Review of « The pitfalls of ignoring topography in snow retrievals: a case study with EMIT», by Niklas Bohn et al. Review by Quentin Libois.

**General comments**

This paper presents a novel retrieval algorithm based on optimal estimation to retrieve snow properties (grain size, dust and algae contents, liquid water content) from hyperspectral satellite images in the solar spectrum. The main originality of the algorithm is to account for local topography by including the slope angle in the retrieved parameters. The algorithm is applied to two images acquired above Patagonia by the spaceborne instrument EMIT. Accounting for the local slope has a significant impact of the dust and grain size retrievals, but is less critical for algae and liquid water content which are characterized by localized spectral features. These retrievals are also used to compute the instantaneous radiative forcing of light absorbing particles, which is also very sensitive to slope effects. The spatial gradients of the retrieved quantities are discussed on a physical basis, supporting the reliability of the retrievals. A preliminary sensitivity study is performed, which highlights how the aerosol optical depth in the atmosphere and the slope angle impact the snow reflectance. The apparent drop in the blue range of snow spectra is discussed in details to highlight that topography is largely responsible for this common feature of snow reflectance as measured from space. Some limitations of the current algorithm and suggestions for improvements are also provided.

The paper is overall well written, although the abstract and introduction could probably be greatly improved, in particular to better remind the existing strategies already used to account for topography in snow retrievals from space and the link between the present work and previous studies from the same authors. More technical information about EMIT would be appreciated and the relevance of the retrieval algorithm to other spaceborne instruments could be elaborated. Some technical details lack in the presentation of the algorithm but could easily be provided. My main concern is about the representation of snow by a collection of spheres to simulate the directional reflectance. This issue is only very briefly mentioned in the discussion, without any quantification of the potential impacts. It certainly deserves much more attention. Also, the accuracy of the retrievals is not discussed, while the theoretical framework used would make it easy to investigate, and would strengthen the conclusion that retrievals of LAP are possible with EMIT while they were apparently challenging with previous spaceborne instruments. For these reasons I recommend that these points be carefully treated before the paper can be considered for publication.

**Specific comments**

1) The abstract is probably too long. It could more efficiently start with the relevance of monitoring LAP and grain size. Also the objective is not clearly stated, neither the main novelty compared to previous work. In general it is not very clear and would deserve some general rewriting (see some suggestions in the technical comments).

2) The introduction as well could notably be improved. What is mainly missing is information about algorithms already used for LAP retrieval, and to account for topography (e.g. Picard et al., 2020). EMIT is selected because it supposedly has a larger SNR so what would happen if existing algorithms were applied to EMIT? Why such a motivation to build a new retrieval algorithm? This should be better motivated. Also, how does this study complement recent previous work from the same authors?

3) EMIT is central in the present study. However it is nowhere described in details. In particular information on the spatial resolution (which is very critical) is lacking. Likewise, information about

its spectral resolution and radiometric accuracy would be very useful. An important question being: why using EMIT and not any other spaceborne hyperspectral (or multi-spectral if it would be enough for the purpose of the study) sensor.

4) The authors represent snow as a collection of spheres, although it has been known for a long time that this is not appropriate, in particular to describe the anisotropy of snow reflectance. The quantitative impact of such an assumption on the retrievals is not investigated, which is detrimental to the overall quality and impact of the study. I'd encourage the authors to test the retrievals with other datasets of snow directional reflectance (from observations or models depending on the availability of the data, see some suggestions in the technical comments). Also, adding a figure to illustrate the snow HDRF used in the forward model would probably help the interpretation of the spectra and their sensitivity to illumination and viewing geometry

5) I feel like some technical details in the algorithm are missing. First, the equation for the forward model would deserve more physical explanations, beyond a reference to a paper. Then, snow grain size is not properly defined. Is it a radius, a diameter, averaged or effective over a prescribed size distribution, or on the contrary a monodisperse collection? As a consequence, the way liquid water content is accounted for is not sufficiently clear. Likewise it is not clear if the treatment of LAP relies on mass absorption coefficients or any other optical quantity. The instrumental noise is not detailed either while some quantitative information to highlight the high SNR would be appreciated.

6) One originality of the retrieval algorithm is to use the optimal estimation in combination with a forward model to retrieve the parameters of a state vector (instead of retrieving for instance reflectances). However only the most probable solutions of the problems are presented, without any reference to the associated uncertainties. Given that the objective of the study is to demonstrate that EMIT can be used to retrieve snow properties that are not accessible with other instruments, mentioning uncertainties is key to convince the reader that the retrieved quantities are reliable, in particular given that there is no ground truth. For instance it is quite questioning that the retrieved dust quantities can be strictly zero (Table 2). Beyond the uncertainties I'd encourage the authors to further investigate the correlations between the retrieved parameters, which could help understand how compensation between variables can affect the quality of the retrievals.

**<u>Technical comments</u>**

l.2: solar <u>radiation</u> would be better than <u>illumination</u>

l.3: at negative temperature melting is not critical, but metamorphism is

l.6: maybe specify LAPs <u>on/in snow</u>?

l.11: not clear what dust properties EMIT measures

l.12: what is a "target mask"?

l.15: anisotropy of what?

l.16: why "forward scattering"? Not clear

l.18-20: quite difficult to understand in an abstract

l.21: it would have been helpful to detail earlier (e.g. l.15) what are the snow properties to be retrieved l.22: use µg $g^{-1}$ instead, as well as for all units

l.23: such a forcing seems huge! It's because it's instantaneous.

l.25: is the "blue hook" something sufficiently well known (it is not to me) to appear as is in an abstract?

l.25-26: the link with runoff and climate models is definitely not obvious. Either to be removed, or expanded

l.30: I don't see why having the highest albedo of all natural surfaces is a reason for playing a key role...

l.31: "cooling effect" is a bit surprising to read. Snow does not cool the Earth, or at least it depends with respect to what? Ok if you say "more snow-covered surfaces will tend to cool the Earth"

l.37: LAPs are not the only reason for snow darkening (or at least albedo decrease). Metamorphism has a similar effect

l.39: Sun's energy is unclear → where the solar spectrum peaks? Where the sun irradiance is maximum?

l.59: not clear what "not tied to physical units" means

l.61, 62: what are EnMAP, PRISMA?

l.68: not clear what these references correspond to

l.69: absorption <u>spectral features</u>?

l.73: not clear why mapping arid surfaces informs about transport and radiative forcing. EMIT should be more clearly introduced

l.83: the transition from the Lambertian assumption issue to the topography issue is too fast. Is there a link between both?

l.89: what does "rapidly shifting terrain" mean?

l.91: at first order and satellite footprint scale the mountain topography probably dominates snow depth variability, nope?

l.95: could you explain what does this algorithm

l.96-97: not clear what is the atmospheric radiative code and the snow one. Also it suggests that 3D effects (reillumination by neighboring slopes) are not accounted for by such a model. Do you confirm?

l.123: what tool was used to compute these HDRF?

l.138: it’s not obvious to me why the backward reflectance decreases with less direct irradiance (it means comparing backward and side scattering). Some explanation detailing the equivalent incidence angle of diffuse illumination would be helpful

l.141: could you clarify whether “scattering by surrounding objects” can actually be modeled.

l.144: I regret that EMIT has not been introduced before in more details. In particular its spatial resolution seems to be a critical quantity if it is meant to see independently distinct mountain slopes instead of a mixture of various slopes.

l.163: I think units (here and elsewhere) should not be italic

l.172: can you clarify whether you invert independently the individual pixels, or not.

Eq. (2): I think it could be better explained in terms of the various contributions. Also, transmittance is a physical property (of the atmosphere for instance). Here it seems that it includes the partition between direct and diffuse irradiance. I’d recommend to explicitly mention the direct/diffuse partition. Also, I don’t know what “atmospheric path radiance” is. Is it related to the spherical albedo of the atmosphere? By the way spherical albedo has not been defined before.

l.185: I’d expect the HDRF to depend also on the direct/diffuse partition. Regarding the incidence angle what is the motivation to have it in the state vector instead of using a DEM? How would the results with fixed vs retrieved incidence angle compare?

l.186-187: Not very clear. Do you mean that previously the HDRF was in the surface state vector? Also I’m afraid to read that you assume spherical particles for snow (confirmed l. 198), which are very inappropriate, in particular when it comes to computing HDRF. Database exist for more realistic snow BRDF data (from either measurements of models). If not detailed elsewhere, could you clarify how many snow layers you use in the model.

l.187: how many streams are used for the DISORT simulations?

l.190: do you mean that the dimension was larger previously due to the multispectral dimension?

l.197: not only the asymmetry parameter matters, but also the detailed phase function for that kind of applications

l.199: how is then defined the snow grain size? Including the liquid water coating? What about the size distribution of snow particles?

l.204: would you have any reference to support that algae are similar in Greenland and Patagonia? Otherwise why would you believe this? Also could you clarify what optical property is defined. Only a mass absorption coefficient? The same question holds for dust.

l.213: what do you mean by “atmospheric aerosols”? Those assumed in MODTRAN?

l.222: it should be clear what measurements are included here. Multi spectral or also multi-pixels?

l.226: then why using a prior at all if in the end it does not constrain the cost function?

l.227: for the model to be well-posed it should be proved that measurements at distinct wavelengths are actually independent, and the number of spectral channels should be mentioned (to be compared to the number of parameters to be retrieved).

Eq. (4): here again this factor is very dependent on the actual phase function of snow, which is likely to be different than that of spheres.

l.230: the main advantage of optimal estimation is to provide an estimation of the posterior error, which is not discussed at all. It would be worth adding this uncertainty range for the retrieved parameters of interest.

l.248: I think the units should be like W $m^{-2}$.

l.264: how do EMIT spatial resolution and SRTM match (or not)? Is SRTM averaged somehow to find incidence angles comparable to EMIT retrievals?

l.268: the correlation between snow grain size and slope is tricky. You could either argue that the retrieval is homogeneous for snow grain size in a mountainous terrain with various slopes, suggesting that accounting for slope corrects for an apparent heterogeneity of snow grain size when assuming flat terrain. Or you give a physical reason why snow grain size can differ depending on the slope… Looking at the correlations between retrieved parameters may help clarify this point. A too strong correlation may indicate compensation between both variables.

l.271: I would not necessarily say that snow grains of 200 microns (at least if it is the radius) are small.

Fig.7: it should be clear somewhere that EMIT L2A is the standard EMIT product ignoring topography

l.299: it is not clear what the single-transmittance model is (what wavelength?).

l.303: I think this “hook” behavior should be better identified in the figure. Is it the too strong decrease in the blue visible in the HDRF? I believe the direct/diffuse partition, that greatly changes in this spectral range, if not properly accounted for can also contribute to this hook.

l.311: I don’t see in this paragraph the sensitivity to assuming a Lambertian snow surface. Unless both impacts are combined altogether. In this case it would be worth separating both to disentangle the impacts, and point what assumptions is most critical.

l.325: only here is the direct/diffuse partition explicitly mentioned, while I think it would be valuable to clarify its treatment and impact earlier on.

l.333: the 3 digits may be a lot for such an estimation.

Table 2: any comment on the fact that algae can be zero somewhere, and present elsewhere?

l.375: “small” is awkward. Preliminary?

l.376: this point suggests that their could be correlations between the retrieved parameters. You could look at these correlations to inform about the independence (or not) of the retrieved parameters, which is trivial with optimal estimation. The underlying question being for instance:

can the retrieval algorithm return stronger AOD and lower LAP in snow, which may result in more or less the same apparent radiance at TOA?

l.382: this suggests that AOD cannot be accurately retrieved, unless it is the blue end of the spectrum that puts most constraint on AOD (rather than the longer wavelengths). Could you expand on that?

l.395: AOT or AOD?

l.399: what would be the impact of not considering blue wavelenghts in the retrievals? What variables would be most affected, and to which extent?

l.401: this physical explanation for the blue hook could have been given earlier on, and a bit more detailed.

l.402: can you expand on these laboratory measurements?

l.411: where does this assumption come from?

l.412: you could also refer to Picard et al. (2016) who suggest absorption is in between Warren and Brandt (2008) and Warren (1984).

l.423: on which basis do you argue that the spherical assumption is the best general shape? As a suggestion, Malinka (2014, 2023) has developed a general mixture model that works very well to estimate the "optical shape" of snow. Maybe it's relevant as well for snow BRDF. See also Dumont et al. (2021).

l.426: much larger than what? Why couldn't it be large spherical particles?

l.427: It's definitely a good idea, and I would strongly suggest to further investigate this in the present paper.

l.430: I guess one of the EMIT objectives is to map this variability in dust optical properties, so it might be worth referring to this and directly related studies.

l.442: as the spatial resolution has never been discussed it's hard to guess how critical are these mixed pixels.

l.453: how would you calculate snow fractional cover? By including it in the state vector?

l.471: the link between this work and melt runoff and climate model input are not clear, but this might be clarified if it sounds important to the authors.

**References**

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