

Interactive comment on “Temporal and spatial variability in precipitation trends in the Southeast Tibetan Plateau during 1961–2012” by X.-L. Zhang et al.

Anonymous Referee #2

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General comments: This study presents the trends in annual and seasonal precipitation over the southeastern Tibetan Plateau. Based on the references made in the introduction chapter and the reviewer’s own literature review, the novelty of this manuscript is questionable. As referee #1 already pointed out, numerous studies exist dealing with the same parameters (precipitation plus max and min precipitation), with similar time scales, and mainly covering the same region (e.g. Li et al. (2010), Lu et al. (2008), Tan et al. (2010), Li et al. (2007), Duan et al. (2008), Wu et al. (2005), Li (2011), etc.). The authors’ response does not really solve this controversy because even if they focus on the Southeastern TP, not much “new” information can be given compared to what has been presented already by all the other literature! By only using one method (the com-

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monly used MK test) on few datasets, the results and conclusions do not present novel concepts, ideas, tools, or data, and are not compelling enough to approve publication in *Climate of the Past* (which in my point of view is also not the right journal, as this study is rather focusing on the present conditions based on the most recent past).

Specific comments: The authors engage in describing how the topographic conditions are responsible for the identified trends. This is rather weakly described in a simple and only qualitative way. To improve their results and conclusions some quantitative investigation into the topography seems rather necessary, especially considering the large mountainous terrain (of an area of at least 40,000 sqkm) and the small number of used weather stations (14). As this study focuses on the topography an interpolation with kriging in combination with a profound digital elevation model might be interesting. Only data from 14 stations is available, hence, I strongly recommend incorporating further sources of data such as re-analysis, modeling, or satellite sources. An addition could be also the use of rather specific indexes that prove the variations in precipitation more evidently. In climate research numerous methodologies exist to analyze (extreme) precipitation data. Hence, I strongly recommend to additionally using at least one more appropriate method. For example recurring cycles in dry and wet episodes could be investigated by applying a Fourier analysis and/or a wavelet analysis. Another feature of scientific interest is the linkage to changes in river discharge and/or surface water/snow accumulation, which is the major concern in this area anyway.

For the introduction chapter, the responses/additions to referee #1 are merely helpful as these are only common statements, of which the most have been already stated in other papers. The authors should put their focus on a more specific topic/feature (which hasn’t been investigated yet) and apply more advanced methodologies and indexes.

Technical corrections: All maps (Figure 1, 2, 5, 7, 8) lack a good descriptive character. This means that e.g. the neighboring provinces/countries/oceans/rivers need to be shown and named, especially in Figure 1 and 2. As Figures 5, 7, and 8 show only one parameter; using different shadings of the circles or even exchanging them with

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the actual value might be much better decipherable for the reader than they are now. Additionally Figure 7 and 8 should be integrated into one figure only by applying shapes and shadings according to the trend and magnitude, respectively. The information in most of the tables is not much needed as most significant values have already been described in the text. Please note: a non-significant trend is not significant and, hence, might only show a positive or negative tendency (and not a positive or negative trend)! Please revise accordingly.

Interactive comment on Clim. Past Discuss., 11, 447, 2015.