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

Anonymous Referee #2

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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 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 suggest-

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ing 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.

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.

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

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.

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).

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.

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.

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

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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 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.

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.

- P1, L27: "The magnitude and [. . .] local production rates." > Although you go into further details later in the introduction, citations are missing here.

- 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.

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- 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.

- 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.

- 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?

- 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).

- 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?

- 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.

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

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

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

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

- 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.

- 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).

- 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.

- 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.

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

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”

- 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”.

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

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

- P4, L122: “specdal” > “spectral”

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

- Table 2: is lacking the first 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.

References

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

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