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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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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

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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.

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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.

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