

Interactive
Comment

Interactive comment on “The Heidelberg iterative cloud retrieval utilities (HICRU) and its application to GOME data” by M. Grzegorski et al.

M. Grzegorski et al.

Received and published: 18 July 2006

Point to point response to Anonymous Referee 1

We would like to thank the reviewer for his numerous, constructive and helpful comments on our paper. We tried to change several points mentioned by the reviewer but also disagree with some of the points raised by the reviewer. In such cases we give a detailed justification of our point of view. A detailed description of changes are outlined below. The comments of the reviewer are cited using italicized letters.

1) I find that the split of the introduction in two parts dedicated respectively to the description of GOME and to the description of the different cloud retrieval approaches is not appropriate. No such detailed descriptions should be present in the introduction. It

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

should be dedicated to a general description of the context and of the issues dealt with in the paper and to introduce the paper's outline. Subsections 1.1 and 1.2 should be inserted after the introduction.

1.2) This section describes a number of algorithms with the introduction of the corresponding acronyms (FRESCO, IFCA, GOMECAT ...). As I say below, it would be better to merge it with subsection 4.1 for clarity. Furthermore, the 2 last § are dedicated to the description of algorithms that are not used in the intercomparison. It may be interesting to mention them in the introduction, but their detailed descriptions is unnecessary. The description of the 7 algorithms compared to HICRU is complicated enough.

Sect. 1.2 is now inserted after the Introduction as Sect 2. The structuring of the introduction into subsections is now avoided. The introduction is therefore shorter now. But the text of Sect 1.2 is left in the introduction because of the following reasons:

- **important work related to the discussed subject should be mentioned at the beginning, including appropriate references**
- **the reader should know the major concepts of cloud retrieval before the new algorithm is described**
- **Sect. 4.1 and Sect 1.2 should not be merged, because they contain different subjects. Chapter 1 discusses the basic physical quantities used for cloud retrieval from GOME data (intensity based, O2-A band, Ring effect etc.). Chapter 4 discusses only the retrieval of cloud fraction, which is intensity based for all algorithms except ICFA (but ICFA is not discussed in detail, because the problems of the algorithm are discussed in the papers of Koelemeijer et. al.). In Sect 4.1, I therefore want to limit the discussion to the retrieval methods for an intensity-based cloud fraction, which is the basic information needed to understand the intercomparisons.**

The last two paragraphs of Sect. 1 indeed discuss algorithms not included in the intercomparison. As proposed by the reviewer, the discussion of these algorithms is reduced.

2)Section 2 and 3 both describe the HICRU algorithms. They should therefore be gathered in a single section untitled as section 2 with section 3 being 2.3) (see following comment about section 2.3).

Sect. 2 and Sect. 3 are merged as proposed by the reviewer.

2.1) From where comes the “daily solar spectrum” used in HICRU ? This should be stated in the paper.

The word "daily solar spectrum" is replaced by "solar spectrum from the operational dataproduct" to be more precisely. In 2001/02 for some of the months daily solar spectra are unavailable due to problems with the instruments (see discussion about degradation, Sect. 3.1, below).

2.3)This section starts with a rather weird title: “Color space analysis is NOT used by HICRU”! So why make such a detailed description of it ? The authors state that “This will be realized by algorithms currently in development”. I think this subsection therefore unusefully extends the length of the paper and should be kept for papers describing those developments. The important point of this subsection is the introduction of the CRUSA and OCRA algorithms used in the intercomparison. I therefore suggest that those algorithms have to be described together with the other methods in the merged 1.2 and 4.3 (see comment below) subsections before the intercomparison.

There seems to be a misunderstanding by the reviewer. Algorithms in development do NOT provide color space analysis. Moreover, the chapter discusses, why we think that the cloud algorithms are not improved through the usage of color space analysis. It seems, that the chapter is a little bit confusing and therefore the following changes are done:

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

- The arguments against color space analysis in general are added in Sect. 2.2. The major argument - PMD 1 should be omitted - is already stated at the end of Sect 2.2 and we add a sentence there, that we therefore do not want to use color space analysis.
- We only mention general arguments against color space analysis now and neglect arguments, which hold for special applications of color space analysis only.
- The beginning of Sect. 2.3 is added at the end of Sect. 2.2 (p. 1644, line 2-6 and line 24-25), but with several changes. The text p. 1644 line 7-23 is deleted.

3) This section is very important because it describes how the thresholds are retrieved, that is, the heart of the method. While the iterative method is correctly described with the help of Fig.2, some important issues remain:

3.1) The authors mention “the irregular instrument degradation dependent on the time of measurement” as an argument to use short periods of time, without further details. In order to assess the validity of this argument, it would be useful to have elements of answers to the following questions. What are the frequency and duration of those degradations and are they clearly detectable? What is their impact on the determination of cloud fractions?

The degradation of the instruments is discussed in Sect. 2.2 with appropriate references. It is impossible to describe the details of the degradation; this would be a large discussion containing only results from the cited papers. Irregular degradation is not clearly detectable, especially we have not distinguished between effects of instrument degradation and problems due to the lack of solar spectra in 2001/2002. We analyzed time series of the PMD intensities (corrected by SZA and solar spectra) as the algorithm directly uses them. From time series

we expect errors of the thresholds up to 8% for long periods of time through degradation and the lack of solar spectra for some months in 2001/2002. This error is now given in a table containing the pre-defined thresholds (see below). The error is lower for stage 2, 3, and 4 of the retrieval and generally the error in the cloud fraction is significantly lower than the error in the lower threshold, because the intensity of the cloud is higher than the intensity of the surface.

The word “predefined” appear twice in this subsection. Once about “the assumed maximum variation of the surface albedo” and once about the “sum of the average value and a predefined threshold”. What are those values ? Are they critical to the determination of the pixel cloud fraction ? Did the authors make sensitivity studies to optimize them ? With what results ?

In both cases the same thresholds are discussed. The thresholds are determined using time series of PMD intensities and case studies to estimate the expected maximum variation of the thresholds for the considered period of time due to the change of the surface albedo, the degradation etc. The values of the pre-defined thresholds are now presented in a table. The caption contains a short explanation including the estimation of the degradation effects. The details are not included in the text, because we want to keep the text as short as possible.

The authors mention periods of 25 days with a footnote stating that “in practice, only 9 days of data are considered”. I found this footnote confusing and I think a clearer formulation is needed. Is it 25 or 9 days ?

The footnote is changed to make the point more clear. The footnote describes an important detail, but the information is only useful for GOME specialists. We decided to provide the information as a footnote, because it is indeed confusing for people not specialized in GOME retrieval.

3.2 What does “Pixels definitely not representing completely cloudy pixels” means quantitatively ? Same question as before about the “PREDEFINED absolute and rela-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

tive thresholds”. Why are there a relative and an absolute threshold this time and how are they used ? The statement “more than predefined absolute and relative thresholds” is unclear.

As in Sect. 3.1, the thresholds are given in a table together with some information

4)In general, the intercomparison section is well structured and interesting with a general description and the case studies clarifying particular differences between the algorithms. The biggest discrepancies found in Fig. 6 are indeed addressed by the case studies and Fig. 7 to 10.

4.1)As I mentioned before, I think the details from the description of the other algorithms from subsection 1.2 should be merged with this subsection. This would have the advantage to clarify the structure and to avoid repetitions. The reader would have the characteristics of all the algorithms in mind when coming to the intercomparison and it would not be necessary to go back to the other subsections (1.2 and 2.3) of the paper to find the information.

We don't want to merge the Sect. 1.2 and 4.1 for the reasons discussed above

4.2.1) The PMD test algorithm is introduced for the first time at this stage to “support the interpretation of the data”. The usefulness of the PMD-test algorithm is supported by the intercomparison, but a brief description of the PMD test algorithm with the main differences with HICRU should be included in subsection 4.1 where all the other algorithms are described.

The characteristics of the PMD test algorithm are summarized in Table 2. The algorithm is treated in the same way as all the other algorithms. Maybe, the reviewer has not seen the table? I am sorry, that Table 2 had become very small through the editing process. The table is therefore hardly readable and should have a size of one DIN-A-4 page, but ACP was not able to produce a table larger than a half DIN-A4 page. But this will of course be changed for the final ACP

version, because the limitation exists only for ACPD. Nevertheless, I have added some additional information about the algorithm in a footnote of the table, because there are no references for the test algorithm.

About the comparisons with FRESCO: the authors compare results from HICRU to an old and a new FRESCO version. The old one has known “shortcomings” and will probably not be used anymore in the future. I suggest to briefly mention the improvements in FRESCO and their implication on the intercomparison with HICRU, but to eliminate the old FRESCO version from the plots and to focus the discussion on the new version to make the paper and the figures clearer.

5) I suggest to skip the discussion about the old FRESCO version in the conclusion as in the core of the manuscript.

In Sect 4.2.1 for some algorithms (HICRU, FRESCO, GOMECAT) different releases are included. In all cases significant conclusions are reached with respect to important aspects discussed in the paper like the influence of the surface albedo on the cloud fraction. Therefore the changes proposed by the reviewer are not included.

The term "old" version and "new" version of FRESCO may be confusing, if a new version of FRESCO will be released. Therefore a definition of old (version 1) and new (version 3) is added in the text and in the conclusions. Version 2 was not supported by KNMI and is therefore not part of the intercomparison. The data of version 3 is received from the TEMIS homepage. Version 4 is planned for mid-2006. Version 1 is used in several scientific papers.

4.2.4) This case study came unexpected to me after the two previous ones. Solar zenith angles are reappearing without any explanations. I knew from subsection 3.2 that they play a role in the determination of the upper threshold for HICRU, but I don't understand why they appear abruptly in subsection 4.2.4 while they were not discussed in 4.2.2 and 4.2.3.

The case study for high SZA might be somehow surprising, but we think it is important to demonstrate the "subpixel effect" using high solar zenith angles, because the subpixel-dependency of the upper threshold is especially important for high solar zenith angles (see Fig. 5). The case study is left unchanged, but we changed the title to "Case study over ocean: Subpixel dependency for high solar zenith angles" to make the aim of the study more clear.

4.3)The author should introduce this subsection by a sentence explaining why it is important to make a "detailed intercomparison between HICRU and FRESCO" and not with the other algorithms. Furthermore, I understand that the use of a complete month of data improves the statistical significance of the intercomparison, but some of the conclusions are similar to what was discussed before like the overestimation of the cloud fraction over deserts by FRESCO. The new point is the detailed explanation of the discrepancies concerning high cloud fractions. This subsection may therefore be a bit shorten. The 2 last sentences of the second § are rather long to explain differences for "0.25% of the measurements" which doesn't sound significant.

Sect. 4.1 explains, why especially FRESCO is important for the intercomparison. But we agree with the reviewer that the reasons should also be discussed at the beginning of this section. We therefore changed the first sentence, which now refers to Sect. 4.1. There are several arguments, why the intercomparison with FRESCO is especially important. This should be discussed in the introduction to the intercomparison in detail, because one reason is the different methods used by the two algorithms, which has to be explained together with the description of the different algorithms

Our responses to the other arguments of the reviewer are as follows:

- **The reviewer says correctly, that the intercomparison between HICRU and FRESCO is more significant if one month of data is used for intercomparison and that the differences between HICRU and FRESCO for high cloud**

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

fractions are discussed for the first time. These are important points. But I think that there are more significant conclusions in the Section. A further important point is the intercomparison between land and ocean. For ocean, we found a significant better correlation between HICRU and FRESCO (correlation coefficient > 0.99) as if all surface types are included. This is a further very important conclusion. Note, that intercomparison between FRESCO and HICRU is especially important because both algorithms use a similar concept of effective cloud fractions, but different detectors and completely different retrieval methods. This is not the case for the other algorithms. The effective cloud fraction is the most important cloud quantity for tropospheric trace gas retrievals.

- The differences between HICRU and FRESCO for low cloud fractions have to be discussed again, because the plots are showing features, which are not present in the figures discussed before. This is done in the paper.
- We disagree with the reviewer, that 0.25% of the measurements discussed in two sentences of the second paragraph are irrelevant. These measurements all refer to very low cloud fractions, which is the most important case for the correction of tropospheric trace gases. Further on, the differences can be quite high (up to 20% effective cloud fraction). The reasons for these differences are still unclear, but the findings should be stated in the text.

Overall, there are several important conclusions in Sect. 4.4. A text covering one column of a ACP-DIN-A4 page and 3 figures are adequate to describe these conclusions. Therefore the chapter is unchanged except of the first sentence.

The English is improved through further proofreading and I am grateful to the reviewer for his corrections.

Regards,

S1872

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Michael Grzegorski on behalf of the co-authors,

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 1637, 2006.

ACPD

6, S1864–S1873, 2006

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

S1873

EGU