Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2015-326-RC2, 2016

Atmospheric Measurement Techniques Discussions
Open Access
EGU

Interactive comment on "Cirrus cloud optical and microphysical property retrievals from eMAS during SEAC^{4}RS using bi-spectral reflectance measurements within the 1.88 µm water vapor absorption band" *by* K. Meyer et al.

Anonymous Referee #2

Received and published: 11 February 2016

General comments:

The work presented in this paper focuses on an alternative dual-channel technique to retrieve thin cloud properties from airborne spectral measurments. The paper is well written and can be of interest for any reader willing to implement a similar method with other instruments. The technical procedure is described in details and leaves no room for scientific ambiguity.

Final publication in AMT should be obviously warranted, since just few minor changes would be needed to improve clarity also for the non-experienced reader. However, one

Full screen / Esc

Printer-friendly version

Discussion paper

major issue is still unclear and namely: why have land pixels been discarded in the analysis?

In Page 8 line 163 and following it is stated that: *In addition, the case studies selected here only include ocean scenes for which the surface is dark, thus the contribution of surface reflection to measured TOA cirrus reflectance is expected to be negligible*

But in the abstract the algorithm is termed to be very effective in surface screening and in delivering more accurate results. I would have expected the inclusion of a bright scene case study along with the analysis of a dark water surface. How can be the effectiveness of the algorithm be judged if the underlying surface is barely reflective?

Even if it can be clearly guessed by the experienced reader that the influence of the surface albedo is minimized, I think that it should be made explicit. Therefore I suggest to investigate the dependency of the 1.83/1.93 mkm retrieval technique also as function of surface reflectivity with an another case study and provide a table with relative errors in COT and CER among the presented algorithms and methods.

Minor comments:

p7 l155: Only the range 1.83-1.93 mkm is shown in Figure 1. It would be nice to show also the 1.38 mkm for illustrative purposes.

p12 l254: Notwithstanding the remarkable resemblance between image c) and d), could you please provide a justification of neglecting the error in the cloud phase discrimination? I think it is important for the reader to understand the assumption behind this choice.

Why aren't low-level warm clouds - in a) the brightest cumulus and in c) the red-coded at the top scans - showing up in d) too? Is it because of the discussion on the coverage of the LUT (p12 l264) or because of a suboptimal flagging?

p13 l294 and ff: The discussion on plot c) on CTH. It seems to me that the OE-IR mehtod delivers considerably correlated results across variables, namely CTH, CER

and COT. Could you provide more details on this, even if OE-IR is not really the focus of the present manuscript?

Additional details on the cloud phase (discrimination, see above) also would be appreciated, because when looking at Fig.5-c, is difficult to see whether the green and red lines are all in ice and not mixed-phase.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2015-326, 2016.