RC1: Anonymous Referee #1

The manuscript from Bohn et al. deals with a relevant problem of imaging spectroscopy of snow and ice: topography. The authors developed a new methodology to correct imaging spectroscopy data acquired from the EMIT satellite mission for the effect of topography and they conclude that high error in LAPs-induced radiative forcing estimates are possible if the topographic effect is neglected. The study is based on two EMIT scenes in Chile and Argentina without any field validation of the retrieval. This issue partly weakens the outcomes of this study, and I suggest to refine the conclusions in accordance. Several hyperspectral satellite mission will be launched in the future (e.g. SBG, CHIME) and new data will be available for retrieval of surface parameters of snow and ice. The results of this manuscript raise important questions regarding the uncorrected topographic effect in the context of parameter retrieval. I think that the manuscript is interesting both for the cryospheric and remote sensing community, and it can be accepted only after minor comments listed below are taken into account.

We thank the referee for the positive feedback and the constructive review. We updated our conclusion by adding the following statements:

"A validation with field measurements is still missing for the presented approach, but comparisons to Landsat 9 and Sentinel-2 reflectance values as well as to estimates of radiative forcing of previous studies indicate an accurate performance of the retrieval framework."

And:

"Future work must include a thorough validation effort, and …"

Below, we address the line-by-line comments by providing respective responses and by indicating the changes to the manuscript.

lines 69-70: What are the expected signal-to-noise ratio for those two missions? Please provide some numbers.

The expected signal-to-noise performance for both SBG and CHIME is ≥ 400 in the VNIR and ≥ 250 in the SWIR. We added these numbers:

"Future orbital imaging spectroscopy missions, such as NASA's Surface Biology and Geology (SBG) (National Academies of Sciences, Engineering, and Medicine, 2018) and ESA's Copernicus Hyperspectral Imaging Mission for the Environment (CHIME) (Rast et al., 2019) will address this problem by providing high signal-to-noise ratios (SNR) of more than 400 in the visible-to-near-infrared (VNIR) and more than 250 in the shortwave-infrared (SWIR), as well as high spectral and spatial resolution."

In the introduction, I suggest to add a brief discussion on the attempts that have been made to model snow albedo in complex topography (e.g. Picard et al. 2020). In fact, those studies already show the "blue hook" that is described later in your manuscript.

Thanks for this suggestion! We agree and extended the discussion about modeling snow albedo in complex terrain by adding references to Picard et al. (2020) and Donahue et al. (2023):

“Specifically, Picard et al. (2020) demonstrated the sensitivity of snow albedo measurements to surface slope based on spectral data taken in the field, and proposed a correction approach to retrieve the intrinsic albedo. Using a digital elevation model (DEM), this local geometry, including surface slope and aspect, can be calculated and incorporated in atmospheric modeling schemes in order to correct for spectral distortions in the retrieved surface reflectance (Richter and Schläpfer, 2017). However, given the complex terrain of mountainous regions, and the current unavailability of coincident radar/lidar and imaging spectroscopy data in orbit, reliance on fixed DEMs may introduce additional retrieval errors, not only due to variability in local snow depth, but also because of uncertainties in the DEM product itself (Dozier et al., 2022). For instance, Donahue et al. (2023) showed that topographic correction with coarse and non-coincident DEMs introduces significant errors in estimated snow albedo from air- or spaceborne imaging spectroscopy of up to 20%. Overall, a mature and comprehensive modeling of topography for spaceborne imaging spectroscopy data over mountain snow has not yet been demonstrated, and only a few studies have applied a limited post-hoc correction at the airborne scale (Painter et al., 2013a; Seidel et al., 2016).”

Figure 1: I suggest to add the grain size value (100 um and 1000 um) also in the plot. HDRF should be also displayed in the label, in order to be consistent with the main text.

We updated Fig. 1 accordingly.

line 154: the spatial, spectral and temporal resolutions of EMIT data should be provided here. Furthermore, I suggest to add a scale bare to Figure 3.

We added information about spatial, spectral, and temporal resolution of EMIT images to the introduction:

“Extending over a wavelength range of 380-2500 nm with a spectral resolution of approximately 7.5 nm, EMIT images provide a pixel size of around 60 m on a 74 km wide swath. The temporal revisit time is variable depending on the orbital cycle of the ISS, and ranges between one day and more than a week (Thompson et al., 2024).”

We also added a scale bar to Fig. 3.

line 256: this info should be provided in the methods. Which bands have been used to calculate ndsi? You used Ndsi<0.0: this is strange, please verify which threshold that you applied to identify snow/ice areas.

We agree and moved the description of our snow mask to the methods in Sect. 3.1. In addition, we updated our approach of identifying snow-covered pixels by now following the procedure of Dozier (1989). We now use top-of-atmosphere (TOA) reflectance ρ_{TOA} of EMIT bands at 485 nm (blue), 567 nm (green), and 1648 nm (SWIR), and marked a pixel as snow-covered when $\rho_{\mathrm{TOA},485} > 0.16$, $\rho_{\mathrm{TOA},1648} < 0.25$, and NDSI > 0.4. The latter is calculated using the bands at $\rho_{\mathrm{TOA},567}$ and $\rho_{\mathrm{TOA},1648}$. We added this information to the text:

“It has to be noted that our approach is only applicable to snow- and ice-covered pixels. We identified these areas by following the procedure of Dozier (1989). We utilize TOA reflectance rho_TOA of EMIT bands at 485 nm (blue), 567 nm (green), and 1648 nm (SWIR), and considered a pixel as snow-covered when rho_TOA,485 > 0.16, rho_TOA,1648 < 0.25, and

NDSI > 0.4. The NDSI is the normalized-difference snow index calculated using rho_TOA,567 and rho_TOA,1648 (Dozier, 1989).”

Also, we updated Fig. 6 accordingly and added the following:

“We currently apply an NDSI threshold of 0.4 to determine snow-covered pixels.”

line 265: Geophysically? I never read/heard this term..

We agree that this term sounds strange and removed it:

“Topography can influence the size of snow grains.”

line 266-268: this is true only during a period of time. When air temperature is low, this may not hold true.

Yes, that is a good point. We added the word ‘can’ to express that it is a potential process but not always the case:

“Air temperatures can decrease with increasing elevation, which abates melt processes, causing drier snow with smaller grain size.”

line 282-284: how you can be so sure without any field validation?

That is a fair argument. We removed the respective sentence.

line 333: the variance explained by this regression is very low. The reasoning should be more conservative.

We agree that our statement is disproportionate in this context and reverted to a more conservative reasoning as suggested:

“Interestingly, the effects on the snow grain size retrieval are of a similar magnitude as on the dust estimations.”

Figure 9: this figure is impactful. I would be very curious to see at least reflectance data from one multispectral mission (Landsat 8-9 or Sentinel 2) acquired in the same period over the same spots. This would confirm that the multi-transmittance approach provides a sound correction for HDRF.

Thanks for the comment, this is a great idea! We pulled the respective L2 surface reflectance images from Landsat 8 and Sentinel-2, which captured our study area under clear sky conditions on September 14 and 12, 2022, respectively. Landsat 8 observed the region two days later than EMIT, but we believe that it is still valuable for a comparison. The comparison to both of these multispectral instruments is indeed of particular interest because standard L2 reflectance products from Landsat 8 are not corrected for topography (Yin et al., 2022), while those from Sentinel-2 are (Louis et al., 2021; Santini & Palombo, 2022). We updated Fig. 9 by adding the Landsat 8 and Sentinel-2 reflectance values for the six selected locations, adjusted the figure caption, and added the following paragraph to Sect. 4.2.3:

“To further validate the performance of the multi-transmittance approach, Fig. 9 is complemented by reflectance values derived from Landsat 8 and Sentinel-2 images, acquired over the six selected locations on September 14 and 12, 2022, respectively. Landsat 8 observed the region two days later than EMIT, but we believe that it is still valuable for a comparison. Note that standard L2 reflectance products from Landsat 8 are not corrected for topography (Yin et al., 2022), while those from Sentinel-2 include a terrain correction accounting for local slope and aspect (Louis et al., 2021; Santini & Palombo, 2022). This comparison provides an additional indication for the accuracy of the multi-transmittance derived reflectance spectra, given by their good agreement with Sentinel-2 results, particularly for locations S3, S4, and I2, which exhibit surface tilts of 10-20°. In contrast, the non-corrected Landsat 8 data rather follow the results from the EMIT L2A product.”

Section 4.2.3: in general, I like this narrative but I think that the error in Rf estimates should be put in the right context since no field validation data are provided in this study. I suggest at least to compare your estimates with previous results in the same area (e.g. Rowe et al. 2019 and references therein).

In order to address this comment, we added subsection 5.4 ‘Field validation’ to the discussion section with the following content:

“One essential part of remote sensing retrievals is still missing for the presented multi-transmittance approach, which is the validation with field measurements. This can be explained by the remoteness of our study sites in South America, but also by the fact that this work has rather been designed as a theoretical proof of concept. A good indication for the accuracy of our proposed inversion method is already given by the comparison to Landsat 8 and Sentinel-2 data in Sect. 4.2.3. However, to further compensate for the lack of validation and to put our results into the right context, we provide a comparison to findings from previous studies.

We look at the estimated error in radiative forcing when topography and anisotropy are not considered in the forward model. Previous work in the Chilean and Argentinian Andes reports daily or annual averages of LAP radiative forcing, or even the mean over a period of multiple years (Rowe et al., 2019; Cordero et al., 2022; Figueroa-Villanueva et al., 2023). Their values range between 0 and 10 W m^{-2}*, mainly investigating the influence of black carbon, which was not the focus of our study. In contrast, our method provides the instantaneous radiative forcing due to LAP, which allows a reasonable comparison to similar work conducted in the Sierra Nevada, CA, or the Rocky Mountains, CO (Painter et al., 2013a; Seidel et al., 2016). Even though estimated in a different geographical location, their values of up to 400 W* m^{-2} *agree well with the range of LAP radiative forcing retrieved from the multi-transmittance approach (see Table 3).”*

Section 4.2.4. Here your results should be put in context with other modeling results (Picard et al. 2020)

We added acknowledgment of previous findings about the ‘blue hook’ in the discussion section by referencing the work from Picard et al. (2020):

“As one of the major results of our study, we confirm conclusions from previous publications (e.g., Picard et al., (2020)) that remotely sensed snow HDRF can form an artificial downward

hook in the blue wavelengths, when retrieved over challenging mountainous terrain without considering the local topography, i.e., observation and illumination geometry."

Section 5.1: I encourage the authors to briefly review other studies where the "blue hook" is visible (e.g. Naegeli et al. 2015; Di Mauro et al. 2017; Kokhanovsky et al. 2022).

Thanks for the encouragement. We added a brief review of other studies to Sect. 5.1:

"The downward hook has been observed in various measurements from different types of instruments, including endmember spectra of ice surface materials derived from airborne APEX data (Naegeli et al., 2015), spaceborne Hyperion observations of bare ice (Di Mauro et al., 2017), or reflectance of Antarctic snow derived from the orbital PRISMA sensor (Kokhanovsky et al., 2022)."

Line 412: I agree with this point. In fact, a bending in the blue band is displayed also in the imaginary part of the refractive index of ice. More discussion should be added on this point in the manuscript.

We agree that more discussion on this point would be valuable. However, we decided to move a detailed investigation of the blue hook to a subsequent manuscript, which has recently been submitted as a brief communication to The Cryosphere as well. We would like to point the referee to this resource, which can be found at https://egusphere.copernicus.org/preprints/2024/egusphere-2024-1681/.

I think it's important to mention that often field spectroscopy data display a "upwarding" hook (e,g. Painter & Dozier 2004), that has been also modeled by Picard et al. 2020. This can be also found in imaging spectroscopy data for snow in particular slope/aspect conditions.

Thanks for mentioning this important point. In fact, we already describe this opposite behavior in lines 403-407: "Similarly, we can also observe an upward hook in cases where the local solar zenith angle is significantly larger than at top of atmosphere. Again, it is caused by an erroneous ratio of direct to diffuse irradiance, this time under the assumption of much more direct illumination than actually present, i.e., a red-shift in the solar irradiance. Opposite to sun-facing slopes, the blue reflectance is now much more overestimated than green or red reflectance since it has to compensate for larger radiance deviations in the blue spectral range when running the forward model." To include the referee's suggestion, we added the following sentences to the second paragraph of Sect. 5.1:

"Previous simulation-based studies have also modeled the upward hook (e.g., Picard et al., 2020)."

And:

"This behavior has likewise been observed in field spectroscopy data, although being rather induced by saturation or linearity effects in the solar energy rich blue wavelengths (Painter & Dozier, 2004)."

lines 428-429: this would be interesting.

While being very important, we believe that an investigation of different grain shape representations and snow and ice layer models would go beyond the scope of this manuscript. However, we would like to inform the referee that we are currently working on another study that investigates alternative approaches to model ice layers. Amongst others, it includes an approach to combine DISORT simulations with the model proposed by Whicker et al. (2022), which encloses air bubbles and a Fresnel layer between the ice and a thin snow cover.

line 430 and on: This is crucial because the OPs of dus are strongly dependent on its mineralogy and source area (Di Biagio et al. 2019). Using optical properties from Colorado is clearly a strong approximation here. I suggest to go in more detail regarding the possible differences in dust mineralogy between those two regions.

We fully agree with this comment and improved the discussion of dust optical properties in Sect. 5.2:

"The use of dust OPs poses a different challenge, as they strongly depend on mineralogy and source area (Di Biagio et al., 2019). Several sets of dust OPs from different geographic regions, derived using diverse techniques and data, are publicly available. They have been obtained from any combination of field samples, spectral measurements, and linear mixing modeling, with Sahara, Colorado, Greenland, and Mars being the most prominent regional types (Polashenski et al., 2015; Skiles et al., 2017b; Balkanski et al., 2007, Singh et al., 2016). However, only a few studies have considered specific dust minerals when assessing their impact on snow melt (Lawrence et al., 2010; Kaspari et al., 2014; Reynolds et al., 2014). For our study, we selected only one type of dust OPs representing rather large particles, measured from samples that were collected in the San Juan Mountains of southwestern Colorado (Skiles et al., 2017b). In the lack of dust OP characterization in South America, we believe that the Colorado type is closest to the dust type found in Patagonia. This is especially supported by the finding that very large dust particles are often present in patchy snow of arid environments (Skiles et al., 2017b). Moreover, studies of the geochemical composition and mineralogy suggest that both the San Juan and the Patagonian dust are significantly dominated by quartz with 30-50 % of the total mineral mass (Lawrence et al., 2010; Demasy et al., 2024). Such analyses will be facilitated on even larger geographical scales by the EMIT mission objective, which is providing an improved understanding of the mineralogy of dust particle source regions, and enabling an enhanced identification and classification of dust OPs and their distribution around the Earth's snow-covered areas."

line 450: I have the feeling that this threshold is quite low. I suggest to justify in detail this choice also showing frequency histogram of NDSI over the study area. Other possible classification methods can be applied to get snow cover from hyperspectral data (e.g. maximum likelihood, support vector machine etc.). Did the authors tested other methods?

No, we did not test other methods, but we agree that an NDSI threshold of 0.0 was indeed quite low. We updated the derivation of our snow mask by now following the procedure of Dozier (1989). We now use top-of-atmosphere (TOA) reflectance ρ_{TOA} of EMIT bands at 485 nm (blue), 567 nm (green), and 1648 nm (SWIR), and marked a pixel as snow-covered when $\rho_{\mathrm{TOA},485} > 0.16$, $\rho_{\mathrm{TOA},1648} < 0.25$, and NDSI > 0.4. The latter is calculated using the bands at $\rho_{\mathrm{TOA},567}$ and $\rho_{\mathrm{TOA},1648}$. We added this information to the text:

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rho_TOA of EMIT bands at 485 nm (blue), 567 nm (green), and 1648 nm (SWIR), and considered a pixel as snow-covered when rho_TOA,485 > 0.16, rho_TOA,1648 < 0.25, and NDSI > 0.4. The NDSI is the normalized-difference snow index calculated using rho_TOA,567 and rho_TOA,1648 (Dozier, 1989).”

Also, we updated Fig. 6 accordingly and added the following:

“We currently apply an NDSI threshold of 0.4 to determine snow-covered pixels.”

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