

Interactive comment on “Retrieval methods of effective cloud cover for the GOME instrument: an intercomparison” by O. N. E. Tuinder et al.

Anonymous Referee #1

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GENERAL COMMENTS

The paper by Tuinder et al. presents an intercomparison of fractional cloud cover products derived from GOME satellite measurements. As emphasised in its introduction, an adequate knowledge of cloud effects is of prime importance for several applications such as the retrieval and the geophysical interpretation of atmospheric trace species and actinic fluxes. Basics of typical retrieval algorithms are described, which helps understanding differences between the studied products. Small subsets of GOME effective cloud fraction data derived with those different algorithms are then compared to each other and also to synoptic surface observations.

Although the reported work is interesting and could help the GOME user with valuable and original information, there is a real concern with the scope of the paper. The way

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results are reported and the hasty conclusions are inconsistent with the apparent objective. On many occasions in the abstract and in the introduction it is evident that the objective of the work is linked with the use rather than the development of cloud fraction products. Statements like 'The retrieval of density columns ... are ... dependent on a correct description of the partially cloudy scenes in the field of view' suggest that the study aims at assessing to what extent current algorithms meet quality requirements specific to GOME applications. Even the title mentions '... cloud cover FOR ...' instead of '... cloud cover FROM ...' Within this scope, the presence of a cloud is less important than its effect on the target application. An algorithm can provide a very bad cloud product for weather forecast applications but a reliable product for the retrieval tropospheric species. Accordingly, the reader would rather expect a discussion on the usability of current cloud data products for the claimed target applications (tropospheric species and actinic fluxes) and, possibly, a few recommendations.

Unfortunately, the apparent objectives have been somewhat forgotten in the following sections. The conclusion tells us that ICFA reports lower values than other algorithms, but nothing about the impact of this result on the determination of ghost columns, air mass factors or actinic fluxes. A detailed study of this impact with systematic argumentation obviously falls out of the scope of the present paper, however, there is no doubt that the reader expects more than just a classification of the algorithms from lower to higher mean values. References to results from previous studies of cloud effects (e.g., Koelemeijer and Stammes) could improve the quality of the discussion.

Although crucial, the concept itself of effective cloud cover is not discussed satisfactorily. The prime objective of GOME cloud clearing algorithms is not to derive cloud parameters (actually, GOME is not designed as a cloud sensor and its type of measurement contains only limited information about clouds) but to take cloud effects into account, hence the wording of 'EFFECTIVE cloud cover' instead of simply 'cloud cover'. This aspect is partly eluded in the paper. Actually, what is the physical sense of an effective cloud cover? The notion of effectiveness raises many issues: from a physical

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point of view (and not a numerical point of view), how does an O2 A band derived cloud fraction compare with a broadband cloud fraction? Is a cloud fraction retrieved from measurements near 760 nm representative of the cloud effect on UV actinic fluxes? What is the impact of biasing the clouds to an optical thickness of 50? By the way, is GOME affected by clouds with small optical thickness? What would be the best suited effective cloud fraction to the calculation of ghost columns and air mass factors? Is a cloud fraction derived from statistics on broadband reflectance measurements suitable to the retrieval of molecules absorbing in the UV (O3, BrO, SO2, OCIO, HCHO)? To what extent does it make sense to compare synoptic broadband observations made by human eye to spectroscopic measurements acquired by a satellite spectrometer?

In its current form, the manuscript reaches poor conclusion and leads to a misleading perception of GOME cloud products. However, the well written description of the cloud algorithms is informative and the potential of the reported work is significant. Therefore the manuscript would be acceptable if it were reorganised and if the conclusion were further developed according to the aforementioned remarks. The referee suggests two ways of clarifying the scope of the paper. The paper is publishable as a modest contribution if it emphasises the comparison of cloud fractions independently of the application, or the concerns outlined here (concept of effective cloud cover, usability for intended GOME applications) can be addressed and the paper published as a significant contribution to the literature.

SPECIFIC COMMENTS

In general, parameters affecting directly and dramatically the radiative transfer (e.g. surface albedo and aerosols) are not taken into account properly in the discussions. Please explicit a little more their contribution and the way they are treated in the algorithms.

Page 624, Line 24: Only GOME papers are referred to, although cloud corrections have also been discussed for TOMS and other satellite sensors. Please add at least

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relevant TOMS references.

Page 625, Second and third paragraphs: Two beams are missing: light reflected by the ground or scattered by the PBL (significant in the visible), and light reflected by the sides of the cloud (acts as a lens).

Page 625, Line 26: 'THREE more methods': More than three methods have been developed.

Page 626, Line 10: What is the justification of selecting only August? Limiting the study to August cuts the range of SZA and of parameters affecting the radiative transfer in the troposphere such as atmospheric turbidity and ground albedo.

Page 626, Line 21: Please specify that the backward scan pixel is much larger and why it has been rejected for the present work.

Page 627, Equation (1): The cosine approximation for the length of the optical path is certainly valid in August (high sun elevation in the Netherlands) and in the NIR (where the O2 band is found), but not at shorter wavelengths and at low sun elevation. A strong aerosol load also modifies the path of the radiation. Is this simple formula used in any circumstances? Or are corrections used where the light path is not straightforward? If not, what is the impact of such an approximation, especially when several spectral ranges (R, G, B) are combined together?

Page 628, Equation (3): There is some confusion about λ used both as general argument and as integrand.

Page 629, Line 14: '...the ICFA method produces an effective cloud fraction ...': Actually, all methods produce an effective cloud fraction. See general comments on the concept of effectiveness.

Page 630, Line 1: 'Except for snow and ice': How do the different algorithms work under such circumstances?

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Page 635, Section 3.6: Please specify in this section that synoptic observations are made by human eye.

Page 636, Section 3.7.2: What could be said about snow/ice conditions? Are threshold-based cloud algorithms able to discriminate maximum reflectance values arising from high surface albedo and from clouds, respectively?

Page 636, Section 3.7.2, 2nd Paragraph: The ITCZ presents many days where clouds form after the passage of GOME, hence it is likely that cloud-free pixels occur with a sufficient frequency even within one month. On the opposite, polar areas in wintertime can be completely covered by clouds during longer periods.

Page 638, Section 3.7.5: The difference in observed field of view is well described, but the difference in sensitivity is not taken into account. The spectral sensitivity function of the eye is quite different from that of a spectrometer. See general comments on the concept of effectiveness.

Page 638, Line 26: It is said that clear sky composites were constructed matching at best the studied conditions, that is, August of the same year. Why have the authors developed their own databases instead of using the standard algorithms? The latter have the advantage of being published and used in the literature. How valid are present results for studies based on these published, standard products?

Page 640, Line 11: 'This has significant effect on the local actinic flux' Please illustrate shortly this effect.

Page 641, Line 25: The difference in RMS between OCRA and others does not seem to be significant compared to other differences.

Page 643, First sentence: This cloud-side effect affects satellite measurements as well.

Page 643, Line 26: Do the authors see an explanation for the observed discrepancies at large cloud covers?

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Page 644, Lines 13-15: Indeed, SYNOP ground-based observations seem to be biased compared to ATSR-2 and GOME satellite data. What is the opinion of the authors about this important result?

Page 645, First paragraph: Is that confirmed, or just a possibility? Please explain why it would not affect other methods.

Page 645, Line 22: It is not completely true to state that the 'capabilities' have been described. The scope of the reported results looks different. Please rephrase.

Page 646, Line 4, '... all four methods retrieve the cloud fraction as coming from ... a high effective optical thickness.': What is the impact of this bias on target applications? See general comments.

Page 646, Line 16, '... may be better candidates ...': This conclusion about the superiority of PC2K and FRESCO is somewhat hasty. The patchy behaviour of OCRA and its dependence on ICFA databases are certainly not the sole arguments to be taken into account. Please develop a more convincing argumentation.

Table 5: There are many tables in the paper, of which the usefulness can be questionable. This is particularly true for Table 5 where we learn mainly that SYNOP differs strongly from other cloud data. More generally, what could we learn from that kind of statistics? Are they representative or do they depend on the choice of the orbits?

TECHNICAL COMMENTS

For publication in a European journal, British spelling is preferred: 'behaviour', 'modelled', 'normalised', 'minimisation', 'optimisation', 'centre', 'analysed' etc.

Page 632, Line 5: The comma is not at the right place.

Page 634, Line 27: Please replace 'en' by 'and'.

Page 637, Line 2: Please replace 'uses' by 'used'.

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Page 643, Line 23: Please replace 'indicate' by 'indicates'.

Page 644, Line 19: Please replace 'surface areas' by 'surface type' or equivalent.

Page 660, Figure 4: What is the colour scale? If not done so, please use the same colour scale for both panels.

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 623, 2002.

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