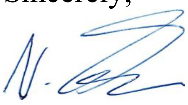
Niklas Bohn
Jet Propulsion Laboratory
California Institute of Technology
4800 Oak Grove Dr
Pasadena, CA 91109
urs.n.bohn@jpl.nasa.gov

Dear Nora,

This letter accompanies our manuscript "Do we still need reflectance? From radiance to snow properties in mountainous terrain: a case study with EMIT" (egusphere-2024-1020), which we resubmit for consideration by *The Cryosphere*. We appreciate your time and effort in dealing with our manuscript and for encouraging us to resubmit it. We hope that we have addressed all remaining minor concerns raised by you and the reviewers in this revised version of the paper and in our point-by-point answers, which follow below. In the revised version of the manuscript, the changes are highlighted in magenta.

We hope that this new version of the manuscript meets the quality criteria necessary for publication in The Cryosphere and look forward to future correspondence. Many thanks for your help again.

Sincerely,



Niklas Bohn
Jet Propulsion Laboratory, California Institute of Technology
Pasadena, CA, USA

Edward H. Bair
Civil Group, Leidos, Inc.
Reston, VA, USA

Philip G. Brodrick, Nimrod Carmon, Robert O. Green, David R. Thompson
Jet Propulsion Laboratory, California Institute of Technology
Pasadena, CA, USA

Thomas H. Painter
Joint Institute for Regional Earth System Science and Engineering, UCLA
Los Angeles, CA, USA
****

Ms. Ref. No: egusphere-2024-1020
Do we still need reflectance? From radiance to snow properties in mountainous terrain: a case study with EMIT

Anonymous Referee #1

I thank the authors for addressing my comments and I apologize for my late re-review. I'm satisfied with their answers. I have just two minor comments on the revised version, detailed below.

We thank the referee for re-reviewing our manuscript and for the positive feedback. Below are our responses to the two minor comments.

1- I see that the title has been now changed according to a specific request from R#2. I'm ok with that, but I suggest that the authors should try to answer to the rhetoric question in the title. In fact, at the moment I can't find a specific section in the discussion part of the manuscript. I suggest also to include one sentence in the abstract.

That is a very good point, and we agree that we have not really answered the question in none of the different sections. We updated the second half of the abstract (lines 14-22):

"Moreover, the term 'surface reflectance' is often used with ambiguity in the literature, which instantly raises the question if we still need this quantity as a retrieval product. In this contribution, we present a novel forward model that couples the MODTRAN atmosphere radiative transfer code with a physics-based snow reflectance model that utilizes the multistream DISORT program. Our model allows to estimate snow surface and atmosphere properties directly from measured radiance. We apply the approach to EMIT images from Patagonia, South America, and compare our results to the EMIT L2A products that retrieve surface reflectance as a free parameter. We find discrepancies in snow grain size of up to 200 μm and in dust mass mixing ratio of up to 75 μg g^{-1}. Furthermore, we demonstrate differences in instantaneous LAP radiative forcing of up to 400 W m^{-2}. We conclude that we still need reflectance, but only if clearly defined and preferably as a modeled quantity within the forward model."

In addition, we added subsection 5.5 "Do we still need reflectance" to the discussion (lines 569 - 577):

"Remote sensing retrievals that utilize measurements of reflected sunlight will always need a reflectance term within the forward model as this is the essential quantity to summarize the radiative transfer of photons through a material. However, our findings confirm that this term needs to be treated carefully and should either refer to the intrinsic reflectivity of the target material or even more accurately, be defined as a bi-directional reflectance distribution function (BRDF) providing coupled, angle- and illumination-dependent reflectance terms (Vermote et al., 1997; Schaepman-Strub et al., 2006; Verhoef and Bach, 2007; Guanter et al., 2009). Furthermore, our study shows that using surface reflectance as a modeled quantity within the forward model rather than retrieving it as a free parameter improves the accuracy of estimated biophysical snow properties. In the absence of well-parametrized surface radiative transfer models, this can be balanced by a clear definition and consistent use of the surface reflectance term."

Finally, we added the following sentence to the conclusion (lines 591-592):

"Surface reflectance is still needed as a modeled quantity within the forward model but must be consistent and clearly defined."

By the way, I saw also a new paper just published on TC on this same topic (Wilder et al. 2024). Probably it's worthwhile add this reference in the bibliography of this manuscript.

Absolutely! We added this reference in lines 98-100:

"Only recently, Wilder et al. (2024) introduced a more mature and comprehensive modeling of topography for spaceborne imaging spectroscopy data over mountain snow, but most studies apply a limited post-hoc correction at the airborne scale (Painter et al., 2013a; Seidel et al., 2016)."

And in lines 109-110:

"This facilitates the consideration of local surface anisotropy and topography in the forward model and removes dependency from external DEMs (Carmon et al., 2023; Wilder et al., 2024)."

And in lines 201-203:

"Aligning with Wilder et al. (2024), we remove dependency from digital elevation models by adding θ_i *to* $\boldsymbol{x}_{SURF}$*, which ensures "physical consistency, temporal coincidence, and spatial alignment" (Carmon et al., 2023)."*

And in lines 477-479:

"Being able to accurately retrieve the local incident angle directly from the measured radiance though, as shown by Carmon et al. (2023) and Wilder et al. (2024), and confirmed by our manuscript, adds one constraint to the shape of the reflectance in the blue wavelengths, facilitating a more reliable retrieval of AOD."

2- I see that the blue hook section has been removed, since a more detailed manuscript (also submitted to TC) focuses on this issue. I'm ok with that. I suggest to improve cross-citation between the manuscripts, in order to make clear the distinction of those two studies.

Thanks for this comment. We further improved the cross-citation between the two manuscripts by adding lines 96-98:

"Overall, there can be a handful of factors biasing snow reflectance shape and magnitude, especially in the blue wavelengths, as demonstrated in a complementing study by Bair et al. (2024)."

References:

Wilder, B. A., Meyer, J., Enterkine, J., and Glenn, N. F.: Improved snow property retrievals by solving for topography in the inversion of at-sensor radiance measurements, The Cryosphere, 18, 5015–5029, https://doi.org/10.5194/tc-18-5015-2024, 2024.

Anonymous Referee #2

In this paper, a combined optimal estimation technique for simultaneous retrieval of retrieval of snow and atmopsheric parameters from EMIT data is presented. The method is new and specifically the inclusion of diffuse-to direct irradiance estimation makes it worth publishing. Also, the authors did a good job in augmenting the paper in comparison to the first version. But still, I think there should be some mandatory changes done in the text to avoid ambiguities.

We thank the referee for the positive feedback. Below are our responses to their comments.

Some specific comments:

- The title asks if one still needs reflectance, but no answer to this question is given int the text - and one should be careful answering this as the confusion between AOD and snow properties shows the risks if omitting reflectance as an intermediate product.

That is a very good point, and we agree that we have not really answered the question in none of the different sections. We updated the second half of the abstract (lines 14-22):

"Moreover, the term 'surface reflectance' is often used with ambiguity in the literature, which instantly raises the question if we still need this quantity as a retrieval product. In this contribution, we present a novel forward model that couples the MODTRAN atmosphere radiative transfer code with a physics-based snow reflectance model that utilizes the multistream DISORT program. Our model allows to estimate snow surface and atmosphere properties directly from measured radiance. We apply the approach to EMIT images from Patagonia, South America, and compare our results to the EMIT L2A products that retrieve surface reflectance as a free parameter. We find discrepancies in snow grain size of up to 200 μm and in dust mass mixing ratio of up to 75 μg g^{-1}. Furthermore, we demonstrate differences in instantaneous LAP radiative forcing of up to 400 W m^{-2}. We conclude that we still need reflectance, but only if clearly defined and preferably as a modeled quantity within the forward model."

In addition, we added subsection 5.5 "Do we still need reflectance" to the discussion (lines 569 - 577):

"Remote sensing retrievals that utilize measurements of reflected sunlight will always need a reflectance term within the forward model as this is the essential quantity to summarize the radiative transfer of photons through a material. However, our findings confirm that this term needs to be treated carefully and should either refer to the intrinsic reflectivity of the target material or even more accurately, be defined as a bi-directional reflectance distribution function (BRDF) providing coupled, angle- and illumination-dependent reflectance terms (Vermote et al., 1997; Schaepman-Strub et al., 2006; Verhoef and Bach, 2007; Guanter et al., 2009). Furthermore, our study shows that using surface reflectance as a modeled quantity within the forward model rather than retrieving it as a free parameter improves the accuracy of estimated biophysical snow properties. In the absence of well-parametrized surface radiative transfer models, this can be balanced by a clear definition and consistent use of the surface reflectance term."

Finally, we added the following sentence to the conclusion (lines 591-592):

Surface reflectance is still needed as a modeled quantity within the forward model but must be consistent and clearly defined.

- You have apparently decided to use the definition of HDRF as of G Schaepman-Strub. This should be clearly stated, as HDRF may be defined that way, but the purely physical definition of Nicodemus is different: HDRF is the BRDF integrated over the incidence hemisphere (ie. white sky). Please change (at least) the wording: instead of 'the HDRF is defined as' use 'in the this paper, the defnition of 'HDRF' as proposed by Schaepman-Strub is used. Even better would be to call this 'bottom-of atmosphere reflectance'.

Thanks for this comment. We agree and implemented the referee's suggestion (lines 121-123):

"In this study, we follow the definition of HDRF as given by Schaepman-Strub et al. (2006), which is based on the nomenclature of Nicodemus et al. (1977) but incorporates the adaptations to the remote sensing case from Martonchik et al. (2000)."

- The sentence in the abstract 'previous methods..' is wrong and should be deleted. There are many methods around which use physical models for well defined BOA reflectance retrieval (what you call HDRF) under consideration and also BRDF models are somtimes linked directly. (and the definition of reflectance used in this paper is also physcially shaky).

We thank the referee for this comment. We do not fully agree with this statement. In our opinion, the sentence is not 'wrong' but probably too inclusive. We absolutely acknowledge that methods exist that incorporate physical models for BOA reflectance/HDRF retrievals. However, it is also correct that methods exist that either neglect physical effects of the surface or utilize the BOA reflectance/HDRF as an intermediate non-physical quantity, without proper error propagation from the atmospheric modeling and obtained from statistical modeling. Furthermore, we specifically refer to the case of accurate retrievals of snow surface properties. We therefore softened our expression, but did not remove it (lines 12-14):

"However, some methods still either neglect physical effects of the surface or utilize the surface reflectance as an intermediate non-physical quantity, in part without proper error propagation from the atmospheric modeling and obtained from statistical modeling."

- page8: the omission of adjacency may have a strong impact on the results and the given reference does not corroborate that this is not a problem. By nature, snow covered areas are in mountainous terrain and neighbouring slopes as well as the aerosol scattering both lead to strong cross talk to the neighbourhood. This also hampers the validity of the 'HDRF' you have been using as it assumes a flat terrain. A discussion about adjacency should be added.

Thanks for this comment. We agree that Guanter et al. (2009) do not corroborate this finding, and thus, removed this reference. Also, the term 'less critical' that we used in this context is not appropriate. However, Picard et al. (2020) conclude that '…the upward- and downward-looking sensors are affected by additional illumination coming from the slope itself and the neighbouring slopes. [This effect] becomes significant for slopes larger than about 15˚. The theory for large slopes is analytically tractable in several particular cases but is more complex than for the small slopes and requires information or assumptions on the neighbouring slope, which limits its interest in practice.' Also, snow-covered areas are not 'by nature' in mountainous terrain. Yes, most of the Earth's mountains are snow-covered, but high-latitude landscapes, including vast plains and ice sheets, are also snow-covered, particularly in the winter season. Finally, the HDRF we are using does not assume a flat terrain. In fact, we understand it as the ratio of reflected spectral radiance at a local solar and view geometry to the radiant flux that would be reflected from an ideal Lambertian surface, illuminated and observed under the same conditions. We slightly modified our justification of not including adjacency effects to accommodate the referee's suggestion (lines 187-190):

"While acknowledging their impact on reflectance of larger slopes, we currently exclude adjacency effects of neighboring pixels as their modeling is complex and requires assumptions on the neighboring terrain which may introduce additional uncertainty themselves (Picard et al., 2020)."

- Snowy areas are often in northern latitudes with small solar zenith angles; excluding extrem solar angles and low apparent reflectances may exclude too much (maybe add a remark about that).

Yes, absolutely right, snowy areas are often to be found in high latitudes. However, we believe the referee means large solar zenith angles, i.e., small solar elevation angles. If yes, we totally agree. However, we do not fully understand what this comment refers to. Why and how did we exclude extreme solar angles and low apparent reflectance? The EMIT sensor does not cover latitudes beyond ~55° due to its orbit on the ISS. Thus, we are not able to utilize EMIT images from those regions the referee is alluding to. Moreover, we actually included examples of large solar zenith angles and low apparent reflectance by applying our retrieval approach to pixels facing away from the sun (see Figure 7a and Figure 10, panels S4 and I2).

- The uncertainties in Table 2 are error propagation, but no errors towards real measurements yet; the AOD uncertainty is somwhat strange as it allows values below zero (?).

Yes, correct, these uncertainties are propagated through the Optimal Estimation based retrieval framework and are taken from the diagonal of the posterior covariance matrix. And yes again, we do not have error estimates yet obtained from comparison to in-situ measurements. We are aware of this and already included this topic as Section 5.4 in the discussion. In fact, all posterior uncertainties theoretically allow values below zero by nature as they represent the standard deviation around the mean value of the posterior distribution of each state vector element. We agree that, in practice, negative values are of course unrealistic. We added a sentence for clarification to lines 407-409:

"This can not only lead to error distributions extending into the negative value range, but also ignores other local minima solutions, if they exist, and even the local estimate may under predict errors (Hobbs et al., 2017; Cressie, 2018)."

- processing time seems to be critical - please add the current processing times.

Good point! However, it is difficult to give exact numbers as these highly vary depending on the computing system. At least, we added a comparison to the traditional processing as presented in Thompson et al. (2018) to lines 269-271:

"It usually finds a solution after five iterations on average with an overall processing speed-up of about four orders of magnitude compared to the traditional approach as presented in Thompson et al. (2018)."