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Hydrology and Earth System Sciences Discussions

Interactive comment on “Stratified analysis of satellite imagery of SW Europe during summer 2003: the differential response of vegetation classes to increased water deficit” *by* A. Lobo and P. Maisongrande

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This manuscript represents a substantial effort towards teasing out the effect of regional climatic extreme events on vegetation temporal dynamics. These kinds of analyses are critical to understanding the way landscapes will respond to climatic variability and the successful integration of meteorological and remote sensing data represent a significant hurdle towards progress in this area. In this case, the authors have taken on the challenge of analyzing regional responses to a single summer drought (2003). The region analyzed exhibits dramatic variation in climate and vegetation, particularly the nature and timing of herbaceous vegetation dynamics. Furthermore, the drought

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effects included both rainfall and temperature anomalies that varied greatly over the region of analysis. These factors complicate the authors' study, and make generalizations regarding the possible effect of future climatic variation difficult to assess. I feel there are a number of specific changes and additional analyses that could enhance the presentation of the current work. I hope the authors are able to accommodate some of these changes within their manuscript.

The authors compare historical average climatology (1961-1990 for Spain & 1960-2000 for France) to the summer of 2003 in order to determine the spatial extent of P-PET anomalies. In contrast, they only use 1999-2002 as a baseline for August NDVI patterns, which are then compared to August 2003. The authors should make some attempt to demonstrate that 1999-2002 NDVI (and corresponding climate) data are “normal” enough to use this limited period as a reasonable comparison to the 2003 data, particularly since the climate means used do not contain any data from the period 1999-2002.

As mentioned in the text (and above), the period 1999-2002 isn't sufficient to even establish a natural range of NDVI variation, so what makes the authors sure that the mean over this period is appropriate? Regardless, a 3 year period for comparison does not seem sufficient to provide context for the 2003 event, particularly since one may expect the NDVI of 2003 to be correlated to the 1999-2002 NDVI patterns regardless of the 2003 P-PET anomaly, which is based on a comparison to the historical climate record mean.

Since NDVI data extend back over 2 decades, I believe that a more robust assessment of the sensitivity of NDVI to P-PET anomalies could be accomplished. Such an analysis would provide context for the situation in 2003. A key unanswered (but answerable) question seems to be: “To what extent were observed anomalies in NDVI during the summer of 2003 significant compared to the natural variation of NDVI for each of these vegetation types during the historical NDVI period of record?”

The authors use of a drought index defined as P-PET includes both rainfall and temperature effects which could affect NDVI independently. Why not simply use a P anomaly, if reduced rainfall is the expected driver of NDVI changes in the summer of 2003? Alternatively, if temperature is a factor, couldn't temperature anomaly be determined independently? I would suggest the authors analyze each of these factors independently - the summer of 2003 represented anomalies in both rainfall and temperature that each had spatial distributions that may have affected each of the vegetation types separately. Given the availability of historical and current data on both rainfall and temperature, the use of a combined index seems most expedient but not necessarily most illuminating, particularly as future climate change scenarios indicate that temperature and rainfall anomaly patterns may vary independently.

The authors should define NDVI anomaly more clearly, particularly as it appears to have different magnitude depending on what figure it is plotted in! In Figure 4, anomaly magnitude varies from .12 to -.12, In Figures 8 & 9, the range is from 10 to -60. In Figure 11, it is from -2 to -14. In Figures 12 & 13, the values are from 0 to -0.4Ě It becomes clear that these data can't all represent the same anomaly, but in every case they are labeled and described in the Figure caption as "2003 August Anomaly". What data are actually being reported in each of these Figures? NDVI - (NIR-VIS)/(NIR+VIS) - should always vary between -1 and 1, so why are values between 100 and 220(!) reported in Figure 14? Unfortunately, the presentation is sufficiently confusing as to create uncertainty about the authors' results and discussion.

The authors report in the caption of Figure 1 and 2 that they are depicting PET, but the value is negative and the text refers to the P-PET index? Shouldn't the anomaly (i.e. difference?) between average P-PET (negative) and P-PET in 2003 (generally more negative) be positive? But in Figure 2, it is negative? Again, what is the definition of anomaly here?

The authors rightly make a distinction between the two broad geographic zones of herbaceous vegetation that exhibit separate peaks in NDVI in spring and summer.

Given this fact, it is not at all clear why they use a single measure of climatic anomaly (P-PET summed over June-August) as the measure of water stress in each of these landscapes. If the NDVI of herbaceous vegetation in the northern part of their study area peaks in spring, why expect any effect of summer drought on NDVI during August?

The authors attribute the fact that tree vegetation shows reduced sensitivity to NDVI variation as a sign that these trees are somehow more robust to the drought conditions. But the NDVI values of trees must necessarily depend on the canopy architecture (i.e. LAI) of the tree vegetation, which is fixed at the start of the summer. In this way, trees have little ability to adapt their LAI (or NDVI) to changes in prevailing climate conditions within a single year. In contrast, herbaceous vegetation is much more variable in its LAI (cf. http://dx.doi.org/10.1016/S0034-4257(02)00054-8). These differences in life-history could account for different responses to a single-year drought anomaly, independent of any special ability of tree vegetation to access additional soil moisture during droughtĚ Again, it seems that a historical analysis of the baseline sensitivity of each landcover type to changes in the chosen drought index (P-PET) would be necessary to make any strong inferences regarding the specific responses to the 2003 event.

Finally, very recent work has provided context for the effect of the 2003 drought on carbon balance in these same regions (http://dx.doi.org/10.1038/nature03972). Although this work has appeared since the author's submission, it seems appropriate to include a mention of the article in this discussion to provide additional context for the current study.

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