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Interactive comment on "Distinction between clouds and ice/snow covered surfaces in the identification of cloud-free observations using SCIAMACHY PMDs" *by* J. M. Krijger et al.

Anonymous Referee #2

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Review on the manuscript:

'Distinction between clouds and ice/snow covered surfaces in the identification of cloud-free observations using SCIAMACHY PMDs' by

J.M. Krijger, I. Aben, and H. Shrijver

The paper addresses a very important issue: the discrimination of cloudy and ice-covered scenes in satellite cloud retrievals. Such an identification can be interesting in itself and for the investigation of cloud properties. It is definitely important for the cor-

rect interpretation of remote sensing of tropospheric trace gases from space. The work convincingly demonstrates the principle possibility to distinguish between clouds and ice/snow covered surfaces using observations from the near infrared spectral range. They authors also develop a simple cloud algorithm using experimentally determined threshold values for the characterisation of the properties of individual satellite observations. This algorithm seems to work well for the selected case studies, but I have the impression that by far not the optimum use was made from the available information. In particular not enough emphasis is given to the physics 'behind' the algorithm. Instead, arbitrary thresholds are selected to satisfy arbitrary criteria. From a paper which introduces a new technique I would expect a deeper discussion of the most important fundamental mechanisms (scattering on cloud particles versus reflection on the surface) as a function of wavelength. I have also other major concerns:

-the presented algorithm is based on assumptions which are not proofed to be fulfilled (in particular concerning the dependence on viewing angle and SZA).

- no comparison with established cloud algorithms for UV/vis observations (e.g. FRESCO) is performed. Thus the quality of the part based on the visible PMDs can hardly be assessed.

-the algorithm is adjusted (and validated!) using collocated MODIS observations. No sufficient information on the details and the quality of the MODIS cloud results are given.

In the current version the paper is not suited for publication. After addressing the raised points I recommend publication in ACP.

Specific points:

1) In the abstract some examples of troposheric trace gases are given. However, several important gases are missing, e.g. BrO, HCHO, SO2, H2O, and CO. Especially BrO and SO2 is often observed over snow and ice-covered surfaces.

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2) line 25: I suggest to replace ‘or’ by ‘and especially’

3) line 33: I suggest to remove N2O

4) line 54: better 790 instead of 800 nm to be consistent with abstract.

5) lines 57-59: Add HICRU algorithm and respective reference, e.g.

Grzegorski, M., C. Frankenberg, U. Platt, M. Wenig, N. Fournier, P. Stammes, and T. Wagner, Determination of cloud parameters from SCIAMACHY data for the correction of tropospheric trace gases, Proceedings of the ENVISAT & ERS Symposium, 6-10 September 2004, Salzburg, Austria, ESA publication SP-572, (CD-ROM), 2004.

6) equation 1 and following paragraph: This information is never used in the following and is thus misleading. Just say that the polarisation sensitivity of the PMD 2,3,4 is small and is thus neglected.

7) line 93/04: This statement is not clear to me. The second step is also part of the ‘cloud algorithm’?

8) line 120: Fig. 2 should be Fig. 1? (real Fig 2 is never mentioned in the text)

9) Fig. 1 & Fig. 2: Both plots are ‘nice’, but what do they really tell? (What do you want to tell the reader?) The assignment of colours to the different PMDs is arbitrary and has no meaning. I recommend to remove both figures.

10) line 130: What do you intend to say with ‘The image shows that PMD 2,3, and 4 can be used as broad band intensity measurements’? Isn’t this clear at all?

11) Fig. 3: Why do you show a colour composite of PMD 4,5, and 6? There is no expression of the PMDs by real colours at all! This is confusing and misleading. If you want to show that PMD 5 can be used to discriminate between ice/snow and clouds, then just show individual figures for PMD 4 and PMD 5. You don’t use PMD 6 later on. Why do you use it here?

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12) line 150: What is the justification for the assignment to different colours? Non of the used PMDs cover the colour which is assigned.

13) line 154: What is the justification for the expectation that for cloudy scenes the intensities should be equal? Wouldn't this depend on the cloud type, altitude and solar zenith angle?

14) line 155: What do you mean with 'visually inspected'?

15) line 158: Why do you use two names 'whiteness' and 'saturation'? This is confusing.

16) line 163: You state that your saturation value does not depend on viewing angle and solar zenith angle. There is no justification given for this statement. I have doubts that it is really true. I would especially expect a dependence on the solar zenith angle. Please provide a diagram showing your saturation values as a function of viewing angle and SZA.

17) paragraph after line 165: Please provide references for the MODIS algorithm. How are the MODIS cloud mask values are derived?

18) line 181: What is the justification for averaging the MODIS cloud values? Are the values proportional to cloud properties, e.g. cloud fraction?

19) line 185: What do you mean with 'wrong signature'? How well does the MODIS algorithm describe reality? Are there any validation results?

20) line 205: For your case study you selected scenes with sharp cloud contours. What will happen for optically thin clouds without sharp contours?

21) Did you compare your algorithm (visible part) with existing cloud algorithms (e.g. FRESCO)?

22) line 239: An insufficient calibration should only result in a bias of the ratio of PMD 5 and PMD 4. (If signal to noise is really a problem, you can't use the PMD observations

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at all). It would be interesting to see, if the ratio shows a lower fixpoint for snow/ice covered surfaces.

23) line 243: Again you state that the ratio is independent on viewing angle and solar zenith angle without proof. Please provide a figure with ratio as function of SZA and viewing angle. (What is the characteristic of the BDRF of snow/ice?)

24) line 250: How strongly does the surface albedo of snow and ice differ for different conditions (e.g. new or old snow)? How would this affect the ratio?

25) Fig. 7 & Fig. 8: You find different distributions for Antarctica and Europe and you adjust your thresholds accordingly. Do you have an explanation why the ratios for cloudy scenes are different for both scenes? Could this be a remaining solar zenith angle dependence? (see points 16 and 23).

26) section 5: This is no independent validation, because you compare your algorithm to another algorithm to which it was previously adjusted.

27) Fig. 5, 10, 11: I suggest to add 'MODIS' to the legend.

28) Fig. 6: What is the reason for the different spectral dependence (of blue and red line) within the spectral range of PMD 4?

29) Do you have an explanation for the 'blue values' with ratios > 0.4?

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 815, 2005.