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Atmospheric Measurement Techniques Discussions

Interactive comment on "Cloud detection for MIPAS using singular vector decomposition" *by* J. Hurley et al.

Anonymous Referee #1

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General Comment:

The paper by Hurley et al. presents a new method for the detection of clouds in IR emission spectra by using singular vector decomposition (SVD). The method is described in detail and first comparisons with other standard detection method, like a colour ratio based method, show the excellent sensitivity and improvement of the novel approach. The paper is well structured and concentrates on the technique itself and first applications, therefore AMT seems the right place for publications. However, parts of manuscript show severe technical weaknesses (missing and unused references, figure ordering, etc.). For the resubmission the authors should take adequate care on the inner consistency of the manuscript. Finally, there are some specific comments (see below) the authors should address in more detail.

Specific comments:

1) The problem of misinterpreted large water continuum as cloudy spectra is mentioned when the cloud index approach is described. However there are no comments on this topic when applying the SVD method to the modeled spectra data base. Can you please specify if the SVD method can avoid this problem, which would be definitely a substantial improvement compared to other methods and if this is the case it should be highlighted in the summary and/or abstract.

2) The 2nd paragraph of section 3.1.1 should be restructured. Some statements are not comprehensible (e.g. p1190, l22 see below). Please reorganise figures and make more clear why refinements in the CI threshold will 'result in obviously clear cases being almost completely flagged as clouds', which is not obvious from Figure 2 and 3.

3) Section 4: As far as I know the radiative transfer model RFM in not including single or multiple scattering effects. The author should address why scattering isn't an issue for the SVD approach, (which is based on these modelled spectra) and if potential uncertainties are introduced by this simplification. In addition, horizontal homogeneity of the cloud layers seems to be assumed in the model calculations and should be stated in the text.

4) Section 5.1: From Figure 4 only 80% instead of 90% are related to the first three singular vectors. But this should have no implications on the further analysis.

5) Section 7: The discussion of the noise error is difficult to follow should be rewritten (last two paragraphs). The 'order of 10000' for the fit coefficients is given without physical units.

6) The detection of Polar Stratospheric Clouds (PSC) haven't been investigated in this paper. However, it looks obvious that this is an useful approach of the method as well. At some place in the manuscript this restriction should be clearly stated (tropospheric clouds only) and an outlook on PSC detection with the SVD method may be given. Is

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the noise error a potential problem for the PSC detection?

7) Section 8: The method ends up using only the 960-961 cm^{-1} range for the definition of a threshold profile. Why it is better to use this small range instead the information content of the full range (827-970 cm^{-1})?

8) The threshold method has been applied down to 6km (Fig. 10). The PDFs of the lower altitudes are looking quite different to the levels above, which indicates potential problems with the method at the lower levels. The author should give some information on potential altitude restrictions of the method.

9) Conclusions: The Effective Fraction threshold of 0.0025 for the SVD method should be introduced somewhere in the sections before. It is also not obvious from the presented figures, how such a precise and quite small threshold can be deduced.

Minor comments:

p1189, l19: The given threshold of CI$>$4 for cloud-free conditions is only correct for a specific selection of micro windows like the ESA operational cloud index. The corresponding wavenumber regions should be indicated.

p1190, l22: It is not obvious to the reader why the CI method should incorrectly diagnosing many clear spectra as cloud-contaminated. Please present some details.

p1191, l18: The problem with the water continuum is already quantified in Spang et al., 2004. The authors should at least add this reference to the Greenhough one or just replace it.

Technical corrections:

The order of Figure 2 and Figure 3 needs to be swapped.

Fig 3: units of x-axis seem to be wrong if one compare with figure 11, please check.

Figure 10 and 11: Time period or number of profiles as well a potential latitude restric-

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tion should be stated in the figure legend.

References in the text should be given in the format 'Author et al., YYYY' if the list of authors of the cited paper includes more than two persons (not just 'Author, YYYY').

p1187: Reference for citation 'SAGE, 2002' and 'ISCCP, 2008' are missing.

p1189: The citation 'Dudhia, 2004' is in conflict with 'Dudhia, 2005' in the reference section.

References in general: The author should carefully check the used references in the paper. There are a number of missing references (see above, or Remedios, 2001, etc.) or not used references in the reference section (e.g. Baran et al., Baum et al.):

page 1191: typo - misdianosed change to misdiagnosed

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 1185, 2009.