

## ***Interactive comment on “Variability in snow cover phenology in China from 1952 to 2010” by C. Q. Ke et al.***

### **Anonymous Referee #3**

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The authors present a study on spatiotemporal variations and trends in snow cover days (SCD), snow cover onset date (SCOD), and snow cover end date (SCED) using observational data from 672 climate stations in China. The period of analysis is from 1952 to 2010.

Overall the manuscript contains a lot of valuable information and is well organized for the most part. However, I feel there is room for improvement. In the following, I have listed several recommendations and questions to the authors.

Major recommendations:

2.1 Data: The authors selected 672 stations for their analyses. How many climate stations are contained in the original dataset? This information should be added to the

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text. When using nearest neighbor interpolation to fill data gaps, was the correlation between the time series tested over common time periods? Especially in the western and northwestern regions with low station density, the nearest neighbor might show a quite different snowfall pattern.

2.2 Methods: In this chapter, the authors need to provide information about the correlation analysis. What correlation coefficients have been calculated? If the authors calculated Pearson Product-Moment Correlation Coefficients, were the data tested for normal distribution? How was the significance testing done?

3 Results: Fig. 2: What is the time period covered? It needs to be the time period covered by all climate stations used (1980/81 to 2009/10). If not, and the result is based on means over different time periods, comparability is a problem.

The results of the correlation analysis reported in the subchapters 4.1, 4.2, and 4.3 of the Discussion chapter should be in the Results chapter.

Minor recommendations:

Page 4474, row 6: Please omit this part of the sentence “China is the main large snow cover distribution area in the middle latitudes and the Northern Hemisphere [...]”. There are vast areas covered by snow in other regions of the middle latitudes and the Northern Hemisphere as well.

Page 4475, row 20: “[...] a SCD is defined [...]” instead of “[...] an SCD is defined [...]”

Page 4479, rows 22-23: “The Tarim Basin is located inland, with relatively little precipitation, thus snowfall there is extremely rare (Li, 1993).” Snowfall is not rare in the mountains surrounding the Taklamakan desert. Please correct this.

Page 4480, rows 17-20: The authors define heavy-snow and light-snow years based on the SCD anomaly using two requirements. However, more snow cover days do not necessarily coincide with more snowfall. Therefore, I recommend the authors to name

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it “year with a positive (negative) SCD anomaly”.

Fig. 1: Please add the symbol for the climate stations to the legend.

My last comment is an idea beyond the scope of this paper. It is an idea for future research. The authors have already looked at the relationship between the Arctic Oscillation and SCD. I encourage the authors to also look at the Siberian High Intensity (SHI) defined as the mean sea level pressure averaged over the center of the anticyclone (40°N-60°N, 70°E-120°E) (Gong et al. 2001; Gong and Ho 2002) and its relationship with SCD.

Gong, D.-Y.; Ho, C.-H. (2002): The Siberian High and climate change over middle to high latitude Asia. *Theoretical and Applied Climatology* 72: 1-9.

Gong, D.-Y.; Wang, S.-W.; Zhu, J.-H. (2001): East Asian winter monsoon and Arctic Oscillation. *Geophysical Research Letters* 28: 2073-2076.

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