

Biogeosciences Discuss., referee comment RC2  
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## **Comment on bg-2021-47**

Anonymous Referee #2

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Referee comment on "Seasonal ecosystem vulnerability to climatic anomalies in the Mediterranean" by Johannes Vogel et al., Biogeosciences Discuss.,  
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## **Review of 'Seasonal ecosystem vulnerability to climatic anomalies in the Mediterranean' by Vogel et al.**

### **General comments**

In this paper, the authors quantify vulnerability of FAPAR, as a proxy for ecosystem productivity, to variability in temperature and soil moisture in the Mediterranean. They disentangle the vulnerability on seasonal timescales, with land cover type and regionally. They clearly show how, in general, FAPAR is reduced due to cold temperatures in the winter, then to hot temperatures in early spring, followed reductions due to hot and dry conditions in late spring and summer, although productivity is small in summer due to soil moisture limitation. These are clear albeit, not surprising, results.

The paper is very well written considering style and structure. The methods are clearly explained and being simple, they make sense and may easily be applied in other regions (by others). In that sense I do not see major obstacles preventing publication of the paper in BGS.

However, having read the paper, I continue asking myself what point the authors are trying to make. I will explain my remark in two points.

First, the authors use satellite and model data to quantify ecosystem vulnerability in an original and interesting way. However, the paper is rather clean or academic in the sense that it only uses these data streams to apply some statistical/mathematical procedure resulting in maps. Data treatment always produces results, but they are not meaningful without validation. The authors do not confront their results with ground truth data or a

soil-vegetation-atmosphere model for validation. How do the readers know that the results actually make sense? (see also my comment to line 231). Additionally, the authors take a rather straightforward route towards the final results, where some reflection is necessary, e.g. when assuming time lags (see my comments to lines 311 and 325).

Second, the authors write about potential applications, e.g. at the end of section 4.1. There the authors suggest that the method may be used to detect (the effect) of trends in temperature and moisture. For the readers, this is where the paper becomes really interesting, but the authors do not take up their own challenge.

The paper as it is now, contains a comprehensive, rounded whole, which is good. I am aware that I might be asking for protruding tentacles to the story, which disturb the entirety of the paper and make it lengthier. However, I think the paper needs those tentacles to appeal to the audience, get cited and make an impact. Therefore I ask the authors to address the two points mentioned before publishing the paper in BGS.

### **Specific comments**

Lines 90-96: Although the introduction and methods are clearly structures in general, in those lines some things are repeated (e.g. the improvements of the somo product, they being ECV's, etc.)

Line 105: is sigma calculated over the entire year or per month? I think per year, but this does not become entirely clear from the paper.

Section 2.4: 'The terminology ... is confusing ...'. Yes it is. I have always used the term vulnerability more or less in the sense of sensitivity combined with risk, so FAPAR would be vulnerable to temperature, because it is sensitive and temperature extremes do occur. As such, I had a hard time understanding your definition, and I presume I am not the only one. Could you make an effort to explain better your definition and how it is different from other definitions? This will help many readers to understand the paper.

Line 171: 'Sparse vegetation never shows vulnerability to hot conditions...'. I was confused here. Since the extremes are defined by the 10<sup>th</sup> percentile, each pixel must have extremes. Why is there no vulnerability? Do I understand correctly that this is because the extremes in T and somo do not cause a significant change in FAPAR, because the vegetation is dormant? I think these lines would benefit from a better explanation.

Figure 6: Please write out the abbreviations for BALkan, Italy and France, etc., there is enough space in the figure and it would help the readers by not having to turn back to

previous pages.

Line 222: '... in a transitional or wet system...': don't you mean '... or dry system...'?

Line 231: '... presumably because August is outside of the growing season.' Why don't you show this, you have the data.

Line 267: 'Depending on the plant... beneficial or detrimental.' Please explain this process wise.

Line 311: Why didn't you play with the time lag? It would be very interesting if you could show that your method is capable of quantifying the time lag for specific ecosystem types.

Line 325: It seems simple enough to apply different averaging windows to T and somo. Why don't you take the time to do this properly?