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

Anonymous Referee #2

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Dear Editor,

The authors use 17 years of repeated end-of-summer Landsat images to investigate the change in bare-ice albedo over the ablation area of 39 large glaciers in Switzerland. In doing so, this study has the merit to address an important research question currently debated in glaciology regarding the potential lowering of the albedo of ice surfaces. This is an important topic in the context of the significant demise affecting mountain glaciers around the world. Capturing whether the spatial distribution of surface albedo of glaciers exhibits trends in the current context is important to characterize processes and longer-term effects associated with the widespread retreat.

To do so, the authors rely on near-yearly Landsat (TM/ETM+/OLI) Level-2 data corrected for atmospheric effect. An empirical formulation by Liang (2001) is used to estimate broadband albedo. This is aimed at being applied to bare-ice surfaces only,

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thus relying on an algorithm to determine the snowline elevation and segment the area over which "bare-ice" albedo is computed. The evolution of albedo is then retrieved per pixel and trends assessed via statistical testing and regression analysis.

When all glaciers are considered globally, no trend in mean albedo seem to emerge. Nonetheless, the authors find that statistical testing of trends per-pixel reveal that most of the ablation areas exhibit trends significant at the 80% level, in which case albedo change equally increases or decreases, and about 10% of the ablation area exceeding the 95% confidence level, and where a decrease in albedo is observed to prevail. The authors suggest a number of processes at work to explain this trend, namely accumulation of debris, presence of organic material, and the role of lithology in the area producing loose light-absorbing material potentially accumulating on bare ice.

In my view, and despite the merit of addressing this important question, I find the proposed manuscript requires substantial methodological improvements and significantly better support for the conclusion being drawn before this work be acceptable for publication in The Cryosphere.

General comments

My main concern is that the method claims to assess the changes of albedo on bare-ice. However, most of the ablation areas of the glaciers under consideration exhibit large medial moraines and changing debris covers on sides and terminus, capable to affect strongly the albedo signal. It comes that much of the changes found to be significant enough appear obviously related to changes in the spatial distribution of debris rather than a "darkening of bare-ice" surfaces as suggested by the authors. I am however concerned that the study gives only little acknowledgement to the fact that the target has changed in most instances, but rather insist on the fact that significant areas of bare-ice are perceived to darken. I find this insufficiently supported, if not misleading in view of the data and results provided. Removing from the analysis all areas where the significant change in albedo may be associated with the surface not being bare-ice at some point of the chronology has the potential to change substantially the message of this study, and I believe can compromise the significance and robustness of its current conclusion.

Specific comments

P1L17: Is it “to” or “at higher altitude”? I suppose the authors mean “to” but since the meaning would be different with either preposition, it is important to correct this.

P2L13: remove “necessarily”

P2L15-18: Although it is true that the use of MODIS data to retrieve surface albedo on mountain glaciers is complicated by a relatively coarse resolution, it is not “unsuitable” as the authors claim. Since Dumont et al. (2011) the use of MODIS data to measure temporal variations in glacier surface albedo has proven to be successful to inform about changes occurring on alpine glaciers, see for example:

Dumont, M., Gardelle, J., Sirguey, P., Guillot, A., Six, D., Rabatel, A. & Arnaud, Y. (2012). Linking glacier annual mass balance and glacier albedo retrieved from MODIS data. The Cryosphere 6 (6), 1527–1539. (Doi: 10.5194/tc-6-1527-2012.) Sirguey, P., Still, H., Cullen, N. J., Dumont, M., Arnaud, Y. & Conway, J. P. (2016). Reconstructing the mass balance of Brewster Glacier, New Zealand, using MODIS-derived glacier-wide albedo. The Cryosphere 10, 2465–2484. (Doi: 10.5194/tc-10-2465-2016.) Davaze, L., Rabatel, A., Arnaud, Y., Sirguey, P., Six, D., Letreguilly, A. & Dumont, M. (2018). Monitoring glacier albedo as a proxy to derive summer and annual surface mass balances from optical remote-sensing data. The Cryosphere 12, 271–286. (Doi: 10.5194/tc-12-271-2018.)

P2L28: The study focuses on glaciers which exceeds 5km2, yet Table 1 reports three glaciers smaller than that.

P2L33: I don’t think “of high accuracy” is meaningful or well used in this sentence. I suggest removing it.

P3L1: I believe there is a contradiction between the emphasize put on the fact that the study seeks to characterize changes in surface albedo of “bare ice only” (mentioned 8 times in the abstract and 6 times in the introduction) to discover now that the medial moraines have been kept in the analysis. A number of glaciers such as Aletsch exhibit relatively large medial moraines that are expected to affect strongly albedo estimates. Furthermore, over the time period of the study, it is reasonable to think that the location of the moraines may vary within or beyond the 30-m pixel resolution of the Landsat images (not mentioning the modulation associated with co-registration variability between images), thus convoluting their spectral response with that of bare ice, and potentially creating perceived changes in albedo value.

P3L5: Landsat 7 sensor should be named ETM+ throughout the text, tables and figures.

P3L5: Although it leaves little doubts that the authors are referring to the Level 2 surface reflectance product LEDAPS for Landsat 5-7 for and LaSRC for Landsat 8 OLI, I would suggest these products are named accordingly to their rightful designation for clarity. Since those surface reflectance are not correcting for topography nor shadow effects and that this can compromise the accuracy of albedo estimates, I believe the product being used deserve a more comprehensive description in relation to the context of the study. In particular, it would be important to review what is corrected for in those products, the expected accuracy and limitations of those corrections, and how suitable it is for the study at hand in the present context.

P3L5: As the authors must know and have experienced, all ETM+ data post May 2003 are affected by the SLC failure leaving significant areas of the glaciers missing observations. I must admit I am somewhat surprised that this is not mentioned once, despite a number of images being affected as illustrated in Figure 3, and despite this possibly having noticeable consequences on the computation of mean bare-ice albedo in Figure 4. To me, the inclusion of those data in the analysis would need a specific test to quantify how much the mean albedo may be affected by SLC-off data. This could be done relatively easily with a SLC-on dataset from which a SLC-off would be simulated using the mask of a SLC-off epoch.

P4L8: “All reflectance data were downloaded”

P4L10: The authors relied on their own cloud classification approach. Since cloud masks are provided as part of the LEDAPS and LaSRC products albeit having known issues, the authors could explain and discuss the reason why they favored a custom algorithm, and how this was assessed to deliver more useful images. I did download a number of the LaSRC images used in this study and left wondering why a custom cloud detection algorithm was needed here, especially given the relatively limited number of images and the visual assessment being made to select those less of not affected by clouds at all.

P4L16: the citation should be Liang (2001), not 2000, same in P19

P4L18: It would be desirable to use the same notation as Liang (2001) with \alpha_i used to represent narrowband ground reflectance of TM/ETM+ in band i.

P4L19: the symbol b_n is not the spectral band number but should be the narrowband ground reflectance of TM/ETM+ in band i. As commented above, using \alpha would make it consistent with Liang (2001) and general understanding of the quantity used.

P4L22: The narrow to broadband conversion assumes ground reflectance on horizontal surfaces. The LEDAPS and LaSRC products account for topography only in terms of the control of elevation on atmospheric effects, yet the control of topography on the modulation of irradiance on varying slope and aspects, hence on the measured satellite radiance in mountainous regions, is ignored. In other words, the surface reflectance products assume a flat surface and the relative geometry between irradiance, the target, and observation direction is not accounted for, let alone the higher orders of effects related to the topography such as terrain reflected irradiance and modulation of the observed reflectance by the BRDF of the target under the said geometry.

Although this effect may be limited on relatively flat glacier tongues and/or when considering only variations rather than absolute values of albedo through time, this and the disconnect between the variable topographic setting under study and the relevance of the empirical formulation of the albedo cannot just be ignored by the authors.

P4L22: I find the claim that the albedo products are of “very high accuracy” and “deviate by less than <0.001 on average from more sophisticated approach” not well informed, if not misleading. It suggests that albedo values retrieved by the empirical method, on the basis of atmospherically but not topographically corrected data may be 100 times more accurate than reference albedo estimated from an albedometer. I can find the sentence in Naegeli et al. (2017) claiming such “accuracy”. I however understand this is the mean difference between Liang albedo applied to synthetic APEX data compared to a rigorous albedo derived from the full spectrum. At least in this case the source of data is all APEX and thus topographically corrected. All in Naegeli et al. (2017) suggest that albedo derived from L8 can hardly meet this target even on average. If to comment on the accuracy of this formula (and provided the limitations associated with the use of non-topographically corrected data and other sources of uncertainty is justified), a more useful number would rather be the standard error of this comparison between APEX and APEXLiang, namely 0.11. This is far more realistic with what can reasonably be expected from albedo retrieval from satellite.

P4L25: the definition of such hard threshold on albedo derived from non-topographically corrected data is disputable. Again and related to my previous comment, this should be reviewed and/or critically discussed in view of variability of albedo associated with the topographic control on irradiance.

P4L9: “(0.25<\alpha<0.55)” not “(0.25>\alpha<0.55)”

P7L9: snapshot is singular

P7L10: The use of “ideal case” is value-laden. This sentence should be rephrased more objectively.

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P8L2: The authors seem to consider equally epochs one (sometimes two) year(s) apart and sometimes just a few days apart to compute trends. I don't think this is an acceptable methodology. When several images are available for the same summer on a grid cell, I think only one estimate should prevail (or an average maybe) for this year and trends only derived from a "yearly" record. This also applies to the linear regression through all available data points. Note also that it is recommended that the Mann-Kendall test be conducted with only one data point per time period (see Chapter 12 in Helsel and Hirsch, Statistical Methods in Water Resources, U.S. Geological Survey, Techniques of Water-Resources Investigations Book 4, Chapter A3).

P8L14: the areas of nodata in ETM+ are not to be referred as striping (which is usually associated with radiometric calibration) but the Side-line-correction failure that occurred in May 2003.

P8L15: the 2004 image seems to be 8/9/2004, not 9/9/2004. All dates in Figure 3 must also be double-checked for erroneous date reported in Table 2 as stressed in a later comment.

P8L16: The authors acknowledge cloud shadow exemplified by one image in the upper accumulation area. They then claim that "bare-ice area is almost always well represented and inferred albedo is realistic thus allowing monitoring through time". I find this claim particularly vague and not supported. What do the authors mean by "realistic"? In what sense would realistic "allow" monitoring? Just looking at both 1999 ETM+ images on Aletsch glacier, the 10/08 is unusable due to cloud cover on most glaciers of interest and in particular Aletsch, while the 11/09 image shows Aletsch tongue severely impacted by cloud shadow, not even to mention Fiescher glacier. Right from this first date, the quality of the image begs the question about how much consideration was given to the "realistic" retrieval of albedo. Beyond the inherent accuracy of the albedo retrieval method which I think is misrepresented here, there is also no consideration of factors such as the different radiometric quality of TM/ETM vs OLI (whiskbroom vs pushbroom, 8b vs 12b radiometric resolution) or imperfect coregistration between image, and the potential of these factors on the quality of the albedo signal.

To me the accuracy of the albedo retrieval method once all source of uncertainties are considered is the main factor that must be given consideration before seeking to make inferences on changes. So far and despite what the authors may suggest earlier, the albedo retrieval for a single pixel can hardly be proven to perform at better than 0.1 accuracy if not worst. A discussion of all environmental factors that may even degrade this further would be welcome. For example how cloud shadow as well as topographic shading whether in the upper reach or on the tongue of glaciers is handled by the methodology remains far too obscure. Yet the relative share of any of such phenomena on some dates could potentially result in variation of albedo. Furthermore, obviously the average bare-ice-albedo are computed on varying areas depending on the classification of the snow/ice limit. Naegeli et al. (2017) themselves reported on substantial variation in albedo up the tongue of Findelengletscher. This should be put into perspective with a mean albedo obtained from a variable number of “bare-ice” pixels at different years.

P10L5: In the context of the relatively large uncertainties being involved in the albedo estimates and interpretation of Table 3 and Figure 5, I find the phrasing that “trends were significant yet at low level” rather misleading. When focusing on those pixels with trends significant at 80% confidence (most of the bare ice), Table 3 reveals a symmetrical distribution (arguably Gaussian) of occurrences exhibiting either positive or equally negative trends. The trends themselves are estimated within the .05/decades or .1 overall magnitude over the approximate 20 years of the study; in other word, barely what we could hope as the uncertainty of the albedo retrieval method. To me this is rather showing that the level of detection (or signal to noise ratio) is simply not enough to be conclusive. Presenting this results and Figure 5 suggesting there are trends significant at least at 80% level everywhere is to me contrary to an alternate interpretation being that this level of detection is not suitable to evidence any obvious trend at all. I think it would be fairer that the interpretation of this result and its significance stress the limitations of the methods rather than suggesting that there are indeed trends, potentially driven by some physical cause. At this stage, my interpretation of the results given the proposed methodology is simply that it can only be inconclusive.

Turning now to the small portions of pixels exhibiting high confidence (95%) of a trend. In this case, the negative trend apparently prevails. Simply looking at the spatial patterns of occurrences of those pixels in Figure 5 and 6 exemplifies what could be expected of the redistribution and spatial variation of debris and medial moraine on most glaciers. A clear example of this is Gorner glacier. Comparing the 11/9/1999 image to recent 25/9/2013 or 30/8/2015 immediately reveals that all those areas of "highly" significant darkening actually don't qualify as "bare-ice" but rather occur mostly due to spatial variation in the distribution of debris. Visually, there is an obvious widening of the medial moraine on the main trunk, retreat of the glacier front and that of tributaries that are exposing rocks, and obvious down wasting with lateral moraines material falling on the glacier. The fact this drives some pixels to appear as exhibiting a strong decrease in albedo is in fact not so much reliant on the analysis presented in the paper to be revealed. More importantly, I am concerned that the paper may suggest a darkening of "bare ice" as if this was a subtle trend associated with increasing concentration of LAI on glaciers when the areas where the changes occur appear mostly to be those exhibiting a step-change in surface type altogether. In view of this, the way the authors elaborate in P10L10 on probable causes associated with such a progressive darkening such as the growth of algae and bacteria is to me far too speculative at this stage, and finally not supported by any new data in the present work.

P10L12: Further to my comments above, I find that the authors far underestimate or seem to lower the role of the debris and changes in the distribution of moraine material in their observations of albedo changes. Based on my visual interpretation of the images used by the authors, it is obvious that there is more than a mere "possibility" than the areas of significant changes are associated to increase in debris cover and changes in medial moraines. I find the authors suggesting that this may only affect

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“certain grid cells” not supported by observations.

P12L5: The authors claim that assessing the uncertainties associated with the albedo estimate is beyond the scope of this work. I don’t think this is acceptable and as stressed several times in my comments, I believe this and would require a far more thorough consideration of the uncertainties than presently offered, for any inferences being made about processes at work to be deemed robust.

P12L8: What do the authors mean by “better result”. This is unspecific, value-laden, and should be unsupported by a stronger argument.

P12L9: As demonstrated above, this study does NOT “focus on bare-ice areas” only. Furthermore, the data quality issue (e.g., SLC off, cloud shadows, saturation) can severely affect that albedo retrieval.

P12L13: The authors claim that “manual checks” revealed low frequency of misclassified pixels compromising the albedo retrieval. My own check alone on the first two dates revealed immediately that the biggest glacier in the study (Aletsch) is severely affected by clouds and cloud shadow to the extent that I don’t believe a realistic estimate of albedo could have been obtained across many parts of the glacier tongue. In view of this, one cannot be satisfied by the unsupported claim of the authors.

P13L1: While it is true that using 2016 outlines of glaciers in the context of the current glacier demise would have reduced the dominance of ground becoming exposed dominating the change in albedo at the terminus and lateral moraines, Figure 5 and 6 still reveal that the significant changes in albedo remain associated to areas of probably thick debris deposition. It begs the question about what would have been the signal and conclusions of this work if the analysis had focus on a (conservative) mask of bare-ice, meaning a mask where pixels throughout the study period can be observed as free of thick debris. I think this is the main methodological issue that the authors should address to revise this work.

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P13L5: what does “most scenes” means in this context? This sentence is ambiguous and should be clarified.

P13L8: This and later paragraphs relate to a specific methodology and analysis that should be introduced earlier. The steps taken to assess uncertainty should be integral of the research design and reporting of results.

P13L11: The authors stated in the previous paragraph that the uncertainty analysis was performed on Findelen glacier, but now indicate that the 39 glaciers were considered. Please clarify.

P14L1: The repeatability of mean albedo determination on (supposedly) bare-ice areas of Findelen glacier is reported in Table 4, leading to an assessment of uncertainty claimed by the authors on the albedo (pixel-wise) being 0.026. It should be clearly indicated that this reports on the precision (repeatability) of the albedo retrieval only, not its accuracy. I also note that 0.026 is obtained by simply averaging the four estimates corresponding to each year. I am not convinced this simple averaging is providing a fair assessment of the repeatability that can be obtained from this approach. As highlighted in my comment on Table 4, I have concerns about the very small value reported for 2016 and it would contribute to lower the perceived precision. I could not find images in table 4 for 2014 so it leave only two other instances in 2013 and 2015 together suggesting quite a substantially larger precision than 0.026.

P14L1: The fact that the assessment of trends does not appear to change in view of the perturbations is not truly surprising. As discussed above, I don’t think the methodology and level of detection is conclusive enough to elaborate on areas exhibiting trends at the 80% CI in the context of the current methodology. It would be more informative to test how this level of significance changes when increasing the perturbation on albedo to more realistic uncertainty levels given all other environmental factors. The area exhibiting most change, generally a strong albedo decline are apparently greatly controlled by a redistribution of debris and moraines, hence does not qualify as bare-ice throughout the study period. It is expected that those areas remain confidently detected as areas of strong change in albedo. In conclusion, I cannot agree with the author's statement that the "inferred trends in local bare-ice albedo are considered to be robust despite the uncertainty in the albedo retrieval".

P14L11: the expression "snap-shot uncertainty" is vague and unspecific. I recommend that this unfamiliar and uncommon wording is revisited to express more plainly what uncertainty the authors are referring to.

P15L17-24: This far-reaching interpretation is not supported by any tangible results presented in present study. Presented as it is now and provided some of the weaknesses of the methodology, this equates more to a relatively general hypothesis rather than one directly informed and supported by the data and results presented here.

P17L9: I believe the authors mean "snap-shot" and not "snap-short" yet I maintain that the use of this "new" terminology is not specific enough to make it informative of a clear source of uncertainty, and hence would advise against the use of it.

P17L11: The authors insist in their conclusion that "meteorological conditions preceding the acquisition (. . .) need to be considered". I however saw no such consideration in the manuscript despite most images exhibiting various stages of snowline retreat, some of them with obvious short-lived snowfall.

P17L12: I am actually quite concerned that one of the main conclusion point is that "highly significant darkening" affect about 10% of the ablation areas. Based on my own interpretation of many of the images used, much of this darkening can be attributed to change in surface type from bare-ice to debris, rather than a "darkening" of bare-ice via the accumulation of LAI for example. To me the point raised by this study rather stresses the potential accumulation of debris on the lower reaches of mountain glaciers in the context of retreat.

Table 2: Landsat 7 sensor should be named ETM+

Table 2: Although it is understandable that rounding may bring variations in numbers at the decimal level, it would be preferable that in 2008 when 0 km2 cloud is reported, this correspond to 0% as well. Table 2: Some of the information reported in the table 2 appears to be incorrect. I could not find images for quite a few dates: I suggest 8/9/2004 (not 9/9/2004); 22/9/2006 (not 20/09/2006); 27/08/2014 (not 26/08/2014); 12/09/2014 or 28/09/2014 (not 1/9/2014 and 27/09/2014). The sensor in 9/9/2013 appears to be L8 and not L7. This also affects Table 4.

Table 2: I am perplex about the reporting of clouds in 25/9/2013 and the reason for reliance on a secondary date 9/9/2013. I downloaded both dates and it appears that clouds in 25/9/2013 indeed marginally affect the east of area (d), however barely above the terminus of Bachi and Minsti glaciers, both outside the scope of this study. I could not see anywhere else where clouds may have caused an issue and the need to rely on an alternate date. This begs the question about the performance of the cloud classification algorithm used by the authors. It is even more confusing since the 9/9/2013 image (L8 and not L7 as reported by the authors) is obviously far worse with many clouds and the fact that the authors specifically mention this image in P4L13. Looking at both dates, it is obvious that the snowline retreated over the period, thus exposing more of the bare ice, at least for those glaciers not obscured by clouds and as reported in Figure 3. It suggests the use of two dates in this instance may be done to retrieve albedo over larger areas of bare ice, at least on some glaciers, but the reader can only wonder. Even in this case, it would beg the question about the consistency of the data being used and the potential effect on mean albedo. Table 4 shows that several scenes are used for assessing uncertainty, but also shows an inconsistency with table 2 as multiple acquisitions in 2013 and 2014 are consistent, while those in 2015 and 2016 are not reported in Table 2.

Figure 2: “snowline altitude” not “snowline altitdue”

Figure 2: what are SLAconst and rcrit?

Table 4: I could not find any image on 1/09/2014 nor 27/09/2014.

Table 4: In 2016, the number of pixels used for the assessment is 5495, thus representing ~5km2 of supposedly bare-ice surface. I obtained both images and could only map ~3km2 of ice at the most, the rest being mostly the accumulation area still obviously covered by snow. Note also that the lower part of the glacier tongue is severely affected by cloud shadow on 1/9/2016. Beside an issue of size being considered as bare-ice which I can't reconciliate with what the images depicts and shedding doubt on the performance of the snow/ice classification, the albedo variability appears surprisingly small (0.008) when any other years yield about five times larger albedo precision. I believe some clarification is required here.

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