

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 #1

Received and published: 25 April 2019

This article presents an assessment of snow cover changes over the Kola Peninsula. It makes use of MODIS and snow depth data for this. The topic is relevant to TC journal.

However, the manuscript presents too many flaws to be accepted in my opinion. I find that there is some potential for this study, but the way it is presented now makes it impossible to actually understand whether the study is worth being published or not. Indeed, the paper suffers from issues regarding its organisation (for instance some methodological aspects are presented in the results section. . . after some other results that actually use these methods!), its justification (I had difficulties to see from the introduction, which should be rewritten - see below - how novel and important this

[Printer-friendly version](#)

[Discussion paper](#)



study is) and the manuscript contains too many small mistakes and imprecisions about the elements that are presented every now and then, that should have been easily avoided from a careful reading from the authors.

I present below a list of major comments that in my opinion justify rejection of the paper, and after I include a list of specific minor remarks.

Major comments:

All over the paper, I would recommend erasing the word “statistically” in the expression “statistically significant”, as this is a kind of repetition. Indeed, the word significant has a specific meaning in mathematics, which assumes that a significance test is used. In addition, I would recommend that the authors provide information about the significance test that was used in this study, as it does not appear in the text.

Introduction: I find the first part of the introduction (L. 35-68) quite long. We have no doubt that snow is important for many reasons. What would deserve to be developed much more is why this specific study is very relevant, especially for the target area. This only appears a bit from L. 85. Structural issues of the manuscript: L. 81-84 could belong to the data section. L. 99-101 could belong to the methods section. L. 129-137 could belong to the introduction, as it seems to be one of the justifications of the study (“we try to reconcile these opposing findings. . .”).

Rationale of the paper: the novelty of the study and the reason why this is performed does not appear very clearly in the introduction. It is quite important to make it sufficiently clear for the readers.

Discrepancies between the figures and data shown and the methods supposed to be used: beginning of section 4.1.1 presents, according to the authors, SCS and SCE dates. These values, as explained in section 3.2.2, are obtained from processing of MODIS data. However, here, snow depth data seem to be used, without any explanation of how SCS and SCE dates can be retrieved from those. Update: actually

[Printer-friendly version](#)[Discussion paper](#)

this appears in section 4.2.2 (L. 303-305)!! There are serious issues in the structure and organisation of the paper. . . I think that the comparison of these SCS and SCE dates obtained from different data and methods should be discussed and references provided, as scales issues (among others) could occur.

Potentially erroneous calculation and discrepancies between data presented and description: in Table 4, SCS, SCE and SCD evolution trends are shown. I would logically expect the trend of SCD evolution to be equal to the trend of SCE evolution minus the trend of SCS evolution. This is however never the case, please explain why. In addition, the caption states that these dates are presented for MODIS 2000-2016 data but the column about the available data starts from 1936! Fig. 5 and associated text: where do these pixels come from? Are they pixels around the stations? Are you talking about the 8 pixels corresponding to the 8 stations, times the 16 years of MODIS data? I am lost again here.

Just a thought/proposition: Maybe present the MODIS analysis first and then the comparison with snow depth?

Specific remarks:

L. 14-16: the middle part of this sentence seems a bit poor, grammatically speaking.

L. 20: please consider adding a hyphen between locally and calibrated

L. 20-21: it is not understandable how MODIS-derived data can give information about the past half century, as MODIS satellites were launched in 2000. Please consider clarifying this sentence.

L. 28: stationS instead of station?

L. 31: "hydrology" is quite a vague field. You can be more specific.

L. 69: please specify what period it relates to.

Table 1 and 2: please add a column with the number of the station as used in Fig. 1.

[Printer-friendly version](#)[Discussion paper](#)

In addition, please try to be consistent (e.g. we have “1962-2005” and “1984 – 2016”). Also, do not start a sentence with “And”.

L. 154-158: I am not sure this is really interesting. In my opinion, such checks must be done for every data collected or used.

L. 164: Aqua was launched in 2002.

L. 165-167: Could you provide a reference for that? In addition, since NDSI is used, and although it is considered equivalent to FSC (L. 171), I recommend using in the remaining of the document only the term NDSI, not a mix between NDSI and FSC.

L. 185: I think that replacing “reached” with “exceeded” would be more informative.

L. 193: I would remove “dates” (as SCD the date of a duration has no meaning) and replace “were” with “was”.

Table 3: please uniformise the format of SCE dates provided.

Table 4: why is there a blank line after the Murmansk line?

L. 246 and 248: please replace “shows” with “show”

L.248: actually Lovozero shows a decrease of the end of the snow free season (-4.4 days/decade).

L. 295: please provide some details about why there are missing data in SCE and SCS data, as is done for water bodies.

Figure 4: North direction is missing (same for similar maps). Please add the meaning of white pixels (missing data I guess).

Table 6: I would replace “Mean” with “Mean difference” in the second and sixth column.

L. 332: deviation to what?

L. 372: Do you mean Figure 7?

[Printer-friendly version](#)[Discussion paper](#)

L. 382: “this is likely due to. . .”: I would either prefer an evidence or a deletion of this assertion.

Table 7: I am very surprised to find a “-80.2” trend value in this table. And even more surprised not to see it discussed by the authors.

L. 450: there are six stations in Fig. 1, not four.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-9>, 2019.

Printer-friendly version

Discussion paper

