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Atmospheric Chemistry and Physics Discussions

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

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1. General Aspects

1. What can be learnt from the paper in its present form?

The paper draws attention on the importance of accurate cloud products from GOME observations. This is required for two major reasons:

a) Direct interest in cloud properties and their impact on the radiative transport throught the atmosphere.

b) Impact of clouds on the retrieval of atmospheric trace gas products, especially for tropospheric species. Four cloud retrieval algorithms are compared to each other and to ground based cloud observations.

The presented work allows to get a (limited) impression on the (currently limited) quality of the GOME cloud products. Also some reasons for discrepancies are discussed and recommendations are given.

What could be learnt from the paper in an optimised form?

The potential of a comparison between different GOME cloud products is by far not sufficiently exploited. In particular, the following shortcomings were identified in the present version of the paper:

a) Only the cloud fraction is compared. However, some cloud products also include the cloud top height (e.g. PCRA and FRESCO). Also from the ground observations information on the cloud top height is available. The cloud top height is a very important information and it is strongly recommended to include it also into the comparison.

b) The ICFA algorithm is known to be subject to important shortcomings. This is also mentioned in the paper. These shortcomings are known within the GOME community, and for accurate cloud related studies the ICFA data should be used only very carefully. In this respect it is not understandable, why the ICFA algorithm is in the centre of the presented studies. It should, at best, be included among other cloud algorithms (since it was the first standard product of the official GOME products). The comparison should instead focus on the other cloud products, e.g. on FRESCO or PCRA which yield both cloud fraction and cloud height.

In particular, scatter plots between PCRA, OCRA, and FRESCO should be presented. Such comparisons would be very helpful for the reader and for the “GOME user".

c) Too much effort is spent on statistics and correlation plots but too less effort is spent to identify the systematic discrepancies between the different cloud products and the respective reasons. This is what the reader really needs!

What is time consuming and probably misleading in the present form?

Several Figures seem to be misleading, also information is missing in several Figures.

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Too much redundant information is presented. In particular, in Figure 11 only one GOME orbit would have been totally sufficient. Showing instead four orbits reduces the size of one single orbit plot and makes it hardly readable.

It is also questionable that many technical details of the individual cloud algorithms are presented. Here it would be much better to reduce the information to the physical principles, the main features of the algorithm, the most important shortcomings and adequate references. It is also questionable why in some cases the authors have developed their own version of the algorithms rather than used the standard version. This procedure needs at least explanation, but should better be avoided.

Summarising the above points I come to the conclusion that the paper needs fundamental reorganisation. However, since the addressed topic is of great importance I like to strongly encourage the authors to apply the recommended modifications.

2. More specific comments

1. Abstract. It would be very valuable to add some reasons for the observed discrepancies between the different cloud algorithms and to give some recommendations which cloud product may be used for which purpose.

2. Introduction, page 624, line 23. It is stated that the retrieval of trace gas columns or profiles depends “to some extend" on the “correct description of the partially cloudy scenes in the field of view". This statement must be formulated much stronger! Especially in the case of tropospheric trace gases (NO_2, SO_2, BrO, HCHO, H_2O) the cloud influence is usually the dominant source of uncertainty.

3. Introduction, page 625, line 18. Please add that also the sensitivity of GOME for tropospheric trace gases can be significantly different for the discussed cases.

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4. Introduction, page 626, line 10. It is absolutely unclear to me, why the three months (August of 1997, 1998, and 1999) were selected. At least some explanation would be needed. However, it seems to me that it would be much more helpful and inspiring to select different months in different seasons, when the meteorology and/or the ground albedo is systematically different.

5. Section 3.2 (also relevant for the following sub sections). It would be helpful to the reader to shorten the whole section to the essential minimum, to include relevant references instead, and to directly discuss the shortcomings (section 3.7.1).

In particular, it should be stated, that ICFA has strong limitations (especially because of the usage of a climatological cloud altitude), and that ICFA should be seen as a more traditional “standard product".

6. Section 3.2, page 628, line 3. The reflectivity inside the absorption band also strongly depends on the optical depth, and the ground albedo.

The decisive difference for the reflectivities inside and outside the absorption band is that the reflectivity outside does NOT STRONGLY depend on the cloud altitude.

7. Section 3.3, page 629, line 22. Although two of the included cloud algorithms yield also values for the cloud top height (FRESCO and PCRA). Also from the ground based cloud observations information on the cloud altitude is given. It is therefore a mystery to me why the cloud top height is not included in this study. It is an important loss for its own. However, probably even more important is, that necessary information for the interpretation of the results of the cloud fractions is thrown away.

8. Section 3.3, page 630, line 20. Also an important reason for small variations of the measured intensities could be changes of the solar zenith angle during the selected period.

9. Section 3.4, page 633, line 7. Since ICFA was used for the calibration of OCRA, the known and important shortcomings of ICFA will influence the OCRA results. This

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must be seen as a fundamental limitation of OCRA and should be stated. Also some indication should be given on the magnitude of the effect.

10. Section 3.4, page 633, line 10. Why have the authors determined different scaling factors than the author of the original algorithm? How meaningful can then the results of this study be for the original OCRA cloud product?

I really wonder why the authors did not apply the original values. Did they at least communicate with the author of the original version of OCRA (Albrecht von Bargen) and what was his opinion on this?

11. Section 3.5, page 634, line 27. There are several differences between ICFA and FRESCO. Please insert “most important" before “difference".

12. Section 3.7, page 635 and following pages. I suggest to include the sub sections about the shortcomings of the different cloud algorithms directly into the describing sections.

13. Section 3.7.5, page 638, line 13. The results of GOME are strongly dependent on the solar zenith angle, which changes systematically with latitude. For many products of GOME (including the cloud data) the derived results therefore must downright be expected to be systematically different for different places on the world.

14. Section 4.2, page 641, line 11. The large underestimation of SYNOP cloud fractions by the ICFA cloud fractions (39%) must have been expected because of the strong difference in the respective pixel sizes. This finding, however, does not tell anything meaningful about the ICFA algorithm itself. Therefore, this circumstance should be either mentioned here or the comparison between ICFA and SYNOP cloud fractions should be excluded at this place.

15. Section 4.2, page 641, line 17. It is stated that PCRA and PC2K “behave very similar". This statement seems to be symptomatic for the whole paper. A direct comparison of both PMD algorithms would have yielded very valuable information for the

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reader and GOME data user. Instead, however, both PMD algorithms were compared separately to a third algorithm, ICFA. Especially, here ICFA is not very well suited for this comparison because of a) its important shortcomings and b) the large ground pixel size compared to the PMD measurements. From the results of the comparisons with ICFA, then conclusions are extracted about the relation between PCRA and PC2K.

Please insert instead a direct comparison between all involved PMD cloud algorithms!

16. Section 4.2, page 641, line 22. This information would have been very valuable in section 3.3

17. Section 4.2, page 642, line 17 and following lines. This is an important finding! However, especially here additional information on the cloud top height (e.g. from PCRA) would have been very useful.

18. Section 4.2, page 642, line 20. It is stated that a change of the cloud top height would change the reflectivity or radiance at the top of the atmosphere. However, the most dominant effect of a changing cloud top height is very probably on the optical depth of the O_2 absorption (because part of the total O_2 column is shielded by the cloud) and not on the “radiance" (what was meant here: inside or outside the absorption band?).

19. Section 4.3, page 643, line 8 and following lines. The explanation to Fig. 9 states that SYNOP observations are compared individually (left) or averaged over a GOME pixel (right). Since FRESCO refers naturally to the whole GOME pixel the inclusion of FRESCO in both sides of Fig. 9 is confusing to me. What is shown here and most notably: what can we learn from these findings?

20. Section 4.4, page 645, line 1 and following lines. While the general reasoning is convincing here, I am surprised why the ground albedo should be wrong over the desert. It was stated in section 3.5 that the surface albedo used by FRESCO is determined from GOME observations. Also, the albedo of the desert is not expected to vary

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too much with time and the cloud cover can be expected to be generally low. Thus, especially over the desert the albedo data should be accurate. What is the explanation to this point?

21. Section 4.4, page 645, lines 6–13. This finding can be expected because of the properties of ICFA. It adds no useful information here and should be excluded.

22. Figure 1. In the left part one important light path is missing: Especially for small optical depth also light from the ground can reach the satellite instrument. This is in particular important for the observation of tropospheric trace gases.

23. Figure 7. I recommend to change the x-axis and y-axis for all plots. All plots include ICFA data, thus ICFA cloud fractions should be plotted at the x-axis.

24. Figure 9. X-axis and y-axis should be changed because the SYNOP observations constitute the common relationship of all plots.

25. Figure 9. The plot at the bottom left should be excluded. It seems to be misleading; the relevant comparison (over the whole GOME ground pixel) is shown in the plot at the bottom right (see also point 19).

26. Figure 11. Here, too much “information" is given! As a consequence, important findings which might be in, tend to become invisible. Specific recommendations are:

a) to reduce this comparison to only one GOME orbit. It is already stated in the text that “the behaviour along the orbits is more or less the same". Also, no specific differences between different orbits are discussed in any detail.

b) to exclude the plots showing the difference between ICFA and the other methods. Since the ICFA cloud fractions are subject to systematic errors because of the fixed cloud top height, ICFA is no good standard for the comparison.

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3. Additional, more technical comments

1. Introduction, page 625, line 26. In recent years more than three other methods were developed (e.g. Cloud Retrieval Algorithm Using Image Sequence Analysis (CRUSA), Wenig, M., Satellite Measurement of Long-Term Tropospheric Trace Gas Distributions and Source Strengths – Algorithm Development and Data Analysis, PhD-Thesis, University of Heidelberg, Germany, 2001). Please insert “at least" before “three other methods".

2. Section 3.2, pages 628 and 629, equations 3 and 4. I guess the integration should be over $d\lambda'$ rather than over $d\lambda$. Accordingly, $\Theta(\lambda - \lambda_i)$ should be replaced by $\Theta(\lambda - \lambda')$

3. Section 3.3, page 629, lines 19 and 21. Is “RFCA" correct? Or should it be “RCFA"?

4. Section 3.3, page 631, equations 11 and following equations. Please check whether it might be better to use “λ_i" instead of “λ". In lines 23 and 24 on page 630 and also in equations 7–10 you used “λ_i" to indicate the different PMDs.

5. Section 3.3, page 631, line 19. The subsequent introduction of the different versions of the PCRA algorithm (PCRA, PCFA, PC2K) is really confusing! Please clarify this at the beginning. Also please state why which version is finally used.

6. Section 4.1, page 639, line 18 and following lines. I wonder whether the term “anti-correlation" is appropriate for the findings in the histograms of Fig. 6. The scatter plots in Fig. 6 by all means show a correlation rather than a anti-correlation.

7. Section 4.2, page 642, line 26, 27. Please replace “caused by a number of factors" by “dominated by their different spatial extensions".

8. Section 4.2, page 642, line 28. Please add the information on the number of surface observations within a GOME ground pixel to the Figure caption of Fig. 8.

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9. Section 4.3, page 643, line 23. Please replace “indicate" by “indicates".

10. Section 4.4, page 645, line 2. What is meant by “$\pm$0.3"? A cloud fraction can not become negative.

11. Figure 4. A colour scale for the reflectance values is missing.

12. Figure 10. What can we learn from this Figure? I suggest to remove it.

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