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Hydrology and Earth System Sciences Discussions

# *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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Please note that a revised version of the manuscript is provided as a supplement file.

## 1 Response to anonymous referee #1

*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 recently 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 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*

*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 refererence. "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 collagues?) 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.*

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 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.*

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 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.

## 2 Response to anonymous referee #2

*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 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 eveolution 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 ex- pose 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 (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.

*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.

*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 $m^3$ (even if the 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 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 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 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 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).

## 3 Response to anonymous referee #3

*This study presents a modeling framework for assessing drought characteristics over France, during the past 50 years (1958-2008). In general, this is a well written paper, and the topic is appropriate for the Hydrology and Earth System Sciences journal. The methodology is generally sound, and it is explained relatively well. The coupling, albeit offline, of the three models is a particular strength having implications in assessing droughts over regions with no in-situ measurements, and representing different types of drought (potentially adding socio-economic models). However, there are a number of points that need clarification and minor revisions which are outlined below.*

The authors thank the referee for his/her fruitful comments (in italics below) on the manuscript.

*Although using a model to evaluate drought can offer some advantages over just using point observations (e.g. space-time continuous fields, indirectly observed variables), I think some discussion of potential uncertainties with respect to the 10 year validation versus 50 year simulation, would be a nice addition.*

Some comments on this source of uncertainty have been added to the Discussion part

*I think having larger figures would be beneficial for the reader (especially 4 and 5).*

I totally agree, but their size has been reduced by the journal editors for the hessd formatting. They will be in full page in the final version, so they would hopefully be easier to read. As for figures 4 and 5, they are bound to be presented both rotated on the same page.

*How are the timing results (section 5.2) affected by the sensitivity to the chosen drought*



*threshold? That is, would a 30% threshold lead to different results? Although the authors have provided some significance testing, I think providing some physical association like climate teleconnections (admittedly not easy to do) would strengthen the argument.*

This is a very interesting question, and it would be worthwhile investigating it further. I expect results might be somewhat different for e.g. a 30% threshold because of the temporal shape of the drought events. However, sensitivity analyses on the threshold level are very time-consuming and could not be performed within this project. I am not convinced about links between climate teleconnections and drought seasonality patterns, because no clear pattern can be spotted on meteorological droughts, and thus the signal appears to come from the land surface hydrological system itself. This view could be tempered by the impact of temperature (and thus evapotranspiration) anomalies that may come from global-scale teleconnections. This should definitely be worthwhile investigating further.

*In Section 5.3, would it be more appropriate to use the mean duration of the identified individual drought events instead of the local-scale duration? The same drought event might cover two distinct areas but not concurrently, therefore using the local-scale duration, as valuable as it may be, could underestimate the actual event durations.*

I definitely agree, and that's why the mean duration of spatio-temporal drought events has been looked at in Section 6.1

*In Section 5.4, how is the mean magnitude exactly calculated? If it the monthly severity divided by the time period, I would expect that it would be lower for longer periods. This is not explained ver y well in the text.*

The definition of the magnitude of an event is given in Section 2.31. and in Figure 2, together with the definition of the other local-scale drought descriptors. The magnitude is the absolute value of the sum of drought index values during the event.

*p. 6457 (lines 3-4): I would change "economic impacts" to "impacts" in general, droughts have external costs as well.*

I agree, and social and environmental impacts have also been mentionned in the text.

*p. 6459 (lines 19-20): Since the reference is in press, it would be useful to add a short summary of the validation results.*

The reference is now available on-line: doi: 10.1002/joc.2003. Some comments have been added in the revised text to summarize the validation of precipitation from the Safran reanalysis.

*p. 6467: The introductory part of Section 4 is a little confusing. Was the 3-month period choice arbitrary? Why is the RDI chosen instead of other indices? It seems to be related to hydrological drought, is that right? Are there other studies that look at drought over France that were not included here?*

Yes, the 3-month time scale has been chosen arbitrarily for illustrative purposes. As mentioned in the text, the RDI has been chosen as it is a hydrological drought index that is based on observed streamflow (as opposed to SFI which is based on computed streamflow). To the knowledge of the authors, no other study looked at hydrological droughts at the scale of France. Comparing results with an independent hydrological (i.e., at the end of the land-surface hydrological cycle) index enables to identify possible discrepancies that would result from errors in the propagation of events through the hydrological cycle as modelled with the SIM suite. This last comment has been added in the text.



*p. 6469 (line 17): change “on the contrary to” to “in contrast with”.*

The text has been modified accordingly.

*p. 6474 (line 5): what about hydrological droughts?*

It is not possible to derive spatio-temporal characteristics of hydrological droughts as SFI series are point series, and not gridded series like SPI and SSWI. It would be possible to derive a gridded index based on runoff, but the downstream propagation of streamflow could not be taken into account.

## 4 Response to anonymous referee #4

*I agree with the other reviewers that the authors are to be commended for this comprehensive analysis of drought in France and also recommend the paper for publication.*

The authors thank the referee for his/her fruitful comments (in italics below) on the manuscript.

*The paper would, however, benefit from more concise and more scientific presentation. In particular, it is very descriptive and mixes discussion into the presentation of the results, which makes it difficult for the reader to extract the key findings about propagation of drought and scale. These could definitely be highlighted better.*

The text has been revised and the Discussion part expanded to hopefully better highlight the key findings.

*The abstract should not only state was analyzed but also needs one or two sentences on the findings/results.*

Summary sentences have been added to the abstract.

*The introduction lists a lot of studies, but falls short on summarizing what groups of studies actually found and which conclusions they drew that are relevant to this study. The aims are a bit hidden within the text and could be brought out better, preferably together with some research questions such as in the beginning of section 5 (where there should be only results and no repetition of research questions).*

The introduction text has been revised and now includes the questions that were initially in section 5.



*In sections 2.2 and 2.3 in particular I found it difficult to distinguish between review of methods and the exact choice of methods that were applied in this study and why. This needs to be made clearer.*

Section 2.2 has been restructured in order to make clearer the distinction between review and approach chosen. The text in section 2.3 has been modified to hopefully make clearer such distinctions.

*The presentation of drought characteristics are very good and illustrative. By immediately jumping into comparisons with the literature, however, the great results of this study seem to drown a bit. The work would be highlighted better if the results were presented plainly first just as they are, and all discussion with respect to the literature was moved to a more structured and more expansive discussion section.*

Indeed, comparisons with literature do closely follow the presentation of examples of drought propagation, and reviewer #1 found that it increases confidence in the modelling results. However, no immediate comparison with literature has been made for drought event characteristics, as no previous study looked at them in France. Some comments/comparisons have been moved to an expanded Discussion part.

*Discussion and conclusion would benefit from more focus on the multi-level and multi-scale aspects that were actually found. Otherwise the title promises too much.*

The discussion and conclusion parts have been expanded and some more comments on multilevel and multiscale aspects have been added.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/6/C3332/2010/hessd-6-C3332-2010-supplement.pdf

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