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Interactive Comment

Interactive comment on "Temporal and spatial variability of extreme snowfall indices over northern Xinjiang from 1959/1960 to 2008/2009" *by* S. Wang et al.

Anonymous Referee #2

Received and published: 12 May 2014

General comments:

The paper attempts on quantifying monotonic trends in "heavy snow" indices in the northern region of China, Xinjiang, and generate spatial fields of results by using a standard interpolation method "universal krigging". While the purpose of the research could be a-priori interesting for the reader of Natural Hazards and Earth System Science, the outcome is certainly disappointing, and it does not reach the standards for an international journal such as NHESS. My recomendation is to reject the manuscript; however I encourage the authors to re-think about their research, and how they could make it suitable for an international publication. For this I specify the main drawbacks of

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the manuscript, as well as some of the (countless) technical errors that I found. Given that this is an open discussion, I would like as well to express the concern that I feel as a scientist, when I read revisions such as that of the Referee #2 in which only a couple of comments are provided alongside with three references, that are, surprisingly, authored by the referee.

Specific comments:

Presentation/quality

The first impression when one reads the paper is confirming (as Referee #1 pointed out) the poor language/English that it contains, that even non-English speakers as myself can find countless grammatical, wording, and spelling errors along the manuscript. Sentences such: "Actually, there is a bunch of researches devoted to the trends of snowstorms over a variety of regions" or, "Mentioned above make it easy to identify snow days and their snowfall amounts. What needs to be mentioned is that the precipitation is usually measured by rain gauge in China. In case of snowfall, the snow amount captured by the rain gauge is taken from observation site to room. When the snow in rain gauge melted at room temperature, the melted-water amount is measured in unit millimeter (mm)", are clear examples of very bad use of English, that alone could be a reason for rejecting the manuscript.

The figures are as well of very poor quality, lacking any kind of edition (such as Fig. 2), or entirely uninformative (such as Fig. 4). Some paragraphs, especially those in the methodology section, are dispensable. For example, the statistical explanation of the Mann-Kendall tests is given in thousands of previous papers, and more important, in the original references, so there is no need to write it down, maybe with some references it would be enough. Also the whole explanation of the universal kriging interpolation method seems to me disproportioned, especially when the objective of the paper is not comparing interpolation methods, or measuring the goodness and suitability of the used one. Moreover, as I argue in the next paragraph, the spatial interpolation of results in this study seems to me inadequate, given the small amount of cases (stations) for such a wide and topographically complex study area.

Lack of representativeness/lack of knowledge of climatological basics

In the introduction, the authors make a quick description of the study area, but they forget to talk about the extension and other geographic features, which are essential for the further interpretation of results and suitability of methodology. At a guess I'd say that the area of the region is about 0.5 million Km2, which is about the size of countries such as Spain or Sweden. Two mountain chains are observed in the northeast and southwest of the region, with a wide basin in between. Nothing is said about the well-known gradients of precipitation (orographic precipitation) and temperature (adiabatic lapse rate) of mountain areas, which accumulate much larger amounts of snow than the plains. I'm afraid that 18 pluviometric stations are not representative of such a wide and diverse area, and this invalidates any spatial interpolation to be made (none can imagine an interpolation of snow indices in, let's say, Spain with only 18 meteorological stations). None of the 18 stations are located in the mountains, only a few are in the foothills, and the majority are located in the inner basin, therefore the tendency surfaces show in Fig 3 are totally arbitrary. There are more suitable interpolation methods when orography is present such as the co-krigging or the multiple regression method (Ninyerola et al, 2000), that enable introducing the elevation as a co-variable, thus obtaining more reliable results.

Mislead of scientific concepts

From the scientific perspective there are as well various inaccuracies that the authors should take into consideration for any further investigation. For example, they use the term "trend" inadequately, as it only should be used when the Mann-Kendall coefficients are statistically significant. Example: Page 7069 line 4: "Upward trends were observed at 17 out of 18 stations, while only one station exhibited a downward trend (Table 4). The highest upward trend occurred at Urumqi station, and the downward trend occurred at Altay station (Table 6). MK significance test for the trends in the time series of the SX1day showed that 10 out of 18 stations had significant upward trends (at $p < 0.05$), accounting for 55.6% of the total stations (Table 5)."

If only 10 out of 18 stations showed significant trends, they shouldn't say "upward trends were observed at 17 out of 18 stations". They should rather use the term coefficient, i.e.: positive coefficients were observed in 17 out 18, 10 of which were statistically significant, indicating upward trends...

The authors use the non-parametric Mann-Kendall test to search for the significance of trends, and they justify it because you don't have to assume any distribution of data, and because it is not influenced by outliers; however they use the parametric Pearson test to compute for the magnitude of trends, and this test it's influenced both by the presence of outliers and by the distribution of the sample. The use of both tests is contradictory, and the results of table 6 are confusing. What does the bold and italics refer to, p-level of the Mann-Kendall test or p-level of the Pearson test? If the amount of change is given by the Pearson test, it is erroneous to use the p-level of MK test, as both tests have different sensitivity. The Thiel-Sen slope estimator (Yue et al., 2002) should be used instead of the Pearson test, as it complements the MK test, giving the value of the slope and thus the magnitude of any existent trend. Moreover, data series should be checked for autocorrelation (Yue et al., 2002). The existence of autocorrelation can lead to erroneous rejection of the null hypothesis in the MK test, thus removing any serial correlation is highly advisable prior to run MK test.

I have serious concerns as well about the suitability of the data used for this kind of analysis. Firstly, the authors are assuming (should the readers assume it too?) that any precipitation recorded during the December-February period is below 0°C. This should be demonstrated in the manuscript. Secondly, the authors indicate that the daily precipitation types were discriminated, but then, for the calculation of the indices, we don't know if they are using daily precipitation, or daily amounts of snow... Thirdly, as indicated by Referee #2, there is no explanation on how the indices were calculated.

This whole methodology part is full of assumptions and rather obscure and needs a lot more detail to be reliable.

Finally, the authors use the concept of “extreme” without a real comprehension of its meaning. From a statistical perspective extreme refer to unusual, or very little frequent (in the extremes, or tails of the distribution), and from a meteorological perspective it includes as well an exceptional magnitude of the event. As we don’t know how the indices were calculated we cannot really appreciate if they refer to extreme events. The terminology used by authors is as well confusing. What does “snowfall” refer to? Is it amount of precipitation? Once again, how do we know that all this precipitation was in the form of snow? They should rather use the concept of Snow Water Equivalent (SWE). The values shown in table 2 are hardly representative of an extreme amount of snow. Is really 10 mm of SWE in 24h considered a heavy snowfall in China?

Ninyerola, M., Pons, X. and Roure, J. M. (2000), A methodological approach of climatological modelling of air temperature and precipitation through GIS techniques. Int. J. Climatol., 20: 1823–1841. doi: 10.1002/1097-0088(20001130)20:14<1823::AID-JOC566>3.0.CO;2-B

Yue, S., P. Pilon, B. Phinney, and G. Cavadias. 2002. The influence of autocorrelation on the ability to detect trend in hydrological series. Hydrological Processes 16:1807-1829.

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