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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Vegetation vulnerability to drought on southeastern Europe" *by* Patrícia Páscoa et al.

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Summary: The authors present a study that investigates the co-variability of vegetation greenness (based on remotely sensed NDVI) and water availability (assessed through SPEI) over a territory that roughly encompasses the state of Romania. The authors further split up their analysis by altitude and land cover type; and also look at a severe drought episode that occurred in Romania around 2000/2001. While the results are technically sound, and in general a regional-scale analysis of vegetation responses to meteorological variability in different land cover types is interesting, I believe the study could be strongly improved by a suite of potential further in-depth analyses and discussion. The presentation of the study's results appears mainly descriptive, and a discussion of broader hypotheses, processes or wider implications is missing. An

important weakness that needs to be addressed is that correlations are in some cases directly interpreted from a process point of view (e.g. in the Abstract, "[...] a positive effect of dryness on the vegetation activity") - which completely disregards the fact that there are likely confounding meteorological factors that are not analysed. I'll provide a few suggestions below.

Response: We appreciate the overall good appreciation of our manuscript by the reviewer. The issues addressed by the reviewer are commented below.

Major comments: 1. * Interpretation of negative correlations between SPEI and NDVI * The authors identify a number of negative correlations between NDVI and SPEI, mostly in early spring and in higher altitudes; and mostly with the shorter time scales of SPEI (e.g. Fig. 5). These are mentioned for instance in the Abstract as one of the highlight results ("a positive effect of dryness on the vegetation activity", p. 1, l. 19/20). This kind of interpretation of a simple correlation analysis is strongly misleading (and the paper relies too much on inferences of this kind): First, correlation does not imply causation (in principle).

Response: We appreciate the reviewer's concerns regarding the interpretation of the results obtained with the correlation analysis. Firstly, we would like to stress that we did not intend to overemphasise the existence of a causation relation based on these results. Furthermore, we intent to write carefully aiming to avoid misleading interpretations about those relationships. When we mentioned 'the positive effect of dryness on vegetation activity' we were pointing to the relationship observed, by means of correlation analysis, when years with low vegetation activity are coincident with years of negative SPEI values (drought conditions). This behaviour is similar to the ones obtained in several other works for several other regions in the world using the same methodology (Ji and Peters, 2003; Lotsch et al., 2003; 2005; Quiring et al., 2010; Brown et al., 2008; Wang et al., 2015; Vicente Serrano et., 2012; 2013; 2014; Gouveia et al., 2017). However, we have included other methodologies, namely the one proposed by Gouveia et al., (2009) and applied over different regions (e.g. Fertile Crescent - Trigo at al., 2010;

China - Barriopedro et al., 2012, Madeira - Liberato et al., 2017) that together help understand the effect of these variables.

2. Second, from a process point of view, it seems more likely that dry springs coincide with warm temperatures / high radiation, which appear a much more likely driver of vegetation activity at high altitude sites (and changes in meteorological drivers from spring to summer and in high altitude sites are not entirely new: See for instance: Jolly et al 2005, GRL; Wolf et al 2016, PNAS; Sippel et al 2017, ERL).

Response: We thank the reviewer for the suggested papers. One of our main goals was to map the vegetation response in the area to drought conditions, and although we did not intend to focus on the altitude gradient, this issue had to be explored, since it is intimately related with the maps obtained. We did not think the results we obtained regarding the altitude range were a scientific novelty, and we have cited in the text several authors that obtained comparable or similar results in the study area (Gouveia et al., 2017, Sidor et al. (2015)). However we acknowledge the relevance of the driving mechanism suggested by the reviewer, as well as the additional references suggested by the reviewer and will therefore also include in our discussion the points indicated by the reviewer.

3. This appears also likely, as not only the correlation pattern reverses, but also the relevance of time scales reverses (i.e. in summer it's long SPEI time scales, in spring vegetation responses appear to be on rather short time scales...) While the authors mention that it seems likely that temperature or radiation, and not water availability is limiting vegetation activity in spring, and in higher altitudes (p. 9, l. 279 onwards), a discussion or even further analysis of these likely confounding factors (that are correlated with SPEI!) is missing. Hence, in particular, if the true control of NDVI in spring and at high altitudes would be temperature and/or radiation, and not SPEI, is it meaningful at all to analyse and interpret SPEI/NDVI correlations only?

Response: We understand the reviewer's point, however we should stress that the

aim of our work is to analyse and characterize the relationship between drought and vegetation activity using a multiscalar drought index and vegetation indices obtained by satellite. The processes behind these relationships are out of the scope of this work. As we recognize that radiation and temperature are important factors that should be included in drought impacts, we opted by using the SPEI. SPEI incorporates a simple water balance, and if temperature increases vegetation activity, it may affect the available water, thus affecting the vegetation activity. For this reason, we consider that the analysis of NDVI/SPEI correlation may provide important information.

Moreover, and accordingly with the above mentioned goals, we did mention that it was likely that temperature or radiation was a limiting factor in the high altitudes, based on other works that have shown this (Nemani et al., 2003). Nonetheless, we cannot rule out the influence of precipitation in the vegetation activity, particularly since other works have shown that precipitation does have a correlation with vegetation activity in the study area, at high altitudes (Levanič et al. (2013), Sidor et al. (2015)). We have mentioned this in the Discussion and Conclusion section (please see page 9, lines 272- 279 and page 10, lines 293-296

Although recognizing the great interest in analysing radiation and temperature and also SPEI and NDVI over high altitudes is not the goal of this work, as it is not possible to do this kind of analysis with gridded data with 0.5° of spatial resolution. Nonetheless, we will improve the Discussion and Conclusion section, in order to include other effects of these meteorological variables, that may not be very clear in the original manuscript, as well as the role of the SPEI time scale.

4. Would it be possible to assess NDVI anomalies jointly, using both SPEI datasets and temperature/radiation datasets? (using for instance partial correlation analysis or other statistical techniques that might be able to account at least to some degree for the covariation of SPEI and temperature/radiation).

Response: This analysis would certainly provide important information, but, due to the reasons presented above, we consider it is outside the scope of this work, which is to assess the influence of drought on vegetation in the study area. The analysis proposed by the reviewer will enlarge the paper and defocus the manuscript.

5. Also, in light of current literature it would be interesting to analyse whether there are carry-over effects from warm/positive spring conditions towards negative summer conditions? These have been recently found in observations (Buermann et al. 2012 ERL, Wolf et al. 2016) and models (Sippel et al. 2017); and it would be really interesting to analyse whether there is a correlation between spring and summer NDVI conditions?

Response: The effect of past climate conditions is included in the different SPEI time scales considered in this work, although the results were not discussed considering the proposed point of view. We do consider it is an interesting point and will include it in the discussion. It is possible that pixels showing both positive correlations in July and negative correlations in June are reflecting the referred carry-over effect.

An in-depth analysis of the carry-over effects mentioned by the reviewer would need a longer time series, in order to include the different climate conditions on both seasons on a pixel basis. It is likely that the dataset used in this work is not long enough to this type of analysis. However a discussion of these aspects will be included in Discussion, mentioning the works referred by the reviewer.

6. Nonetheless, it would be very interesting to see an analysis that takes into account not only SPEI, but for instance also temperature/radiation in a partial correlation analysis (in which I would guess that the "positive effect of dryness" disappears if controlled for temperature/radiation).

Response: Please consider the response to comment 4.

7. * Descriptive presentations of results * The present text is very hard to read, as it presents numbers over numbers from the analysis, and it appears very descriptive overall. It would probably provide for a better readability if the authors would clearly

state 2-3 hypotheses that they investigate, and refer back to these throughout the text. For example, on p. 3, l. 85, the authors state three objectives: "i) to map the vegetation response to drought conditions, ii) to identify the vegetation types that show the strongest response to drought severity and iii) to study the impact of a severe drought episode on the vegetation activity." The readability of the paper might improve if the authors would structure their Discussion&Conclusion Section also along three objectives/ hypotheses, and refer back to them in the Discussion&Conclusion section.

Response: We thank the suggestion to improve the readability of our work. We initially decided to discuss the results only in the last section, but taking into account the comments from all the reviewers we will include in the Results section some interpretation, so it won't be as descriptive. Accordingly, we will also improve the Discussion section, as suggested.

8. * Analysis tools and discussion * The authors use two widely used analysis metrics, namely SPEI and NDVI. However, none of these two metrics is based directly on measurements: SPEI relies on an estimate of the evapotranspiration component; NDVI is remotely sensed vegetation "greenness". While these two metrics are widely used as proxies for dryness and vegetation activity, respectively, it still needs to be emphasized (e.g. in the methods section) that both metrics are only highly simplified proxies of dryness/vegetation activity; and thus that some care needs to be taken in interpretation of results due to these methodological choices.

Response: We agree with the precautionary note raised by the Reviewer. We will include in the Data and Methods Section more information regarding the indices, such as their advantages and caveats. We would like to stress that we have included in the Introduction information that justifies the use of these indices, such as the difficulty to directly quantify droughts (thus the need to use a proxy).

9. In a similar vein, it would be very interesting to see a discussion on how the results of this paper compare to analyses of direct vegetation growth measurements (e.g.

FLUXNET) in response to meteorological variability. There are some papers available, some that analyse also stratified by land cover type /vegetation type: e.g. Schwalm et al. 2010 GCB; von Buttlar et al. 2018 BG.

Response: We have cited in the Discussion Section several authors that used tree-ring data and crop yield data to assess the impact of drought/climate variability (Levanič et al. (2013), Sidor et al. (2015), Potopová et al., 2016). Unfortunately, we have not found works by other authors using FLUXNET data in this region that we could use to compare. Nonetheless, it is our opinion that the methodology used is robust, which is reinforced by the similarity with the results obtained by the mentioned works. A reference to the mentioned papers will also be included in Discussion.

Minor comments: âĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤâĂŤ 10. p. 1, title: shouldn't is say "vegetation vulnerability to drought IN southeastern Europe"

Response: We thank the reviewer for the correction.

11. p. 1, l. 17 "on July and August" should read "in July and August". Several more writing issues across the text.

Response: We thank the reviewer for the corrections. We will perform a thorough review of the manuscript in order to correct other issues.

12. p. 4, Section 2.2 on NDVI data. Are NDVI time series deseasonalized? This would be reasonable for looking at vegetation activity anomalies. Is there any pre-processing done on NDVI values other than discarding values below 0.1.

Response: The monthly time series of NDVI were detrended. Detrending is a common procedure when performing a correlation analysis. On the other hand, since the detrending was performed on the monthly values, the resulting anomaly is in reference to the monthly values and therefore we do not see the need to deseasonalize the time series.

13. p. 5, l. 136. A threshold of -0.84 appears very subjective. Could you maybe make

a plot to show where this threshold lies across the SPEI values obtained in the study?

Response: As mentioned in the Introduction, one of the reasons making it so difficult to directly quantify drought is the difficulty to precisely identify the onset and end of a drought event. Bearing this in mind, the choice of a threshold in this situation is inherently subjective. We have chosen this threshold based on the probability distribution function of the SPEI. Really, any threshold is always subjective, but given different vulnerabilities and responses of natural vegetation to droughts, it is really difficult to establish objective thresholds. Considering the threshold -0.84, the frequency of SPEI values indicating drought (moderate, severe or extreme) is around 35% in the entire period used to compute the index. We show on Fig.1 the requested plot, for the time scales considered in the study, but we do not think necessary to include in the manuscript.

14. p. 5, line 150: What about sensor ageing?

Response: The dataset used is the Collection 3, which is the result of the reprocessing of Collections 1 and 2, and Toté et al. (2017) concluded that this version is more stable over time when compared with the previous dataset. Changes in illumination associated with sensor drift may introduce some artificial trends in the time series, but if occurring, these were eliminated during the detrending of the time series. No other trend analysis was performed in this work that could be masked by changes associated with sensor drift.

15. p. 6 line 187: should read "shown IN fig. 3"

Response: We thank the reviewer for the correction.

16. Fig. 4: "Significant correlation" (figure caption) and "Corr > 0" (title of figure) must be a diffent thing.

Response: The figure caption will be clarified, in order to clarify this issue. It will read: "Percentage area showing significant correlations in the month of June (left, negative correlation) and July (right, positive correlation)."

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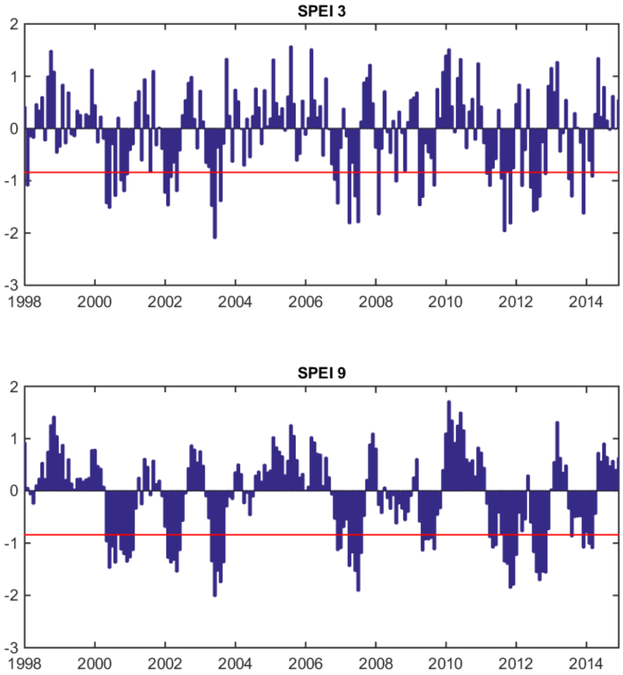

Fig. 1. SPEI time series, averaged over the study region, computed with 3- (top) and 9- (bottom) months' time scale. The red line indicates the -0.84 threshold, used to assess the drought condition