Interactive comment on "Snow cover variations across China from 1953-2018" by Xiaodong Huang et al. Anonymous Referee #1

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1. **Comments from Referees:** The end date of 31 December 2013 is problematic. It makes more sense to use the end of a snow season or hydrological year. Small point, but because the complete 2012-2013 season was available for analysis, should the title not use the year range 1952-2013? More importantly, why was the seemingly arbitrary decision made to only utilize data through 2013, which means the reported trends do not include the six most recent winters. This means the reported trends have reduced relevancy and timeliness. As noted below, there were some extreme snow events in China during the 2009-2012 time period, so it would be interesting to see if these strong anomalies continue to emerge between 2013 and 2019.

Author's Response: Firstly, on behalf of all authors, we appreciate your affirmation and also great comments for this manuscript. Based on your suggestion, the snow data from 2014 to 2019 is collected through our effects, which extend the period from 1951 to 2018. The hydrological year which spanned from July 1 of the current year to June 31 of the subsequent year is used when analyze the snow variations in this study, thus, the data period used between July 1, 1951 and June 30, 2018. The title of the manuscript is modified as: "Snow cover variations across China from 1951-2018"

Author's changes in manuscript: The data and results are re-analyzed based on extended dataset.

2. **Comments from Referees:** Lines 85-92: It's not clear how time series of varying length at individual measurement locations were utilized in the analysis. Do the map figures (e.g. Figures 2, 4, and 6) show all stations with at least 10 years of data as noted on line 86? If so, does this mean there are different start and end dates for individual stations in these figures, or do all points over the same time period? Do some points represent trends calculated over only 10 years of data? In the figures which show regional averages (e.g. Figures 3, 5, and 7) how was variability in station time series length accounted for in the averaging procedure? More details on how the snow data were handled is needed in the first part of Section 2.1.

Author's Response: The proportion of length records in years for each stations during the period of 1951 to 2018 are calculated and mapped in Fig 1 based on your suggestion. There 67

stations which the snow records less than 10 years were abandoned in this study based on data quality control. More details on how the snow the snow data handled is described in section 2.1. Thank you for your comments.

Author's changes in manuscript: Please see the section 2.1 about data description and the figure 1.

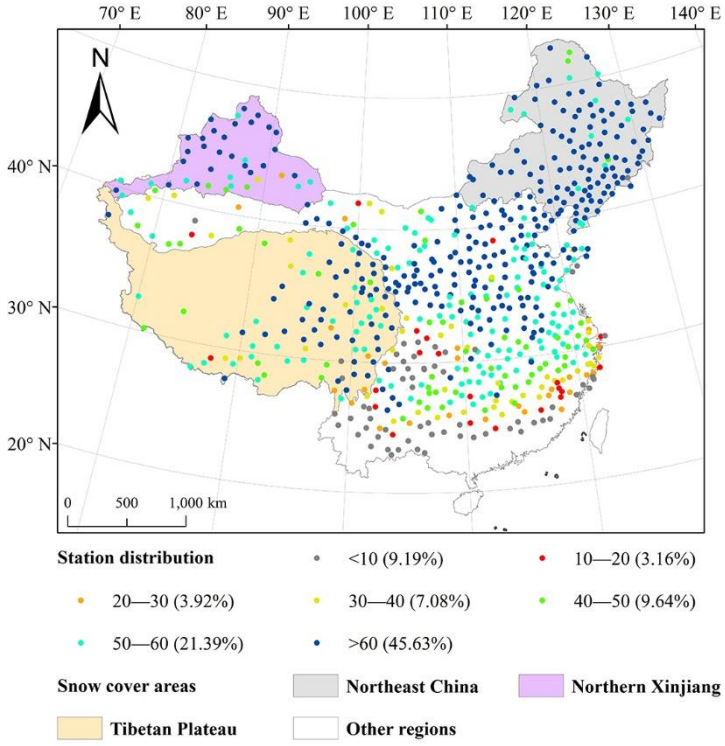

Figure 1. Geographical locations and the proportion of length records in years of each meteorological stations in China mainland.

3. Comments from Referees: Some of the analyzed snow variables are consistent with typical metrics of assessing snow cover, such as maximum snow depth, snow cover days, snow onset, snow end days, etc. but some of the snow variables are unusual and lack a clear physical driver that can be related to climate variability. I suggest dropping annual average snow depth and cumulative snow depth. In the case of snow onset date (SOD) and snow end date (SED), do these mark the beginning and end of continuous snow cover, or do they include ephemeral snow cover events (such as the first time snow accumulates, even if it melts again before continuous snow cover begins)?

Author's Response: We are agree with you. The accumulative snow depth lack a clear physical drive that can be related to climate, and also hard to explore its variation. But the annual mean snow depth ($SD_{overall}$) can somehow reflect the interannual variation of snow accumulation.

Thus, the accumulative snow depth is dropped based on your suggestion. In order to avoid the impact of ephemeral snow in snow phenology computations, the SOD is defined as the first date of the first three continuous snow records during the snow accumulation period, and the SED is defined as the last day of the last three continuous snow records during the snow melt season, respectively. Thank you for the comment.

Author's changes in manuscript: Please see the revised manuscript in page 103-115.

4. **Comments from Referees:** Section 2: the text describing Mann-Kendall and linear trends in parts (a) and (b) is not easily readable – it's essentially a series of equations which describe fairly straightforward statistical techniques. Sections (c) and (d) however provide only very short descriptions of more complicated techniques. Should the wavelet analysis and structural equation modeling be retained in the paper (see comment #8 below) the text in parts (c) and (d) must be expanded, while the readability of parts (a) and (b) should be improved.

Author's Response: Thank you for the comment. We are sorry that we didn't give more information for our methodologies. In the revised manuscript, the detail description for UF and UB in M-K test, as well as wavelet. Thank you again.

Author's changes in manuscript: Please see the section 2.2.

5. **Comments from Referees:** Section 3.1: I think most readers will not be clear on the terms UF and UB as utilized in the text. The definition of UF can be discerned from Section 2.1, but this is not the case for UB.

Author's Response: More detail definition for UF and UB are described in Section 2.2. Thank you for the comments.

Author's changes in manuscript: Please see the section 2.2.

6. **Comments from Referees:** Figures 3 and 5: the strength of these trends seem to be driven strongly by the early and late years in the time series. Without a handful of large anomalies during the first five years and the last five years, the trends appear to be near zero. Can some comments be added on the influence of these outliers at the beginning and end of the time series?

Author's Response: This anomalies may be caused by factors such as temperature, participation and the oscillation period of the snow cover. But we don't have the supporting data to prove it. Sorry for the response, and thank you for the comment.

Author's changes in manuscript: Please see the section 3.4.

8. Comments from Referees: Many of the overall trends are quite modest (e.g a decrease in snow cover days of 0.1 days per decade, which translate to not even a full day over the 70-year period of record). The lack of trends is itself not an issue – it's still useful to report this in the literature given the long time period. But with the lack of strong trends evident in the Mann-Kendall analysis, I don't believe the wavelet analysis and structural equation modeling are warranted. My suggestion would be to remove this analysis – Figures 8 and 9 are difficult to interpret and don't add much insight. instead, some additional analysis could add more value and innovation to the paper. I was left wondering two things: -Are these trends consistent with satellite-derived data such as the NOAA snow chart climate data record (https://climate.rutgers.edu/snowcover/index.php) which extend back to 1977? -Can snow cover trends over China be attributed to temperature and/or precipitation trends? In general, it appears the snow cover season is getting shorter (Figure 7) but the snow depth is increasing (Figure 3). How do the spatial patterns of seasonal temperature changes and precipitation change (including the proportion of snow versus rain) compare to the station observations of snow?

Author's Response: We are completely agreed with your comments. Based on your comments, The wavelet analysis and structural equation modeling results are deleted. The snow variations trends are compared with the results from remote sensing observations as well as the simulation results from CMIP5 model. We rewrote the discussion section, and focus on the consistent with other current results to further prove the findings in this study. Unfortunately, we don't have climate data to prove the snow cover trends which conclusions drawn from this article. In any case, we still appreciate your great suggestion.

Author's changes in manuscript: Please see the detail discussion in section 4.

9. Comments from Referees: A couple minor comments: -Line 17: the term 'jump points' is used here and throughout the manuscript, but I think 'break points' is a more appropriate term -Figure 1: Given the emphasis placed on elevation for some of the interpretation, can shading be added to this figure to show elevation?

Author's Response: The 'jump points' are revised to 'break points' based on your suggestion. For figure 1, because of the too many colors used to distinguish snow zones and meteorological stations, the use of shading relief as the background will affect the reading of the figure. We

tried many schemes, but can't find an ideal way. Thus the original scheme was kept. We are sorry we can't meet your comment. Please forgive us.

New references updated:

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