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# *Interactive comment on* "Global trends in extreme precipitation: climate models vs. observations" *by* B. Asadieh and N. Y. Krakauer

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We thank the reviewers for their careful readings, and appreciate the opportunity to submit a revised and improved version of our paper. Our responses to the points raised are as follows:

Referee #1 :

1) First paragraph of section 3: The authors should specify the method used to calculate statistical significance of the trends. Do they use one of the two methods in the appendix or another method?

* The statistical significance for trends at 95 percent confidence level is based on P-

value being less than 0.05 in the linear regression. The explanation is added as the last paragraph of section 2 in the revision.

2) The Q_median statistic seems to be misinterpreted in the paper. As defined in appendix A2, it is an estimate of the trend, but page 11377 (line 19) says it is a "confidence level", page 11382 says it is a "slope of trend", and table 1 presents it as not having any units (the units are presumably mm/day/yr). In table 1, Q_median and the ordinary least squares slope ("b") agree for the observations (0.0504 versus 0.0521) but not so well for the models (0.0230 versus 0.0314). This discrepancy should be commented on.

* The text is edited in the 6th paragraph of section 3 and the unit of Q_median [mm.day-1.year-1] is included in the revision. More discussions on this are added to the 4th paragraph of the Results section and the 2nd paragraph of the Discussion in the revision. Also the boxplot of the Qmed is added to the Figure 2 in the revision. As explained in Appendix A2, the Qmed sign reflects the direction of trend, while its value indicates the magnitude of the trend. The confidence interval in Sen's method is calculated using Q_min and Q_max values, which are not discussed in the main text. The comparison should be made between b and Qmed values of each model separately. Qmed boxplot is now added to the Figure 2. However, the boxplots of b and Qmed for climate models are very similar on global and continental scale for different percentiles (Fig 2.c and d, respectively). Comments on the discrepancies are now included in the 4th paragraph of the Results section.

3) Some of the observational results are similar to those of Westra et al, J. Climate, 2013. For example, Westra et al show the percentage change in annual maximum precipitation per K of global warming at each latitude, similar to Fig 5d. I think this paper should include a few sentences regarding the differences in methodology and any differences in results. Both Westra et al and this paper show a higher rate of change per K at some lower latitudes - could this be related to the higher rate of change per K found for tropical extremes by O'Gorman, Nat. Geo. 2012, or is the analysis too

uncertain?

* The results of the present work are now compared with that of Westra et al. 2013 in the last paragraph of the Results section in the revision. We also comment on the possible significance of the large trends seen in the tropics.

4) The figures are generally difficult to read. The fonts are too small, and the titles given above panels are too long and have many unexplained abbreviations. In Figure 3, the marker sizes have meaning but they are too small for this to be of use to the reader.

* Figure titles are edited in the revision. The Figures are also shown in larger sizes now.

5) page 11380 lines 12-14: It is stated that "extreme precipitations and flooding" seem to be underestimated by climate models, but this is not supported by the results presented: a) this paper doesn't analyze flooding, b) according to page 11376, the lower values in the models are because of the different scales for models and observations and are therefore not truly an underestimation, and c) this paper doesn't provide error bars for the observational trends and so it is not possible to conclude that the trends are underestimated in models. (It seems unlikely that the difference of 10%/K versus 8.3%/K mentioned in the conclusions is statistically significant.)

a) The word "flooding" is excluded in the revision. b) Scale differences between models and observations have always been a challenge in the comparison of the results, according to the earlier studies. The present study have used a gridded observation product and also spatially and temporally subsampled models as observations in order to limit the uncertainties in the results, although it is not practical to totally eliminate them. c) The present study utilizes the non-parametric Mann-Kendall method to calculate the Z-score which defines the level of confidence in the identified trends, where the higher values of Z indicate higher confidence level in the results. Table 1 indicates that the average observational Z-score is higher than the average in the models which indicates that the observed trends are statistically more significant than the modeled ones.



HESSD
11, C5840–C5845, 2014

The paper does not claim that the difference in the mean magnitude of the precipitation trend between observations and models is significant given inter-model variability, but states that extreme precipitation has increased faster in the observations than in the model mean. We acknowledge that the uncertainties still remain.

6) Caption of figure 3: Model disagreement for the trends is quantified at the gridbox level and is said to be a measure of the discrepancy between the climate models. However, these differences could easily be due to the influence of non-forced variability (which will be different in different simulations), rather than any differences between the models. The maps are also said to show the underestimation of the trends by the models, but both more positive and more negative trends are apparent depending on location. (The absolute magnitudes of the gridbox trends are expected to be smaller compared to observations because an average is taken across independent model simulations with their own variability in each case.)

* The stated discrepancy is regarding the significance of the trends and not the trend magnitude. The sentence is edited accordingly. The model projections are selected for similar scenarios for all climate models, but we agree that non-forced variability is also a contributor to difference in trends between models. The underestimation statement is excluded from the Figure 3 caption in the revision. We agree that the absolute gridded trend values are expected to be smaller, where Table 1 shows that the average of modeled absolute extreme precipitation value is smaller compared to the observations. However, the conclusions are made based on the relative changes in the extreme precipitation and percentage of change per K global warming.

7) More care is needed to distinguish between the global average of relative trends and the relative trend in the global average. For example, the abstract says that "The global average of observational annual-maximum daily precipitation has increased ... 8.5% in relative terms". I think this number refers to the global average of the relative trend at each location, but it is not clear. Similarly ambiguous statements are made in the conclusions.

* The sentence is edited in the text as “On global scale, the observational annual-maximum daily precipitation has increased by an average of 5.73 mm over the last 110 years, or 8.5% in relative terms” in the revision.

8) The appendix should be referred to in the paper.

* The Appendix is referred in the last paragraph of the Introduction section in the revision.

Referee #2 :

1. Consider adding few lines or citing a paper to explain the justification of using the linear regression, which is assuming that the annual block maxima (daily rainfalls) is normally distributed when block maxima should follow GEV.

* We now briefly discuss with references to earlier studies, the question of what distribution to assume for anomalies in maximum precipitation, e.g. normal versus GEV. We also mention that an advantage of non-parametric methods (like Mann-Kendall and Sen's methods utilized in this study) is that they provide trend estimates independent of the distribution of the data.

2. Since two types of methods-parametric and non-parametric, were applied, few lines could be added in discussion section explaining what were the similarities and differences in the results comparing the two methods.

* Discussions on this are added to the 4th paragraph of the Results section and the 2nd paragraph of the Discussion in the revision. Also the boxplot of the Qmed (from the non-parametric method) is added to the Figure 2 in the revision.

3. Consider citing Chou & Neelin (2004)'s work when referring to wet getting wetter and dry getting dryer in page-11372, line-1.

* Chou & Neelin (2004) is included in the revision.

4. Titles of all figures are wordy and not clearly readable. Additionally, for figure-2: the

red dots are not clearly labeled.

* Figure titles are edited in the revision. The red dots in Figure 2 represent model outliers, which is now explained in the caption as well.

5. In page-11376, line-7, the statement “The 19 climate models give 19 global averages . . .” gives an impression that 19 climate models were used while 15 were used with 19 runs.

* The sentence has been edited to “19 climate model runs” in the revision.

— Enrico Scoccimarro comment:

* Dear Enrico Scoccimarro, we appreciate your comment on the present study. The mentioned paper is now referenced in the Introduction part.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 11369, 2014.