

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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Received and published: 15 February 2010

General Comments

This is an excellent paper on the spatial and temporal dynamics of meteorological, agricultural and hydrological droughts in France. Whilst this a novel and particularly valuable contribution as a regional study (as the authors note, this has not been carried out before in France), it is also a valuable and timely contribution to the international research community. Whilst space-time drought analysis have been well advanced re-

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cently in the USA and on global scale, this kind of work has not been applied in Europe so comprehensively; there are also significant methodological benefits in the current paper, compared to previous approaches. As such, it is an internationally significant dataset and suite of analyses. The methods used are robust and allow consistent application across levels, space and time. The paper is very well presented, with very clear graphics which do a good job of synthesising a great deal of complex information in a manageable way to the reader. The authors should also be congratulated on doing such a good job of ensuring a top-quality manuscript which generally needs only minor attention. The paper is almost publishable as-is, but some minor English adjustments would enhance it; equally, in my view, a slight rebalancing of the paper could make a big difference, with some detail cut from earlier on and replaced with a little more penetrating discussion later.

The authors first thank the anonymous reviewer #1 for his/her very positive general comments on the manuscript. They also thank him/her for the specific and technical comments (in italic below) that will lead to significant improvements on the manuscript.

Specific Comments

Whilst the authors do consider the limitations of their work, I think it would be well worth them underlining in the discussions and conclusions that this is all based on modelling assessments. There is some consideration of uncertainties in section 7, but I feel this could be strengthened. At several places in the paper, they acknowledge that the model has been tested elsewhere (e.g. section 2.1), and do make comparisons with independent observed data (e.g. section 4). However, it may be worth strengthening the discussion, to make it more transparent and to underline an ongoing need to provide "ground-truth" for these sorts of broad-scale modelling assessments.

Some assessments of the modelling suite have been added in Section 2.1, and a

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whole subsection of the Discussion part is now assigned on the different sources of uncertainty.

The paper feels a little on the long side in places. Overall length is probably OK, but perhaps there could be more discussion of the implications of this work later in the paper, at expense of some of the detail in earlier sections (as noted in the technical comments). The authors describe some applications of the method in the conclusions, but there are probably wider implications of this study (for drought monitoring in Europe, for example) which could be brought out in the discussion.

Some more perspectives have been added in the Conclusions part.

I was confused by the section on seasonality (5.2), and Fig 7. Which I think is slightly ambiguous and potentially misleading. This may just be my reading, but I would be grateful if the authors could clarify this.

Figure 7 has been slightly changed to hopefully remove any ambiguity. See below in the responses to technical comments.

Language generally very good – worth a thorough proof read, many minor corrections below but some generic points (over use of "indeed" and "besides. . ." times; some confusion with plurals).

The authors thank the reviewer for his detailed review of the language and the grammar. All suggested corrections (omitted below in the technical comments part) have been taken into account in the revised version.

Technical comments

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6457, 9. The authors point to the three types of drought identified by Wilhite and Glantz (1985). Other authors discriminate drought types along different lines (e.g. separating groundwater and streamflow). Perhaps just worth an additional sentence to set the wider background of drought type discrimination, before referring to W & G model.

A sentence has been added to make that point.

6458, 29. "Sect. 4" – earlier sections referred to as "Section 3" etc. Also "Sect." is used at top of 6459. Check consistency through paper.

This follows the journal recommendations for authors, with abbreviations only within a sentence.

6459, 25. The term and concept "force-restore" may not be familiar to many readers. No need to go into detail as there are references to other work, but might be worth a very brief sentence in parenthesis with reference. "force-restore (i.e. . . .)".

A short sentence in parenthesis has been added to describe it briefly.

Section 2.2.1 Do the authors need to cite all of this work (e.g. the multiple studies carried out by Sheffield, Wood and colleagues?) If the aim is just to refer to where other authors have applied SPI-type analysis to soil moisture and runoff, this whole section could be trimmed down, with just a few references, without losing much.

The paragraph has been trimmed down.

Section 2.2.2. Could probably also be trimmed down by being sparing with refs to previous work. This may allow a fuller description of the method used in this paper (which only gets a short para at the end). As it is, this last para is not that informative.

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This section has also been reorganized and some more details on the method have been added.

6464, 9. " . . .of all drought indices". This is a bit ambiguous (sounds like referring to SPI, SSWI, SFI). Surely this is the distribution in time of all values for a particular drought index for a given location.

You're right. The sentence has been slightly modified to remove the ambiguity.

6466, 24. "It thus hides periods of less extensive but less extreme drought conditions". If these periods are hidden, it is not clear that this statement follows on from the graph; should the authors refer to an example?

I wanted to emphasize that some periods that were dry but during which the index did not reach the chosen threshold may not appear on this graph. This ambiguous sentence has been removed.

6468, 1. Onwards. Comment: this is very useful as it does allow a direct comparison with observed data, and this section is very well informed, with good cross references to previously published work in France and elsewhere; this does provide the reader with increased confidence in the results from the modelling work.

The authors thank the reviewer for this positive comment.

6470, 12. There is some tension here between the use of the term "about a drought event" and then the first bullet point which asks how often it will occur. In the strict sense, a drought event can only happen once. Perhaps the final sentence should read "...about drought events". And then the bullet points be made plural "how often do they occur" "when do they start" "how long do they last" etc. Or just change the first bullet

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to read "how often do droughts of this type occur" or similar.

These suggestions have been adopted in the revised text (and the list of questions moved to the introduction as suggested by reviewer #4).

6471, 3. Is the spatial variability of SPI really so limited? There appears to be quite a reasonable gradient in SPI12.

Yes, indeed. This specificity for the 12-month time scale has been added in the text.

6471, 6 – 10. The authors pick out general patterns, but there are clearly exceptions (very sandy, low clay area in NW does not have many events), and the authors do not really explain why there are differences between the centre and SW dependent on vegetation. The authors should explain more fully and/or underline that these are only general observations and more work is needed to explain the patterns.

Yes, the general pattern mentioned in the text is the one that can be explained the more easily. As noted by the referee, there are other patterns that would need more work because they are the consequence of physical processes (soil and vegetation) with opposite effects on the shape of droughts. The text has been revised accordingly.

6471, 20 – 28, and Caption to Fig 7. MAJOR COMMENT: I am confused by this section. I thought the idea would be to test for situations where the frequency of events in the most frequent starting season is not higher than that which would be expected by chance (1 in 4), and hide these. But reading the text, it sounds like the authors are referring to the significant cases being those shaded black. If they are shaded, I don't see how the reader is meant to refer to the season in question. This makes this section very difficult to interpret. Please clarify the process and how the reader should interpret the black shading in the fig.

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The cases with black points did actually indicate where the proportion of events is significantly higher than the 1 in 4 probability. At the original size, cell colors were visible below the points. Unfortunately, after the journal editors reduced its size, it was not the case any more. Figure 7 has thus been modified in order to remove this ambiguity: pale colors are now attributed to non-significant cases, and vivid colors to significant cases.

6473, 13 – 14. Is this necessarily true? In many areas the streamflow droughts are shorter than the soil moisture droughts (although it is quite difficult to tell with the streamflow points comparing to the gridded points).

That was a mistake. What I meant was actually "shorter", and the text has been revised accordingly. But the point made by the referee (comparison of streamflow points to gridded soil moisture) is very relevant, and it would be interesting to compare for example indices of catchment-averaged soil moisture with corresponding streamflow indices.

6475, 4. This is a very important point. Whether these are independent events from a climatic or drought management point of view or not is an open question. Perhaps the authors should comment in more detail on the extent to which they see these sort of events as a truly unified entity as opposed to a construct of the method. Whilst the method is undoubtedly useful in integrating the space and time elements in characterising an "event", presumably the individual phases have different signatures in terms of climatic causes and actual spatio-temporal evolution. The authors should perhaps consider this in the discussion.

Yes, indeed. The spatio-temporal independence of events should ideally be based on atmospheric considerations, and one may think about using the occurrence and persistence of distinct weather types. A comment on this has been added to the

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Discussion part.

Section 6.2. Is generally a good way to present this information. Perhaps the authors should address why the significance of this (why identify these benchmark droughts) – what is the practical utility of these findings?

These findings about benchmark events would be useful if a similar analysis is made at the spatial scale relevant for water resource management (catchment/water resource zone). A comment on this has been added in the conclusions.

Section 7, discussion. I would like to see this expanded, given the effort that has gone into such a comprehensive analysis over these three dimensions. The work has generated a mountain of data, but there is really only brief consideration of implications (6477, 14 – 21, and end of conclusion. The end of the discussion touches on usefulness of seasonality, but what about the drought duration & frequency findings, e.g. for regional water management, What is the significance for the scientific community?

The Discussion part has been restructured and expanded, and some more comments have been added in the conclusions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 6455, 2009.

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