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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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First of all, we wish to thank the reviewer for reading our manuscript and for taking the time to provide us with insightful comments on our manuscript. In this document we will comment on the reviewer's main concerns.

The reviewer states that (s)he has difficulties to see the novelty of this research. With our study, we aim to contribute to the debate on the use drought indices (for agricultural drought in particular), and how Earth observation products can be used for this. We did not aim to provide an analysis on the (cor)relation between vegetation and soil moisture from a more process-based point-of-view, since, as the reviewer correctly

points out, many other studies have already looked at this. While the other references are relevant and we are more than happy to discuss them in a revised version of the manuscript, it seems that the Peled et al., (2010) paper is the most relevant to the context of our work. We note, however, several important differences, for instance its focus on seasonal rather than monthly timescales, its use of modelled rather than observed soil moisture, its focus on correlations rather than discrete drought events, and the now more historical period (1982–2002) which excludes many of the well-documented drought events that have occurred over the past 2 decades (including 2003 and 2018) that are covered in our study. We think that the main contribution of our work is providing an answer to the question how well one type of agricultural drought can be predicted by the other type from Earth observation data – an issue that we believe is novel, given that it is rare to see studies that make an explicit distinction between soil moisture and vegetation when discussing agricultural drought. The IPCC, for instance, defines 'Agricultural drought' as moisture deficits in the topmost one metre or so of soil (the root zone) that impact crops, without acknowledging that this relation is ambiguous when vegetation and soil moisture are analysed jointly (as we do).

The second point the reviewer makes is that (s)he found there is a lack of analysis and discussion of results. More specifically, the reviewer missed the analysis on lags and land cover in the current study. Our reason for not including this was that NDVI is used for real-time monitoring of agricultural drought in several national and European platforms. This data is used without lag, and hence in the framework of our research question (how should agricultural drought be defined?) we initially thought this was of less relevance. However the reviewers' comments have made us rethink this decision, and we believe an analysis of NDVI with a 1-month lag can provide useful insight because this is often assumed as a working hypothesis (as is confirmed by the reviewer). It should be noted that according to our hypothesis, whether the 1-month lag actually works might depend on the climate setting.

We repeated our original analysis, but now including a lag between soil moisture and vegetation data was taken into account. Fig. 1 shows the results with this lag, as compared to the no-lag analysis shown in Figure 6 in the manuscript. Though the results are actually very comparable to the original analysis, it seems that in this case the skill scores are generally lower compared to the situation without lag. This indicates that a lag between the two variables does not explain the low skill scores obtained when attempting to predict one type of agricultural drought (soil moisture) by another (vegetation), thereby further strengthening our conclusion that soil moisture droughts should not be used as a proxy for vegetation drought.

Regarding the analysis based on different land covers, we refer to our supplementary material. There, we included several figures showing the NDVI and soil moisture anomalies for two different land covers (grassland vs. forest). We opted not to include these analyses in our main manuscript as there was a substantial difference between the amount of pixels of each land cover in the each drought event (Fig. 2). Robust results, comparing all of the studied drought events, could thus not be generated. What the figures in the Supplementary materials do show is that compared to the NDVI anomalies (NDVIA) in all pixels (Fig. S11), NDVIA in grassland pixels (Fig. S8) reacts much more strongly to a decrease in soil moisture than NDVIA in forested pixels (Fig. S7). One would therefore assume that the skill scores would indeed be higher over grassland pixels than over forested pixels.

To test this hypothesis, we repeated our original analysis for the two land cover types (as discussed in the supplementary material). Here, we include results of an analysis with an 80% threshold, meaning that pixels are included in the analysis only when at least 80% of its surface belongs to one of the two land cover types. The skill scores for grassland cover and forested cover are given in Fig. 3 and 4, respectively. These figures indeed show that there is a difference in skill when the two types of land covers are considered, as rightfully suggested by the reviewer. Results should be interpreted with care though, as the number of pixels considered per event differ strongly (Fig. 2). Regardless, we'd be happy to include these analyses in the manuscript or the

supplementary materials, dependent on the reviewer's preference.

The reviewer additionally comments on Figure 4 in the manuscript, where it can be seen that for some droughts, the area in drought for SM seems to precede the NDVI area in drought. That is indeed true, most notably for the 2015 and 2018N event. The suggestion of the reviewer that this is caused by the fraction of forest seems unlikely, based on the values given in Fig. 2. We would argue that these patterns, i.e. a soil moisture drought preceding a vegetation drought, are to be expected due to the lag that is common between soil moisture and vegetation anomalies, as shown, amongst others, in several papers cited by the reviewer (Chen et al., 2014; Nicolai-Shaw et al., 2017). In a possible revision of our paper, we will discuss this behaviour as well.

The reviewer finishes off with raising some questions regarding our analysis. The reviewer mentions that frozen soils and snow cover may have caused low soil moisture values in April and May for the 2002 drought, if not properly masked. Though we did not mask the soil moisture data ourselves, retrieval under such conditions is highly uncertain and therefore masked in the CCI combined soil moisture data set (Scanlon et al., 2020), which we used. Having said that, it has been found that there are still some issues with insufficient masking of snow or frozen soil conditions in this data set (van der Vliet et al., 2020). The low soil moisture values in April and May 2002 could thus indeed have been caused by low temperatures. We will include this in the manuscript.

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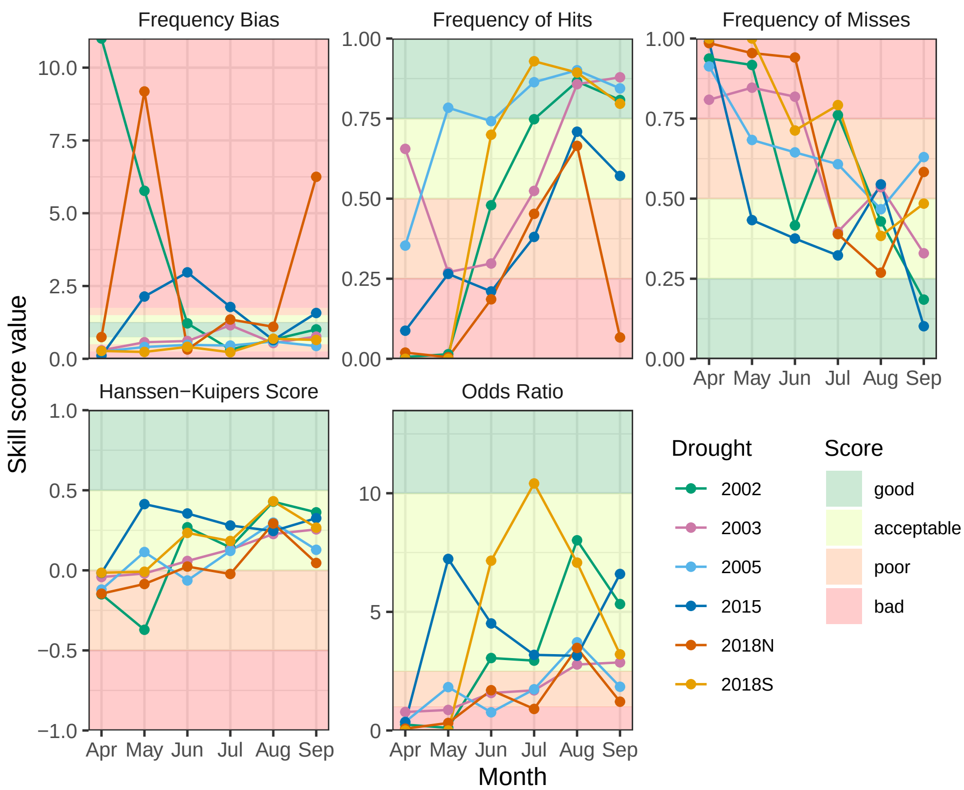

Fig. 1. Skill scores for soil moisture drought at time t-1 as a proxy for vegetation drought at time t.

	80%				90%			
	Grassland		Forest		Grassland		Forest	
Event	SM	NDVI	SM	NDVI	SM	NDVI	SM	NDVI
2002	146	146	1060	1066	43	43	350	350
2003	730	844	51	232	300	335	5	49
2005	138	138	52	52	67	67	13	13
2015	553	560	116	122	193	196	25	27
2018N	18	18	631	532	4	4	136	136
2018S	355	355	52	52	100	100	5	5

Fig. 2. Number of pixels for each drought event when the different (80% or 90%) thresholds for two types of land cover are applied, as defined in the Supplementary information

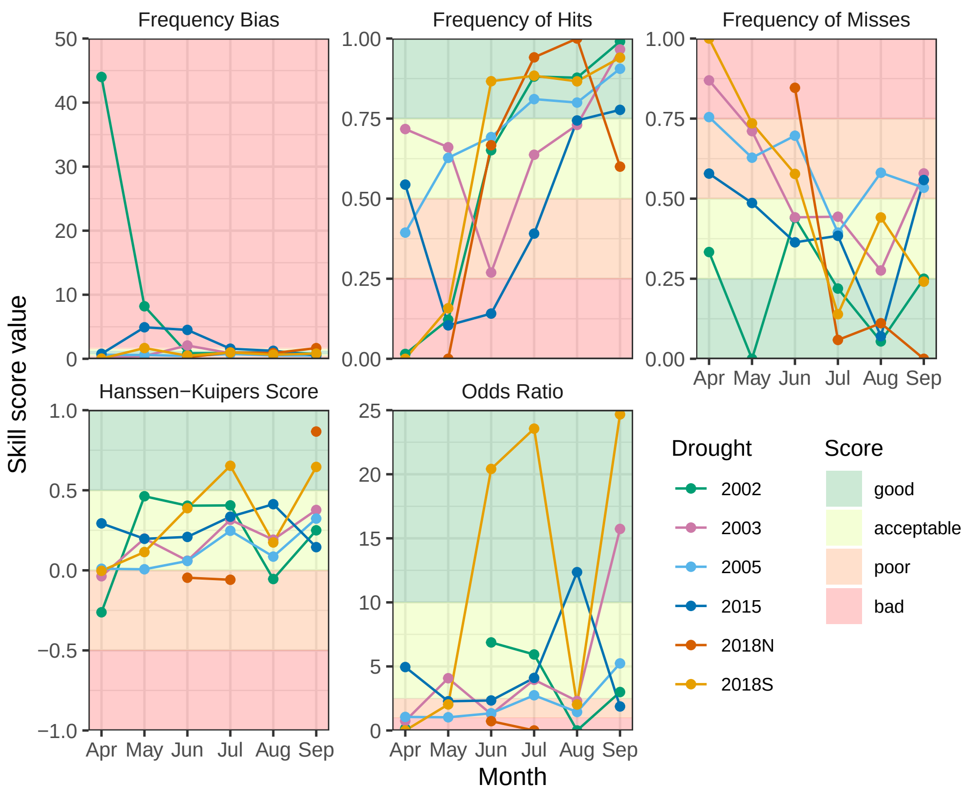

Fig. 3. Skill scores over grassland pixels, as defined in the supplementary material. An 80% threshold was used.

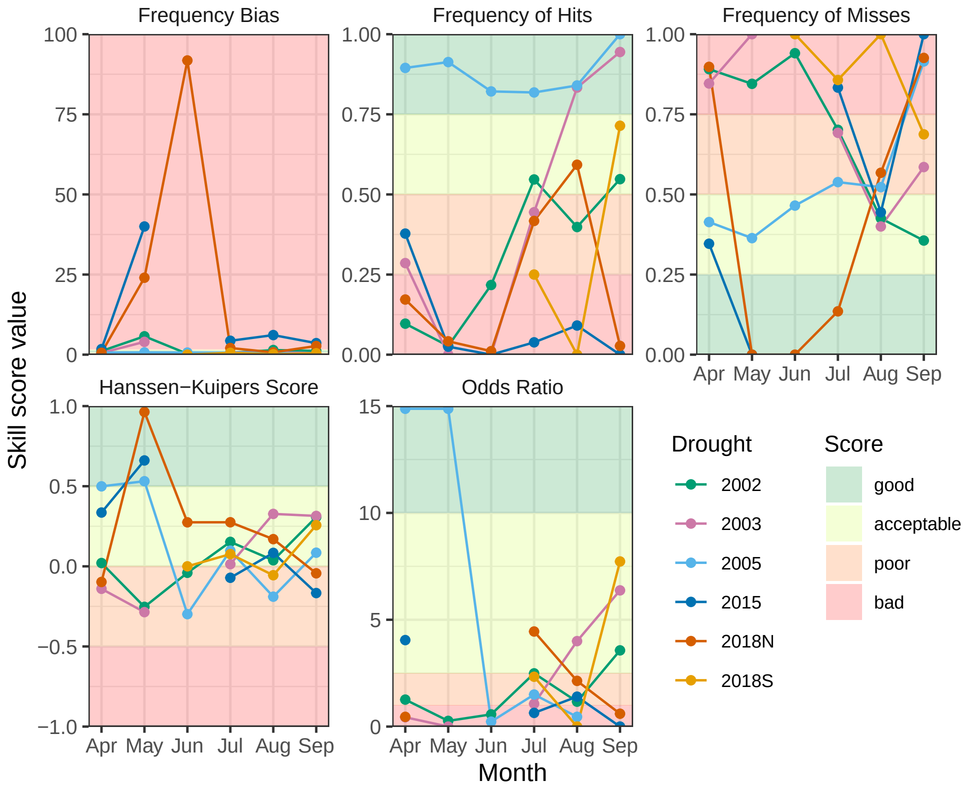

Fig. 4. Skill scores over forested pixels, as defined in the supplementary material. An 80% threshold was used.