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The Cryosphere Discussions

Interactive comment on "Assessing snow cover changes in the Kola Peninsula, Arctic Russia, using a synthesis of MODIS snow products and station observations" *by* Rebecca M. Vignols et al.

Anonymous Referee #2

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General comments and recommendation:

This paper aims at describing the snow cover evolution over the last decades in the mountains of the Kola Peninsula (Arctic Russia) using local meteorological and snow stations and MODIS satellite data. The article is well organized, and the text is well written. Also, the method used to replace the missing data in MODIS is interesting. However, I would recommend a rejection of this article for three main reasons: (i) the time series used to investigate the trends are really short to get significant signals. This is mentioned in the text, but the discussion about the climate signals is very limited, and some of the author's conclusions regarding the trends sound speculative. As an exam-

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ple, they point out that wind changes could be responsible for contrasted patterns of regional snow trends, but without showing any analysis of the wind, and without investigating other processes that could involve for example temperature and precipitation changes; (ii) The comparison between the MODIS and the station data is not well presented. In particular, I could not understand in what extent the difference highlighted between satellite and station data is related to the missing data in MODIS (in relation to clouds) or to the differences between the sensors and their respective uncertainties; (iii) The analysis of the snow cover start (SCS) and snow cover end (SCE) dates as well as the snow cover duration (SCD) is also not very clear using the maps in which it is difficult to localize the mountains and the water bodies. Finally, the authors mention in their conclusion that further research using climate data (model outputs and I would recommend also to use meteorological data) will be used to investigate the climate factors that drive the snow cover changes in the Kola Peninsula. To conclude, I encourage the authors to work on the three points mentioned previously, and to include in their study an analysis of the snow-climate interactions, to produce a more complete article. In addition to this general recommendation, a point-by-point list of comments is presented below.

Point-by point comments

Abstract: OK

Introduction:

P3L66: was shorthening -> was shortened?

L70/75: The authors describe the high spatio-temporal variability of snow cover/depth at the regional scale, focusing in particular in opposite signals in terms of trends. They should mention here that because of internal variability of the climate system, a local positive trend is compatible with a negative trend over a longer trend. This can explain contrasted regional trends (e.g. Mudryk et al., 2017; Connoly et al., 2019) that can happen just by chance, depending on the chaotic nature of the climate system.

Section 2: Climate of the Western Mountain Regions (WMR):

The authors present a nice description of the climate of the WMR and the Kola Peninsula. However, there is a general confusion when describing trends of vegetation, snow cover and temperature estimated from other publications because these ones focus on disjoint periods. For example, the authors should be more cautious when drawing a parallel between a warming over 1965-2015 (Demin and Volkov, 2017), a decrease by 15 to 20 days of the length of the summer in the Kola Peninsula in the 1930 to 1998 interval, and a 44% increase in winter precipitation recorded over the Northern taiga forests in the Kola Peninsula over again another period (Høgda et al., 2001). Overall, the authors should be more careful when describing the co-evolution of these variables for which the observations are not available over common periods.

Section 3:

L. 155: what is the uncertainty related to the unofficial data https://rp5.ru? If you use this data, you should present them in the manuscript.

Section 3.2.1:

Could you explain what are the MODIS bands?

L. 165: Âń This is a snow cover index that directly relates to the presence of snow in a pixel and is a more accurate description of snow detection compared to Fractional Snow Cover (FSC) products (https://nsidc.org/). Âż -> could you explain why is it more accurate and why you finally consider FSC$\sim$NDSI?

Section 3.2.2: data processing

Could you include an evaluation of the sensitivity of the method used to compute the SCE and SCS dates to the choices of the thresholds 5 days for SCS and 10 days for SCE? This could be done to check if this assumption impact the final result.

Section 4: Results and Discussion

L. 245: Âń However, over the time period also covered by MODIS (2005 to 2016), a statistically significant trend is identified in the SCE ($p < 0.01$), wherein the snow cover season has been ending earlier at a rate of 1.45 days/decade. This is a result of the year 2017 being a very anomalous year (see Fig. 2), thus the inclusion of such an outlier year decreases the statistical significance of the trend Âż -> It also demonstrates the low confidence level corresponding to this trend: Even with p-value<0.05, the power of the statistical test is probably very low because of a small sample compared to the low signal-to-noise ratio.

L. 277: Âń In contrast to these findings, using phenological evidence, Kozlov and Berlina (2002) found that the length of the summer in the Kola Peninsula decreased by 15 to 20 days in the 1930 to 1998 interval. Âż -> I would say that a shorter summer is not incompatible with a shorter snow cover annual duration.

L. 315-335: It is not clear whether the authors focus on the differences between MODIS and the data station due to the missing values or due to the sensors themselves, and also if they focus on the SCE and SCS numbers or on there trends. For instance, at L. 316: ÂńOn average, the difference is 8.6 and 10.4 days between the station and MODIS SCS and SCE dates respectively. There is a slight bias in the MODIS dates with higher errors being positive, so finding an earlier date than the station data. On average the mean bias of the SCS and SCE dates are + 1.2 days and + 5.0 days respectively.Âż -> this is not clear to my point of view. I do not catch the differences between the numbers showed in these two sentences. What is the exact definition of the mean and the mean offset in Table 6? Another example at L. 349: Âń the differences are highest for Kandalaksha station with an offset of up to 41 days Âż we cannot see the number 41 in Table 6. To clarify the text, you should systematically reefer to the numbers shown in the Tables.

Table 6: You could compute the differences MODIS – station data excluding the dates for which MODIS is not available. Then, you could differentiate the bias related to the missing years from the bias related to the sensors themselves.

L. 360 and Figure 6: we cannot visualize exactly the location of the mountain and the water bodies on the map. The correspondences with the map of the Figure 1 is not straightforward. Contours showing the topography or any geographical information would helped for the interpretation.

L. 368: I do not see the “two dark streaks Âż in Figure 7a.

L. 374: “Overall, the standard deviation is much higher for the SCS than the SCE and SCD Âż -> I see lower deviation for the SCS than for the SCE and the SCD in many areas of the map shown in Figure 7. Did I misunderstood something in the maps?

L. 376: “SCD is the most inter-annually uniform snow cover parameter Âż -> this is true in the plain, but false in the mountains (if I understand well the map?), where the standard deviation of SCD can exceed 30 days (a larger signal than the SCS in the same areas).

L. 412: “low attitude zones Âż -> Low altitude zones

L. 412: Âń However, the main clusters of significant trends in the low attitude zones are negative over water bodies and are positive along the eastern edges of the mountain ranges. Âż -> this is difficult to see on the map without the contours delimiting the topography and the water bodies, and without the names of the mountain ranges for someone who does not know the local geography.

L. 420: “The increasingly early end to the snow cover in the higher altitudes may be due, in part, to increased blowing snow (Demin, personal communication) as a result of the overall trend towards stronger winds in the Kola Peninsula (Roshydromet, 2005). Âż -> this sounds very speculative, precipitation rates and temperature changes could also play a major role.

Conclusion:

L. 438: “In this analysis, we find that for 85.8 % of pixels investigated (SCS and SCE combined) the deviation in the MODIS-derived dates is less than 20 days Âż -> This

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not clear. Are the authors speaking about the standard deviation of SCS/SCE over the whole domain, or about the deviation between MODIS and other data?

L. 442: Âń There is high inter-annual and spatial variability in the long-term snow cover trends in the WMR of the Kola Peninsula Âż I agree about the large variability of the snow cover, but to my point of view, 2000-2016 is a really short period. The shortness of the period used in this study is probably the main reason for the lack of statistical significance found in the trends.

L. 449: Âń These differences in snow depth as well as some of the trends in the snow cover season are probably explained by wind scouring that occurs in the WMR Âż. There is no justification for that. Precipitation and temperature variability could also explain the spatio-temporal variability of the snow parameters and trends variability. Wind data should be presented.

Figures:

The Figures have a good quality, but the lack of geographical characteristics (mountains, water bodies, names of the different areas) does not allow an efficient understanding of the corresponding text.

References:

Connolly, R., 1; Connolly, M.; Soon, W.; Legates, D.R.; Cionco, R.G.; Velasco Herrera, V.M. Northern Hemisphere Snow-Cover Trends (1967–2018): A Comparison between Climate Models and Observations. Geosciences 2019, 9, 135.

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