Review comments 2

Summary

The present study assesses the skill of seasonal forecasts of soil moisture over the entire Mediterranean and Northern Europe region, and seeks to identify any potential correlations between crop yield (maize and wheat) and the primary pattern of forecasted soil moisture. The authors of the present study found that the skill of seasonal forecasts is dependent on the specific region analysed and on the vertical soil layer. This finding is in agreement with previous literature on the same topic in the Central Mediterranean region [2]. However, this study offers a more comprehensive analysis of the skill degradation with lead time on the different soil layers, finding that this is mostly due to noise rather than intrinsic predictability. Therefore, an EOF-filtering approach is utilised on soil moisture data, thereby providing a more realistic field that is conducive to the development of early-warning systems. This methodology has the potential to enhance the reliability of hydroclimatic indicators derived from seasonal forecasts. In particular, this work presents a noteworthy finding regarding the correlation between maize yield and soil moisture anomalies in the Balkan region. This demonstrates the potential for their methodology to be utilised for drought monitoring and seasonal forecasting in summer cropping systems. The text goes on to provide some insights into the decoupling between yield and climate-driven moisture variability due to irrigation regimes. It is evident that the work requires refinement, particularly with regard to the elucidation of skill degradation and the presentation of data, as well as the enhancement of coherence. Not withstanding these observations, it is deemed suitable for publication in HESS. It is my conviction that the paper would be improved by the implementation of one major comment and a number of minor comments that, in my opinion, could render it more robust.

We sincerely thank the reviewer for such a positive review and comments. We provide below a detailed response to the reviewer’s comments.

Major Comment

The primary concern pertains to Figure 3 (Section 4.2) and the underlying rationale for the observed degradation in skill level. As illustrated in Figure 3, there is a marked difference between this figure and the other figures (shown in the supplementary materials, S1 to S3) for the normalized PC time series in soil layers 1, 2 and 3. It is needed to ascertain why the transition from hindcasts to forecasts (as delineated in Lines 219-222) should exert an influence solely on the deepest soil layer. Furthermore, with the exception of the jump around 2015-2017, the PC1-SWVL4 time series demonstrate significantly divergent behaviour between SEAS5 and ERA5 between 1981 and 2015, indicating a distinct temporal evolution (in contradiction to the assertions made in Lines 220-222). This point requires further elucidation, for example through the presentation of comparative illustrations of soil moisture anomaly between SEAS5 and ERA5, in order to ensure a comprehensive investigation, given its status as the initial significant finding of the paper.

In both ERA5 and SEAS5, soil moisture is corrected by a point-wise Simplified Extended Kalman Filter applied only to the upper three soil layers (the top 1 m). The deep fourth layer receives no analysis increments and evolves freely within the model; combined with its multi-year memory, this is why it exhibits discontinuities at ERA5 production-stream transition points (Hersbach et al. 2020; de Rosnay et al. 2014). The influence of the hindcast-to-forecast transition on the deepest soil layer can therefore be explained by the long spin-up time of this layer, combined with the fact that it receives no observational updates from data assimilation.

We agree that the two datasets show divergent behavior and that this was not accurately described in the text. We believe that the PC1-SWVL4 reduction in SEAS5 is mostly controlled by a model drift associated with the long free-running land-surface simulation. Yet we think that from 2015 the drift is mixed with a real long-term signal, particularly after 2015 when the reduction accelerates, and later, also seen in ERA5. Since our study deals with seasonal prediction, long-term variability is of secondary importance. This is demonstrated by our improved ability to predict yield when this PC is removed from the analysis despitethe fact that we can’t disentangle the signal from the model artifact. We thank the reviewer for this important comment. In the revised manuscript, we added a discussion along the lines of the two paragraphs above.

Minor Comments

• In the introduction (Lines 47-50) the reader is also referred to [1] for a recent comparison of three different soil moisture reanalyses in Italy.

Thank you for the suggestion. We are happy to add this reference in the introduction.

• Figure 1 (and also Figure 6): Why the correlation shows large negative values (dark blue) only in SWVL4 in the southern part of the domain? Maybe such negative values (which disappear when removing PC1), are linked to the skill degradation (see Major comment).

In Figure 8, the negative correlation disappears when reconstructing the soil moisture after removing the first PC from both ERA5 and SEAS5. This suggests that the negative correlation may be related to the artificial drift in SEAS5 shown in Figure 3.
This point will be clarified in the revised manuscript.

• Why there is a large region in Eastern Africa with white colours? Are there missing data? Moreover I would add statistical significance to such correlations in the Figure (e.g marking statistically significant areas with hatches).

Yes, we checked the raw ERA5 and SEAS5 soil moisture datasets. In the SEAS5 data, soil moisture values are missing over parts of eastern Africa, particularly in the deeper soil layers. As a result, correlation coefficients could not be computed in these regions, which appear as white areas in the correlation maps. This will be clarified in the revised manuscript.

• In Figure 5 percentages are related to SWVL3 and not to SWVL4

Yes, thank you very much for observing over minor details. We will change it in the revised manuscript.

• Table 3, Please express the number of pixels as percentage with respect to the total number of land grid cells to make the data more readable.

Sure, we will mention it as percentages, so that it will be more convenient to read.

• Line 470 ”Removing PC1 isolates mesoscale precipitation and root-zone moisture anomalies that directly affect crop growth.” This sentence is too strong in my opinion and would require the introduction of rainfall variability in the study to check its validity. I would remove such sentence.

Minor edits

Sure. We will write these types of sentences in a balanced manner in the main manuscript.

• Line 124: Please check the symbol k

Sure.

• Table 1: I suggest to remove it and keep description and source only in the data availability statement at the end of the paper.

Sure, we will do it

• Line 186: Remove bold text Figure 1

Sure.

• Figure 1: Remove Figure title, Caption is already enough

Sure, we will remove the figure1title.

• Lines 206-207: Please complete the sentence.

Sure, will check it.

• Line 214: Remove bold text Figure 3

Yes, will do it.

• Table 2 is not adding any information about the results with respect to Figure 4, please consider to remove it

Sure, we will make changes in the revised manuscirpt.

• Figure 7, please check caption

Sure, we will make it clear in the revised manusrcipt.

References

1. Hersbach, H., Bell, B., Berrisford, P., Hirahara, S., Horányi, A., Muñoz-Sabater, J., ... & Thépaut, J. N. (2020). The ERA5 global reanalysis. Quarterly journal of the Royal Meteorological Society, 146(730), 1999-2049.
2. de Rosnay, P., Balsamo, G., Albergel, C., Muñoz-Sabater, J., & Isaksen, L. (2014). Initialisation of land surface variables for numerical weather prediction. Surveys in Geophysics, 35(3), 607-621.