

Hydrol. Earth Syst. Sci. Discuss., referee comment RC2  
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## Comment on hess-2021-569

Anonymous Referee #2

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Referee comment on "Long-term trends in agricultural droughts over Netherlands and Germany: how extreme was the year 2018?" by Yafei Huang et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-569-RC2>, 2021

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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.

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 indices 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."*

## **Abstract**

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

### **▪ 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)

### **▪ 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. 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  $K_c$  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.

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  $T_a$  through the sink term,  $S$  in Eq. 2

## ▪ 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)

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

## ▪ Discussion

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

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