

Interactive comment on “Impact of precipitation and increasing temperatures on drought in eastern Africa” by Sarah F. Kew et al.

Anonymous Referee #1

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This paper attempts to demonstrate trends in different hydroclimatic variables and how they may relate to recent droughts in eastern Africa. While it is laudable that many (mostly model-based) time series have been used to address uncertainties, the documentation of these data is somewhat confusing, the presentation of results lack clarity and the interpretation/discussion of findings is rather vague. In general, the material is presented in a way that makes it hard to follow the implications of the chosen synthesis method, the differences among models and regions, and the overall conclusions; also, rather long-term trends than droughts are being analysed.

Main comments:

Already the title is somewhat misleading, as you do not really analyse droughts but (modelled) annual soil moisture and climatic trends. I understand the argument that

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soil moisture and PET may be proxies for agricultural drought, but the connection of this analysis to drought and even food security is too vague. Moreover, the analysis of long-term annual trends probably tells little about the (shorter-term) droughts. The attempt to interpret the recent drought years as part of the overall trends is too limited. I'd suggest to rephrase the setup in terms of that you analyse hydroclimatic trends over the study region rather than suggesting the analysis is on droughts. Moreover, the study may better fit a specialised hydrological or climatological journal.

Introduction: lengthy, with some passages that do not straightforwardly lead to the study's objectives or promise too much. Specifically, I think, the statements on food production (p. 3 lines 3-15) are not needed; it could be much more straightforwardly said that you analyse four variables without attempting to construct such an argument (which you instead could shortly point to in the Discussion/Conclusions). The paragraphs on p. 4 also belong rather to the Discussion. The study's leading questions should be much more concrete, and focused on the East African region.

Study region: It is not clear how the three criteria were applied: homogeneous precipitation (is it really homogeneous by the way, at what time scale?), livelihood zones, expert judgment? And is it really so that the final results are only aggregated for these 6 zones, and only based on annual data? This should be said clearly early on, as it limits the scope of the analysis (while arguably increasing robustness).

Datasets: It would be very helpful if there was a summary of the methodological approach in the very beginning of the Methods or as part of the Introduction. Figure 2 and the following text is not easy to follow; a well-structured and annotated table showing all data, acronyms, time periods and references would be way better.

Methods: Not clear to me why "global" temperature is used and what the purpose of this analysis is. Is it done for all time series, and why not just use the original data? What "validation tests" were done, and if they were more or less qualitative you may still have applied a quasi-objective criterion of whether the seasonal cycle "resembles" the

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observational data (which actually). What is a return period for a specific year, 2018 (e.g. Fig. 3)? Why does w@h require no fitting?

Page 13 point 4: So different time periods are mixed in your synthesis product? Doesn't that produce biases or at least merit discussion?

Section 3.2 is weak; I do not see a convincing approach to drought analysis here, and why is but one illustrative example explained which also only says that there is a marginally significant trend over the whole time period? This seems to be also something that should go to the Results.

Results: I am not sure if this is the best selection of figures to portray results, and whether the set can be extended (note, the methods part has more figures than the main text's results part). Are also maps possible? Why focus text on the SS region only? In any case, the results section is way too short, and the reader gets lost on what figures, tables, data you refer to in the Results' text. A clearer presentation of key findings is needed, plus a more academic style (terms like "looking at" etc. should be more precise analytically). The order of presentation also need improvement, maybe variable after variable.

Discussion: rather a list of shortcomings (which does not build trust in the analysis) than a discussion of the main findings and their relevance. Surely every analysis has caveats, but in this paper the robust patterns need to be highlighted and then discussed in terms of their plausibility and potential further studies to be done as follow-up.

Conclusions: too long and not really conclusions but an extension of the Discussion.

Detailed/technical comments:

Abstract: Study period needs to be mentioned. Line 12, "Nevertheless...", this info is not needed here. Line 14/15, this is self-evident and no novel conclusion of this study I'd say.

Introduction: Line 30, GCM is the abbreviation for General Circulation Models. Page 4

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lines 20-27: can be deleted

Datasets: what is the original spatial resolution of the different data, and how were they aggregated? W@home data: using the counterfactual climate dataset seems to make no sense here? Page 8 line 27: why not shown, what sort of analysis is this? Line 33: what is refET? Page 9 line 3-13: belongs to Discussion, as not studied here and probably not relevant for the historical time period. Line 17: what is the relevance of the RCPs here, as you do not analyse future periods.

Discussion: page 19 line 17: where is this subannual analysis presented, and why not part of the Results (same for the analysis of PET differences, page 20 line 22)? Page 20 line 21: uncertainties and origin are given: not so clear, and this is also in contrast to what is presented in Table 3.

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