

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

Anonymous Referee #2

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General:

The authors present a new method for detection of cloud from MIPAS limb-emission spectra.

Since this is a novel and promising technique which is clearly presented, the paper should be published in AMT. However, beside some minor modifications, the comparison with the operational cloud detection algorithm is insufficient and should be extended (for details: see below).

Specific:

p. 1186, l. 2-5:

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For my feeling the first two sentences of the abstract are more a motivation belonging into the introduction but not in the abstract. In the rest of the abstract: concentrate more on summary of work done and give some more quantitative conclusions.

p. 1186, l. 12: 'filling more than 30% of the measurement field-of-view (FOV).'

Where does this number stem from? The statement also does not include the cases of thinner clouds.

p. 1186, l. 17: very sensitive even to thin clouds only marginally filling the MIPAS FOV'

However, the settings of the operational method are adjusted to avoid thick clouds but not to detect very thin ones. So this cannot be given as an argument in support of the new method (see comment on chapter 9 below).

p. 1186, l. 26: However, current cloud detection algorithms often miss much thin cloud in satellite measurements'

It should be pointed out that this is not only due to the detection algorithms, but to the sensitivity of the measurement technique (e.g. nadir vs. limb).

p. 1187, l. 18: '(primarily for trace gases such as CO₂, O₃. . .)'

Perhaps better '(primarily for temperature and trace gas retrievals such as O₃. . .)'

p. 1187, l. 25: 'measurement frequency increased to nominally every 1.5 km in the troposphere'

It should be clear that this is the case not only in the troposphere but also in the lower stratosphere. Further the term frequency might be misleading? Perhaps better: tangent point difference.

Chapter: 4 Ensemble of simulated clear and cloudy MIPAS spectra

Here the assumptions/simplifications made for the radiative transfer simulations of cloud are missing, e.g. is the cloud considered as horizontally homogeneous (sym-

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metric with respect to the tangent point), have single/multiple scattering effects been taken into account, what kind of cloud has been simulated (composition, particle size distribution)?

Later (e.g. in the conclusions/discussion part) there should be some discussion on possible effects of the applied assumptions on the conclusions of the study.

p. 1194, l. 25: 'to share reconstructive responsibility of the raised cloud radiance baseline'

Could you explain this statement a bit more detailed? I've understood that the integral of the spectrum is a clear signal for clouds? Does this not reduce the sensitivity of the method to the cloud signal?

p. 1195, l. 24: 'since any of the initial pieces of information (here, the spectra) can be reconstructed by using as few as three singular vectors'

Isn't this formulation a bit too idealized? With a limited number of singular vectors the spectra can of course not be totally reconstructed but only within some error margin.

p. 1195, l. 24: 'The basis of this work is the hypothesis that a cloud-contaminated spectrum can be decomposed into components coming from the clear atmosphere and those due to the cloud itself.'

Radiative transfer in general is a non-linear problem. Could you try to give some justification why this assumption of linear combination should work here even in case one deals with cases which are optically not thin?

p. 1196, l. 18: 'of cloud singular vectors v_{cloudy_i} which are orthogonal to the clear singular vectors v_{clear_i} '

Is each cloud singular vector really orthogonal to each clear singular vector? Could you explain why?

p. 1198, l. 1-13 and Figure 7

Could you comment on the fact, that apparently only the first cloudy singular vector seems to be necessary to simulate cloud. How much are the singular values of the 2nd singular vector smaller than those of the first one?

Figure 8

The numbers given for the errors in the titles are strange: the differences between the red and the black curve seem to be much less. Are these integrated error numbers for the whole spectral range? In that case the units are wrong.

p. 1200, l. 6: 'For the case at hand, whereby $\sigma=50 \text{ nW} \dots$ '

Is this the noise for the unapodised or the apodised measurement?

p. 1202, l. 17:

Could you give the reasons why only the reconstructed radiance in a small spectral interval is used for cloud identification instead of e.g. the magnitude of the first singular value of the decomposition or the integral over the whole reconstructed cloudy radiance divided by the clear radiance?

p. 1203: Chapter 9, Comparison of current cloud detection methods

In my opinion this comparison is insufficient, since the threshold of the operational cloud detection method is not intended to detect even thin clouds, but only those which could strongly affect the temperature/trace-gas retrievals from MIPAS observations. This at least should be mentioned clearly and a comparison should be made with cases where the cloud-threshold of the operational method is set more sensitive.

This is strongly connected with a further possible advantage of the singular value method which should be discussed/quantified: the differentiation between a cloud and a high water-vapour atmospheric continuum signal which is a problem when a more sensitive cloud-threshold of the operational method is applied. Perhaps this could be performed on basis of the simulated cloudy/non-cloudy spectra.

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Technical:

p. 1186, l. 6: 'high-resolution'

Please specify more clearly that high-resolution in the spectral space is meant.

p. 1191, l.16:

Misdianosed -> misdiagnosed

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

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