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Discussions

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

**J.-P. Vidal et al.**

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The referee comments are recalled in italics and followed by the authors' responses.

*The paper proposes a methodology to estimate a transient probability distribution of yearly hydrological variables conditional to ensemble projections. Specifically, yearly anomalies and rolling means over 30 years of anomalies of MAM7 are analysed. The projections are derived from a model chain involving GCMs, statistical downscaling methods and hydrological models and the contribution of each model chain member to total uncertainty is quantified using quasi-ergodic analysis of variance.*

*General comments*

*The authors investigate the relevant topic of future changes to low flow behavior and*



*makes use of transient projections which is important for water management for specific years. The paper is generally well written and provides relevant and timely references. It also presents clear figures to support their statements.*

The authors would like to thank the referee for this positive evaluation of the manuscript.

*However, I see some points that need to be addressed before I feel confident in recommending final publication:*

1. *Since projections are used on hydrological models I miss the description on how these models were tested on robustness. If a hydrological model is not robust - in this case particularly targeting low flow-, I do not trust indications that are made with projections, i.e. in changed conditions. See for instance the simple recommendations made by Klemes (1986).*

   We understand this point of view, and this comment calls for different elements of response.

   First, as mentioned P12657L11, ORCHIDEE has not been calibrated and incidentally shows a very low performance on various low flow metrics. Moreover, only manual sensitivity tests have been performed to select J2000 parameters.

   Second, for GR5J, MORDOR, CEQUEAU and CLSM, tests of robustness have been run by following the approaches recommended by Klemeš (1986): split-sample tests have been performed over two consecutive periods P1 (1980-1994) and P2 (1994-2009). Results on different metrics (including low flow metrics) are summarized by Sauquet et al. (2015, p. 63-69). They show that all models tend to have difficulty in simulating low flows. Moreover, as mentioned in the manuscript P12671L1-7, differential split sample tests have been performed by considering 5-year subperiods with contrasted climatic conditions: 1983-1988 (cold and snowy), 1988-1993 (dry and quite cold), 1993-1998 (wet and snowy), 1999-2004 (wet and warm) and 2004-2009 (dry and warm). The results for all these tests on different metrics (including low flow metrics) are summarized by Sauquet et al. (2015, p. 70-72). They show that all calibrated models seem equally robust with regard to their low flow simulations. Other differential split sample tests have been performed with CLSM and are summarized by Magand et al. (2015). The results from all these tests prompted us to comment in the manuscript on the necessity to include parameter uncertainty in future uncertainty assessments (see P12670L25 to P12671L12). As mentioned in the manuscript P12671L5-7, detailed results of split-sample tests will be presented in a follow-up paper.

Third, and most importantly, this manuscript focuses on the decomposition of uncertainties, independently of the quality of the models, be they GCMs (Global Climate Models), SDMs (Statistical Downscaling Methods) or HMs (Hydrological Models). To this aim, only anomalies with respect to the REF period (1980-2009) are considered throughout the manuscript, in order to remove the effect of potential biases in low flow indicators. What may be relevant to the present paper is an assessment of how the models are able to simulate the observed interannual variability of low flow anomalies. All models show a very good interannual variability of MAM7 anomalies, except for the low-elevation catchment (Verdon@Sainte-Croix) in summer where their performance is a bit lower. The above statement are however not valid for ORCHIDEE which shows only a fair performance.

Some of the above comments will be added to Section 5.3.

2. *The paper reads nicely and logic until the discussion starts. Here there are many parts that actually would belong to the Methods and Results sections. Please, restructure for better readability of the entire paper. (See also Specific comments)*

   We will restructure the manuscript to (1) integrate the analysis of the origins of divergence of low flow responses from different HMs (Sect. 5.2) in the Methods section, keeping only the comparison to findings from other studies, (2) move specific comparisons to other studies currently in the Results section to a ded-



icated subsection of the Discussion section. For the sake of readability of this document, specific comments corresponding to this main comment will not be recalled below.

3. *The authors introduce convincingly the benefit of transient projections. Hence, I would expect a discussion on this benefit underlined with the results that are presented as well as concrete examples for application. Particularly, the time of emergence and related uncertainties are not discussed (see also Specific comments).*

   Some comments will be added to the revised manuscript to discuss the benefit of a transient decomposition of uncertainties, for example for assessing the time of emergence of the change signal on low flows for an individual year or for 30-year time slice averages. Such comments will be included on a subsection discussing the advantages and the limitations of the QE-ANOVA approach. See also responses to specific comments below.

*Specific comments*

- *12652L20 Does the reference present the low number or does it propose alternatives? (not clear from its placement); name these alternatives briefly*

  Peel et al. (2015) actually proposes an alternative to circumvent the low number of GCM runs, by stochastically generating time series based on resampled GCM projections. This will be made explicit in the revised manuscript.

- *12653L5 why is it called comprehensive, briefly state why*

  All possible combinations of the available GCMs / GCM runs / SDMs / SDM realizations / HMs are considered in this dataset. To hopefully be even clearer, each run of each GCM has been downscaled with each SDM, and each realization of this downscaled climate projection dataset has served as forcing for each of the HMs.

- *12654L17 does the higher elevated catchment contain glacierized parts?*

  The Durance@Serre-Ponçon indeed contains some glacierized parts mainly located in the Écrins massif, accounting for around 20km$^2$ in 2006-2009 (Gardent, 2014, p. 181) and shrinking (Gardent et al., 2014). These parts represent only 0.5% of the catchment surface area and glacier melt has therefore little influence on the low flows at Serre-Ponçon.

- *12654L21-L25 I wonder if these reconstitutions and their related uncertainties could influence the outcomes of the uncertainty contribution partitioning. Please, clarify.*

  Reconstituting natural streamflow is a prerequisite of any climate change effect on hydrology in regulated catchments like the Durance one, in order to remove anthropogenic influence from reservoir operations that may vary from year to year. Such reconstitutions – that of course carry some uncertainties – are here only used to calibrate the hydrological models to hopefully simulate the natural component of the catchment hydrology. In the present study, these models are only used with forcings from the downscaled GCM projections. We therefore hardly see how these reconstitutions may influence the uncertainty contribution partitioning as they were used in a similar way by all calibrated hydrological models.

- *12655L16ff the basic principle is introduced, but since three different SDMs are used it would be good to briefly introduce the specific differences among them, or earlier refer to Table2*

  An earlier reference to Table 2 and previous references (which both contain additional information and differences between SDMs) will be made in the revised manuscript.



- *12656l12-17 How often and how much was the temperature corrected? -> possible impacts on results? And impacts on the interpretation in 12668L2 "identical"?*

  The temperature correction (occurrence and amount) is highly dependent on the SDM considered. For example, few and generally small corrections are required for d2gen which include the large-scale temperature at 700hPa above the catchment, corrections are higher for dsclim that includes the large-scale T2m above France as a predictor, and again higher for analog that does not include any temperature-related predictor. Such a correction may therefore contribute to reduce the difference in downscaled projections from the different SDMs, at least for the temperature at the scale of the whole Durance basin, the spatial aspects being unchanged. It may therefore contribute to reduce the SDM uncertainty part in the overall uncertainty. This discussion will be added to the revised manuscript. Concerning the second point of this comment, there is no impact on the interpretation of the sentence P12669L2: when a specific combination (GCM / GCM run / SDM / SDM realization) is considered, meteorological forcings (downscaled gridded projections) are indeed identical for all HMs.

- *12657L13 what are the consequences of this initially coupled mode if any?*

  There is no direct consequence as they are here used here in a forced mode. This sentence was simply intended to highlight the initial purpose of such models – which is different from the one of rainfall-runoff hydrological models – and therefore the potentially lower adequacy to such catchment-scale modelling. It will be clarified in the revised text.

- *12657L14 Here I miss the description on how the hydrological models were tested on robustness (Klemeš 1986)*

  See response to main comment #1.

- *12657L19 is there a practical motivation for choosing the MAM7 and not any other low flow metric?*

The choice of the MAM7 was guided by the requirement for (1) an annual indicator, and (2) an indicator commonly used internationally for operational purposes. This will be clarified in the revised text.

- *12658L5 is there snowfall already before November in the higher catchment?*

  Based on data from the 1980-2009 period, snowfall may happen in late October in the Durance@Serre-Ponçon but in limited amounts.

- *12665L26 -12666L2 Methods not Results – also I find this Time of Emergence very appealing and would appreciate more details and thoughts on applicability on it*

  See response to main comment #3. The concept of Time of Emergence (ToE) has been introduced by Giorgi and Bi (2009) and popularized by Hawkins and Sutton (2012). The only requirement for applying this concept is an estimate of the multimodel signal of change and an estimate of the natural/internal variability. Some discussion will be added to the revised manuscript.

- *12669L26-28 actually, less snow pack can have two natural reasons related to precipitation: 1) less precipitation fell in general or 2) precipitation fell as rain; these two would have different effects and would not necessarily result in more water for winter low flow*

  The referee is right. But the fact that precipitation totals are identical for all HMs (for a specific GCM / GCM run / SDM / SDM realization) makes reason 1) not relevant here. What is left is therefore reason 2), hence our difficulty on interpreting this result. We would be happy to have more external insights on this particular point, as mentioned in the manuscript.

- *12671L13 I would appreciate a discussion on the time of Emergence and its relevance for application respectively the limitations that are related to this metric; could it be influenced by the initial calibration for instance?*



  See response to main comment #3. We do not believe the ToE metric could be influenced by the calibration processes, but some additional analysis may confirm this. What is true, however, is that the ToE is intrinsically linked to the choice of the reference period chosen for calculating the anomalies (see Hawkins and Sutton, 2016, for some relevant comments on that issue). It is also highly linked to the quality of the estimates for both the multimodel mean signal and the internal variability. The time series approach used here makes this estimation rather robust. This would have not been the case with other uncertainty estimation approaches such as the one proposed by Yip et al. (2011) as the contribution of internal variability to total uncertainty variance is here very high.

- *12671L25- 12672L4 the benefit of transient quantification of uncertainties should be discussed before appearing in the conclusions - potentially comparing to other studies that used other than low flow variables and then leading to applicability particularly for the water management with the focus on low flow as pointed at in the conclusions 12672L24f*

  Some discussion on this point will be added to the revised manuscript. See also the response to main comment #3 and responses to specific comments on the Time of Emergence (ToE). Benefits for the water manager will be discussed, and notably how such results may inform robust adaptation strategies and how they may change the focus of such strategies compared to previous studies that looked only at changes in 30-year average quantities.

- *12689 I like this Figure very much!*

  Thank you.

- *12692 Figure4 Durance@Serre-Ponçon makes me wonder how suitable GCMs in higher Alpine catchments are. ECHAM5 and CNCM33 show opposite signals over the entire period (winter). Could the authors add some words on the suitability of GCMs in high Alpine catchments?*

The performance of GCMs in higher Alpine catchment is actually not really relevant here. Indeed, the downscaling step makes use of GCM predictors not necessarily located above the specific catchment. Geopotential height predictors used by all 3 SDMs are for example considered over a large spatial domain covering a large part of France.

*Technical comments*

- *12650L8 and L9 "of" ->"for"?*

  We believe the appropriate use of "to take account" is with "of".

- *12650L12 "possible transient futures" rephrase!*

  We may replace it by "transient possible futures" if required.

- *12650L16 "most elevated", only two catchments are studied -> change*

  This will be replaced by "more elevated".

- *12650L19-21 Unclear, rephrase*

  This will be replaced by "The time of emergence of the change signal is however detected for low-flow averages over 30-year time slices starting as early as 2020."

- *12651L20 either "paragraphs propose" or "paragraph proposes" (I guess the latter?)*

  This will be replaced by: "The following paragraphs propose...".

- *12652L25f reformulate for better understanding*

  This will be replaced by: "Lastly, the majority of hydrological change studies so far mainly focused on uncertainties in the streamflow regime."



- *12652L26 when -> while*

  The sentence will be modified as: "Some of them [...], but relatively few..."

- *12653L1 "possible futures", please change to "future possibilities" or similar*

  We would like to keep the wording as “possible futures” as we believe it conveys the appropriate concept.

- *12653L5 move 1980-2065 after hydrological projections*

  We will rephrase the sentence as: "[...] transient hydrological projections over the 1980-2065 period..."

- *12653L12 verb missing after critically*

  We don’t think any verb is missing, as the sentence draws a parallel between "relative contributions of model uncertainty...." and "[relative contributions] of both large-scale and local-scale components of internal variability". We may add numbers in brackets to make it more explicit.

- *12655L7 add "the" before year*

  The sentence will be modified accordingly.

- *12655L9 that -> these GCM runs*

  We unfortunately don’t understand the modification proposed by the referee.

- *12655L10 runs -> is*

  We believe using the verb "to run" is valid here.

- *12656L1 predictors*

  The plural is indeed appropriate here.

- *12659L5 NFS would mean N* F* S mathematically speaking, please change to S N F or similar throughout*

  There is obviously a misunderstanding over "NFS". NFS refers to Noise-Free Signal, an abbreviation already used by Hingray and Saïd (2014). It will be made more explicit and defined earlier on in the revised manuscript to remove any possible confusion with mathematic notations.

- *12661L16 did Hingray and Saïd do the same of did they overfit - not clear from this sentence*

  The sentence is indeed unclear and will be rephrased. Hingray and Saïd (2014) also used a linear trend not to overfit interannual fluctuations.

- *12668L13 "snowpack building" rephrase*

  We propose to replace it by "snowpack accumulation and snowpack melt".

- *12671L27 change "account of" into "into account" and place after "variability"*

  We believe the two formulations are equally valid, but we may use the proposed one in the revised manuscript.

- *12692 correct to "catchments" in the caption*

  This typo will be corrected.

- *12701 add "the" before year 2065*

  The sentence will be modified accordingly.

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