The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-92-AC2, 2020

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "Small scale spatial variability of bare-ice albedo at Jamtalferner, Austria" *by* Lea Hartl et al.

Lea Hartl et al.

lea.hartl@oeaw.ac.at

Received and published: 20 August 2020

We have copied the reviewer comments into this document and respond to them individually below. Author responses are below the respective reviewer comments and separated by —- throughout the document.

Reviewer comment:

In this paper, the authors present a comparison between spectral reflectance measurements of bare ice carried out in the ablation zone of the Jamtalferner glacier, Austria with concurrent Sentinel-2 and Landsat-8 acquisitions. In a first step, the spatial variability of the manually acquired surface albedo across the ablation zone of the glacier is presented, highlighting large differences in reflective properties from dry clean ice

Printer-friendly version

Discussion paper

to surfaces covered in mineral and organic debris. Secondly, the paper focusses on comparing the field measurements with atmospherically-corrected satellite reflectance products to investigate whether physical processes related to deglaciation are fully captured by optical Earth Observation sensors. Results show that the differences observed between the ground-based and satellite measurements are not uniform depending on the wavelength, the sensor or surface type. The authors conclude by suggesting that further in-situ monitoring efforts are needed to be able to use satellite-derived reflectance for glacier change monitoring.

General assessment

The comparison of in-situ surface reflectance measurements with satellite-derived products is of great interest for anyone involved in space-borne observations of glaciers and more generally glacier surface processes monitoring, and in that sense, the work here is timely and most welcome. I particularly commend the use of openly accessible world-wide available satellite data rather than higher-resolution commercial data, making the applications available to a wider audience. The article is overall well written, apart from a couple of minor approximations (see detailed comments). However, the manuscript presents two major shortcomings that leave the reader missing significant information (see General comments paragraph below).

In summary, this article would have merit for publication in The Cryosphere if the major points referred to below are addressed. Currently, the Methods and Discussion sections are insufficient.

—-

Author response:

We thank the reviewer for their time and the detailed and constructive commentary. The points of criticism are valid and we will address them (if given the opportunity) in a revised version of the manuscript, following the suggestions by both reviewers.

Printer-friendly version

Discussion paper

To remedy the main shortcomings of the Methods and Discussion sections as specified in this review, we propose to:

1) significantly expand the Methods section, particularly where it concerns the measurement protocol for the in situ data collection and the atmospheric conditions during data acquisition.

2) restructure and rewrite the discussion section to address the specific issues pointed out by the reviewer in the comments below. We accept that the current version is too vague and contains some parts that are better suited for the Introduction, while lacking detail in other areas. Both reviewers have made comments on specific issues that need to be expanded upon in the discussion and we agree that doing so will improve the manuscript.

We are happy to follow all suggestions made by the reviewer within the constraints of the data available to us.

—-

General comments

The first deficiency mentioned in the paragraph above concerns the presentation of the Methods. The ground measurements of spectral reflectance presented in Section 2.2 (7 lines) are largely insufficient for a piece of work dedicated to comparing ground measurements to satellite products. Indeed, the section barely skims over the way measurements were collected and crucial information is lacking to clearly understand the comparisons made.

—-

See response to specific comments below.

—-

1. When were the measurements collected? No date or time of measurements is

Printer-friendly version

Discussion paper

provided in the section describing ground measurements. The reader has to wait until Section 2.3 to understand that the measurements were acquired on 4th September 2019. Over what time period (start and end of acquisitions) was the data acquired? This is of significant importance for the comparison of the data, e.g. did the surface have time to change between the satellite overpass and the ground measurements?

—-

Ground measurements were taken on 4th September 2019, between approximately 10 am and 3 pm local time. The Sentinel overpass occurred at 10:20 GMT on Sept. 4. The Landsat overpass occurred at 10:10 GMT on Sept. 3. We will specify exact times for the first and last measured profiles in the revised manuscript and begin the section describing the ground measurements by stating the date and time period of the data collection. We propose adding a brief overview of how the surface may have changed over the course of these two days in this section, with more detailed considerations on the significance of possible changes for our analysis in the discussion section.

—-

2. There is no description of the environmental conditions during the acquisition, e.g cloud cover. Even a small amount of cloud cover, such as the presence of rapidly changing cirrus can introduce uncertainties of several percent in the measured reflectance.

—-

We will add a description of the environmental conditions during data acquisition in the methods section and will add commentary on possible uncertainties introduced by changing atmospheric conditions during the time period of the ground measurements in the discussion. The study site is free of cloud cover in both satellite images. Description of conditions during the ground measurements will be based on notes made by the field team and data from a nearby weather station.

Printer-friendly version

Discussion paper

—-

3. The method for measuring the distance between the points on the profile is indicated, but how were the measurements geo-located in the field? Were there any GPS points acquired (especially as the authors refer to “GPS profile” in figure 1), with what uncertainty? The uncertainty in the positioning of the ground spectra may impact your point-to-pixel comparisons (to be addressed in the Discussion also).

—-

GPS points were taken at the start and end point of each profile line, using a standard handheld GPS device. The horizontal accuracy for these devices is typically in the range of 3-5m. We will specify this further in the revised manuscript (considerations on terrain dependent accuracy and exact GPS model). We propose to add a quantitative estimation of the uncertainty in the point-to-pixel comparisons due to the GPS accuracy in the discussion.

—-

4. The measurement protocol is not described sufficiently, leaving the reader with a number of interrogations: how were the measurements carried out: was the ASD fibre optic handheld or placed on a device to reduce operator interference (Fig 3 in Wright et al. 2014, Kimes et al. 1983)? Did the authors use an optical lens on the fibre optic (if so, what field-of-view)? What height was the collector from the surface / spectral panel when performing the measurements? A description of how the measurements were performed is desired, or at the least, if the authors were following an existing protocol, a reference to the article is expected.

—-

The fibre optic was handheld and used without an optical lens, at a distance of 30cm above the ground. We based our usage of the ASD device on the descriptions in Naegeli et al. (2017) and Di Mauro et al. (2017), who carried out comparable mea-

surements on glacier surfaces. We will reference these studies in this section and add specifics on where our protocol differs from theirs in the revised manuscript.

—-

5. The description of the processing of the raw ASD is missing. There are numerous steps to be carried out during the processing of data, including the application of instrument or spectral calibration files. In the current state, the description of the processing is too vague.

—-

We will add a step by step description of the data processing in the revised manuscript. We used a feature of our instrument that saves the white reference measurement to the RAM of the instrument. When this option is enabled, subsequent reflectance measurements are calculated with respect to the reference and the result of this calculation is saved to the output data file, such that there is no separate file for the reference.

—-

6. The authors are not clear about the physical quantities measured. The title reads "Small scale variability of bare-ice albedo at Jamtalferner, Austria", and the author summarise the body of work on broadband and spectral albedo. However, in the methods, the field acquisitions are referred to as spectral reflectance and the (limited) description of the measurement protocol leads the author to believe that the authors are recording hemispherical–conical reflectance. The ground measurements are then compared to surface reflectance products derived from Sentinel-2 and Landsat-8. Particular care should be observed when describing remotely sensed quantities and I recommend that the authors verify inconsistencies throughout the paper. Very useful references in that sense are Schaepman-Strub et al., 2004, 2006 (besides an important corpus on the subject).

—-

The reviewer's assessment here is correct. We will make sure to remove these inconsistencies and clearly define the measured quantities. Thank you for pointing out the publications by Schaepman-Strub et al., these are indeed very helpful.

—-

The second shortfall mentioned in the overall remarks concerns the Discussion, that does not do justice to the paper. Indeed, in its current state, the section repeats the introduction and doesn't address the rich results obtained by the authors. The key points presented in the results are barely brushed past and the discussion on the limitations of the methods employed and possible explanations for the results obtained are missing. The paragraph starting P8, L247 would deserve (consequential) expanding in regard to the results obtained. By restructuring the Discussion section, significant value could be brought to this otherwise valuable contribution to the observation of glacier ablation zones based on optical Remote Sensing.

—-

We accept the reviewer's criticisms of the discussion and will follow suggestions on how to improve it as stated previously. This will include a significantly expanded discussion of the content of the paragraph specified above.

Specific comments

- P1, L14: in the Optical Remote Sensing community, ground reflectance is commonly referred to as Bottom-Of-Atmosphere (BOA) reflectance. I am not suggesting to replace the term, but maybe add a mention to BOA.

—-

We will add a note on this in the revised manuscript.

—-

- P1, L27: "The magnitude and [. . .] local production rates." > Although you go into

Printer-friendly version

Discussion paper

further details later in the introduction, citations are missing here.

—-

We will rewrite/restructure this so that the citations are in the right place and more concisely match the information in the text.

—-

- P4, L106: Figure 2 and 3 seem irrelevant in the context of this paper that focusses on the comparison of ground and satellite acquisitions of reflectance and not the evolution of the surface properties over time. I suggest their removal, as they cloud the overall message. Rather, the satellite images (used in the study), of the glacier tongue with the profiles overlaid would be a nice addition to the paper.

—-

We are happy to follow these suggestions and replace the current figures 2 and 3 with new figures showing the satellite images and profiles.

—-

- Section 2.3: Table 3 would benefit being completed with additional information on the Sentinel-2 and Landsat-8 acquisitions, such as acquisition time or the angular information (solar and viewing angles). A column with the corresponding ground measurement information would be a plus.

—-

We will add additional columns as suggested.

—-

- P5, L126: The acquisition time of Sentinel-2 is not specified: yet this information is important to investigate the differences between the measurements from both sensors.

—-

We will specify this in the text and add it to table 3 as suggested in the previous comment.

—-

- P5, L139: Did the authors consider integrating the spectral measurements using the (available at least for Sentinel-2) spectral response of each band? Do the authors think that the difference with the average would be negligible or not?

—-

We expect that the differences would vary, potentially strongly, depending on the band and will assess whether it is feasible to include this as an additional part of the analysis.

—-

- P6, L175: This is an interesting find. Have the authors considered the difference in viewing/solar geometries between the two acquisitions? The strong anisotropy of the ice could partly explain the differences (see the previous comment). Basic simulations of ice reflectance (using e.g. Malinka et al. 2016) could help investigate this point. To be clear, this is not expected from the authors, but a point that could be worth thinking about for future studies. Another factor that could influence the differences observed could be the different atmospheric corrections schemes used (a reference in the Discussion would be of value).

—-

We agree that the differences in the geometries/anisotropy are important considerations in this context and should be discussed in greater detail in relation to our results. We will look into the work of Malinka et al. in this context but agree that the complexities of simulating reflectance are such that they are probably better addressed separately in future work. We will also make sure to add some comments on the issue of atmospheric correction schemes in the discussion.

Printer-friendly version

Discussion paper

—-

- P6, L183: This suggests that for surfaces with strong sub-pixel variability the resolution of the images is essential for an accurate description of the surface. The representativeness of field sampling when comparing in situ measurements to satellite images is of particular interest in the snow and ice community. Did the authors consider investigating the sensitivity to resolution by degrading the 10m bands to 30 then 60 meters?

—-

We think this would be an interesting addition to the analysis and will included a sensitivity analysis in the revised manuscript.

—-

- P7, L200: Very interesting find, which links to the question of the representativeness of the in-situ sampling. It would be nice to see this point further discussed in the Discussion section.

—-

We agree that representativeness of the in situ sampling is an important issue and will discuss this in more detail in the revised manuscript.

—-

- P7, L206: Again, this key result deserves some discussion.

—-

Will make sure to give this appropriate room in the discussion.

—-

- P8, L222-226: the observation is repeated from the introduction.

—-

Will remove/rephrase.

—-

- P8, L228: This paragraph should be placed in the context of the results of this study and is overall too vague.

—-

We will rephrase this to contextualize it better with our results and specify our thoughts on the importance of choosing appropriate spatial and temporal resolution for measurements depending on the processes to be studied in a more detailed manner.

—-

- P8, L234: Again, the paragraph reads like an introduction and doesn't have a place in the discussion.

—-

Noted & agreed.

—-

- P8, L244: Some lines of reflection in the context of the authors' study, such as discussing the anisotropy of ice in line with the differences in overpass geometries would be most welcome here.

—-

We will add more context and discussion of the effects of anisotropy, overpass geometries, and atmospheric conditions as they relate specifically to our study.

—-

- Figure 4: is the highlighting of the maximum and minimum spectra necessary? A

single emphasised black spectrum of the mean and the others in light grey could be clearer (if the authors agree).

—-

Will reconsider the coloring choices and assess how to make the figure clearer.

—-

- Figure 6: in the printed manuscript, the tape measure is unreadable in the photos. Adding a small simple scale bar int the pictures would help grasp the scale of the images. This is an interesting figure showing the important variability of reflectance across the glacier.

—-

We will edit the pictures in the figure to add scale bars.

—-

- Figure 7: the caption is unclear and the reader has to read Section 3.2 several times to understand the figure. The term “ground measurements” for satellite images (P20, L419) is confusing. I would suggest revising the caption to clearly state what the blue and orange bars represent.

—-

We will rephrase the figure caption to improve clarity.

—-

- Table 1: why are the PROMICE network measurements not referenced (Fausto and van As 2019)? They have been used for satellite calibration also.

—-

This was an unfortunate oversight on our side. We will add a reference to PROMICE

Printer-friendly version

Discussion paper

to the revised table.

—-

Technical corrections

- P1, L12: exits > exist. // - P1, L16: at dark spectra > for dark spectra - // P1, L 25: “so that darker bare ice is exposed” > I suggest specifying "in Summer” to be more precise. // - P2, L33: ”gap of knowledge” > “knowledge gap”

—-

Will change as suggested.

—-

- P2, L39: “comparatively high resolution” > Comparatively to what? Please be more specific. Sentinel-2 and Landsat-8 could be referred to as “medium resolution sensors”.

—-

We suggest changing:

“2) Compare commonly used, comparatively high resolution satellite-derived reflectance products with in situ measurements, highlighting areas in which further study is required if ongoing processes related to deglaciation are to be fully captured by satellite data.”

To:

“2) Compare reflectance products derived from Landsat-8 and Sentinel-2 data with in situ measurements, highlighting areas in which further study is required if ongoing processes related to deglaciation are to be fully captured by satellite data.”

The comparative statement was meant mainly in reference to the resolution of MODIS, but this is probably better expressed elsewhere in the text in a more specific manner, which we will do in the revised manuscript.

—-

- P2, L59: “in relatively recent times” > Please be more specific.

—-

Propose to replace with “...throughout the last decade”.

—-

- P3, L86: “different kinds of remote sensing” > this phrasing is a little vague, could you clarify?

—-

Suggest changing:

“....albedo products derived from different kinds of remote sensing data...”

To:

“...albedo products derived from airborne imaging spectroscopy and Landsat and Sentinel data...”

—-

- P4, L122: “specdal” > “spectral”

—-

It should be “SpecDal” and refers to the python package we used to process the data. We will rephrase the sentence to make this clearer and cite the documentation. https://specdal.readthedocs.io/en/latest/index.html

—-

- Figure 9: please specify the wavelength of band 3.

—-

Printer-friendly version

Discussion paper

Will specify the wavelength in the figure caption.

—-

- Table 2: is lacking the first column header

—-

Will add missing column header.

—-

- Table 1, 2 and 3: I am guessing that the authors will format the tables correctly in the next iteration? They are currently unpleasant to read.

—-

Yes, we will properly format the tables.

References Reviewer

Fausto, R.S. and van As, D., (2019). Programme for monitoring of the Greenland ice sheet (PROMICE): Automatic weather station data. Version: v03, Dataset published via Geological Survey of Denmark and Greenland. DOI: https://doi.org/10.22008/promice/data/aws

Kimes, D. S., J. A. Kirchner, and W. Wayne Newcomb. "Spectral radiance errors in remote sensing ground studies due to nearby objects." Applied optics 22.1 (1983): 8-10. Malinka, Aleksey, et al. "Reflective properties of white sea ice and snow." Cryosphere 10.6 (2016).

Schaepman-Strub, G., et al. "About the importance of the definition of reflectance quantities-results of case studies." Proceedings of the XXth ISPRS Congress. 2004.

Schaepman-Strub, Gabriela, et al. "Reflectance quantities in optical remote sensingâAËŸTDefinitions and case studies." Remote sensing of environment 103.1 (2006): 27-42.

Wright, Patrick, et al. "Comparing MODIS daily snow albedo to spectral albedo field measurements in Central Greenland." Remote Sensing of Environment 140 (2014): 118-129.

Author references:

Naegeli, K., Damm, A., Huss, M., Wulf, H., Schaepman, M., & Hoelzle, M.: Cross-Comparison of albedo products for glacier surfaces derived from airborne and satellite (Sentinel-2 and Landsat 8) optical data, Remote Sensing, 9(2), 110, 2017. Naegeli, K., Huss, M., & Hoelzle, M.: Change detection of bare-ice albedo in the Swiss Alps, The Cryosphere, 13(1), 397- 412, 2019.

Di Mauro, B., Baccolo, G., Garzonio, R., Giardino, C., Massabò, D., Piazzalunga, A., Rossini, M., and Colombo, R.: Impact of impurities and cryoconite on the optical properties of the Morteratsch Glacier (Swiss Alps), The Cryosphere, 11(6), 2393, 2017.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-92, 2020.

Printer-friendly version

Discussion paper