

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

Anonymous Referee #1

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General comments :

This paper describes an empirical cloud retrieval algorithm developed in order to improve trace gas retrievals from the GOME and SCIAMACHY instruments. It makes use of a threshold method applied to measurements by the Polarization Monitoring Devices (PMD). The main advantage of using the PMD's being to improve the spatial resolution relative to what is currently achieved with the detectors dedicated to trace gas retrievals. The description of the method is followed by a thorough comparison with the other existing algorithms. The issue is important and concerns a wide community of scientists working with and using GOME and SCIAMACHY data. Furthermore, the paper contains some convincing arguments about the method and about the improve-

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ments it brings relative to the other methods presented. I therefore think this work is suitable for publication in ACP. Nevertheless, I find that the manuscript is difficult to read because of its structure and of the repetitions that it implies, making sometimes the manuscript unnecessarily long. This problem unfortunately prevents the reader to sort out the main results from this thorough study. At the same time, some particular points are lacking of precision with, for instance, the extensive use of the word “pre-defined” which doesn’t say much without further details. I give the following specific comments in order to help the authors to improve their manuscript.

Specific comments :

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 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.

2)Section 2 and 3 both describe the HIRCU 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).

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

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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.

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 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?

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?

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

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ative 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.

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.

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.

About the comparisons with FRESKO: the authors compare results from HICRU to an old and a new FRESKO 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 FRESKO and their implication on the intercomparison with HICRU, but to eliminate the old FRESKO version from the plots and to focus the discussion on the new version to make the paper and the figures clearer.

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

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understand why they appear abruptly in subsection 4.2.4 while they were not discussed in 4.2.2 and 4.2.3.

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 shorter. The 2 last sentences of the second § are rather long to explain differences for “0.25% of the measurements” which doesn’t sound significant.

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

English:

Even though I am not a native english speaker myself, I found a number of syntax and grammatical errors in the manuscript. I therefore suggest a careful check of the text by a native speaker if possible. I just give a few corrections about some obvious mistakes I found in the manuscript.

p. 1640 l. 17: “affected” p. 1641 l. 23: “found” p. 1642 l. 16: the sentence is not correct “...that a cloud is a ... and that a GOME pixel...” p. 1642 l. 20: “depends” p. 1642 l. 22: “is especially” p. 1643 l. 6: “cause problems” p. 1643 l. 9: “choose” p. 1645 l. 11: “would only work” p. 1646 l. 11: “is then compared” p. 1648 l. 14: “with other assumptions” p. 1650 l. 18: “is included” p. 1651 l. 9: incorrect sentence which gives to understand that the “database” is retrieving “monthly albedo maps”. p. 1655 l. 29: “representative” p. 1656 l. 7: I would replace “assumed” by “retrieved” p. 1657 l. 9: I would replace “specialities” by “specificities”.

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