Evaluation:

Scientific significance: Good
Scientific quality: Very Good
Presentation quality: Very good.

General comments
This paper is welcome in that it highlights the uncertainties in structural and
methodological choices in constructing a dataset.   It should be published with
minor revisions.

The paper is relevant to the goals of the journal, and it explores uncertainties in a
comprehensive manner. It is not so novel or new in that the data have been
presented elsewhere. The paper is well written and complete.

Several overall comments are as follows:
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.

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.

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!

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.

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

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.

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
P 2120 l12 'tie'? l 23 "a… values";
P 2124 l 21 araising
P 2128 l 22 is increases
P 2129 l 12 liekly
P 2132 l 3 ass

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

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

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

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