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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Climate change and snow cover trends in Iceland" *by* Darri Eythorsson et al.

Anonymous Referee #1

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General comments:

This study analyses past, present and future climate of Iceland. The authors present a combined analysis of remote sensing of snow cover and global climate simulations to study the impact of climate change over Iceland. Specifically, the authors intend to find recent (2000-2016) snow cover trends over Iceland as well as long term (1950-2100) trends in Koppen-Geiger climate classifications and try to link them to each other.

The proposed research questions are interesting in the literature context that is presented in the introduction. However, the data and methods used are fundamentally the same as those published in Eythorsson et al. (2019) (See section 2 of the cited paper). The authors use the same version of MODIS10A1 daily snow cover product, for the same period and same resolution. They also apply the same data processing

and calculate trends by the same method and significance test. The same applies to the Koppen-Geiger climate classification analysis from the NASA NEX (ensemble of 21 Global Circulation Models from CMIP5) dataset for historical and future climate conditions. Most parts of the text in this section are only slightly rephrased. This makes that the majority of the study presented here is refining (zooming in) the results from Eythorsson et al. (2019) for Iceland. To that respect, Figure 2 in this study shows the same results as Figure 4 in the former study but only for Iceland. The same happens to Figure 4 of the current and Figure 2 of the former study.

An element of novelty that the authors present here is the stratification of trends into elevation bands. However, elevation bands are defined based on a 20x20m resolution, while MODIS is 500x500m resolution and the GCM output 0.2x0.2ËŽ (in the order of tens of km). No method is described as to how the different resolutions are matched for the analysis of the data. This is an important issue because the elevation bands are narrowly defined. For instance the coastline (0-100m) covers a relatively small area in Figure 1 and might be represented by only a few grid points of the coarse GCM output. This makes that the stratified results by elevation band in Figure 5 might not be representative and therefore not significant depending on the method used to match resolutions.

The authors find significant increasing snow cover frequency trends over large parts of Iceland, although these trends should be treated with caution even if supported by statistics because they cover only a 17 year period and could respond to extreme events or interannual variability. Therefore, some of the statements in the discussion and conclusion should decrease their strength. Furthermore, the results are similar to those shown by Gunnarsson et al. (2019), who also used MODIS10A1 over Iceland to find increasing snow cover trends for the same period, although in this case they used sophisticated gap-filling methods. Regarding the Koppen-Geiger trends, significant trends are found in line with warming temperatures, where cold climate are replaced by warmer climates, but these are not significant during the MODIS period and are as

well contradicting increases in snow cover. For this, the authors introduce a Figure 6 in the discussion where they show increasing precipitation trends over Iceland that could explain increasing snow cover despite the warming of the climate.

I think the article provides a potential interesting analysis of the link between climate change and snow cover in Iceland, with a large data analysis of different types and significance tests for all the results. Furthermore, the discussion and conclusions are well written and provide interesting insights into effects of climate change over Iceland. However, most of these conclusions are highly similar to those in Eythorsson et al. (2019) and Gunnarsson et al. (2019). The strong overlap with these two other studies together with the major issues commented above make me suggest that the article is not suitable for publication in HESS in its current form.

Specific comments (minor/major issues):

Introduction:

- The authors present an interesting literature review context but the introduction does not have a proper structure. The first paragraph (lines 24 to 32) gives a short overview of the study and contains the research questions. This paragraph should therefore be located after the literature review (after line 67). The hypothesis should be placed after the research questions.

- I do not understand why the hypothesis is that recent warming has resulted in decreasing snow cover, given that the MODIS data used and the period of study are similar to those of Gunnarsson et al .(2019), who mostly found increases or no change in snow cover.

- No results (lines 70-74) should be included at the end of the introduction.

Methods:

- The subsection tools and datasets misses important information about the characteristics of the data and what period the data cover. The level of detail provided in section

2.1 in Eythorsson et al. (2019) would be more appropriate.

- The elevation zones are narrowly defined considering the coarser resolution of the other data in the study. Three zones or even just two (e.g. 0-500 and 500-2000) might suffice to observe changes based on elevation. The sensitivity of the chosen elevation bands could also be tested, by choosing several different bands and checking if the same results are obtained.

- Gap-filling methods provide extra sources of uncertainty and using the “raw” data is a good idea. However, the distribution of the 60 valid observations/year per pixel should be tested to check if it remains the same over the MODIS period. It could be that in different years the 60 valid observations occur during periods with less or more snow cover (e.g. some years in spring and some in autumn). This would make the interpretation of the trends more confusing and difficult.

- An explanation should be provided about how the SCF trends are sorted by elevation zones, given that the elevation zones are based on a 20x20m DEM but the SCF trends have 500x500m resolution and the GCM 0.2x0.2 degree. The SCF trends are calculated from the same exact data (MOD10A1.005 Modis Terra snow cover daily product), resolution (500m), filtering (and method (slope estimator and significance test) as in Eythorsson et al. (2019).

- The test applied in section 2.3.1 to validate trends based on known land surface changes is a good approach, but I do not see whether the 3 locations found to be impacted by land surface changes are then removed from the analyses. Also, given the clear changes observed in Figure 3, this test should be applied to every pixel in the study. If there are other pixels with these kind of changes they should be removed from the study analysis.

Results:

- As mentioned above, section 3.1 here is similar to section 3.2 in Eythorsson et al

2019. Section 3.3 ressembles section 3.1 of Eythorsson et al. 2019.

- How are the resolutions matched for the analysis in Figure 5? For instance, is one big pixel from the GCM assigned to a specific elevation based on the elevation band dominating that pixel? Or is the value of the GCM pixel assigned to all the pixels that cover the same area? In the first case some small elevation areas like coastline would barely be represented. In the second, the same trend would be counting for several different pixels. The best approach would be to use different elevation bands or use a coarser DEM resolution. The same problem applies then to Table 1 (Should be numbered Table 2).

Discussion and conclusion:

- Significantly decreasing SCF was observed at the retreating termini of all major glaciers. Does that mean that the decreasing/increasing SCF in the study is affected by changes in glacier area? This could have an impact in the results. Or is the glaciated area removed from the trends?

- The authors provide an interesting approach to attribute increasing snow cover to increasing precipitation in the discussion by showing a new figure of trends in precipitation. This should be tested for the MODIS period to see if in fact the years with higher snow cover frequency correlate with years with higher precipitation.

Technical corrections/clarifications (line by line):

- There are parts of the text in results that explain again the Methods (e.g. lines 187 to 194 are similar to lines 112-119). Also lines 106-110 are fundamentally explaining the same as 151-157.

- The title could be more original.

- Other minor and technical corrections are included in the pdf attached.

Please also note the supplement to this comment:

https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-564/hess-2019-564-RC1-supplement.pdf

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