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Interactive comment on "Snow cover variations across China from 1952–2012" *by* Xiaodong Huang et al.

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General comments The authors have used the Chinese Meteorological Administration (CMA)'s daily snow depth dataset for China that covers the period, 1951-2013, to study and describe the annual variations in snow cover across China over the period, 1952-2012 (because they define the hydrological year as July-June, they were unable to include 1951 or 2013). This dataset is constructed from ground-based meteorological measurements taken from hundreds of meteorological stations distributed across most of China. The authors derive several different metrics of snow cover from this dataset and compare and contrast the annual trends for each of these metrics. They also describe separately the trends for the three main snow cover areas in China (northeast China; northern Xinjiang; and the Tibetan Plateau) as well as the overall trends for all

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China.

Although it is disappointing that the dataset ends in 2013 (and the analysis ends in 2012), the results should still be of interest to readers of The Cryosphere, and more broadly, the wider climate science community. This is especially so for three reasons:

1. Much of the analysis of snow cover trends described in the literature is based on satellite-derived estimates (in particular, the "Rutgers Snow Lab" dataset, which covers the period 1967-present), while the analysis in this paper is based on an independent ground-based dataset.

2. The snow cover trends for China have received some discussion and debate in the literature lately because they appear to be different from much of the Northern Hemisphere, e.g., see Wei and Dong, 2015; Wang et al., 2017; Chen et al., 2016.

3. Recently, some of us (and others) have been encouraging more research into the differences in climatic trends in different regions within China, e.g., Soon et al., 2018; Li and Yang, 2019; Soon et al., 2019. Therefore, this study is also useful because the authors provide the breakdown for the trends of each of the three main snow cover regions in China as well as the national trends.

However, in my opinion, the authors should compare and contrast their findings with other equivalent datasets. At a minimum, the authors should compare their results to:

1. The satellite-derived snow cover extent dataset maintained by Rutgers University Global Snow Lab (Robinson et al., 1993; Robinson and Frei, 2000; Estilow et al., 2015): https://climate.rutgers.edu/snowcover/docs.php?target=datareq

   Ideally, if the authors are used to working with NetCDF files, they could extract the gridded trends for China (and perhaps even the three regions) from the gridded dataset. However, if not, they should at least compare their results to the Northern

Hemisphere trends over the common period, i.e., 1967-2012. They might also want to comment on the post-2012 trends that are available from the Rutgers dataset since their own analysis finishes in 2012.

2. How do their results compare with the CMIP5 and/or CMIP6 Global Climate Model (GCM) hindcasts for China? KNMI have a useful website which provides access to many of the CMIP5 hindcast results (including "snow cover area"), and if you register on the website, you can apply a "country land mask" to the results, allowing you to extract the hindcasted regional trends for China. https://climexp.knmi.nl/start.cgi

   The Climate Explorer website sometimes has intermittent access, since it is largely the part-time efforts of just one researcher, Prof. van Oldenborgh. However, it is a very versatile website, and if the authors don't have access to CMIP5 or CMIP6 data from elsewhere, they could probably use this to extract the results for the CMIP5 hindcasts. For instance, we recently used this website for our analysis of Northern Hemisphere snow cover trends in Connolly et al., 2019.

They should also discuss in more detail how their results compare to other studies analysing Chinese snow cover (e.g., the Wei and Dong, 2015; Wang et al., 2017; Chen et al., 2016 papers mentioned above), as well as to the trends for the rest of the Northern Hemisphere (e.g., see the Connolly et al., 2019 paper mentioned above).

In a couple of places, the authors hint at how their analysis could be of relevance for anthropogenic global warming, and they uncritically paraphrase a claim from the IPCC's 5th Assessment Report. However, they don't seem to provide any actual discussion of the relevance. Indeed, as I will discuss below, if anything, their results are problematic for the IPCC's claims on snow cover. For this reason, the authors should *either* drop the discussion of anthropogenic global warming *or else* provide a more critical evaluation of how their results compare/contrast with the IPCC report (and other literature, e.g., Connolly et al., 2019).

With that in mind, if the authors were to satisfactorily do all of the above, then the manuscript would be worthy of publication in The Cryosphere. I will provide some additional comments that are more detailed below.

Specific comments

1.) *Relevance for anthropogenic global warming?*

On lines 50-53, you state, "The Intergovernmental Panel on Climate Change (IPCC) has reported that climate warming over the past 50 years is indisputable and that the temperature over the past 50 years is likely to be the highest on average over the past 500 years (IPCC, 2013)". There are several problems here:

- a) There is actually no "(IPCC, 2013)" listed in the references. Instead, the IPCC AR5 Working Group 1 report is currently listed as Stocker et al., 2013.
- b) It is unclear exactly which specific part of the >1000 page IPCC reports the authors are referring to. There are many different *similar-sounding* claims made in the report, but the IPCC actually often make *very* specific and precise claims that have been carefully parsed to simplify their narrative.

  Often if the scope of a particular IPCC claim were broadened to describe the bigger picture, it would make their narrative less compelling. For this reason, the IPCC lead authors are usually very precise in what specifically they are claiming.

  For instance, on p7 of the Summary for Policymakers, the IPCC AR5 WG1 claims, "Over the last two decades, *[...]* **Arctic** sea ice and Northern Hemisphere **spring** snow cover have continued to decrease in extent (high confidence) (see Figure SPM.3). 4.2–4.7" (emphasis added in bold).

  Antarctic sea ice trends over the same period had actually increased, as had Northern Hemisphere winter and autumn snow cover. However, by not mentioning these results, the IPCC were able to create a more compelling narrative of an

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overall “warming of the climate system” (p2, Summary for Policymakers), without making any false claims. In other words, their claims (especially the ones made in the Summary for Policymakers) are **not** made to accurately inform the scientific community of the full context of their findings, but rather to selectively present information which makes their overall narrative seem as compelling as possible.

As an aside, you might have guessed from the above that I’m **not** particularly impressed by the scientific rigour of the IPCC reports. This is true. However, regardless of what you think of the IPCC reports, my point is that, when quoting the IPCC reports, you have to be *very careful* in their specific quotations. Otherwise their statements could be inaccurate.

With that in mind, in the Summary for Policymakers, the IPCC WG1 AR5 claims, “Each of the last three decades has been successively warmer at the Earth’s surface than any preceding decade since 1850 (see Figure SPM.1). In the Northern Hemisphere, 1983–2012 was likely the warmest 30-year period of the last 1400 years (medium confidence).” (Summary for Policymakers, p3) Is that the specific claim you are referring to, or is it something else? Because it is not quite the same as the statement you made.

- c) With regards to the claim which you attribute to the IPCC, it is debatable, and could seem somewhat cherry-picked. Most paleoclimate temperature reconstructions argue that the 17th, 18th and 19th centuries were relatively cold ("Little Ice Age"), but that roughly 1,000 years ago, there was a relatively warm period ("Medieval Warm Period").

  There is, of course, considerable debate over how the Current Warm Period compares to the Medieval Warm Period. The IPCC argues that the Current Warm Period is “likely” warmer. But, notice that even in the IPCC’s claim that I quoted above, they only assign medium confidence and the term “likely” (66-100% chance according to the IPCC “likelihood scale”) to their claim that “1983-2012 was likely the warmest 30-year period of the last 1400 years” (and that they

confine this statement to the Northern Hemisphere).

In other words, the IPCC have actually left it as fairly plausible (up to 34% chance according to the IPCC) that it was similarly warm during the Medieval Warm Period. So, choosing "the past 500 years" as a benchmark (i.e., excluding the Medieval Warm Period) might seem like cherry picking.

Even if you had stuck to the shorter instrumental period (i.e., the last 150 years or so), there is debate over the relative warmth of the recent warm period to the early 20th century warm period. In Soon et al. (2015), we argue that the current land surface temperature datasets have failed to satisfactorily correct for non-climatic biases, including urbanization bias. We developed an alternative estimate of Northern Hemisphere temperature trends since 1881 using mostly rural stations. This new estimate suggests that, while temperatures increased from the 1970s to 2000s, they cooled from the 1940s to 1970s, and temperatures in the 1940s were comparable to present. This could contradict your claim (which you attributed to the IPCC) that the most recent 50 years were the hottest over the past 500 years.

More recently, in Soon et al. (2018), we reviewed the more relevant debate over how the current warm period compares to the early 20th century warm period **for China**. We showed that there is considerable debate over how warm the earlier warm period is relative to the current warm period. Indeed, this debate is ongoing, e.g., see Li and Yang (2019) and our reply (Soon et al., 2019). However, hopefully this should illustrate how you need to be very careful in making these sort of generic statements, when you are referring to the IPCC!

- d) Moreover, it is unclear, why you are making this claim anyway. Later (lines 69-70), you add, "In the context of global warming, the feedback of the snow cover in China on climate change is unknown.".

  I appreciate that it has become "fashionable" to include a reference to "global

warming" or "global climate change" in *any* paper looking at climate trends. However, if you really want to frame your results in the context of global warming (as opposed to framing your paper as a study of regional climate change), then you should be more rigorous, and show the **full** context.

For instance, the IPCC claim on p2 of the Summary for Policymakers that, "Warming of the climate system is unequivocal, and since the 1950s, many of the observed changes are unprecedented over decades to millennia. The atmosphere and ocean have warmed, **the amounts of snow and ice have diminished**, sea level has risen, and the concentrations of greenhouse gases have increased (see Figures SPM.1, SPM.2, SPM.3 and SPM.4). 2.2, 2.4, 3.2, 3.7, 4.2–4.7, 5.2, 5.3, 5.5–5.6, 6.2, 13.2" (emphasis added in bold). Then, on p7, they claim that, "Over the last two decades, *[...]* Northern Hemisphere spring snow cover *[has]* continued to decrease in extent (high confidence) (see Figure SPM.3). 4.2–4.7".

Yet, this paper shows that, **for China**, the amounts of (annual) snow cover have generally increased.

You are not the first to notice this difference for China (e.g., Wei and Dong, 2015; Wang et al., 2017; Chen et al., 2016 as mentioned above). And, in Connolly et al. (2019), we showed that the IPCC's "spring snow cover" claim was not representative of the overall snow cover trends, e.g., winter and autumn snow cover seems to have generally increased over the same period.

With that in mind, I recommend you go with one of two options:

- Option 1: Explain how your results are somewhat at odds with the IPCC AR5's claims and discuss the full context.

  or

- Option 2: Drop the references to "global warming" and stick to describing the trends for China.

2.) *Questions on the datasets*

- a) Do you know if the CMA are planning on updating their snow depth dataset to include more recent years? If not, could you include a comment on why it has not been updated since 2013?
- b) On lines 84-87, you say, “To ensure the reasonableness of statistical analysis, we must ensure that the station dataset used for statistical analysis were longer than 10 years. Therefore, the stations with less than 10 years of records were omitted from the analysis.”. Can you elaborate on how many stations you had in total and how many were omitted after these steps?
- c) Will your results be available as Supplementary Information? If so, this would be great.

  Ideally, as well as the time series you constructed, I would also like to see the individual station results available as Supplementary Information. But, if that is not possible (since it is a CMA dataset), it would be helpful if you could at least provide some indication of the numbers (and possibly locations) of the stations available for each year.
- d) On line 79, you say that the dataset begins on 1 January 1951, but then on line 90, you say, “...since the SD measurements began in October 1951”. Which date is it?

3.) *Results*

- a) Should the title of Section 3 be “Results” instead of “Result”?
- b) On lines 206-207, you say, “The national $SD_{average}$, $SD_{overall}$ and SD_{max} showed increasing trends from 1952 to 2012 under the context of global warming”. What do you mean by this?

First, as explained above, I would reconsider whether or not you want to frame this paper in the context of global warming.

Also, do you mean "anthropogenic global warming", i.e., an expected global warming trend arising from increasing greenhouse gas concentrations? If so, then as we discussed in Connolly et al. (2019), the CMIP5 hindcasts (which do indeed attribute most of the global temperature trends since the 1950s to anthropogenic greenhouse gas emissions) predict that Northern Hemisphere snow cover should have been **decreasing**, and **not** increasing as you find. In other words, your results are actually somewhat problematic "in the context of (anthropogenic) global warming".

- c) But, instead of focusing on global trends, why not frame these results simply in the context of *regional* climate trends for China? How do your results compare with the equivalent temperature and precipitation trends for China?

  In this context, we provided a review of Chinese temperature trends in Soon et al. (2018). You might also find the ensuing discussion, i.e., Li and Yang (2019) and Soon et al. (2019) relevant.

- d) As mentioned in the General Comments section, you should include at least some comparison with the Rutgers satellite-derived dataset (for example). Ideally, I would calculate the regional trends for China from their gridded dataset. But, at the very least, your results should be compared to the overall Northern Hemisphere time series.

- e) As also mentioned in the General Comments section, you should compare how your results compare to the CMIP5 and/or CMIP6 hindcasts of snow cover for China. See Connolly et al. (2019) for a systematic comparison of Northern Hemisphere snow cover according to observations vs. all available CMIP5 model runs.

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- f) Have you looked at the differences between seasons? As we discussed in Connolly et al. (2019), the trends for Northern Hemisphere are quite different for each season. Depending on how you carried out your analysis, this might be beyond the scope of your paper, but you should suggest it as a possibility for future work if you think that this could be done using your dataset.

- g) To be honest, most of the statistical analysis in Sections 3.4 and 3.5 seems unnecessary to me and doesn't really add much insight (in my opinion). Personally, I would remove these two sections. However, perhaps some readers might find it of interest, so I will leave it up to you whether you think these sections are needed or not.

4.) *Conclusion*

Your Section 5 currently reads more like a "summary" than a conclusions. It is ok to include a brief summary as part of the conclusions, but you should **also** provide some concluding remarks and/or some recommendations for further research.

Technical corrections

The written English is not great with some grammatical errors scattered throughout the manuscript. I suggest using the "Check spelling and grammar" option of Microsoft Word or some other similar word processor, and fixing any of the underlined errors that will appear.

Review by Dr. Ronan Connolly

References cited in this review

Note: I am a co-author of several of the papers listed below. However, I dislike when a reviewer uses their review as an excuse to promote their own papers. For this reason, the authors are under no obligation to cite any of my listed papers, but I'm including them in case the authors do find them relevant!

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