

Interactive comment on “Icelandic Snow Cover Characteristics derived from a gap-filled MODIS Daily Snow Cover Product” by Andri Gunnarsson et al.

Anonymous Referee #1

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General comments:

This study presents a complete characterization of snow cover characteristics in Iceland based on a remote sensing product. The authors present the methods used to obtain a gap-filled dataset of snow cover based on a Moderate resolution Imaging Spectroradiometer (Modis), as well as the validation of this product with other satellite data and in-situ observations. Although the context of the novel methods and satellite products is explained well, the study could be better placed in the context of the importance of snow cover studies under climate change. After a successful validation the dataset, the authors analyse the characteristics of snow cover extent and duration over

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the whole of Iceland. Despite the limitations of satellite products in polar latitudes due to polar darkness and clouds, a thorough monthly analysis of snow cover from year 2000 to 2018 is presented. A trend analysis for such a short period is also done. I do not see a major issue on that, since the aim of the paper is not to present a trend analysis and the conclusions are not weakened, but some parts of it require a clarification or a rephrasing of the text.

A data product of this spatio-temporal characteristics over a highly snow dependent region such as Iceland did not exist before and is therefore an advance for snow and hydrological studies. The methods could be further used in other snow dependent regions where the satellite products have coverage, and therefore publication of this article would promote scientific progress. In addition, the article falls well in the scope of the journal since such a complete snow cover product might be of interest for catchment and water cycle studies over Iceland and for operational use as e.g. in the prediction of hydropower generation based on snowmelt. The text follows a logical story and in general is well written, with all sections explained thoroughly in detail.

I suggest accepting the paper for publication after improving some minor issues of the trend analysis, as well as the presentation of some results and figures and the writing of some parts of the text, as I detail below:

Specific comments (minor issues):

Introduction:

- In the first paragraph of the introduction, more references could be used to place the reader in the context of snow cover studies based on satellite products (<https://doi.org/10.5194/tc-5-219-2011>, <https://doi.org/10.1029/2012GL053387> , <https://doi.org/10.1175/2010JCLI3644.1>) and the importance that these have because continental perspectives on snow cover changes due to climate change, based on in-situ observations, are only starting to become available and data is scarce (especially over Iceland) <https://doi.org/10.1029/2018GL079799>

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Data and Methods:

- In both sections the order of “in-situ data” “Modis data” and “Landsat-Sentinel data” should be the same, it is now different and confuses the reader. I suggest 3.1 to be “in-situ” data, 3.2 to be “Modis” and 3.3 “Sentinel”.
- The methods section is quite technical and therefore could highly benefit from a schematic Figure representing the types of data and the types of processing of the data, in the form of a “flowchart”. This would help the reader follow better the whole methods process.

Results:

- Table 1: Can you please provide an explanation or at least hypothesis on why agreement is lower when observations show snow than when they show no snow? For instance, if satellite products were to confuse a cloud with snow, that would lead to a lower agreement when observations show no snow than when they show snow. The difference is big so there should be some reason as for instance that snow is not deep enough for the satellite product to be detected?
- Figure 4: The display with a different colour for every month gives no additional information since nothing can be seen from the colours (except for a few clusters). I suggest that a correlation between landsat and modis is computed for every month and then presented in a table. This would potentially identify in which months MODIS performs best or worse and would give a more complete validation.
- Some statements about Figure 10 and 11 on trend analysis should be treated with more care. Page 15, lines 25-30: “In February and March [...] some areas where snow cover extent recedes over the period”; The statement is too strong, since looking at Figure 10 right column Feb-Mar, trends over almost the whole of Iceland are <1%, which in case of being % change over two months would represent a change of 0.6 days. This rate, considering standard deviations are generally higher than 5 (as seen

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in the middle column) is highly insignificant. A similar statement is written for Oct-Nov, with trends that are generally smaller than 1.5%, and when considering the whole year (Feb-Nov). I suggest decreasing the strength of the statements or showing the trends differently, for instance computing the trend divided by standard deviation (in days, not percentage). This would show on the map to what extent these changes are significant, and might support the significant increasing trend obtained for June when computing the snow covered fraction (Figure 11). Regarding these increasing trends for May, June and July, the significance for such a short period could be explained by the 3 abnormal years in 2013, 2014 and 2015. Although this is stated in the results, I suggest that this is mentioned in the conclusion. Moreover, the conclusion should indicate that an increase is only observed in June or spring, as it is well indicated in the abstract (Page 19, line 1): “The changes over time (trend) analysed for the 18 years showed a slight increase in average snow cover in spring, probably driven by 3 abnormally cold years in 2013, 2014 and 2015. This aligns ...”

Technical corrections/clarifications:

- p.2 L-8: What is the order of the references? It is not alphabetical and not old to new.
- P.2 L-21: Remove extra brackets in (Fig 1,2)
- P.4 L-29: I suggest changing “main objective” for “aim”, since after this sentence a first and second objective are presented.
- P.5 L-14: Join the two references.
- P.5 L-19: Please explain what tile h17v02 is and where it comes from
- P.7 L-4: Abbreviation MCDAT appears here for the first time but it is not explained, please provide the full name for it.
- P.7 L-7: What is the best observation of the day? Can there be two best? What happens then?

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- P.8 L-20: Are the numbers correct? 213.011 matches out of 585.800 is less than 50% accuracy.
- P.8 L-25: "at the bottom"
- Figure 3: Please change the colour scale to a continuous one, otherwise it is difficult to read the map.
- I suggest merging Figures 5 and 6.
- P.12 L-9: While the text indicates that December and January are not available, Figure 8 shows 11 months of data. How is that possible?
- Figure 9: Please increase figure size if possible.
- P.15 L-29: This result contrasts with other studies showing a shortening of melt season and earlier onsets <https://doi.org/10.1175/2010JCLI3644.1>

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