Referee Comment	Author Response
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	The authors express their gratitude to the referee for thorough and insightful review. We have addressed all general and technical comments and which we believe has resulted in a significantly improved article. We agree that the novelty of the study was not clearly stated in the previous version. A description of the novelty of the study has been added to the last paragraph of the introduction section as suggested by the referee. We also agree that the section describing the modelling could be expanded and we have revised and extended the entire modelling section as per the referee's suggestions. The section on the trends in observed and simulated snow conditions has been revised and clarified and a discussion on the physical basis for the observed trends has been added. The technical corrections provided by the referee have all been addressed including the revision of figure and table caption and descriptive text. The description of data availability in Section 7 has been revised and expanded Please see specific responses below.

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.	Good points.

Added to first paragraph to highlight the importance of the study:

"*Understanding of future expected changes to snow in Iceland is important for water resources management as it constitutes a significant portion of the regional hydrological cycle, especially in the interior highlands where the majority of the country's energy production occurs, in hydropower plants developed on glacial rivers.*"

Added to the third paragraph to highlight the novelty of the study.

"The novelty of this study is the analysis of an extended dataset of in-situ records of snow conditions in Iceland combined with reliable remotely sensed dataset of snow conditions in the area and the comparison of these observations with snow conditions simulated using a trusted snow model run with downscaled and bias corrected temperature and precipitation estimates from an ensemble of 21 the CMIP climate models ensemble on a freely available, cloud based, parallel computing platform." |

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).	Good point. The order of these sections has been changed.
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?	It is inherent with satellite observations that they are limited by factors such as cloud cover causing an unequal distribution of observations within years. In this study we used observations from the MODIS instruments that record observations over Iceland twice daily. We did not perform gap-filling of the dataset to prevent introducing another source of uncertainty and thus based the analysis on the observations themselves. As the aim of the study was to investigate long term trends in snow cover interannual variability within specific pixels not considered.
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 refence 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?	We agree that the description of the snow modeling was short and confusing, this section has been restructured and revised. The background literature on the Snow17 model has been expanded in the introduction.

Please note that the simulation period was 1950-2100, as is now clearly stated in the first sentence in Section 2.2.3. |
| Line 114: Why use 1st of April SWE? See Nolin et al., 2021. | We use the 1st April SWE as it has been historically favored as an effective index for streamflow forecasting (Bohr and Aguado, 2001) and is thus a comparable metric with prior studies in the field. We agree with the referee and Nolin et al. 2021 that it doesn't represent mid-winter melt events and is thus problematic for future streamflow forecasting as the probability of these events increase, which is why we also included the SCF metric, which is recommended by |

	Nolin et al. 2021 as a new snow metric for a warming world. This has been clarified in the text in section 2.2.3 which has been rewritten.
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.	Agree. This has been rephrased as: "The statistical significance of the trend in the time series of in situ observed mean annual SCF, SND was estimated using the Mann-Kendall trend test and the significance of trends in MODIS observed SCF was estimated using Sens's estimator of slope method."
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.	Good point. A plot of the number of observation stations against the mean snow depth observed has been added to 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".	Agree, thanks for this suggestion. This has been changed in the manuscript.
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.	Good point. The captions and texts describing figures has been revised for all tables and figures.

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?).	Good point. The figure has been revised to make clear that the white color is at 0. The observations do cover the glaciers and the SCF change on the glaciers is 0 as would be expected. This is now more clear with the revised figure.
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.	The referee is correct. We have revised the text to clarify that symbols represent the IMO stations
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.	Good point. The figure has been revised to show the historical period in grey.
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	Good point, we agree that the text was confusing as to discerning between the consistency of the trends and the „fit" between observed and simulated values. We agree that the trend is indeed opposing between the simulations and the observations.

The text describing Figure 4 has been rewritten and expanded to clarify this point including the addition of a paragraph on the likely reason for the opposing trends.

The authors agree that the opposing trends revealed in this study are important and deserve further investigation in future research and publications. |

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.	Section 7 has been revised and extended. A table has been added with links to all underlying data used.
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.	The references have been checked and revised.
Line 24-25: Better to use change per decade than per century, since the period is 1980-2016.	Agree, this has been changed.
Line 38: Remove "from"	Removed
Everywhere: Add dots in RCP scenarios, it is RCP4.5 not RCP45. Same with RCP8.5. Also, why did you choose these scenarios?	Correct. These dots have been added to the rcp scenarios. Reasoning for the RCP scenarios chosen has been added to section 2.2.3 as follows. *„These scenarios were chosen to represent both a „business-as-usual" scenario (RCP8.5) and a stabilization scenario (RCP4.5) where anthropogenic climate forcing are assumed to be stabilized by the end of the century."*

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.	Changed
Line 43: wrong typed word "thatasc". I guess it should just be "that".	Correct, this has been changed.
Line 116: I think it should be 2.3, not 2.2.3. Revise.	We consider modelling to be part of the processing of data and thus should be a subchapter of 2.2 Data processing. So data modelling should be 2.2.3
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.	Good point. The figure has been both resized and improved in quality.
Table 2: Better % per decade, given the magnitude of the change per year.	Agree, this has been changed.
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…	Good point. The sentence was incomplete and should read „in terms of p values" this has been changed.
Line 156: "are" instead of "is".	Changed
Line 157: change to " SNCM = 4 (fully snow covered mountains)."	Changed
Line 160: full stop after observations.	Full stop added
Line 190: remove "fig" at the end.	Removed
Line 195: "were" instead of "are" at the end of the sentence.	„Are" changed to „were"
Line 224: remove "in" and "a". So "… a decrease in snow cover and snow mass across Iceland, …"	Removed.