

Interactive comment on “Have precipitation extremes and annual totals been increasing in the world’s dry regions over the last 60 years?” by Sebastian Sippel et al.

Anonymous Referee #4

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Overall, I am pleased with the topic of the Sippel et al. paper, which is an evaluation and criticism of some of the methods used in the Donat et al. 2016 paper. More extreme precipitation in the world’s dry and wet regions. This is the type of check-and-balance that keeps our science robust. Sippel et al. address two main criticisms of the Donat 2016 paper, (1) the introduction of a statistical bias when the rainfall data is normalised, and (2) the introduction of another statistical bias based on the regions that are selected as “dry” and “wet” regions. The overall flow and readability of the paper was dense, but not unfollowable. However, I understood the context of the paper, and the authors’ intention, much better after I read the Donat et al. 2016 paper. The authors could use more precise wording to clarify that the methods used were done to recreate the

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results from Donat et al. 2016.

On the topic of the introduced bias from normalising the data; the process of normalising data is pretty common and ensures that areal averages are not dominated by very wet regions. However, this needs to be done with care. The authors unpack and clearly describe the statistical changes that are introduced from the normalisation process. I liked the illustrative example found on page three in lines six through 11 and the quantification of the bias (%) in appendices A and B provided good support for the argument. (Although it isn't clear why these are included as appendices and not tables in the paper). Furthermore, the authors do well to point out the changes that arise by using different reference periods to deconstruct the data (i.e. Figure 2). I note that it was not really clear from reading the Donat (2016) paper why they used the 1951–1980 period to normalise the data.

I don't completely agree with the argument for selecting dry regions. The criteria and thresholds used to define a dry region are very subjective. As Sippel et al. point out, precipitation alone is not enough to determine if a region is wet or dry—e.g. at very high latitudes where even small amounts of rainfall can exceed the potential evapotranspiration. However, the criteria used are dependent on the question to be answered. If the question to be answered is, “How are global precipitation patterns changing?” then an analysis of precipitation alone would be sufficient. If you are trying to address, “Are wet/dry regions getting wetter/drier?” then the hydrology/aridity or climate classification of the region would need to be considered.

The authors quantify the “regression to the mean” bias (as shown in appendices A and B) that arise by defining dry areas as the lowest 30%. The authors further demonstrate that by using the Köppen classification and the Greve (2014) definition that the large trends found by Donat et al. are dramatically minimised. I think this argument is a moot point because, as other reviewers have already pointed out, the HadEX2 dataset does not have data over the world's driest regions (e.g. the Sahara, Western Australia) or some of the wettest regions (e.g. the Amazon or the Maritime Continent region).

A global analysis of precipitation extremes or precipitation trends using HadEX2 data would deliver incomplete results.

Specific comments: 1. Page 4, line 12: mentions a two-sided trend test. Is this the same as the Mann–Kendall test used by Donat et al. and mentioned in the caption of figure 3? It is not really clear in the body of the text why or how this test was chosen.

2. Appendix A, Figure 0, caption: check the spelling of Köppen. This figure was hard to understand. After reading the caption a few times I understood that it is basically built as a table with the first (left) column being the PRCPTOT data and the second (right) column being the Rx1D data. It would be nice to have the rows/columns clearly labelled.

3. Figure 2: The caption mentions red lines. The lines look orange to me.

4. Figure 3: I found this figure very difficult to understand. There is a lot of information that is overlaid on other information. The grey text is too light against the white background.

5. Your methods for producing this graph (grey and black lines) are not clear. You mention the grey lines have been corrected for “statistical artefacts”; I could not find this correction explained anywhere. Which artefacts have you corrected for? Is it the bias from the normalisation? Likewise, the process for producing the black line, or removing the incomplete data, is not explained.

6. The label on the first row of graphs mentioned the Köppen–Geiger climate classification, but the caption references Köppen (1900). The Köppen–Geiger classifications were not published until Geiger (1954 and 1961). Kottek et al. 2006, which was mentioned in the text, is of the Köppen–Geiger classifications. Should the caption reference Kottek et al. 2006 rather than Köppen (1900)?

7. Are graphs 3.e and 3.f from the Greve data, dry+transitional regions? It is not clear from the caption.

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