are a bit overly optimistic for Durance in Winter, i.e. with a realistic evolution of the SSIV, the ToE would be a bit later.
Page 21, lines 13-14, and page 22, line 2: The cross-reference to the corresponding figure in the supplementary material would be beneficial.

Technical comments

Whole manuscript: I noticed that the abbreviation HM for hydrological model has been introduced, but is not used consequently throughout the manuscript. I suggest to stick to HM wherever applicable.
Page 12, line 19: “maximum Snow Water Equivalent (maxSWE)” since it is the first occurrence of the term in the manuscript
Page 19, line 11: “corresponds” instead of “correspond”