Reply to the short comments of Chunyu Dong and Zhiyong Liu

We would like to thank Chunyu Dong and Zhiyong Liu for the provided comments and remarks. Before replying, we would like to mention that Chunyu Dong and Zhiyong Liu are former PhD students of the second author, now working in China.

Short comments of Chunyu Dong

The paper by Tijdeman and Menzel investigated the spatial and temporal variability of soil moisture drought in southwestern Germany using a distributed hydrological model. They analyzed the environmental controls on drought development and duration. Some interesting results are presented in the paper. For example, they find that drought stress tends to occur in warmer and drier locations. This raises an important question, i.e. how did and will climate change affect drought occurrence and severity? Some researchers have conducted some attempts to answer this question at a large scale, e.g. Samaniego et al. (2018). I would suggest the authors do some work on. For example, the authors may simulate the drought stress at different warming and precipitation change scenarios, and see what land covers will be affected most. I know that this analysis is already beyond the scope of this paper. The authors do not need to add this analysis in this paper. I have some other comments below, which may be helpful for the authors to improve this manuscript.

We appreciate the suggestions for future work.

L102. Figure 1a. I would suggest clipping the elevation map and only reserve the Baden-Württemberg. Then you may add a small panel at the corner which indicates the location of Baden-Württemberg in Germany.

Thanks for this suggestion. We will modify Figure 1a accordingly.

L169. In this paper, the SM drought threshold is set to 30% of AWC. Then a binary time series of SM drought stress occurrence becomes the basic data of this study. I am thinking it might be helpful to further classify the SM drought to different levels, for example, moderate, severe and extreme SM droughts. In this case, Figure 4 may demonstrates the temporal variations of cell counts for different drought severity. It may provide some information like whether climate warming has increased the drought severity in this region.

We can definitely add this information to the Figures. For example, see Figure R3 in the reply to RC1. This Figure shows that derived drought characteristics are (as expected) depending on the used severity threshold. However, the relative ordering according to severity is often not affected. In our study, we do not study a sufficiently long record to make strong claims about trends in simulated results.

(4) L170-175. SM drought stress occurrence (Socc) was computed in the basis of calendar year in this paper. Normally, most of the soil moisture droughts in Germany happen between spring and autumn. However, was there some winter droughts over 1989-2018, which began at the end of a year and ended in the following year? If yes, these special circumstances may overestimate the drought occurrences in the successive two years. In addition, how did you calculate the development time and duration for these special droughts? I assume these events are very rare in this region.

Simulations reveal that, according to the used drought definition and the selected agricultural grid cells, there were no multiyear droughts. All grid cells were filled above 30% AWC before the start of the next year and filled to field capacity before the start of the next growing season. Thus, winter droughts were not apparent for the considered agricultural grid cells.

(5) L225-227. This sentence is confusing. What does "the latter" refers to? What I see is that drought tends to develop at warmer locations for all prominent drought years but not for all the other years. Please make it more clear.

The sentence was meant as described above, but we agree that the use of “the latter” was vague and we will rephrase it.

(6) L255-258. It would be helpful to add the significance test of the rank correlations in Table 1

We could add significance, but question the value in thuis particular case, because given the large sample size, low correlations will already be significant. We think the relative magnitude and sign of the correlation coefficients provide enough information.

Short comment of Zhiyong Liu

It is interesting to read the entitled paper “Controls on the development and persistence of soil moisture drought across Southwestern Germany”. The manuscript is well-written and well-organized. I have a few suggestions regarding the method. It seems the authors used the linear correlation and regression models to identify the individual contribution from different controls on SM drought features. However, these two approaches can not differ the co-influencing between the controls (i.e., the soil properties and climate settings). Probably, the partial least squares regression (PLSR) and the partial correlation analysis could be more efficient to identify the individual Kindly see the R function, e.g., plsr and pcor.test in R program. The results based on the PLSR and partial correlation could be different from the current results. The Authors could make some tests based on their sample data. It is only a suggestion.

We will explore the suggested statistical techniques. However, in a revised manuscript, we would weaken our claims about controls, given the remarks of reviewer 1 that these are model based and reliant on assumptions. Therefore, we are hesitant to include additional statistical analyses.