



# ***Interactive comment on “Multilevel and multiscale drought reanalysis over France with the Safran-Isba-Modcou hydrometeorological suite” by J.-P. Vidal et al.***

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## *General comments*

*The paper presents a retrospective analysis of drought events over France, based on variables from a modelling chain that uses a 50-year high resolution atmospheric reanalysis to force a land-surface schema and a hydrological simulation model. Meteorological, agricultural and hydrological drought types are assessed over France and through a range of time scales (1 to 24 months). The topic of the paper is of interest to*

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*the hydrometeorological community and suitable for publication in the HESS journal. The paper is very interesting and well written. It summarizes well the huge amount of computation necessary to tackle the challenges proposed by the authors. The objective of the paper is clearly stated in the introduction and the approach adopted is well described. The results are presented in a easy-to-understand way and contribute to the scientific understanding of the drought-characterization problem. Some parts of the paper could however be improved or made clearer with minor changes. A number of specific comments are listed below. They aim at discussing some scientific points of the paper and suggesting some improvements to the text.*

The authors would first like to thank the referee for his/her fruitful comments on the manuscript. These comments (in italic below) have been particularly useful to expand the discussion part and to hopefully improve the manuscript.

*A main general comment that I would like to stress here is the fact that the identification of drought events is based on reanalysis data from an atmospheric system and on model-based variables obtained from a modelling chain forced by the atmospheric reanalysis. The results are then model-dependent and the fact that they are not based directly on observed variables should not be forgotten.*

I definitely agree with this, that's why some comparisons have been drawn on the spatio-temporal evolution of droughts with an index based on streamflow observations (the RDI index) for example (Section 4). Moreover, a large part of the initial discussion was assigned to the uncertainty in results that are a direct consequence of the choice of the modelling suite. This part of the discussion has been expanded in the revised text.

*In fact, while the authors expose well, in the beginning of the paper (§2.1, pages 6459-6460), the modelling chain used, they do not indicate the performance (comparatively*

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*to observed values) of the SIM model suite, specifically for the variables used to identify the drought events (pre- cipitation, soil moisture, streamflow). It would be interesting, for instance, to have an indication on how the monthly precipitation from the SAFRAN reanalysis compares on average to observed precipitation amounts. This could be added, for instance, to lines 19-20 (page 6459), instead of just sending the reader to a paper in the list of ref- erences.*

Some comments on the uncertainty in precipitation from the Safran reanalysis has been added in the text in order to provide the reader with some confidence on the precipitation data used in this work.

*Also, it is stated that the land-surface scheme and the simulated streamflow values were validated by Habets et al. (2008) (lines 24-25, page 6459, and lines 19-21, page 6460, respectively) for a 10-yr simulation. How about the validation of the 50-yr simulation used by the authors? How does this 10-yr period compares to the 50-yr period used in the paper (for instance, if the 10-yr period is much dr yer/wetter period, etc.). I think this is an important point to validate the results presented in the paper or at least to give the reader more confidence in them. Therefore, it would be interesting to have some numerical information on the performance of the suite for the 50-yr period. It is expected that the drought event identification will be closer to observed drought events only if the model performs well, with no significant bias.*

No detailed hydrological validation of the SIM suite has been performed on the 50-year period. This would be very difficult mainly because of the lack of hydrological data at the beginning of the period, and it is out of the scope of this paper. However, an extensive validation of the 50-year Safran atmospheric reanalysis has been recently conducted (Vidal et al., 2010). The 10-year period (1995-2005) used by Habets et al. (2008) to validate the whole SIM suite is roughly similar to the 50-year period in terms of national-scale precipitation. Some more comments on this validation have been added in the revised Discussion part.

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*Jointly, the fact that the drought events identified are "drought model-based events" should be clearly stated all over the paper, including in the legends of the figures. It should be clear that what is presented is not the results from the analysis of observed time series.*

Cf. responses above

## *Specific comments*

*1. General: figures are too small. This make it difficult to read in them and understand the text describing the results. Please, make them all bigger.*

Figures are indeed very small in the hessd printer-friendly version (but less so in the online version). Their size has been reduced by the journal editors. They will be in full page in the final version, so they would hopefully be easier to read.

*2. P.6456, L.25: what do you mean by "regularly occurred in Europe"? How much "regularly"?*

Actually, there is very little regularity in their occurrence. The adverb has thus been removed.

*3. P.6461 (L.15-27) and P.6462 (L.1-15). I see this part basically as a literature review. I suggest to separate: 1) review of the existing approaches and 2) data and methods used in the paper.*

The structure has been changed in order to take this comment into account.

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4. *P.6462, L.12-15: Could you explain in what the approaches mentioned are different?*

I wanted to point out that the indices produced through this approach are intrinsically relative indices, as opposed to "absolute" indices like streamflow deficits in m<sup>3</sup> (even if the latter may be normalized in a second step). The sentence has been modified to make it clear.

5. *P.6462, L.24-26: Statistical distributions fitting precipitation data depend on the spatial (point or areal) scale and on the time (duration over which precipitation is accumulated) scale of the precipitation variable. Please precise the spatial and temporal scales for which the gamma distribution is suitable, as stated by the authors.*

I totally agree with that. The SPI computation is (nearly) always done with monthly precipitation, and Lloyd-Hughes and Saunders (2002) used a 0.5° gridded dataset. One may argue that point precipitation (as used for example by Vicente-Serrano, 2006) may follow a different type of distribution with respect to the corresponding grid square, but the monthly time scale already ensures a rather important integration of the variable.

6. *P.6463, L.9-11: Why "a great care" is recommended when choosing among the potentially suitable distributions? What are the impacts one expects to have on the results?*

Choosing a poorly fitted theoretical distribution may lead to large errors in estimating percentiles associated with a given value, especially for extreme values. As a consequence, corresponding drought index values may be considerably under- or over-estimated. A sentence has been added to make it clearer.

7. *P.6463, L.23-24: ". . .generating space-time continuous fields of drought index*

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values, for each drought type. . .". How was (if it was) handled the spatial correlation? Specifically in the case of the "hydrological drought", for which indexes were computed at specific gauging stations, how were the "space continuous fields" generated?

It was actually not appropriate to use the word "continuous". SPI and SSWI data are gridded fields computed independently for each grid point, thus with no handling of spatial correlation. As for SFI, outputs are not gridded. The word "continuous" has thus been removed.

*8. P.6464, L.5-9: Did the authors conduct a sensitivity analysis to the choice of the threshold? What impacts do you expect on the results?*

No sensitivity analysis was conducted on this threshold, but it would be a very interesting work to do. Choosing a lower probability threshold would lead to identify only the few most extreme events and possibly introduce a bias on statistics over the 50-year period. This choice will inevitably result from a compromise, but it should be ultimately driven by water management operational thresholds. Comments on this threshold have been added in the Discussion part.

*9. P.6465, L.8-13: From a hydrological point of view, aren't these thresholds for spatial aggregation used for identifying drought events too large for the typical size of French catchments? How does that affect the detection of small-scale droughts (regional scale or watershed scale), useful for operational water management?*

Yes, indeed, these thresholds are very large for small-scale droughts. These values have been chosen rather arbitrarily in order to remove the weakest spatio-temporal links between drought events at the national scale. This national scale is actually rather small for event detection (drought events extend to the European scale), but too large for water management. For operational water management, it would be much more useful to base these thresholds on area percentages of the catchment/water

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resource zone under study. Comments on this have been added in the Discussion part.

*10. P.6465, L.17-19: The authors compute the "mean duration of an event". Would it be useful to similarly compute the "maximum duration of the event" to have a picture of the time-extent of the drought?*

The maximum duration of an event is not a robust variable, that's why it has not been chosen. As a matter of fact, the beginning (resp. end) of a spatio-temporal drought event may (or not) affect only a small area, compared to the heart of the event. However, it would be useful to compute this variable (among others) in order to characterize the temporal shape of an event.

*11. P.6466, L.20-21, please specify the value of the threshold. Maybe it would be useful to introduce both thresholds (the 20%, presented earlier, and the 5%, first mentioned here) together, earlier in the text.*

The value has been specified and this more extreme threshold is now introduced together with the 20% one

*12. P.6468, §4. In the examples presented, the authors highlight two observed events (1976 and 2003) that were identified by their approach. Were there events identified by the approach, but not corresponding to observed ones? It would also be interesting to have a comment on eventual (if any!) "false alerts" or "misses" of the approach. This would clarify, for instance, the limitations of the approach, together with its strengths.*

All observed major events were identified by the method and all major event identified were also observed. The actual detection of a drought event actually depends on the chosen threshold level (see response to comment 9.). Meteorological droughts are identified from atmospheric reanalysis data, so there should be reduced uncertainty

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there. For agricultural droughts, there are very few measurements available in order to assess spatially the correct event identification. For hydrological droughts, the correspondance between computed and observed streamflow values depends heavily on reservoir operations for sustaining low-flows, that were not taken into account in this large-scale modelling study. Some comments on these rather important points have been added in the Discussion part.

*13. P.6470, §5: The fundamental questions numbers 2 to 4 are not correctly formulated. The methods and results presented by the authors do not correspond to a "forecasting approach". Thus, in my opinion, the use of "will" in the questions is not appropriate. These questions should be changed to "When it often starts", "How long it often lasts", "How severe it often is".*

The questions have been rephrased (and are now in the introduction part as suggested by reviewer #4).

*14. §5.1, §5.2, §5.3, §5.4: It should be clearly specified in these paragraphs and in the related figures that the results correspond to drought events identified on the basis of reanalysis data and model results (see my main general comment above).*

This has been specified in the revised text.

*16. P.6474, §6: Why hydrological droughts are not assessed here? From the stations computed weren't there any "national scale" pattern detected? Any other reason? Please clarify this point in the text.*

Hydrological droughts are not assessed here because drought index time series are not gridded and thus no spatial continuity can be ensured. This point has been specified in the revised text. It would be however possible to compute an index based

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on gridded runoff, but the downstream propagation of streamflow would not be taken into account.

*19. P.6478, L.22-23: Please clarify how operational hydrologists could make efficient use of the tools proposed in the paper to improve their water management activities in their local catchments.*

A sentence has been added to clarify how water managers could use this set of indices for drought monitoring.

*20. P.6478, L.24: what do you mean by "the level of the hydrological cycle"? Please, rephrase it.*

The different levels of the land surface hydrological cycle (precipitation, soil moisture, streamflow) are first introduced in the introduction and can be linked to the three types of physical droughts defined by Wilhite and Glantz (1985).

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