



EGUsphere, referee comment RC1
<https://doi.org/10.5194/egusphere-2022-590-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Review of paper

Anonymous Referee #1

Referee comment on "Observed and predicted trends in Icelandic snow conditions for the period 1930–2100" by Darri Eythorsson et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-590-RC1>, 2022

General comments:

This paper provides an interesting analysis of past and future snow conditions in Iceland. The topic is very relevant for the journal The Cryosphere and the authors provide some interesting tools and data to support their research. The study provides a novel contribution to Icelandic snow conditions by combining and comparing in-situ observations, remote sensing estimates and climate model simulations, which had not been done before. The geographical situation of Iceland in the North Atlantic with its maritime and cold climate makes the findings interesting for the scientific community. Nevertheless, in my opinion there are quite a few major concerns that the authors should address before this paper can be published in TC. I believe addressing these concerns would highly improve the quality and especially the trust in the findings. I see the potential for a very good quality and highly-relevant paper after these issues are addressed.

I give a detailed description of the major concerns in the section below. In summary, I first of all believe the authors should clearly state the novelty and relevance to the scientific community of their findings. While this is clear to me as I mention above, they need to make it clear in the introduction. While I liked that the paper is concise and to the point, I found the description of the modelling part too short, with key details and descriptions missing. The statements about their calculated trends are a point of major concern. I think the authors should be more open about what they observe in the figures. While increasing trends in snow cover frequency are observed, decreasing trends in snow cover frequency are simulated. These simulations are then the basis of the conclusions that snow cover frequency will decrease in the future. It is ok if observations and simulations disagree, I find it would be interesting to know the reasons why, rather than claiming something that might be wrong. This could generate mistrust in the findings. The authors should address the technical corrections that I state below, as well as some parts of the text that are literally repeated in the text and in the captions. Finally, it is essential that the authors provide a statement on how their underlying research data can be accessed (as per TC data policy), since at the moment it is only stated that "the data is available".

I apologise for the long review and I positively encourage the authors to address my comments. I look forward to seeing a revised version of the manuscript, which I am sure it will be better and suitable for publication.

Specific comments:

Introduction: The introduction is generally good and concise, but in my opinion it is missing information on the importance of studying changing snow conditions in Iceland and the novelty that this study brings. The first paragraph starts with a general introduction to Icelandic climate and previous studies in Iceland. I miss a closing sentence stating why focusing on changing snow conditions in Iceland is important (e.g. snow is a major component of the water balance in Iceland, the geographical position of Iceland and the North Atlantic influence...). The second paragraph introduces remote sensing, snow cover variables, and the snow modelling. Then the objective is "to analyse observed trends and predict development of snow conditions in Iceland". I can't clearly see from the text where is the knowledge gap that the authors are filling with this study. Have future snow conditions not been analysed for Iceland before? Or has this modelling approach not been used before? Is it the combination of observed and predicted trends? What is the novelty? I do see the novelty and importance of the study, I just think the authors should state that clearly.

To be consistent between sections 2.1 and 2.2, I would change the order of subsections in 2.1, so that In-situ snow observations go first (2.1.1) then remote sensing (2.1.2) and then Climate Data (2.1.3).

Line 100: What if for a specific year there are only valid observations in summer, or only at a different time of the year than other years? How would you handle that, is there a minimum threshold of valid observations, or a defined distribution over the year that the valid observations must follow?

Snow modelling: The description of the modelling part is too short. Although I understand some things are explained in the cited references, there should be a minimum model description with key processes. How are the parameters estimated? Table 1 says they come from Eythorsson et al. 2021, but this reference is not in the reference list! What are the good things of this Snow17 model? What are its limitations? What resolution is used? (Only found out that Snow17 model output has the same resolution as the forcing GCM in

line 176 in the results). Why can't the model be run at higher resolution, given that other model parameters are probably available at higher resolution? What is the simulation period? I only found out in the discussion that it was 2006-2100. This should be explained here. Why starting in 2006?

Line 114: Why use 1st of April SWE? See Nolin et al., 2021.

Lines 117-118: A bit confusing. What are the "calculated time series" and the "distributed observations"? Sen's slope calculated a slope, not a significance. The significance is estimated with the MK test. Please rephrase and make it clear.

Figure 2c: As far as I understand it, each point here is the average snow depth from all available in-situ observations over Iceland for a specific year. However, the authors selected all stations with at least 20 years of available observations in the period 1930-2021. In the case where e.g. in the 1940-1960 there were more station observations available at lower elevations compared to 2000-2020, it could be that the Iceland average was lower in certain years merely due to different distribution of the availability of measurement stations. I am confident this is not the case, but this has to be shown, otherwise the results could be completely wrong. Perhaps a plot showing the availability of ALL stations observations against elevation or against mean snow depth. This could be shown together with Figure 1.

Line 147: I don't think melt rates is the correct word here, since melt rate is the rate at which snow melts, but not the total amount of melt or the duration of the melt season. I suggest "offset the increased winter snowmelt and shortening of the snow cover duration associated with temperature rise". Also in line 220 change "melt rates" for "snowmelt".

For all figures and tables in the results section, the text in the figure captions is also written in the section text: e.g. Lines 148-150 are the exact same lines as 154-155 which is the Table 2 caption; same in lines 160-161 which are the exact same lines as 164-165. Lines 178-179 and 184-185 are also the same. This is not good practice. Caption should give a title to the figure/table and explain the details of the figure that are not self-explanatory. The text should explain what the results in the figure show (a decreasing trend, a high value for X, etc). Text should not be repeated in the caption and the text, let alone a copy-paste... Please change this for all figures and tables.

Figure 3a: The colour bar should have the white colour at 0, so that no trend is shown as white in the map. At the moment it looks like the 0 is at +3, which could give a wrong impression in the figure. Also somewhere on the paper please explain why there no observations on the glacier (is it a limitation from MODIS?).

Figure 3b: It is not clear to me what the difference is between symbols and non-symbols.

As I understand it, significant MODIS trends are shown all over Iceland, and significant IMO station trends are shown additionally as a symbol? Please provide a clearer explanation if so.

Figure 4: For the historical period, why is it shown in red colour as if it was the RCP8.5? If there is a reason, explain it. If there isn't one, then the historical period up to 2006 should be displayed with a different colour.

Trends: There is something inconsistent in the trends and that is in my opinion wrongly explained in the text. Observations of IMO stations and MODIS show increasing SCF trends for the historical period. This seems consistent within all the results and literature shown based on historical observations (except see my comment about Figure 2c above). However, Figure 4 clearly shows a decreasing trend in SCF (and SWE) for the historical period, based on simulations. However, the authors state that "the simulated estimates of average SCF shown in Fig. 4b are in line with MODIS observations over the period 2001-2021" (see line 191). This is not what I see in Fig. 4b: even though the order of magnitude of the SCF values is good (good fit between observed and simulated), the trend is opposite. Table 2 claims an increasing trend in SCD for MODIS, while the simulations in Figure 4 show a decreasing trend for that same period. This tells me there is something wrong either with the observations or the simulations, or simply with the text. The increasing vs decreasing trend problem is even more apparent for the historical period (1930-2020). IMO observations show increasing SCF over 1930-2020, while the model simulations show a decreasing trend for 1930-2020. So why is the simulation showing a decreasing trend in the historical period? And how could this impact the statements that are made about the future regarding SCF? It does not generate much trust in the future projections. I think it could be very interesting to look into the reasons behind this disagreement, without this being necessarily a bad thing for the paper or for the results. There might be an explanation and the authors should investigate it further, providing more convincing results and discussion.

Data availability: The authors state that "all data are freely available", but there is no information whatsoever on where is the data available. Please provide all details about the data and where to find them (IMO data, MODIS, NEX GDDP, etc), with links.

Technical corrections:

Check the references (e.g. in Line 22 it should be "Eythorsson et al., 2018"). Please revise all references in the text are in the reference list and vice-versa.

Line 24-25: Better to use change per decade than per century, since the period is 1980-2016.

Line 38: Remove "from"

Everywhere: Add dots in RCP scenarios, it is RCP4.5 not RCP45. Same with RCP8.5. Also, why did you choose these scenarios?

Line 40: Remove e.g. from (e.g. Nolin et al., 2021). E.g. is only needed to state "for example", but in this case it is Nolin et al. who defined SCF. Same in line 41. It is correct in line 44.

Line 43: wrong typed word "thatasc". I guess it should just be "that".

Line 116: I think it should be 2.3, not 2.2.3. Revise.

Figure 2: Please increase the size and/or the quality of the figure. It is very hard to observe it properly, even when zooming in on the pdf, let alone when printed... Use a better quality format (usually pdf format works great). Also, within the caption I don't think Fig. 2c should be written. (a) (b) (c) should be enough.

Table 2: Better % per decade, given the magnitude of the change per year.

Line 149: Something odd in the structure of the sentence. ", of p values." What does it refer to? Same in Line 154, which is actually the same sentence...

Line 156: "are" instead of "is".

Line 157: change to " SNCM = 4 (fully snow covered mountains)."

Line 160: full stop after observations.

Line 190: remove "fig" at the end.

Line 195: "were" instead of "are" at the end of the sentence.

Line 224: remove "in" and "a". So "... a decrease in snow cover and snow mass across Iceland, ..."