



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

Anonymous Referee #2

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Comments on the paper by Vidal et al., “Multilevel and multiscale drought reanalysis over France with the Safran-Isba-Modcou hydrometeorological suite”

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.

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. 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 to observed values) of the SIM model suite, specifically for the variables used to identify the drought events (precipitation, 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 references. 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 dryer/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

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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. 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.

Specific comments (P=page, L=line):

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.
2. P.6456, L.25: what do you mean by “regularly occurred in Europe”? How much “regularly”?
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.
4. P.6462, L.12-15: Could you explain in what the approaches mentioned are different?
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.
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?
7. P.6463, L.23-24: “...generating space-time continuous fields of drought index 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?

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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?

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?

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?

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.

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.

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".

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).

15. P.6471, L.25: change "situation" to "situations"

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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.

17. P.6478, L.20: change "describes" to "describe"

18. P.6478, L.21: change "but also by changes in temperature" to "but also in temperature".

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.

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

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