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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Ambiguous agricultural drought: characterising soil moisture and vegetation droughts in Europe from earth observation" *by* Theresa C. van Hateren et al.

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This paper discusses whether the term agricultural drought is ambiguous, according to the authors defined as 'a soil moisture deficit severe enough to hamper vegetation growth'. It compares soil moisture derived drought indicators with a vegetation derived indicator and ambiguities, e.g. a time lag in agricultural drought anomalies.

I would recommend rejection for this paper. I don't mean to say the findings are not useful, because they could form additional arguments to existing research. But to my knowledge this work could be specified as a 'discussion/review' . There are no novel findings, unless you would count the activity of comparison of CCI-SM with MODIS-

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NDVI as a novel exercise. Also, the findings of time lag and differing correlations between soil moisture and vegetation derived indicators is also not new, as indicated by the authors who mention the Crow et al (2012) study.

Moreover, and probably more important, the pitch of the paper does not hold up well. The paper argues about a wrong/ambiguous definition of agricultural drought and there are a couple of issues with that argument throughout the paper:

1) The authors mention the current definition of agricultural drought as 'a soil moisture deficit severe enough to hamper vegetation growth'. Where does this definition come from? Please refer to a document. To my knowledge there are already quite a few different definitions of agricultural drought, as well as a variety of agricultural drought indicators. See other reviewers references, and add to that work of e.g. Cao et al (2019), Dalezios et al (2017), Heim et al (2002), Quiring et al (2003).

2) Given the authors' definition of 'a soil moisture deficit severe enough to hamper vegetation growth', the argument that it is ambiguous is not explained well enough (I did not understand at least). The authors claim that in certain soil moisture deficit NDVI shows actual increased growth. But that would not undermine the definition of 'a soil moisture deficit severe enough to hamper vegetation growth'. Maybe the authors have been looking at soil moisture deficits that were not enough to hamper vegetation growth?

3) A few other things need to be clarified. If it is known that the effect of vegetation lags the effect of soil moisture deficit, then the finding of increased growth with lower soil moisture deficit is highly uncertain. After all, the increased growth could be coming from a lagging soil moisture high before the deficit. The finding that correlation between the soil moisture and vegetation derived indicator is peaking at the end of the summer is probably mainly because the soil water deficit is lowest at the end of summer. So the authors are basically claiming that the 'soil moisture deficit is severe enough to hamper vegetation growth', which equals the author's definition of agricultural drought that they

claim was ambiguous in the first place.

Finally, other explanations of the differences found are not touched upon, such as: - groundwater table (e.g. a global version by by Fan et al, 2013, could have been used to identify root water uptake from shallow water tables) - differences in vegetation type; - influence of soil; - influence of fertiliser.

References: Quiring et al (2003): https://doi.org/10.1016/S0168-1923(03)00072-8 Cao et al (2019): doi:10.3390/rs11091066 Dalezios et al (2017): https://www.researchgate.net/publication/320689949_Agricultural_Drought_Indices_Combining_Crop_Climate_and_Soil_ Heim et al, 2002. https://doi.org/10.1175/1520-0477-83.8.1149 Fan et al. (2013). 10.1126/science.1229881

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