

Biogeosciences Discuss., editor comment EC1
<https://doi.org/10.5194/bg-2021-47-EC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on bg-2021-47

Bart van den Hurk (Editor)

Editor comment on "Seasonal ecosystem vulnerability to climatic anomalies in the Mediterranean" by Johannes Vogel et al., Biogeosciences Discuss.,
<https://doi.org/10.5194/bg-2021-47-EC1>, 2021

Both reviewers raise relevant points of criticism to the methodology and implications of this study, where one reviewer recommends to reject, the other has concrete suggestions for extensions to improve the level of interest of the paper. I would recommend the authors to formulate a new version that at least addresses:

- the use of standard deviation as a normalization factor for non-gaussian distributions does introduce the risk of introducing biases. For non-gaussian distributions non-parametric indicators such as inter-quantile ranges are more suitable. However, it is well possible that the time series of monthly residuals resembles a gaussian distribution and justifies the use of a z-score, which could be tested by an indicator such as Kolmogorov-Smirnov or Shapiro-Wilks, or display of transformed time series
- surface soil moisture as such is not a powerful indicator for ecosystem stress, but its success and ability to link to precipitation records, physiological state and deep soil moisture is promoted by the availability of long duration products in which the propagation and accumulation from shallow (short time scale) surface soil moisture to deeper (long time scale) root zone soil water anomalies can be diagnosed. This time scale argument is well addressed in literature on e.g. soil moisture data assimilation and offline land modelling, and could be part of the rationale to use surface soil moisture data sets in your study
- Earlier studies apparently made effective use of this aggregated ecosystem data set to infer statements on ecosystem responses, despite the large diversity of physiological typologies within a single ecosystem class. Reference to this earlier literature including an explanation of why these authors apparently could extract relevant information on vegetation dynamics would be desirable
- some extension to reply to the suggestion of reviewer 2 (using e.g. ground truthing, or derive interpretations of the findings in the context of past or future trends in driving variables) would be highly welcomed and would be a demonstration of the suggested added value of this study for decision support, but I can understand that the paper should not extend its focused scope too much.