**Climate
of the Past**

Open Access

Discussions

Interactive
Comment

Interactive comment on "Investigating uncertainties in global gridded datasets of climate extremes" *by* R. J. H. Dunn et al.

R. J. H. Dunn et al.

robert.dunn@metoffice.gov.uk

Received and published: 21 August 2014

We thank the reviewer for their detailed comments and address each in turn below.

Reviewer

1) It seems patently obvious that any so-called global estimate is highly dependent on the completeness of coverage of the observations. This is for land only. It is not only a matter of coverage, but coverage at the same places: losing one grid square while gaining another still adds discontinuous information. The utility of this dataset is much less in the global value and more in the regional gridded product. The global aspect is overdone in the article.

Response

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The reviewer is right that there are limitations with the spatial coverage of the observational data sets, in that they are only covering land areas and completeness of data is variable. However, the aim of this paper is to investigate the robustness of results on changes in climate extremes, as presented in Donat et al. (2013a) and Alexander et al. (2006) – both studies focussed on "global" changes based on regions for which data are available. For consistency we use the same terminology but also added notes on the coverage limitations in Section 2.

Further, when calculating these timeseries, grid boxes have to be non-missing for 90% of the time to avoid artificial trends from variable coverage. We have expanded upon this in the section assessing the effect of this 90% criterion (Section 3). In this manuscript we have also included the maps of the correlations and the normalised variances to show in which regions there are large differences arising from the different choices in the method.

Reviewer

2) Once again precipitation is not well treated and the inherent intermittent nature of precipitation is done poorly. Talking of "precipitation trends" (abstract) without saying "amounts" and avoiding statements about frequency, intensity, and type are not helpful. This stems back to limitations in the indices used.

With regard to precipitation amount, a recent study has compared datasets and concluded that coverage and changing stations is a major issue and a primary reason for disparate results involving drought studies. These results are very relevant here.

Trenberth, K. E., A. Dai, G. van der Schrier, P. D. Jones, J. Barichivich, K. R. Briffa, and J. Sheffield, 2014: Global warming and changes in drought. Nature Climate Change, 4, 17-22, doi:10.1038/NCLIMATE2067.

Response

We intended that the sub-sampling analyses would address the issue of station net-

works, and how using different stations with different record lengths would change the global averages. Thank you for the additional reference, which we have now added along with further discussion to address the issue of varying station network in Sections 4.7 and 5.

As you point out, for precipitation, global averages are of limited use as they cancel out smaller scale regional effects. What we intended is that the two measures used in the maps give an indication of the level of the uncertainty at the grid box level. Users are then able to combine these with maps of the indices in their analyses.

We appreciate the issue when describing the behaviour in precipitation trends as the quantity being described is also important. However as the different indices measure a range of precipitation properties it is difficult to describe all using a single term. We have gone through the text to clarify where possible what we mean when speaking about precipitation trends and included more natural language appropriate for the index under scrutiny.

We have gone through and reworded sections discussing the precipitation indices to highlight the issues with the spatial variability of this quantity when calculating the indices.

Reviewer

3) There is also a focus on linear trends, yet there is no expectation for a linear trend in any quantity except for limited periods, and there is considerable interest in variability on multiple time scales. For example, a major issue is how the pause on global mean temperatures in the 2000s and associated modes of variability or forcings affect these indices analyzed here. Sorting out the human component is a separate issue and analyzing linear trends is not helpful. For precipitation, there are expectations of large regional trends (wet get wetter, dry get drier, etc) but globally a lot of cancellation and regional influences of aerosols come into play. The authors touch on this p 2132 line 23, but even that statement should be expanded, as it is all cases!

Response

We have added a paragraph on the limitations of the linear trends into Section 4.2 where the maps are explained. We realise there are limitations and that not all indices will be well described by a linear trend since 1951. However, they do provide a simple and well understood summary of the long-term change in the timeseries (see also comments by Reviewer 3). Also, we are comparing the relative differences between different versions of the final gridded data arising from the different methodological choices, rather than assessing the absolute change in the index over time.

For the changes in variability, we agree that this is an interesting point, but feel it is beyond the scope of this already lengthy paper. We are not explicitly assessing these indices over time and linking these to changes in the climate of the planet, but are doing a relative comparison. Where the variability of the global average time series has changed dramatically, this has been noted in the text.

The smaller scale regional changes are captured in the maps to try and overcome some of the limitations of the global average time series. As we note in the paper, if regional studies are to be carried out using HadEX2, we would encourage them to look at the data from this assessment to understand the uncertainties for that region in detail.

Reviewer

4) It would be helpful to have a short discussion of the parent data and number of stations and their distribution, for instance, for precipitation total a comparison with GPCC would be useful.

Response

The number of stations available at each index are listed in Table 1 of Donat et al. 2013a – around 7000 for the temperature indices, and 11500 for the precipitation ones. Distributions of the stations for two example indices are also given in their Fig. 1.

Rather than reproducing this information, we discuss the station number and distribution in the HadEX2 methods section.

Unfortunately it is not actually possible to compare the results with GPCC data since measures calculated by the two datasets are different - GPCC sums all precipitation totals while HadEX2 PRCPTOT only sums days where precipitation is greater than or equal to 1mm. So while we agree with the reviewer that the comparison is a useful one, given that equivalent output is not produced from the two datasets, a comparison would be somewhat meaningless.

Reviewer

5) The polynomial fit (Fig. 11) is awful at large distances and should have been constrained. It has no sound basis.

Response

The polynomial fit was used in HadEX and HadEX2 and therefore is included here as the baseline to compare against. It is the result of a Taylor Expansion of the exponential decay curve. Although the fit is poor at large distances, the important part of the fit is at small distances, where the drop off to 1/e occurs. We chose this particular index and latitude band to show how the different fitting methods perform. We have now added to this figure the same plot for TN90p (T_min > 90th percentile), where all four methods perform much more similarly. This indicates that for this index all 4 methods are very similar up to the location of the DLS.

Also, despite the poor fit of the polynomial at large distances, for all of the temperature indices, and for many of the precipitation ones the correlations and agreement between HadEX2 and the grids based on exponential fits is high, especially for later parts of the record.

We have expanded this section to explain in more detail the results of the different fitting methods (also with reference to the comments of Referee 2).

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Reviewer

6) Table 1 has the list of abbreviations used throughout the paper and its name, along with other information in very tiny font. In many places throughout the text and in figure captions it would help if the name and/or definition were mentioned, instead of just the acronym, which is often not easy to remember and most people will not read this paper end to end. In all the figures, the material in tiny print under the panels is too small.

Response

We have expanded the name of each index the first time it appears in each section and caption for each set of figures (the time series and the maps would count as one set for each of the sub-sections).

We have increased the size of the figures and also the font size in the legends to improve their clarity and readability.

Reviewer

Other comments

There are many minor issues with English or typos. P 2112 l 6; l 13 (correlation between what?); l 26-27 "clearly" twice

Response

Sentence clarified to indicate what the correlations are between. First "clearly" removed.

Reviewer

P 2120 l12 "tie"? l 23 "values"

Response

Clarified sentence.

Reviewer

P 2124 l 21 araising

Response

Spelling corrected.

Reviewer

P 2128 l 22 is increases

Response

Sentence corrected.

Reviewer

P 2129 l 12 liekly

Response

Spelling corrected.

Reviewer

P 2132 l 3 ass

Response

Spelling corrected.

Reviewer

Is "completenesses" a word? (Fig. 1 caption).

Response

We have replaced this by "completeness criteria" where it occurs.

Reviewer

It is not helpful for captions to say "as for Fig ". One has to always refer back.

Response

We have included a little more detail on most of the figure captions to aid in quick interpretation.

Reviewer

Fig. 5: I presume "detrended correlation coefficient" means correlation coefficient using detrended data? Better to say so.

Response

Caption updated accordingly.

Reviewer

Fig 17:what is the purple? Is that red on top of blue?

Response

Yes, we have clarified this in the caption to Fig. 16.

Interactive comment on Clim. Past Discuss., 10, 2105, 2014.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper