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Interactive comment on "Darkening Swiss glacier ice?" *by* Kathrin Naegeli et al.

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We thank referee 1 for the valuable and constructive comments that were helpful to improve the manuscript.

In response to this review, we clarified the main aim of the paper and deleted the link to glacier mass balance to improve the consistency of the manuscript. Furthermore, we expanded the discussion of possible causes and dependencies of the detected albedo changes in the discussion section. Finally, several valuable references to link our study to existing research were added.

Below we respond to all comments by anonymous referee 1. The responses (normal font style) are following the *referees' comments* (displayed in italic font style) directly.

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Discussion paper

The corresponding revised sentences in the manuscript are given in quotation marks.

The authors use late-summer Landsat scenes for retrieving the seasonal minimum of glacier albedo of Swiss glaciers. Then they apply a trend analysis to bare ice albedo in order to detect positive or negative trends in the albedo series. My first concern regards the choice to study trends only on bare ice. Actually, important contribution to the radiative balance of glaciers comes also from the accumulation areas: in fact this part of a glacier plays an important role in determining its mass balance. Albedo decrease in the accumulation area is very important, and I don't understand why the authors did not include this part of the glaciers in their analysis. In the area across the ablation and accumulation zone the impact of light-absorbing impurities is important. Excluding this area from the analysis is not justified in my opinion.

We agree that the accumulation areas of glaciers are important for the radiative budget and consequently mass balance of the entire glacier as mentioned by the referee. We also see the importance of albedo changes in these snow-covered areas of a glacier, especially in connection with glacier mass balance. However, changes in ice albedo compared to snow albedo are not linked to the same processes and thus require a separate investigation. Whereas changes in snow albedo are of rather short-lasting impact, due to snow falls that occur more often and year-round, and thus reset the albedo to higher values again, ice albedo changes are usually more durable. Our main research question is, thus, not linked to the mass balance or changes in the energy budget of the entire glaciers due to changing albedo but addresses the general question of the darkening of bare glacier ice. Moreover, the strong saturation problems in the TM and ETM+ surface reflectance data for snow-covered surfaces would substantially restrict an analysis of snow-covered glacier surfaces. We clarified the motivation in the introduction section.

"Furthermore, when debating the darkening of glaciers, a clear distinction between glacier-wide versus point-based investigations is necessary to be able to clearly separate a darkening effect due to a changing ratio of snow-covered to snow-free areas of a glacier from other processes affecting the reflectivity of glacier surfaces. Moreover, a separation between albedo changes of bare ice and snow is required to correctly distinguish between differing processes and dependencies impacting snow and ice in particular ways."

For example, Gabbi et al. 2015 showed that black carbon and dust have an impact glacier mass balance. They used data from ice cores collected in the accumulation basin of two Swiss glaciers. I think that the comparison with mass balance should be done with averaged albedo over the entire glacier, and not only from bare ice. In fact, it is not straightforward that the mass balance is determined only by bare ice albedo. I'm not surprised that the authors did not find any correlation between those two variables (pg9 ln3-4).

As mentioned in our answer above, the main research question addresses the general darkening of bare ice and not albedo changes of the entire glacier. This is also already made clear in the title of the article. Thus, our study does not aim at explaining mass balance fluctuations with albedo changes. To increase the consistency of the paper, we deleted all parts of the manuscript that link albedo changes to mass balance, i.e. the statement on pg9 ln3-4 and part (b) of figure 4.

Furthermore, in the whole paper I did not find any reference to 'cryoconite' (organic and inorganic sediment found on ice). I think that a discussion about "what" could be the cause of the darkening is necessary in this work to give a broader perspective to the remote sensing analysis. Further discussion should regard also the competing

role of grain growth (due to ice melting) in potential ice darkening.

We agree of not having mentioned the term “cryoconite” in the manuscript. This is now done. Possible causes of the darkening are mentioned at several positions in the paper. We will expand on possible causes and dependencies with a respective sub-section in the discussion section, addressing important points like cryoconite, grain growth, input of debris, surface roughness, etc.

My second concern regards the choice of Swiss glaciers for the analysis, and the validation of the trends. I was a little surprised that Morteratsch glacier was not included in the analysis. As far as I know, it is the only Swiss glacier with a long series of albedo measured with an Automatic Weather Station in the time windows used in this paper (1999-2016). Oerlemans et al. (2009), was one of the first papers dealing with ice darkening in the Alps. In that paper, a decreasing trend of summer albedo was detected (from 2003 to 2006), and associated with dust deposition from lateral moraines. This series could have been a perfect validation for the methodology developed in Naegeli’s paper. I don’t understand why they excluded this glacier from their analysis.

The main aim of the study was to try to detect bare-ice albedo changes based on readily available Landsat science products. We thus decided to focus on only one Landsat scene that comprises most of the larger glaciers in the Swiss Alps. Unfortunately, Vadret da Morteratsch is thus not included. To account for the comment made, we will include some more information about the observed albedo change on Vadret da Morteratsch published in Oerlemans et al. [2009] in the introduction.

Furthermore, they also reference to "Swiss glacier ice" in the title. In my opinion, some validation is needed for the albedo series derived from Landsat. This trend analysis is strongly dependent on the availability of Landsat data during late summer. From table 2, it is evident that differences of more than one month in the dates of the images can create inconsistencies in the albedo retrieval. For examples, early snowfalls in September can generate strong overestimation of albedo.

We agree that the use of only one end-of summer scene demands recognition. In particular, the meteorological conditions prior to the acquisition of the scene are of importance. However, the meteorological conditions prior to the acquisition of the scenes used do not indicate any fresh summer snow fall events that would affect the retrieved albedo values. Moreover, our snap-shot uncertainty analysis revealed that the use of end-of summer scenes within a two-month period (August and September) indicates robust albedo results that can be used to study temporal changes. To clarify the uncertainty resulting from such environmental factors, we will extend the statements in a new sub-section “uncertainty assessment” at the end of the methods section.

Specific comments:

Title: it is correct? I think it should be something like "Are Swiss glaciers getting darker?" or "Is Swiss glacier ice getting darker?"

The suggested changes to the existing title are not changing its meaning. We thus would like to keep the title as it is.

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pg2 ln2: here a brief review of these "controversial discussions" should be reported, in particular for Greenland trends. Please, consider also a reference to Casey et al. (2017).

Thanks for reminding us of this reference. We added a clarifying sentence and the suggested reference.

"The recalibration of the MODIS sensors lead to a reduction in spatial extent and statistical strength of albedo trends over the Greenland Ice Sheet [Casey et al., 2017]."

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pg2 ln3: recently Baccolo et al. (2017) showed that also radionuclides and heavy metals are contained in cryoconite holes, and will be likely released with current and future melting.

Thanks for the mentioning of this reference. We adjusted these sentences and added the suggested reference.

"Moreover, the emergence of legacy contaminants or radionuclides and heavy metals contained in cryoconite holes at lower elevations on Alpine glaciers [Bogdal et al., 2009; Pavlova et al., 2014; Steinlin et al., 2014, 2016; Baccolo et al., 2017] (. . .)."

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pg2 ln6: I personally don't like references to Discussion papers that was not accepted for publication. I suggest to reference a successive paper by the same authors: Goelles Boggild (2017).

We changed the reference accordingly.

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pg2 ln11: here I suggest a reference to Dumont et al. (2012) that used downscaled MODIS data to estimate surface albedo of a glacier in the French Alps. Also a recent paper by Davaze et al. (2018) used a similar dataset to compare mass balance with albedo.

We added the studies that used downscaled MODIS data to infer glacier surface albedo.

"To date, most long-term studies either used point data from automatic weather stations located in the ablation area of a glacier [Oerlemans et al., 2009], coarsely spaced satellite data from the Moderate Resolution Imaging Spectroradiometer (MODIS) [e.g. Stroeve et al., 2013; Mernild et al., 2015], downscaled MODIS data [Dumont et al., 2012; Sirguey et al., 2016; Davaze et al., 2018] or other remote sensing datasets [e.g. Wang et al., 2014] to infer trends in ice albedo."

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pg4 ln1-4: here there is no description on how you used this lithological-petrographic map. Please be more specific on the aim of this analysis. I don't see the added values of this analysis. Objectives must be clearly stated.

We agree that the added value of this analysis was not clearly stated. We thus added some statements in the introduction to strengthen our motivation to include this analysis.

"We examine trends and their significance to better quantify and investigate a possible darkening of glacier ice in the western and southern Swiss Alps from the point to the regional scale. Causes and external factors that might impact bare-ice albedo and explain its spatial and temporal evolution are discussed."

Section 4: In section 4 (Results), also interpretations are found. I suggest to merge this section with the Discussion, or to move all the interpretations and discussion to Section 5.

We agree that some interpretations are already presented in the results section. However, in our opinion they are necessary to present the results to the reader in the right context. We would thus like to keep the separation of results and discussion as is.

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pg8 ln6: "for some analysis". Please, be more specific here.

We realized that this sentence is not of importance anymore and thus deleted it completely.

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pg8 ln29: replace ";" with "."

Changed.

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pg10 Fig.4: In 1999, averaged albedo is already very low (about 0.2 from Figure 4). So it is possible that the darkening trend of Swiss glacier ice already occurred. I think that this discussion should be added to the paper.

We agree that the albedo values in 1999 are already rather low, which could be due to local conditions during the Landsat overpass. Speculating about possibly higher albedo values of bare ice in earlier years (70's and 80's) is delicate as we do not have a dataset that covers this time period. However, we will add a respective

comment in the discussion section to indicate this fact and possible indications for our analysis.

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pg10 ln3: "for some grid cells", please add the percentage.

The percentage and km2 were mentioned in the parentheses at the end of the sentence. We moved the number to the beginning of the sentence.

"For some grid cells, about 15% or 2 km^2, also positive albedo trends significant at the 95% confidence level were detected however."

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pg12 ln2: Results from the lithological analysis should stay here

We moved the respective section from the discussion to the end of this paragraph (Local trend in bare-ice albedo).

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pg15 ln27: here a reference to organic material should be made. Probably the effect of the organic fraction of cryoconite may overwhealm the mineralogic signature of surrounding rocks.

To account for this comment (and others on the same line), we will rewrite the paragraph about dependencies of the discussion section. A more detailed discussion of the role of organic fraction present on a glacier surface will be added.

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pg16 ln 7: a recent paper by Rossini et al. (2018) also explored the relation between ice darkening, roughness and melting in a Swiss glacier

We added the respective reference.

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