

## ***Interactive comment on “The benefit of limb cloud imaging for tropospheric infrared limb sounding” by S. Adams et al.***

### **Anonymous Referee #2**

Received and published: 30 March 2009

#### **General:**

With regard to cloud detection, the paper discusses the advantages of a novel type of infrared emission limb sounders based on 2d-array detector technology. By simultaneously observing the limb scene with high spatial resolution it is possible to deselect pixels affected by cloud from the ensemble of those being averaged in order to obtain spectra which can be used for temperature and trace-gas retrievals. The structure of the manuscript is clear, it is well written and regarding the content I have only minor comments as listed below.

#### **Specific:**

*p. 592, l. 4: ‘In order to achieve the required spectral resolution by the Michelson*

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*interferometer (i.e. in order to resolve the Haidinger fringes (e.g. Hecht and Zajac, 1974) at all path differences) the maximum size of a single pixel of the detector array is limited and is smaller than required for most scientific purposes.'*

This sentence is rather difficult to understand and perhaps not conclusive. In which respect is it 'smaller than required'? – in the vertical it is as small as required for the Dynamics mode. Further, is the pixel size really the design-driver under the aspect that there are other variables which could be adjusted (e.g. the internal angle of the light rays).

*p. 592, l. 10-20*

Can you state the baseline for the given measurement modes? I think they are not exactly those given in the Mission Assessment report of the PREMIER proposal (different spectral resolution and along-track sampling of the chemistry mode.)[http://esamultimedia.esa.int/docs/SP1313-5P\\_REMIER.pdf](http://esamultimedia.esa.int/docs/SP1313-5P_REMIER.pdf)

*p. 593, l. 25*

Here it may not be clear to the reader why a combination of two methods, one which is not sensitive to thin cloud and one which is not validated with respect to thin cloud occurrence should be of advantage for the present study. Perhaps it has to be stated at this point explicitly that in case of thin clouds the presented investigation might have larger uncertainties.

*p. 594, l. 22: 'and 0.5 km vertically'*

Can you give a reference for the 0.5 km vertical resolution of the BT-cloud field?

*p. 596, l. 6*

Is there a reference for the quoted numbers of cloud sensitivity or can you give an explanation under which conditions they have been derived? Additionally, what are the lowest IWCs present in the COSMO-EU dataset?

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*p. 596, l. 10-17: 'So far we have considered the LOS to be an infinitesimally thin line...'*

It is not really clear if the FOV and the movement of the satellite, described in the paragraph, is taken into account for the following investigations.

*p. 597, l. 23: 'the optical thin clouds, which are in particular subvisible cirrus, are not detected by the nadir sounding geostationary weather satellites because their emissions relative to the strong emissions from the ground and altitudes below the level of interest do not induce a clear signal at the sensor.'*

In principle the statement is correct, but perhaps only 'their emissions' could be misleading because the detection is due to absorption (of lower atmospheric radiance) and emission of the cloud. The underlying quantity is the optical depth of the cloud.

*p. 597, l. 25... p. 598, l. 5*

'Opaque' is used in the comparison with SAGE on the one hand for opaque in nadir direction and on the other hand for opaque in limb-direction. It should be made clear which 'opaque' is really meant.

*p. 597, l. 25: 'With an instrument measuring only at C-mode resolution a great many profiles were not usable for trace gas retrieval.'*

It should be pointed out that this is valid under the assumption that, if only a very small part of the C-mode FOV is affected by cloud, there is no temperature or trace gas retrieval possible at all, which is a very strong assumption. Perhaps you can make a reference to chapter 4.3 where it is more relativised.

*p. 602, l. 23: 'The method for deriving the BT cloud data underestimates the cloud occurrences in the midlatitudes'*

Can you tell why this is the case, in addition to the insensitivity to thin clouds? Is this also described by Kerridge et al., 2004?

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## Technical:

*Figures 7-9*

'LME' is used in the figure title instead of COSMO-EU, but it is nowhere explained.

*p. 596, l. 3*

(') after 'track' missing.

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Interactive comment on Atmos. Meas. Tech. Discuss., 2, 589, 2009.

# AMTD

2, C15–C18, 2009

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