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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.

Anonymous Referee #1

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The paper is interesting and offers a new approach for retrieving dust properties, i.e. singular vector decomposition (SVD) of brightness temperature spectra from high spectral resolution measurements from IASI aboard the METOP satellites. Apart from commonly used LUT approaches, this technique provides an alternate method for extracting the optical/microphysical properties of dust aerosol from observed spectra without having to prescribe a-priori key surface and atmospheric state parameters such as surface emissivity, water vapor, etc, which contribute to the total IR signal at the TOA.

Although the paper looks promising, I do feel there are areas which require further clarification in order to further expand and support the key concepts presented. In some

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cases it is felt that either there is not enough information given for the reader to fully capture the main points or simply the main points get lost in the text. It is highly suggested that the authors revisit the paper to ensure all points are adequately addressed and fully supported. This will help the paper significantly. The review comments are as follows.

Major comments (requiring further clarification, etc):

1. Lines 21-25, page 464. The words, 'Not yet existing', should be used with caution here. Please see reference below which describes a global infrared land surface emissivity database that provides spectral coverage over 10 discrete wavelengths from 3.6-14.3 microns at 0.5 deg spatial resolution. Recommend adding this reference and re-wording the phrase to indicate that a database has been compiled and is available.

Seemann, S.W., E. E. Borbas, R. O. Knuteson, G. R. Stephenson, H.-L. Huang, 2008: Development of a Global Infrared Land Surface Emissivity Database for Application to Clear Sky Sounding Retrievals from Multi-spectral Satellite Radiance Measurements.J. Appl. Meteor. Climatol., Vol. 47, 108-123.

2. The reviewer is not fully convinced that the problem of surface emissivity is avoided by using SVD (lines 1-3, page 465). Surface effect information may still be contained in the first 2 singular vectors, perhaps less than what is found in v3-v5, but could still be present. Is the function of C [1, 2] (Equation 13) to account for such effects? Rather than adding a term that adjusts the AOD by an amount that's dependent on the surface type (i.e., mostly negative over desert soils, for example), can information from the global database (Seeman et al.) be utilized to constrain the problem to minimize these impacts even further?

Also, rather than state, "one possibility of avoiding the problem of surface emissivity. . .." (Page 465, line 1), instead suggest saying 'minimizing the problem of. . .' Avoiding is quite strong and as discussed on page 476 (line 1), the equivalent optical depth still contains some surface signal. This is really more of a way to 'minimize' rather than

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'avoid' the impact.

3. In section 3, there are a number of steps and assumptions going into retrieving the dust properties, including data pre-processing, dust model selection, SVD, etc. At times the text was found to be quite lengthy and even a bit wordy, and consequently was not easy to follow. It is highly recommended that the authors add a flow chart or table outlining the key steps and assumptions used in this section. This will significantly help improve the readability and flow of the paper and will certainly let the reader know what the key points are up front. As a side note, it seems the text in section 3 can also be trimmed down somewhat and made more concise.

4. Page 468, last 2 sentences. What is meant by "typical spectral behavior of mineral dust"? This is not clear and needs to be explained further. Are the authors referring to the commonly seen V-shaped signature of the dust spectrum? Please elaborate.

5. Table 1 (score test scheme for dust) and accompanying text (pages 468-469) are not entirely clear. Not enough information is provided to support how these scores were determined, how the wavelengths were selected, etc? Please provide more description. Also, there appears to be 2 test no. 3's? Is this correct?

6. Page 486, lines 8-13 (cloud-masking). Although the methodology does not utilize a dust flag as discussed in De Souza-Machado et al. 2010 for separating dust and cloud, the method still relies on a filtering process (equations 4-5) to minimize the impacts of cloud contamination on the retrieval. This approach still requires some criteria for masking these 'potentially cloudy cases'. Therefore recommend that this section be reworded to point out that although not like other masking algorithms (i.e., brightness temperature differences, etc), this technique pre-filters the data to effectively remove/mask clouds to focus exclusively on dust.

7. Section 5. There is a lot of information presented and at times was a bit difficult keeping track of the details. It would be worth the effort to perhaps arrange the key points in this section as bullets. Advantages and disadvantages of the method could

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be identified maybe as a table or to list them directly. This way all the main elements are clearly visible to the reader.

8. As with the previous comment, the same may also help improve the readability of section 6 and would set apart the main points.

Other concerns (technical points, etc):

9. Line 17, page 464: States IASI has better spectral and spatial resolution than AIRS. For those readers not familiar with IASI, suggest including a table detailing the instrument characteristics for comparison.

10. The authors' choose to use microns as their unit for wavelength, however typically in IR work, wavenumber is commonly employed. Recommend that somewhere in the text this be mentioned and to identify the range of the wavenumber domain used in the analysis. On page 467 line 7, for example, the authors might consider stating the wavenumber equivalent for the TIR window region. This would also clarify the number of channel values used ($\sim$420?).

11. In the general case, the high extinction of many common minerals in the IR is around 9.5 microns (page 467, line 1) but as a word of caution, this really is dependent on the composition of the particles. For example the sulfate gypsum exhibits a peak absorption $\sim$ 8.7 microns (1149 cm-1). Recommend this sentence be rewritten to emphasize the dependence of extinction in the IR on the types of minerals present in dust.

12. Page 468, line 4: what is meant by "...some filtering of IASI...."? This is a bit vague. Recommend saying, "a 2 step filtering of IASI observations is applied" This is more specific. Again a flow chart (see previous comment) would help here.

13. Page 470, line 4. Is 'emissivity' here in reference to the dust or is it from the surface?

14. Is there viewing zenith angle dependence in equation 16?

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15. Is it possible to adjust the color scale for dust AOD in Figure 2? It is rather difficult to detect/differentiate against the greyscale as most AODs seem to be ∼0.3 or less. This will help with making more meaningful comparisons.

16. On page 483, line9 talks about Fig. 7 over the months of Feb, May, and Aug 2010, however the caption for Fig. 7 identifies the year as 2009. Which is correct?

17. Page 474, lines 13-15. What about the effects of the water vapor continuum on the retrieval?

18. In the case of heavy dust plumes at the source region, what provisions might allow this retrieval methodology to use a more coarse-mode optical model? The exclusion of MICM in OPAC perhaps may not work as the authors point out due to the absence of the central peak; however it seems reasonable that other dust models besides MICM may exhibit the desired extinctive features to be compatible with this approach.

19. Is 11-17 March 2009 a sufficient number of days to characterize the singular vectors? Seasonal dependencies and perhaps inter-annual differences may possibly introduce variations in the resulting singular vectors if applied to other time periods.

20. It would be interesting to see a figure showing how well the singular vector (3-5) based representation of the equivalent optical depth does in comparison to that derived from equation 6 (i.e. the measurements) since this is a key step in the paper's methodology.

As a side note, is there any sensitivity on the retrieved parameters if, for example, equation 8 allows for more than 5 singular vectors?

21. Has this methodology been tested using other dust models? Although there are a lot of uncertainties in the TIR, it would be quite interesting to see how this methodology responds to different particle parameters. Following the same methodology in the paper may also provide additional insight into the non-sphericity of dust particles.

22. Page 491, lines 9-10. Can't the effective radius retrievals from IASI observations

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be compared with those from AERONET?

23. It was not apparent as to how the transfer coefficients (Equation 18) for 10 to 0.5 micron conversions of AOD, were obtained and what values were actually used. Please clarify.

Minor comments (grammar, word usage, etc):

24. Page 463, line 6: 'thus' can be removed

25. Page 463, line6, the words, '. . .for which thus mineral dust information is important. . .' is awkward. Please reword.

26. The wording, "also interact with tropical storm strengths (page 463, line 13) seems awkward. Dust can interact with tropical storms not their strengths. This can impact their strengths, however.

27. Recommend removing the word 'also' on page 6, line 24.

28. Page 483, line 13, ". . .time series are. . .' should read '. . .time series is. . .'

29. Page 479, line 9, '. . .dust uplift. . .' should be '. . .dust uplifted. . .'

30. Page 468, last sentence, "An overview over..." may read clearer as "An overview of. . ."

31. Page 469, line 21, "denomination" is believed to be "determination".

32. Page 471, lines 8-9. The word 'reported' appears twice.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 461, 2011.