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Hydrology and Earth System Sciences Discussions

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.

The article analyses the future drought severity in France for the twentieth century considering three scenarios of greenhouse gasses emission (A2, A1B and B1). The authors use reanalyzed data from a model developed in METEOFRANCE and they focus on two different drought indices: the SPI and the SSWI on two different time scales with the purpose of analysing the drought severity in the twentieth-first century. I consider that the article provides novelties related to the drought science. On

the one hand, the area/magnitude/duration analysis using drought indices and climate change scenarios has not been previously used. The authors show that this approach is very useful to illustrate how climate change processes may increase the severity of droughts. This approach introduces much more information than the typical analysis based on the magnitude/duration estimations based on punctual data. In addition the study is also showing the need of drought adaptation measures given future drought projections and it provides a relevant message on how drought severity may increase in the future and how drought adaptation approaches are completely necessary to reduce the possible derived impacts. Therefore, I consider that the article deserved to be published in HEES since it provides relevant results and messages, not only useful for scientists but also showing a new methodological approach with interest for managers and policymakers.

The authors would like to thank Sergio M. Vicente Serrano for this comment.

I am going to provide two main criticisms to the article, mainly conceptual. The first one is related to the drought index, SPI, used in this study. The authors use this indicator to determine future drought severity in France. Although the SPI is now considered as the reference drought index by the WMO (see Hayes et al., 2010 - authors should cite this reference in page 1627 line 10), the SPI is calculated considering precipitation data. There are several evidences that indicate that drought severity is not only related to the precipitation variability but other variables, mainly potential evapotranspiration (PET), is also having an important role, even more under the current climate warming scenario. Thus, in page 1635 the authors indicate the possible effect of upward trend in temperature (affecting PET) to increase the soil moisture droughts in the last years. Precipitation-based drought indices including the SPI rely on two assumptions: i) the variability of precipitation is much higher than that of other variables, such as temperature and potential evapotranspiration (PET), and ii) the other variables are stationary *(i.e. they have no temporal trend). In this scenario the importance of these other variables is negligible, and droughts are controlled by the temporal variability in precipitation (See Vicente-Serrano et al., 2010). Nevertheless, when the authors analyse the drought projections under three different scenarios, they are using the SPI although they implicitly affirm that global warming is having a certain influence on drought severity. I think that to determine future scenarios in drought severity, it would be better the use of drought indices that consider both precipitation and potential evapotranspiration like the sc-PDSI or the SPEI, which may be also calculated on different time scales, like the SPI, but it is also sensitive to evapotranspiration changes (see Vicente-Serrano et al., 2010 and 2011). I am not requiring that authors make again the analysis using different drought indices but they should include a discussion indicating that the severity of future drought events will be probably worse than that indicated by the SPI, since the role of temperatures will be also important in the future.*

The authors would first like to thank the referee for bringing to our attention the Hayes et al. (2011) reference that summarizes WMO recommendations to use the SPI as the reference meteorological drought index. This would be a first answer to the criticism of the referee about using the SPI in this study: comparisons to the reference drought index prove always useful to put different studies in perspective. The fact that the SPI depends only on precipitation should not discard it from being used in climate change impact studies, as some anthropogenic hydrosystems solely rely on precipitation amount. One example, already mentioned in the manuscript (P1630, L11-17) would be high-altitude reservoirs used for hydropower production that mainly rely on winter snowfall amounts for their resources (cf. Vidal and Hendrickx, 2010) and that are largely present in both the Pyrenees and the Alps.

One further objection - and maybe more important here - to the referee's criticism about using the SPI is that results based on another drought index, namely the Standardized Soil Wetness Index (SSWI), have systematically been shown along with SPI results.

This index takes account not only of precipitation but also of temperature through the computation of the water and energy budget at the soil-vegetation-atmosphere interface by the Isba land surface scheme.

The authors would like here to emphasize that the evapotranspiration computed by Isba is an actual evapotranspiration, and not a potential evapotranspiration. Although some type uncertainties - listed and commented at length in the manuscript in the discussion section (§6.1.2) - are not taken into account in this calculation, the authors consider that the uncertainties in the calculation of a potential evapotranspiration, especially under climate change, are higher or at least as high. Indeed, some studies have recently shown that choosing a specific formula or another (among the various ones available in the literature: Penman-Monteith, Thornwaite, Oudin, etc., which are dependent only on temperature or also on other variables) leads to very different evolutions in a climate change context (Kay et al., 2008, Kingston et al., 2009). Moreover, PET values are highly sensitive to the time step of input data, their quality (worth questioning when looking at future projections), and to the time step of computation (hourly, daily, weekly, monthly). Consequently, the authors consider that results using the Standardized Precipitation and Evaporation Index (SPEI) developed by the referee and others – and that uses the Thornwaite PET which only depends on temperature – should be taken with caution. Nevertheless, the SPEI approach is of considerable interest when no land surface modelling is possible due to data or time constraints. It would be therefore of great interest of comparing the SPEI and the SSWI at different time scales with the same input dataset, in both present and future conditions. The authors are quite willing to participate in such comparisons.

The second main criticism is related to the calculation of a soil moisture drought index on different time scales (3 and 12 months). It is commonly accepted that drought is a multi-scalar phenomenon. McKee et al. (1993) illustrated this essential characteristic of drought through consideration of usable water resources including soil moisture, *ground water, snowpack, river discharge, and reservoir storage. The time lag between the arrival of water inputs and the availability of a given usable resource differs considerably depending on the system under consideration. Thus, the time scale over which water deficits accumulate becomes extremely important, and functionally separates hydrological, environmental, agricultural and other droughts. For example, the response of hydrological systems to precipitation can vary markedly as a function of time. This is determined by the different frequencies of hydrologic/climatic variables. For this reason, drought indices must be associated with a specific timescale to be useful for monitoring and management of different usable water resources. For example, Vicente-Serrano and López-Moreno (2005) and Lorenzo-Lacruz et al. (2010) showed how the response of the river discharges and reservoir storages to climate droughts changes noticeably as a function of the time-scale at which the climatic drought index is calculated. Nevertheless, when the drought index refers directly to a certain usable water source (e.g., streamflows or soil moisture) the concept of time scale is less meaningful for identifying drought conditions. In other words, the severity of the streamflow or soil moisture drought will only depend on the current flow magnitude/soil moisture conditions in a given moment in time. Longer time scales can not be representative of real streamflow/soil moisture drought conditions. Therefore, although the calculation of a hydrological drought index on longer time scales than one month can be useful to be linked to large-scale climate variability or to asses the possible responses downstream (e.g., a reservoir, which storage depend on the upstream conditions during several months), this approach is not useful to detect the real drought conditions in a gauging station/crop field for a certain month, which will only depend on the streamflow/soil moisture recorded that month. Therefore, I do not see any usefulness in using SSWI3 and SSWI12, and really it should be SSWI1 to represent real drought conditions in soil moisture. The climatic drought indices like the SPI or the SPEI are calculated on different time scales to adapt the times of response of different ecological, agricultural or hydrological variables to climatic droughts (see Vicente-Serrano et al. 2011) and to monitor easily the possible drought conditions in a variety of hydrological systems*

(among them soil moisture). Authors should consider it in future drought analysis using the SSWI.

The authors do not agree with the referee when he states that 3- and 12-month soil moisture drought indices are not useful to represent real drought conditions on soil moisture. Consider for example a given crop: knowing the average soil moisture conditions (in terms of water stress for the crop, which actually corresponds to the definition of the Soil Wetness Index) over the growing season (which is usually longer that one month) is to the opinion of the authors much useful for assessing the corresponding crop production! And therefore the use of the corresponding SSWI over n months would allow to look at these conditions within the variability due to climate.

The authors nevertheless agree with the referee that it does not represent the "current" drought conditions if the referee defines the current conditions as "instantaneous". It would be anyway difficult to compute a robust instantaneous soil moisture drought index because of the very high sensitivity of soil moisture (and especially in the higher soil layers) to even moderate precipitation events. The 1-month SSWI would then be an intermediate index between a hypothetical "instantaneous" index and the longer scale indices presented here.

The referee quite correctly states that meteorological drought indices are considered at different time scales for adapting to the different time responses of different ecological, agricultural or hydrological variables. The authors definitely agree with this statement, but the approach developed here is of a quite different nature. The variables considered here (precipitation, Soil Wetness Index, or streamflow like in Vidal et al., 2010) aim at representing directly the ones relevant for anthropogenic hydrosystems, whether they depend on precipitation (e.g., headwater hydropower production), soil moisture (e.g., crop production) or streamflow (multi-usage surface water resources). And specific hydrosystems may rely on those variables at specific time scales, hence the 2 example time scales (3 months and 12 months) considered here.

Minor comment: Lines 24-25. There are empirical studies that show the response of hydrological and environmental systems to the SPI on different time scales (Vicente-Serrano et al., 2011; Lorenzo-Lacruz et al., 2010; Vicente-Serrano, 2007; Vicente-Serrano and López-Moreno, 2005, and references therein).

Cf. the last paragraph of the comment above.

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