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The Cryosphere Discussions

Interactive comment on "Snow cover variations across China from 1951–2018" *by* Xiaodong Huang et al.

Anonymous Referee #1

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In this paper the authors' document trends in China snow cover from surface observations of daily snow depth over the period 1951-2018. The paper results are consistent with previous publications showing mainly increasing snow depth and snow cover above 40N, with decreasing snow cover south of 40N. The main merit of the paper is the period of record (1951-2018) which currently represents the most up-to-date (and longest) assessment of snow cover trends in China. The introduction is well-written and comprehensive, but would be improved with more focus and synthesis of the Chinese snow cover literature, and a clearer discussion and presentation of the study rationale. The data and methods sections are mostly well written, although the methods section could use some additional explanation in a few places (see detailed comments). The trend results are presented clearly, but there is considerable potential to streamline

the presentation. The Structural equation modeling component of the analysis is not compelling; it currently lacks a clear rationale and is based on inappropriate air temperature and precipitation variables. The updated trend results presented in the paper are of strong interest to the cryospheric community. However, the paper provides little explanation of the mechanisms responsible for the trends, which is a major weakness.

Detailed comments: 1. Suggested wording change for first line of Abstract: "Snow cover changes over China from 1951 to 2018 are documented based on an analysis of in situ daily snow depth observations from 730 meteorological stations. The snow cover indicators analyzed included snow depth (SD), snow covered days (SCDs), and snow phenology."

2. The Introduction is well written and comprehensive, but it needs to focus more on China snow cover. I think you could delete the first two paragraphs and replace this with a focussed discussion of the various advantages and disadvantages of the currently available data for monitoring snow cover changes over China. In this regard, your statement that in situ snow depth observations provide the most reliable dataset for analyzing snow cover changes "with a high degree of credibility" will need to acknowledge the strong local scale processes influencing point snow depths, the poor spatial distribution of stations in some regions of China, and the low-elevation bias in the station network.

A summary table of China snow cover trend results from previous studies would be a useful addition to the Introduction given the sensitivity of trend results to the specific period of data analysed. The recent findings by Ma et al. (2020) of the role of changes in winter snow-free periods in snow cover duration trends should be included in the discussion as they help explain why snow cover duration can increase under warming temperatures. A concise synthesis of previous results and identification of knowledge gaps is important for providing a clearer rationale for this particularly study. For example, the SEM analysis presented in subsection 3.2 appears to be an innovative aspect of the paper that needs to be incorporated in the study rationale.

3. What is your definition of “stable” snow cover? Are TP, NX and NC highlighted in Figure 1 because they are the only areas in China with a stable snow cover? Is this also the reason that only these three regions are summarized in the results? The issue of ephemeral vs stable snow cover deserves some discussion particularly in light of the Ma et al. (2020) paper. Related to this, your statistical analyses are carried out at all stations in China not just those with a stable snow cover. How robust are the statistical methods in ephemeral snow cover areas with frequent zero snow cover years and undefined start and end dates to the snow season?

4. In Section 2.2 the use of an annual period seems strange given the snow season is confined to a much shorter cold season. It is also not clear how annual maxima of air temperature and precipitation would assist in diagnosing changes in snow cover. From energy and mass balance considerations variables like freezing degree-days, total solid precipitation and the solid-fraction of total annual precipitation would be expected to have more relevance for explaining variability and change in snow cover.

5. Section 3.1: Can you provide a line or two of text prior to eqns. 3 to 6 to explain what these equations are being derived for? I suggest you add a new section “3.2 Change-point analysis”. Overall I find section 3.1 a bit confusing and statements in the Results section further increase my confusion e.g. page 9 line 4 “The results of the M-K trend test are the same as those of the slope method”.

6. Section 3.2: Please provide some introductory text to your current section 3.2 on why you proposing to employ SEM? What are the hypotheses you are testing and why is SEM the best method? In your statement that “seven factors were screened out”, I think you mean that seven factors were retained for analysis. As mentioned previously, the use of annual maxima in this analysis is difficult to justify for understanding snow cover variability.

I think you would learn more about snow cover variability by defining the regional snow cover response regions from EOF analysis, then looking at the corresponding regional

time series of snow season air temperature, total precipitation, and precipitation solid fraction.

7. Include slope units in Table 2. Why is the China average not included as in Table 3? The same applies to the trend result tables for other snow cover variables.

8. Can you explain how the anomaly time series in Figure 4 is obtained? There should be an error bar for each annual mean, and the error will influence the linear fit through the points. Can you also provide a brief explanation how to interpret the UF and UB curves in Figures 4b and 4d. Wouldn’t the confidence interval in the trend get narrower as the length of the time series increases?

9. What is responsible for the break points shown in Table 3? Are they linked to changes in atmospheric circulation?

10. Section 4.4: The results of the SEM analysis are not very convincing and add little to the paper. The analysis may be more instructive using air temperature and precipitation variables that are more closely linked to snow cover variability.

11. I think your Results section could be significantly shortened if you presented all the snow cover variables together instead of separately. I think this would also help interpreting and explaining the results. At the moment the results are presented in a rather descriptive way following the same format for each variable, which is not very interesting from the readers point of view.

12. The conclusions are largely descriptive and it is hard to find any new insights into snow cover variability and change in China in this paper. As it stands, the only significant contribution of the paper is to extend the period of previous trend analyses. I see several areas where the authors could make potential new contributions: - document the snow response regions of China from EOF analysis of station annual series of SDmax and SCD series - determine the roles of regionally-averaged (over the identified snow response regions) snow season air temperature, total precipitation, and

total snowfall in the observed snow cover series. - find physical explanations for break points e.g. atmospheric circulation changes, increased snowfall in winter storms, fewer snow-free periods (e.g. Ma et al. 2020). - find physical mechanisms for the decrease in snow season gaps documented by Ma et al. (2020)

Literature cited: Ma, N., Yu, K., Zhang, Y., Zhai, J., Zhang, Y. and Zhang, H., 2020. Ground observed climatology and trend in snow cover phenology across China with consideration of snow-free breaks. Climate Dynamics, pp.1-21.

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