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Interactive comment on "Evolution of spatio-temporal drought characteristics: validation, projections and effect of adaptation scenarios" *by* J.-P. Vidal et al.

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The referee comments are recalled in italic and followed by the authors responses.

General points

This paper uses a downscaled climate model to calculate precipitation and soil moisture drought indices over France. It compares these drought events with a high resolution reanalysis and shows that the model does a reasonable job compared with the reanalysis. One concern about this comparison is that the simulation is only for a 50 year period with a limited number of drought events. This limits the statistical analysis

and needs to be explicitly discussed in the paper.

The author agree with the referee that 50-years of data for statistically characterizing spatio-temporal drought events may be a not-so-long period. However, the assessment of the agreement between reanalyzed events and simulated events is made only on the whole distribution (through the use of the Kolmogorov-Smirnov (K-S) test, cf. §4.3 L1636 L14-27 and Fig. 6, P1665) and not on extremes. Moreover, Fig. 3 (P1662) shows that between 50 and 90 observed major events are considered in the statistical analysis, which is a quite reasonable sample size. Furthermore, in order to take account of the uncertainty due to sample size – as asked by the referee–, we had decided not to plot the results of the K-S test, but rather the p-value of the test, which clearly depends on the sample size. And this is shown in Fig. 6 (P1665) of the manuscript.

Future projections are made with the climate model and a couple of theoretical adaptation scenarios. I like the idea of considering adaptation scenarios. However, the adaptation studies are purely theoretical and not necessarily realistic. It concerns me that Figure 1 shows the threshold for drought changing so that it appears that it is defined so that it remains at the 20% level throughout. Therefore this, almost by definition, negates any climate changes and the results in Figures 7-10 for the adaptation scenarios show little change. Are the differences between the adapted future and present statistically significant in Figure 7-10? I suspect that the sampling size is a constraint in such an analysis? I would suggest that more thought would be put into the adaptation scenarios and they are made more realistic.

There are several points worth discussing in this comment. First, yes, the adaptation scenarios considered here are purely theoretical and this is clearly stated at different points in the manuscript, together with the inherent limitations that follows (§3.2 covering P1630-1633, and §6.2.1 P1643-1644). Moreover, the manuscript states right from the abstract that the theoretical adaptation scenarios considered here "call for more realistic scenarios at both the catchment and national scale" (P1620 L25). So the authors definitely agree with the referee that "more thought would be put into the adaptation scenarios and they are made more realistic". This is however out of the scope of this paper, as defined by the three research questions detailed in the introduction (P1623 L1-6). Nevertheless, the authors have already provided some ways forward to derive more realistic adaptation scenarios, that can be found in the dedicated discussion section of the manuscript (P1643 L23 to P1644 L11).

Coming now to the comment on the drought threshold. The fact that this threshold remains at the 20% level throughout is a direct consequence of the definition of the theoretical adaptation scenarios and this is clearly stated in the manuscript: "In both adaptation scenarios, the drought index baseline is simply added to the reference value of the drought threshold, (~ -0.84, see Sect. 3.1.2), in order to generate a time-varying drought threshold. This way, the adaptation scenarios only take account changes in average conditions and not in potential evolutions in variability" (P1632 L10-13). This choice of considering scenarios representing an adaptation to changing normals is hopefully clearly stated in the manuscript (§3.2, P1630 L18 to P1631 L2). These theoretical scenarios can therefore be considered as an upper limit of adaptation effort, and this is already discussed in the manuscript (§3.2.3, P1632 L18 to P1633 L9).

Moreover, the referee sees "little change" in Fig.7-10. Given the definition recalled above, such changes should be interpreted as conditional on "perfect" adaptation scenarios to changing normals, and should be seen as a potential consequence of changes in internannual variability, as clearly discussed in the manuscript: "Additionally, both theoretical scenarios fail to provide a satisfying adaptation to changes in interannual variability, as shown in Figs. 7-10 through the occurrence of several events far longer than the longest observed ones. It strongly suggests that adaptation efforts should not only concentrate on the evolution of median values of water availability, but also on potential changes in its interannual variability." (§6.2.1, P1643 L17-22).

Lastly, the question of the referee about the statistical significance (*"Are the differences between the adapted future and present statistically significant in Figure 7-10?"*) is in our opinion already answered in the manuscript in a dedicated section (§5.2, P1639 L11 to P1640 L10). The statistical significance of the 1958-2100 trends in spatio-temporal drought characteristics are assessed through the use of the Mann-Kendall test and results are presented in Fig. 11 (P1670). It shows that some trends are statistically significant, even when considering the theoretical adaptation scenarios.

Specific comments

-It might be good to discuss how the RCM/GCM/land surface combine together to make the paper more understandable to hydrologists.

The authors do not fully understand this comment, as no RCM is considered in the present study. The statistically downscaled climate projections from the ARPEGE GCM are described at length in the manuscript (§2.2, P1624 L22 to P1626 L2). Moreover, as clearly stated in P1626 L22-26, outputs from the downscaled projections have been used to force the Isba land surface scheme. The meteorological variables used to force Isba are the same as those in the Safran reanalysis (described in §2.1 P1624 L9-20), as stated in the manuscript P1625- L16-18.

-What do we gain from the high resolution information?

The motivation of using 8-km gridded variables is to take account of the spatial variability in both the precipitation and the water and energy budgets. Indeed, both precipitation and parameters influencing the evolution of soil moisture (vegetation and soil properties) are highly spatially variable. Although this paper considers spatio-temporal drought events at the scale of France, it relies on the evolution of droughts defined locally (cf. P1629 L16-18) and on theoretical adaptation scenarios defined also locally (clearly stated in §3.2 P1630 L19-20). Moreover, outputs from this study have also been used elsewhere to characterize the changes in local-scale drought events (see P1640 L22-24).

-What are the limitations of using the reanalysis data?

The reanalysis data are taken here as the reference dataset for present-day climate (see P1624 L18-20 and P1626 L22-24). All analyses are therefore conditional on this choice. The assessment of reanalyzed data against observed data can be found elsewhere in the literature (Habets et al., 2008; Quintana Seguí et al;, 2008; Vidal et al., 2010a,b). It has furthermore to be emphasized that the statistical downscaling method used here has also been calibrated with reference to the Safran reanalysis dataset.

-Why do you not use the soil moisture directly out of the land surface scheme in the GCM?

This is because of the far too coarse resolution of the GCM which prevents any comparison with the reference reanalysis dataset of soil moisture, on top of resulting differences in soil and vegetation properties.

-What soil depth is used in Isba?

This soil depth obviously varies from cell to cell and ranges from 1m to 2m in general. Soil depths are taken in Isba from a 1km resolution Ecoclimap dataset (see Habets et

al., 2008 and Masson et al., 2003, referenced in the manuscript, for more information).

-The spin up is short - have you tested whether it is long enough?

We used here a 2-year spin-up for initializing soil moisture, soil temperature and snowpack in the Isba land surface scheme. The variables affected have relatively low memory constraints and 1 year is usually enough to find a reasonable starting point, as performed for example by the reference land surface simulation over the USA with the VIC model (Maurer et al., 2002). Morever, simulations start in early August, which means that soil moisture and snowpack for example are at very low levels. We have nevertheless decided to consider one more year in our spin-up to prevent any influence of this initialization. Such a spin-up might not be sufficient if we would consider streamflow simulation (especially for catchment with large aquifers), but this is not the case in the present study where we are only interested in soil moisture. For information, Vidal et al. (2010b) computed a 10-year steady-state of aquifers to inform the initial condition of the reanalysis run of the Safran-Isba-Modcou suite.

-I would be interested in finding a little more about the clustering algorithm and definition of drought.

As stated in the manuscript (P1629, L12, L21, L24), more information about clustering and definition of drought events is given in the Vidal et al. (2010b) HESS paper. However, the main relevant information are provided in §3.1, P1627 L6 to P1630 L6. The publication of a R implementation of the spatio-temporal clustering algorithm is furthermore planned by the authors.

-The paper is well written, but would benefit from further read through as some of the

words are not used in the correct context.

The manuscript will be carefully reviewed in order to remove any potential misinterpretation.

-I get a bit confused with all of the different acronyms and projects. Is it possible to make them clearer?

The authors are not sure what acronyms and projects the referee refers to, but they will check that all acronyms are defined the first time they are used in the manuscript.

-What is the baseline defined as?

If the referee refers to the "drought index baseline", it is defined as the threshold under which the standardized index suggests a lower-than-normal value. In the present-day climate, this drought index baseline is defined as zero by construction. This is clearly stated in §3.2.1, P1631 L4 to P1632 L8, and Table 3 (P1659) as well as graphically represented in Fig. 1 (P1660).

-The top figure of Figure 3 is probably unnecessary.

The authors do not agree with the referee on this point. Indeed, it shows that many events only last for a single month, and that even the number of those short-lived events

is well simulated by the modeling suite, and this is commented in §4.1, P1634 L15-19.

-Is the drought definition based on monthly data?

Yes. The definition of variables (grid and time step) used for the analysis is clearly stated in the manuscript, P1626 L1-2 for precipitation and P1627 L1-2 for the soil wetness index. Moreover, the definition of droughts is clearly stated in §3.1, P1627 L19 to P1628 L6.

-Page 1635 line 20 on - I don't think that the conclusion that the events at the end of the simulations are related to climate change can be drawn. Natural variability has a huge impact and it is equally likely that these events are part of natural variability, particularly with such a small sampling size.

From the line referred to by the referee, the manuscript reads: "One interesting feature is that the 3 longest (and with highest magnitude) simulated events for SSWI3 are found to occur in the last years of the simulation, suggesting a downward trend in simulated soil moisture, possibly driven by an underlying upward trend in temperature. This is consistent with results from recent trend analyses in different versions of the PDSI (Dai, 25 2011a,b)." The authors do not suggest that this feature is due to climate change but may simply result from an observed (see Vidal et al., 2010a) upward trend in temperature. And this upward trend adds to natural variability.

In some places there is too much detail which detract from the main message. For *example. -Page 1641 line 25 on is probably too much detail.*

The authors will be thinking of reducing the paragraph referred to by the referee.

-Using the theoretical adaptation scenarios the adaptation has a greater impact at the higher emission scenarios. Is this realistic? Surely the adaptation will find it harder to cope for the higher emission scenarios.

The authors definitely agree with the referee that actual adaptation will find it harder to cope for the higher emission scenarios. And of course the theoretical scenarios developed here are not realistic, as stated and discussed at length in the manuscript (cf. above comments). And, as also stated in the manuscript, they represent an "upper limit of adaptation efforts" (P1633 L4-6). And this is exactly why they "call for more realistic adaptation scenarios" (P1620 L25) and why a whole section of the discussion section of the manuscript is dedicated to ways of deriving more realistic scenarios (§6.2.1, P1643 L2 to 1644 L11).

-How do the adaptation and mitigation combine?

If this question is to be understood in the context of this study, this is definitely a good (open) question, which is asked in the discussion section of the manuscript dedicated to the mitigation scenarios: "It would be useful to actually compare the respective or combined effects of French adaptation and global mitigation scenarios on future drought characteristics, as done recently at the European scale using the E1 emission scenario by Warren et al. (2009)." (P1644 L28 to P1646 L3). The authors believe that

it is a very relevant research topic, which is of course outside the scope of this paper.

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