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Atmospheric Measurement Techniques Discussions

Interactive comment on "Thermal infrared remote sensing of mineral dust over land and ocean: a spectral SVD based retrieval approach for IASI" *by* L. Klüser et al.

L. Klüser et al.

lars.klueser@dlr.de

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The authors thank the anonymous referee #1 for her/his very constructive review of the manuscript. The text of the manuscript will be revisited and shortened, where suggested. Moreover we will follow the suggestions of the referee by adding tables and a retrieval flow chart for supporting the descriptive text in a more intuitive way.

point-by-point reply to referee comments:

1. The reviewer is right that "not yet existing" is a too strong formulation. We were not aware of the Seeman et al. paper, which we will cite in the reference as one exemplary dataset for surface emissivity. Nevertheless the 10 wavelengths presented

in that database would be insufficient for this kind of retrieval, as especially the shape of the spectral signature (the V-curves), being the backbone of the retrieval, is important over the total 8-12μm range. For such application the Seemann database is missing wavelengths as e.g. near 10.0μm. Moreover it was the clear intention for this kind of retrieval to use as low a priori information as possible. The only a priori dataset clearly needed in this approach is the dust extinction spectra. We will highlight this intention more precisely in the revised manuscript.

2. The referee correctly pointed out that the term c[1,2] (eq. 13) is also used for accounting of surface effects in the singular vectors 3-5 (though these effects are rather small, they exist). The singular vectors 1-2 contain the major part of the surface emissivity signal and are therefore not used for the projection of the dust extinction spectra. Accounting for dust signals in the leading two singular vectors, which are initially neglected in the projection of OPAC spectra onto SV 3-5, is the first purpose of c[1,2]. The formulation of c[1,2] may not be straightforward and easy to understand, but a large variety of tests and solution attempts lead to the formulation of this correction term as it is. Otherwise some information about dust would be lost due to surface emissivity effects. We totally agree with the referee that “avoiding the problem of surface emissivity” is a too strong and excluding formulation. Thus we will follow her/his suggestion and reformulate this to “minimizing” or “reducing” the effect.

3. We will provide a flow chart of the retrieval algorithm and data-filtering, which also includes a retrieval status level mask. For a selected exemplary scene a retrieval status map will be added to the revised manuscript in order to explain how the filtering and the retrieval work. This example will also address some other demands of referee #1 for clarification (water vapour, clouds). Given the adding of a flow chart, we will be able to shorten the text of section 3 as suggested.

4. The referee correctly interpreted this formulation. The V-shape of the extinction spectrum in the TIR, which is e.g. seen in the OPAC spectra (and in many other publications) is the major characteristic of mineral dust in TIR and is used somehow by

almost all TIR remote sensing approaches (at least by those exploiting more then one wavelength band). Nevertheless from OPAC and comparisons found in the literature the positions of the V-peaks are not necessarily the same for all dust mixtures and size distributions (see comment 11). We will reformulate and clarify this point in the revised manuscript.

5. We will provide a more elaborated description of these tests, which are the results of widely tested theoretical considerations about the characteristic shape of the dust extinction spectrum and its interplay with the water vapour continuum (see comments 4, 17). The impact of the scoring onto the retrieval will exemplarily be shown with a map of the retrieval status.

6. We agree that the filtering step is effectively cloud masking and we should refer to it as a cloud masking procedure. Nevertheless, it is no external cloud mask and is only sensitive to clouds above potential dust (e.g. deep convection, cirrus) or spatially rather inhomogeneous broken cloud fields as e.g. trade wind cumulus. In contrast to external cloud masks, this procedure does not mask out homogeneous boundary layer cloud, e.g. subtropical maritime stratocumulus. Thus in contrast to solar retrievals it remains possible to detect dust above such cloud layers, as is evident from the example. We will reformulate this passage to address the concerns of the referee and will also refer to these tests as “cloud filtering”.

7. We will follow the referee’s suggestion and add a table with main advantages and disadvantages of the method to the revised manuscript. The text will be reformulated in a more precisely way.

8. Section 6 (conclusions and outlook) will be shortened and parts will be transferred to section 5, where they also will be addressed in the advantages table.

9. We will add this information as a comparison table (also providing the general characteristics of IASI and thus the retrieval – such as ground resolution etc.) to the revised manuscript.

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10. The referee addresses a point which is imminent in most IR related remote sensing work and is surely of concern. We thought a lot about whether to use wavelength or wavenumber. As most TIR dust remote sensing applications are from imaging "broad-band" instruments in the legacy of AVHRR, wavelength is quite common in this community (e.g. also in OPAC and other databases). On the other hand the referee is right that especially in IR spectroscopy wavenumber is the spectral coordinate which is mostly used (also for IASI L1 data). We decided to address the common sense of the dust remote sensing community rather than that of the spectroscopy community in order to avoid unclarity on this side. But we will follow the suggestion of the referee and provide the corresponding wavenumber equivalents to also address the common sense of the spectroscopy community.

11. This is a very important comment, which unfortunately has not been elaborated clear enough in the manuscript. We use the OPAC dust model which does not include variations of dust mineralogy. This problem already has been addressed in former applications (e.g. Holzer-Popp et al., 2008) and has not yet been solved. Nevertheless there are recent publications that at least hematite, which is a major point of concern for shortwave retrievals, seems to play a minor role in the TIR. But this clearly does not mean that mineralogy does not have a great influence on the spectral extinction characteristics of the airborne dust. Although we refer to four OPAC "dust types", unfortunately these are only different by size distribution and not by mineralogy. Recent publications have addressed the impact of different minerals on TIR dust extinction and from case studies differences in mineralogy of airborne dust are reported. Nevertheless we need some "typical" dust mixtures of which the extinction spectra in the TIR are known and available. Otherwise, with allowing for any arbitrary mixture of mineral composition of dust, one would run into large trouble for any retrieval, as the satellite data may not contain enough information. We will stronger emphasize this aspect in the revised manuscript and also point out once more the possibility of easily include additional dust models, also with different mineralogy, into the retrieval (then we may have five or six instead of the formerly three, now four dust models used at the time

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being).

12. We will refer to the flow chart to be added (see comment 3).

13. “Emissivity” will be replaced by “surface emissivity” to clarify that meant here is not the emissivity of the airborne dust.

14. We thank the referee for making us aware of this error in the manuscript. Of course there is the dependence to viewing angle as it is the optical path (AOD/μ0) which determines the upwelling radiance seen from satellite. We will correct eq. 16 for the missing μ0 n the manuscript (it is correctly done in the retrieval).

15. We will follow the referee’s suggestion and provide another colour table which provides a better distinction between dust and greyscale dust free areas.

16. We thank the referee for the hint; of course it has to be 2009.

17. The effect of the water vapour continuum is a more or less linear slope which has opposite effect to that of mineral dust in the 10-12μm range (higher absorption at 12μm than at 10μm). The water vapour signal is also mainly contained in the singular vectors one and two and is besides the surface emissivity one reason to use singular vectors. Otherwise a precise knowledge about the atmospheric state would be required a priori (see e.g. Pierangelo et al., 2004; Klüser and Schepanski, 2009). We will address the effect of water vapour here with a reference to the discussion about WV effects when discussing the use of the MICM dust model. Also the extended discussion of the correction term c[1,2] will address the water vapour influence in the revised manuscript.

18. In order to at least partly address the coarse mode dust near source regions, we can use a mixture of 75% MITR and 25% MICM. This mixture has reasonable size distribution for wind blown dust (the MICM dust model has a very large size distribution which could be regarded as being more representative for sand-like dust or wind blown sand). We will address the new mixture, which better represents the near-source coarse mode dust, in the revised manuscript. As pointed out in response to comment

11, we will be happy to include any new dust extinction representation we are aware of – when it provides additional information (e.g. the Volz dataset is used as input for OPAC and thus does not represent another independent dataset).

19. We had tested the SVD with many different periods form all seasons over the area. The resulting SVs looked always pretty much the same and the retrieval was not sensitive on using one set or another, as for all sets the decomposition of the major influencing signals led to the same. We also tried to use sets of more days, e.g. whole months, but then the data arrays became too large for SVD. Moreover the large region covered by the IASI data going into the SVD provides a sufficient high number of different realisations of atmospheric states over the selected days. Thus we concluded that the period chosen here would be a good representation, as these days are not used for evaluation. Inter-annual differences may be an error source in the long term run, but at least in the comparison of the subsets of the years 2009 and 2010 we have processed so far there is no such problem evident (we also did a SVD for the 2010 period over the Atlantic Ocean and West Africa for testing – the resulting singular vectors again looked very much the same).

20. We see the referee's interest in such comparison figure. Nevertheless the figure looks different for every spectral bin, as the relative importance of the different singular vectors differs for each bin. From the singular vector decomposition (to be exact: from the corresponding singular values) one can learn that the first five singular vectors represent 91% of the spectral variability in the respective data. We have tested with more then the leading five singular vectors for a large number of test cases. The results did not change (the dust signal was not affected by including higher order singular vectors), but the data processing time was much more as for every observation the number of singular vectors weights to be calculated is higher by including more SVs. The characteristic shapes of the tau_eqv spectra can well be reproduced by the leading five singular vectors with some bin-by-bin variation not accounted for (higher order singular vectors are represented by variations which effect smaller numbers of

bins). As the leading two singular vectors build a major part of the spectral variability of the measured data and as surface emissivity is the largest contributor to them, a comparison between tau_sv and tau_eqv at any wavelength results in a more or less uncorrelated cloud of data points. We do not have the feeling that such a plot really helps understanding the retrieval algorithm, thus we decided against showing a plot of one versus the other, although, as the referee correctly noted, this is a key element of the method.

21. Non-sphericity has been identified as an error source we have not addressed in the method. Also the fixed mineralogy of OPAC may represent a significant error source (see response to comment 11). As addressed above we will be very happy to use other dust models (which are really different from OPAC/Volz and others being compiled from the same dust samples), but so far we are not aware of available non-spherical dust models providing spectrally resolved dust extinction in the 8-12μm TIR. We know that from several sides there is work currently carried out to provide dust extinction spectra / refractive indices in the TIR also including different mineralogy and non-sphericity. As soon as they are available we will include them in the retrieval to account for these differences.

22. Generally this could be done, but the full physical inversionproduct from AERONET leads to only very few data points and those only for high AOD. Nevertheless we will look into this and will try to do a comparison with effective radius in addition to that of Ångström exponent.

23. These have been obtained directly from the OPAC database as the ratios between AOD at 10μm and at 0.5μm. We can provide the transfer coefficients for every OPAC dust type used in the revised manuscript.

24. will be done in the revised manuscript

25. We will reformulate this sentence.

26. We agree with the referee and follow her/his suggestion for reformulation.

27. will be done

28. will be substituted

29. will be substituted

30. We agree.

31. will be substituted

32 We will correct this.

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