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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud thermodynamic phase and particle size estimation using the 0.67 and 1.6 μm channels from meteorological satellites" *by* D. Jolivet and A. J. Feijt

Anonymous Referee #3

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GENERAL COMMENTS :

This manuscript discusses a method for retrieving cloud microphysical and radiative properties from visible and near infrared measurements of reflected solar light.

The theoretical basis underlying this method have been already extensively discussed in literature by different authors and this is clearly not the primary interest of this paper. Still, theoretical background is clearly exposed and references provided in the paper are useful and sufficient for its purpose.

The main interest of this paper does not reside in its theoretical background but rather in that : (i) the authors propose an interesting approach to retrieve simultaneously cloud

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thermodynamic phase and microphysical properties (particle size), (ii) the proposed method could potentially be applied to new generation meteorological sensors (namely Meteosat Second Generation MSG).

The first point is a very important topic for remote sensing of cloud properties using passive measurements of reflected solar light because erroneous cloud phase assumption can yield important bias in retrieved microphysical and radiative properties. Other methods currently used for retrieval of cloud microphysical properties tend to separate the cloud phase retrieval from the microphysical (size) and radiative properties retrieval steps. The second point has obvious implication in terms of weather prediction and climate monitoring since meteorological satellites provide both the good sampling rate and long term coverage needed to address these questions. Observation of cloud properties using operational satellites is thus a very important research area which should benefit from experience gained with research satellites/instruments.

Although this paper addresses two important questions, I do not think it is suitable for publication in its present state. My main concern here is that some conclusions are drawn upon a number of assumptions which are generally insufficiently discussed and could significantly limit the range of applicability of the method. I will further discuss in the following specific points which I think the authors should address to make this paper suitable for publication.

SPECIFIC COMMENTS :

"Statistical approach"

The method presented here relies on a statistical analysis of the measured ratio of reflectivity at 0.67 micrometers to that at 1.6 versus reflectivity at 0.67. The authors try to demonstrate in this paper that a statistical analysis of this type of curve (scatter plot) provides sufficient information to select an adequate microphysical model (phase+size) for further retrieval of optical thickness, emissivity and cloud water path (yet, no optical thickness retrievals are attempted). We can first assume here for the purpose of

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discussion that the microphysical models selected by the authors are sufficiently representative of real world clouds and that curves observed for ice and liquid models are indeed usually separated. Two case studies for liquid and ice clouds are presented separately and observations are plotted alongside with theoretical curves. However no actual retrieval results are provided. It could have been extremely useful to select a case study with both ice and liquid clouds and to present the associated retrieval performed over the whole image. This brings a concern related to the method the authors use to perform the statistical analysis. For the purpose of demonstration, two clearly distinct zones corresponding to cirrus and low level liquid clouds have been presumably "hand-selected". We can wonder how the actual segmentation of the image would be performed in an operational environment to distinguish between the different cloud types. No indication is provided here to explain how the different clouds could be actually separated in order to allow for a statistical analysis to be performed over a scatter plot. This has to be discussed because the potential strength and interest of this method really is the statistical analysis approach and I can not imagine how it could be perform on single pixel observation. If the method was to be actually applied on pixel by pixel basis, then the whole method falls down to my opinion.

It is important that the authors provide evidence to support operational feasibility of the proposed method.

"Microphysical models"

I will not argue too much here about the microphysical models used by the authors for their simulation because this question is still very controversial within the community. For instance, the use of single ice crystal models with fixed dimensions in place of size distributions could be discussed. This is not a major issue here but the authors could justify briefly their choices (or at least identify the problem). However, I think the maximum and minimum dimensions used in this study can mask potential problems. The authors write (p 4469) that "... the difference between each cloud particle model will still subsist over the full range of reflectivity (Fig. 3a). For some viewing geome-

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tries there may be overlapping values for very small ice crystals and big liquid water droplets (Fig. 3b). This implies that the cloud phase discrimination may be critical for a little range of cloud particle models." I can hardly endorse this conclusion considering that an effective radius of 16 microns is definitely not that big for liquid droplets and that recent measurements during the INCA campaign demonstrated that impact of small ice crystals have been generally underestimated. Also, the use of spherical particles to simulate "small" ice crystals is arguable since it is now widely recognized that non sphericity has a large impact on optical properties of cloud particles. (ref. to numerous P. Yang, M. Mishchenko, H. Chepfer papers). Hence, I would expect the phase discrimination to be critical over a not so little range of cloud particles. As the authors mention, the shape of the scatter plot can help to remove these ambiguities but it implies that a single cloud (same microphysical properties) can be observed over a sufficiently wide range of visible reflectivity, otherwise the shape analysis does not help much I guess ?

Once again, this raises two concerns about (i) how are different clouds identified and separated from each other on an image (segmentation problem) and also (ii) to what extend can we consider that one microphysical model can be representative of all parts of a same cloud ? I am concerned here about convective systems with thick anvils and convective core for instance.

A discussion of these problems and an evaluation of their impact on the retrievals would be very valuable to the paper. Once again, operational constraint can be an issue which justify some crude assumptions but those should be clearly indicated.

Other comments and minor remarks :

It would be interesting to provide values of the asymmetry parameter together with those of the single scattering albedo. As people tend to use different microphysical models, these values could help interpreting results obtained from different authors.

For consistency with table 2 and 3 and to reply to my concerns about "microphysical models", could you present results of simulations for Re = 5, 10, 15 and 20 microns instead of 4, 10 and 16 ? (figs 1., 2., 3.a and 3.b)

p4471 + Fig5 : "For small values of reflectivity the similarity drops. This is probably due to the value of the surface reflectivity..." It could be also fractional cloud cover (non overcast pixels) or 3D effects. Can you estimate what surface reflectivity is needed to obtain better agreement (similarity) for small reflectivity (< 0.2) ?

I would definitely change the expression "robust method" (abstract and conclusion) to something else... I'd rather focus on the fact that this method is intended at operational use in processing of meteorological satellite data and has to be a compromise between accurate retrieval and the need for a fast, computationally inexpensive method (which can justify the limited number of microphysical models for instance).

TECHNICALS CORRECTIONS :

Fig 3b : change "Calculations are made for a solar zenith angle is 72 ... " to "for a solar zenith angle of 72". Same for 10.5 and 52.0

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 4461, 2003.

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