Comments for manuscript: esd-2019-20

Title: Impact of precipitation and increasing temperatures on drought in eastern Africa
Authors: Sarah F. Kew, Sjoukje Y. Philip, Mathias Hauser, Mike Hobbins, Niko Wanders, Geert Jan van Oldenborgh, Karin van der Wiel, Ted I.E. Veldkamp, Joyce Kimutai, Chris Funk, and Friederike E.L. Otto

Summary

In this paper, the authors obtained the sensitivities of soil moisture, precipitation, potential evapotranspiration (PET) and local temperature to global mean surface temperature (GMST) from numerous datasets using statistical tools, and tried to explain the trend in soil moisture as a combination of trends in precipitation and potential evapotranspiration in eastern Africa.

I believe that the authors did a lot of work to quantify the synthesized values of sensitivities, which may be helpful for the drought analysis of eastern Africa. However, as far as I am concerned, the writing of this manuscript need significant improvement, for example, the logical chain of the paper is poor; some expressions are not appropriate (i.e., temperatures?) and can be confusing to understand. Thus, as scientific research, it does need substantial improvements to presents a sufficiently significant advance to meet the ESD standards.

Major points:

1. The logical chain of the paper seems to be incorrect. The target of this paper is to investigate the impact of precipitation and temperature on drought, which, however, was not quantified in the paper. In fact, the authors only showed the sensitivity of soil moisture, precipitation, PET and local temperature to GMST without any details of the physical mechanism.

2. The method to quantify the sensitivity of different variables to GMST is unclear in the paper. As shown in line 27, page 9, "The method is extensively explained in van Oldenborgh et al. (2019) and Philip et al. (2019)", however, van Oldenborgh et al. (2019) is in review (line 29, page 28) and Philip et al. (2019) is in preparation (line 22, page 27). Therefore, I believe it's better to illustrate some necessary mechanism of the method in the paper.

3. As shown in Figure 3b, the 95% confidence interval for fitted location parameter of precipitation to GMST is quite large. I wonder how precise the sensitivity of precipitation to GMST in the paper is since even the paper itself referred to the fact that "the effect of a changing climate on precipitation is generally much less straightforward" in line 17, page 2.

4. The authors claim to use as many datasets as readily available, provided that the data are sufficiently complete over a long-enough time period. Moreover, there are different hydro/impact models being applied to simulate PET and SM. Two questions are raised here, first, since the accuracy of different datasets may vary spatially, is it reasonable to use as many datasets as readily available, particularly, without applying any additional bias correction (as suggested in Page 6 line 2); second, a very long paragraph is organized here to describe different projects and models, however, differences among these models are not highlighted and the reasons why these projects and models were selected are not clear. Section 2.2 needs serious revisions.

5. The result that 'Precipitation has a stronger influence on soil moisture variability than temperature or PET in the drier or water-limited region' seems to be one of the major conclusions in this study. In fact, there are studies revealing the fact that precipitation is more influential on soil moisture over dry regions and temperature is more influential on soil moisture in wet regions. The authors may need to highlight the novelty of this study in different ways.

Some specific points:

1. Page 1 line 5, we studied trends in six regions or four drought-related variables? I suppose they refer trends in four drought-related variables, however, the statement is not appropriate.

2. Page 4 lines 6-15, this paragraph doesn't seem to be closely related to the topic of this manuscript?

3. Page 4 line 24, a discussion and conclusions are… this is suggested to be changed to discussions and conclusions are…

4. Page 4 line 27, in this section we show… this sentence can be moved to Line 22 before in Section 3 to keep consistency and avoid a one-sentence paragraph.

5. Table 1 and Figure 1 have basically the same information, no need to keep both. Suggest keeping only Figure 1. The authors mentioned that six regions are selected based on livelihood, precipitation zones and local expert judgment, suggest clarifying these criteria clearly in Table 1.

6. In Table 3, the description of '-' (a negative trend) is missing. A small comment, I think table 3 may not be necessary here since similar information has been conveyed in Fig. 6.

7. Lines 6-7 in page 19, not clear

8. Page 20 line 22, we find that … (Prudhomme et al., 2014). It is not clear whether the conclusion comes from the author or from other's work.

9. Page 20 line 31, it is therefore (of) high priority? And line 32…..has been to apply simple, these sentences seem problematic to me.

10. The inconsistency between Figure and Fig. in the manuscript (e.g. Page15 lines 27-28).

11. For soil moisture and precipitation, both low extremes are targeted. Why the distribution functions are different?

12. Are there any proofs suggesting that the CenTrends precipitation dataset is better than others?

13. Content in Section 3.1 is hard to follow due to the poor logic. Suggest reorganizing.