Atmospheric Measurement Techniques Discussions

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Review on amt-2021-143

Anonymous Referee #1

Referee comment on "Cloud-probability-based estimation of black-sky surface albedo from AVHRR data" by Terhikki Manninen et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-143-RC1, 2021

In this paper, the authors attempt to determine surface albedo from AVHRR satellite measurements and with the help of cloud albedo distributions that replace a binary cloud masking approach.

The topic is clearly relevant to AMT and thus the venue is appropriate. As for the research, I confess that the article did not convince me either with respect to the novelty of the content, their representativeness or the analysis and conclusions.

The critical points that I found are: excessive use of concepts published in the past; assumptions too stringent regarding solar illumination and atmospheric state; a database too limited both in time (1 month, June 2012) and in space (only a few ground stations, without any comparison with other satellite datasets to appreciate the advantage of the CP inclusion).

The approach is also unclear to me. If you use only the CP of June 2012, how can you translate with confidence the method also for the months shown in Fig.7 and Tab.3?

In conclusion, the article still seems to me unrefined and not fully mature. It does not deliver a **compelling** message. Perhaps it would be useful to withdraw it, wait and rethink it not so much in the basic idea, which is valid, but in the development of the analysis. So: to have more data available that would allow a deeper analysis and understanding of the variability that inevitably characterizes both the surface and the atmosphere.

I don't like to reject papers and I am conflicted about what judgment to give between major revisions and reject because on the one hand I would like the authors to have the opportunity to improve the work but on the other hand I find that the amount of improvements to be made is so substantial that it would be objectively easier to start over (personal opinion).

Main general comments:

1) I admit I was in trouble reading this paper because the part of the text from pages 5, line 28 to page 7 is a copy-paste of Manninen et al 2004. Although the similarity report gives a result of only 14%, it is surprising how the equations from 1 to 6 are the same, as well as the text with few variations. It is indeed work of the very same author, but I personally find the choice of copy-paste quite bold.

This is not only a matter of form but also of substance: I am led to wonder where is the novelty in this research and the advancement in methods if the section "Theoretical cloud distributions" is taken from an article published in 2004 (17 years ago).

Page 9 - Section 3.2 is also taken from Manninen et al 2004, Section 3.1 p 416, "Surface albedo algorithm". The same thing seems to me to apply when comparing Figure 5 of Manninen et al 2004 and Figure 2 of this paper.

I would like to genuinely ask the authors if they think there is enough scientific novelty in this AMT paper to justify its publication. Unlike the 2004 paper, they ingest cloud probability distributions but the results are still not dissimilar to the 2004 paper, as far as I understand.

I did check the similarity report too, and that 14% does not catch the semantics in my opinion. With some changes one can revamp old text in such a way to avoid a brute force database comparison, but conceptwise you are still sticking to old concepts. The authors seem to be aware of this and by citing every now and then the 2004 paper they avoided to write a much fairer sentence such as (e.g.) "From now on we apply the methodology developed in Manninen et al 2004." Period.

The flavour would be completely different. I honestly don't know how to deal with this situation.

Terhikki Manninen, Niilo Siljamo, Jani Poutiainen, Laurent Vuilleumier, Fred Bosveld, and Annegret Gratzki "Cloud statistics-based estimation of land surface albedo from AVHRR data", Proc. SPIE 5571, Remote Sensing of Clouds and the Atmosphere IX, (30 November 2004); https://doi.org/10.1117/12.565133

2) Unless I missed the information, other than the citation of the pyGAC package, the article makes no explicit mention of any corrections needed for AVHRR channel degradation, nor of the fact that the 40-year AVHRR record is composed of multiple platforms with different local overpass times, relevant for the task for **this** paper.
I imagine that both factors are relevant to the derivation of the surface albedo, both in all-sky configuration due to different atmospheres and black-sky albedo due to different illumination conditions (which I know the authors do not account for, but I am still puzzled by this choice).

3) I was confused by the approach of the paper in that on the one hand it is described as a comprehensive study preparatory to reprocessing the CLARA dataset. On the other hand, however, very limited results are presented in terms of both atmospheric conditions and locations, with very stringent criteria on solar illumination and cloud type.

Specific comments

- P2 L31: "with acceptable spatial representativeness of the site's measurement with respect to the albedo of the surrounding area".

It's not straightforward to me what this passage means. Or rather, I can guess that the authors want to make sure that the albedo around the measurement station does not vary drastically, so that a satellite overpass, that is not perfectly centered, is not contaminated by critically inhomogeneous surface types.
If my assumption is correct, I wonder if it is not useful instead to relax this criterion and analyze just what happens in very heterogeneous surface situations (e.g. coastal areas, mixed topography, urban settlements in arid areas, biologically active water masses).
I imagine the authors could agree that including the above cases would benefit the meaningfulness of their results.

- P3 Section 2.2.2
I would like the authors to explain the reasoning behind the choice of the atmospheric correction approach of Rahman and Dedieu and the selection and filtering criteria of AOD. AI is an index and is it still differentially sensitive to so many aerosol properties and line-of-sights that is interesting (or misterious) to me how it can be used for this task.

P4 L14 : Figure 1 can be greatly improved. I personally would not cut it at 20% but leave the full X-axis domain and the 20% subset as inset. Also in view of the discussion in the next paragraph about the U-shaped distribution. There (P5 L12) Figure 1 is invoked but the U-shaped distribution is not intuitable.
In the ensuing text also it appears to be introduced as a synthesis of AVHRR data given at native resolution 1.1 km and the GAC product (5 km). Information that is not given in the caption of the figure.

- P5 L4-6: "When estimating the cloud fraction distribution over the entire globe in a very coarse spatial resolution, however, it is possible that the extreme values are not achieved at all."

I disagree with this statement. On the one hand, Krijger et al (https://doi.org/10.5194/acp-7-2881-2007) have shown that even at the spatial resolution of GOME (320 x 40 km2) - which is to my knowledge the sensor with the coarsest spatial resolution used in cloud remote sensing - there is a non-negligible probability of having cloud-free pixels. Speaking of the other extreme, CF = 1, we know well that there are synoptic-scale (~1000 km) cloud systems that can be fully covered by the swath of such a sensor.
There are numerous studies comparing CF from GOME with real data and it is clear that the U-distribution of cloud fraction is largely (not completely) independent of the spatial resolution of the instrument. What makes the difference is the algorithm and the class of clouds under consideration.

The first two that come to my mind.
Lutz, R., Loyola, D., Gimeno García, S., and Romahn, F.: OCRA radiometric cloud fractions for GOME-2 on MetOp-A/B, Atmos. Meas. Tech., 9, 2357–2379, https://doi.org/10.5194/amt-9-2357-2016, 2016.

Grzegorski, M., Wenig, M., Platt, U., Stammes, P., Fournier, N., and Wagner, T.: The Heidelberg iterative cloud retrieval utilities (HICRU) and its application to GOME data, Atmos. Chem. Phys., 6, 4461–4476, https://doi.org/10.5194/acp-6-4461-2006, 2006.

So if the authors mean the native resolution of an instrument at the ground (footprint), in my opinion, they are wrong. Alternatively, one could talk about gridded cloud fraction resolution. Perhaps after aggregation with arbitrary temporal and spatial sampling the extremes will never be reached. I invite the authors to reconsider the logic of their reasoning.

P6 L7-9: "The cloud albedo distribution can also be assumed Gaussian, although the standard deviation may be so large, that the result is essentially the same as for uniform distribution."

This is a surprising and simplifiying statement. The albedo of clouds is primarily a function of their optical thickness, which is never normally distributed. It has been shown that the albedo of clouds is better approximated by a beta and Weibull distribution (i.e. Koren and Joseph, 2000).

Koren, Ilan, and Joachim H. Joseph. "The histogram of the brightness distribution of clouds in highâ□□resolution remotely sensed images." Journal of Geophysical Research: Atmospheres 105.D24 (2000): 29369-29377.

P 11 L 11-12: "The difference increases with increasing AOD". Could you expand this sentence and give more information about the AOD values, how they are measured, and the type of aerosol?

P12 L 13: "The chosen limit CP < 20% is a compromise between the quality of TOA reflectance values and the number of pixels available for a monthly mean albedo retrieval" What does that "quality of TOA reflectance" mean? Can you give figures of the radiometric accuracy needed to achieve the results you are presenting? I am convinced that this is important information, since we are talking about a satellite product that should be used as input for other algorithms.

P12 L30: "In addition, the difference between the estimates of the two methods is typically largest for snow-covered areas, where cloud discrimination is very challenging, especially when the sun elevation is low".

I don't understand then the sense of this study, if you are not able to separate and isolate the factors that contribute to the differences in the albedo. The authors rely on this argument several times in the text, but I wonder why they couldn't just look for an RGB image from a high-resolution satellite to show that there really is heterogeneous and patchy snow cover, for instance.

P13 L 6: "The CLARA-A3 SAL will be derived using the CP values instead of the binary cloud mask. The pentad means will be derived technically similarly as the monthly means using pentad distributions of CP."

What is the "pentad" distribution? Why does it need to be introduced here in the discussion of results without any context?

P13 L 7: "Future studies of the CLARA-A3 CP and cloud mask characteristics will show, whether it would be desirable to use both the cloud mask and the CP values as the basis for SAL estimation."

I thought the purpose of this study was really to show that using CP distributions was advantageous over using a CM approach. However, here in the conclusion it says that it has not yet been decided. This statement leads me to think that even the authors themselves are aware of the limited informative value of this study.

P23 Table 3: No statistics of differences are given for the sites.

Minor comments

- P2 Last paragraph of the introduction. I personally am a proponent of a description of the structure of a paper at the end of the introductory section (e.g. in section 2 the data are introduced, while in 3 and 4 the reader finds ...)

- P3 L8: what does the acronym FDR mean? As a section title, expand it.

Typos
- P5 L 10: "Although the cloud probability estimation is complicated various kinds of uncertainties" -> by (?)

- P8 L12 : than -> then