Response (in blue) to comments by Anonymous Referee #2

The authors use Hydrus-1D to simulate water balance to test the impact of meteorological drought on the agricultural drought over 31 (over a 55-long period in 1965-2019) meteorological stations Germany and Netherlands by focusing on the exceptional drought in the year 2018.

The evaluation of this manuscript is based on the following questions:

- *Is it a novel work based on a reliable scientific technique?*
- *Is it clearly structured and well-written?*
- *Are the experimental design and analysis of data adequate and appropriate to the investigation?*

The manuscript is well-written and potentially interesting for HESS. It presents novel work on the extreme drought recorded in 2018 in continental Europe. Nonetheless, this manuscript requires substantial improvement before publication. The manuscript is not well-presented, model set up is oversimplified and data analysis is fair.

Response: We very much appreciate the comments by the reviewer, which helped to improve the manuscript. We will revise the manuscript taking into account the comments.

The main scientific question to the authors: is the 2018 drought an episodic event (as 1976 and 2003) or a consequence of a significant drying trend? From the abstract, the authors state that meteorological drought is episodic and SPI and other rainfall indeces indicate no significant declining trend while temperature-based ET is characterized by an increasing trend. The authors should quantify the probability related to this extreme episodic event from the SPI distribution. Same for the SSI as a consequence.

Lines 384-385: "In summary, the increase of droughts is mainly related to increasing soil moisture deficits, and reduction in actual ET. The main driver is not a precipitation decrease, but an increase of potential ET."

Response: Thanks for pointing out this concern. We will add an analysis which will indicate how much more likely a 2018 drought is now, compared to the past, in terms of SPI, SSI, ET deficit and PPD.

Abstract

Abstract is generally OK. In order to get any feedback or relationship between climate and hydrological response through the use of indeces referred to six months or to the growing season makes sense. I recommend to remove indeces referred to 12 month duration since it ignores the fundamental impact of rainfall seasonality.

Response: Thanks for the suggestion by the reviewer. We prefer to keep indices for the 12-month duration period, as we see that especially 6-month duration drought in summer increases related to increased potential ET, whereas 12-month duration drought is less subject to change. With respect to the SPI and SSI trends in the figure 5, they are trends for the 12-month time period instead of the 6-month period. We will add 6-month SPI and SSI trends in the revised supplements and new text in the MS in an effort to clarify the use of the two time scales.

Introduction

The state-of-the-art is well written. I list other interesting references on meteorological and agricultural droughts. Please see and comment about soil moisture index (Hunt et al., 2009)

Response: Thanks. We will include the recommended references.

1. *Data and Methodology*

The authors should specify that the grass-reference potential evapotranspiration, ET_0 is converted into crop-specific potential evapotranspiration, ET_c by using a time-variant crop coefficient, K_c. Then, ET_p is partitioned into potential transpiration, T_p and potential evaporation, E_p by using a time-variant leaf area index, LAI.

Response: We will clarify this in the revised version of the manuscript and also include the equation used to calculate potential ET according Penman-Monteith, and the parameters adopted there.

Moreover, root depth is time-variant is the crop is annual and root distribution across the root zone needs to be specified. Actual evaporation, E_a and transpiration, T_a are calculated in Hydrus-1D depending on soil surface and root zone pressure head values, respectively. It is therefore clear that the simulation of ET_a depends on time-variant crop characteristics and local soil hydraulic properties. In lines 174-177 I understand that Kc is ignored, LAI and root depth are considered as 2.0 (or 2.88) and 50 cm, respectively. The soil hydraulic properties should be ideally measured. If direct measurements are not available, it is highly recommended to use PTFs based on silt, clay, sand contents, bulk density and organic matter (Weihermüller et al., 2021; Nasta et al., 2021). This study is basically a sensitivity analysis by considering that the spatial variability is quantified only in terms of soil texture classes (the van Genuchten's soil hydraulic parameters are crudely derived from tabulated values in Carsel and Parrish, 1988 in Table 3) and vegetation (assumed pastureland over the 31 stations) characteristics are constant in time and uniform in space.

Response: The five soil types are representative for the domain because they cover well the soil texture triangle and for each location calculations are repeated for these five soil types, covering different possible conditions near the measurement sites. It can be expected that in a region around a measurement site all these different soil types are present. In this study, our main objective is not to determine for each location as good as possible what the soil moisture and evapotranspiration conditions were in 2018 (and other years). This would require the use of all possible information sources including remote sensing information on soil moisture and vegetation states, precise soil and land use land cover information, among others. Our objective was to make a standardized comparison with past years and past droughts. For the further past, especially before the year 2000, remote sensing information is not of good quality, and a data based comparison is not possible. This is the reason why we used a model based comparison, covering all possible soil types.

The pasture was chosen because in this case it is known that all 31 meteorological stations are default located on a pasture. However, we agree with the reviewer that in a similar manner as for soil types, it would have been possible to cover different vegetation types (e.g., grassland, crop land, forest). To some limited extent we took different vegetation states into account by performing calculations with two different LAI time series. We decided not to include simulations for different vegetation types given the large amount of information already included in the paper, and also because we found that for the five different soil types, the drought trends and ranking of the drought years were hardly affected by soil type, in spite of the fact that absolute soil moisture contents showed large differences between soil types. This is also the reason why we did not consider time dependent root zone distributions. For the two LAI time series smaller differences among sites were observed, and the ranking was not affected. We already included a discussion on the impact of vegetation type on the results, pointing to the fact that for deep rooting vegetation rankings could be affected. However, we consider that our model-based comparison already covers many different conditions and think that a further extension is beyond the scope of this manuscript, and will not affect the main conclusions of this paper.

We will improve the clarification of the objectives in this paper, and specify that we want to perform a standardized comparison between years based on a model, to cover different conditions and in particular different soil types. However, a further extension to many different vegetation characteristics is beyond the scope of the manuscript and it is not expected that the main conclusions will be affected by it.

In Eq. 2 remove P and ET from the Richards equation. P and E_p are the climate forcings on the upper boundary. T_p is reduced to Ta through the sink term, S in Eq. 2

Response: Thanks, we will modify this.

1. *Results*

Line 261: The strongest decrease in precipitation is in southern Germany (-2.2 mm/year) sounds really insignificant if compared to its mean annual value. From Fig. 2d, I see that the trend is from 800 mm/year (or so) to almost 700 mm/year (or so). Please explain.

Same problem for ET trends (Fig. 3)

Response: We will clarify this in the manuscript and indicate that although a change of 2 mm/year seems to be small, this mounts to quite a large change over the considered time period.

To tell the truth, I don't understand Fig. 7 and description of Fig. 7. Please, improve the presentation

Thanks for the comment. We wanted to introduce this section with a general overview on how exceptional the year 2018 was from a meteorological point of view. We made the analysis on the basis of the ranking of the year 2018 in the complete time series of 55 years, for the different meteorological variables which influence drought. We will improve the description and discussion of Figure 7 in the text.

1. *Discussion*

Please, be more critical and evidence if there is room for future improvements

Response: We will extend the discussion and will discuss the limitations of the model, and the possibilities to improve the analysis in the future with different model types. We will also discuss additional variables which could be analyzed with more complex models, like for example crop yield. On the other hand, we will discuss in the revised version the limitations of such a more complex, integrated model.

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