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Atmospheric Measurement Techniques Discussions

Interactive comment on "Polarimetric remote sensing in O_2 A and B bands: Sensitivity study and information content analysis for vertical profile of aerosols" *by* Shouguo Ding et al.

Anonymous Referee #2

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Review of "Polarimetric remote sensing of O2 A and B bands: Sensitivity study and information content analysis for vertical profile of aerosols" by Ding et al. for inclusion in AMT

The paper presents a sensitivity study to investigate the use of measurements in the O2 A and B bands for retrieving information about aerosol height and layer thickness. Idealized aerosol profiles, surface abledos, and viewing geometries are used to perform detailed radiative transfer calculations to investigate the sensitivity of simulated top of atmosphere (TOA) radiances and degree of linear polarization (DOLP) to changes in aerosol height and layer thickness across the A and B bands, as well as to determine the information content of the simulated signals (the so-called Degree of Freedom for

Signal, DFS). The main conclusions seem to support the use of polarimetric measurements to retrieve the desired aerosol information in favor of measuring radiance only, but the capability of retrieving aerosols is best for very high aerosol altitudes and relatively thick layers and further requires a large number of measurement channels to make a reliable measurement.

The paper is interesting and results could be compelling, but the paper suffers from some problems so that I recommend major revisions.

First, the paper needs a thorough re-reading by the authors. There are several logical inconsistencies in the statements made in the paper that greatly detract from the clarity of results. I point out those I found, but others might be present.

Further, the paper needs to be read for clarity of grammar and misspellings. Again, I point out those I find.

Finally, the sensitivity results presented seem to vary in conditions in ways that don't make a lot of sense to me and leave me uneasy about how to interpret the results. For example, in the latter part of the analysis the aerosol heights are sometimes assumed to be 8 km, sometimes 5 km. Likewise, surface albedo is sometimes this and sometimes that depending on the particular figure. It feels like the paper was assembled from tests that happened to have been done rather than from a predetermined design. I suggest aiming for some consistency in that regard.

For notes below, the numbering scheme in the paper is unclear, apparently truncating the line numbering to the last two digits, which allows on some pages line 99 to be following by line 0.

Page 6, line 99: replace "twofold" with "two main ways"

Page 6, line 03: "retrieving aerosol profiles"

Page 6, line 08: "Fourier" and ". . .carried on the Japanese. . ."

Page 6, line 09: "...is capable of measurement high spectral resolution..."

Page 7, line13: rather than "spectral samplings" maybe you mean "spectral channels" or "bands"

Page 7, line 26: "...retrieving the corresponding..."

Page 8, line 42: "...scattering generates..."

Page 12, line 02: "Three types of experiments are..."

Page 12, line 04: "...the sensitivity of DOLP and radiance to gamma over..."

Page 12, line 07: "strongly," "moderately", and "strongly"

Page 12, line 10: "results section (Section 3.4)."

Page 12, line 17: Databases of non-spherical dust optical properties suitable for inclusion in radiative transfer models like your are available (e.g., Meng et al. 2010, Meng, Z., Yang, P., Kattawar, G. W., Bi, L., Liou, K. N., & Laszlo, I. (2010). Single-scattering properties of tri-axial ellipsoidal mineral dust aerosols: A database for application to radiative transfer calculations. Journal of Aerosol Science, 41(5), 501–512. http://doi.org/10.1016/j.jaerosci.2010.02.008). I'm not clear on why you could not use those properties in your dust-like simulations. Do you really need the optical properties of the aerosols to be linearized, and couldn't they be anyway given the approximation in, e.g., equation 6? This is not a major issue for me, I'm just curious. Page 13 and Figure 1: Please note the word "wavelengths" is misspelled on Figure 1a & b. Also, I don't understand why Figures 1e & f have a different range on the x-axis. This actually confuses the discussion, to me. Because really what you are showing here is how DOLP varies across different wavelengths, with Figure 1e referring the the A band 755 - 775 nm, and 1f referring to the 685 - 695 nm B band. So really, since you are sorting on gaseous absorption, I think what these two figures are showing is variation in Rayleigh scattering in these two bands.

Page 15 and Figure 2: Figure 2 has numerous problems. The word "Optical" is misspelled Figure 2a. Figure 2b and c please change y-axis labeling to "DOLP" instead of "DOP." Please decide on a consistent wavelength labeling for the y-axes of Figures 2b & e and Figures 2c & f (unless the channels really are different, but it doesn't seem so from the text). The blue line in Figure 2d is for the 1km half width, which looks identical to the profiles in figure 2a which are said to be 2km half width. Something is wrong with one of these figures, and my suspicion is that the results presented in Figure 2a are in fact 1km half width and I wonder if that propagates through the results presented in Figure 2. Now, in terms of what is discussed with respect to Figure 2 could you please include some text on what we expect delta-DOLP to look like? I think that the intent here is that one channel in the pair has no absorption and one has absorption and that the Rayleigh scattering features are basically the same for both channels in the pair. The statement on line 69 that the delta-DOLP decreases with increasing surface albedo is exactly opposite what the figure shows (2b & c), contradicts Figure 1, and the explanation is in any case unclear: why is there strengthened depolarization from the surface which I thought was generally not strongly polarizing?

Page 16, line 96: can you settle on a common set of surface albedos to consider? Here it is 0, 0.2, and 0.5, but in Figure 2 it is 0, 0.05, and 0.2, and in Figure 1 it is 0.05, 0.2, and 0.5. Clarity is lost by these different combinations.

Page 16, line 98: actually, at 150 degree scattering angle the DOLP increases as albedo increases (gets less negative).

Page 19, line 41: Should refer to Figure 5

Page 19, line 53: why are you using a height of 8 km? I'm guessing because you get better results this way, but really how common are aerosols at 8 km? 3 - 4 km might be more realistic.

Page 20, line 68: Should refer to Figure 7a

Page 20, line 70: I think you said the assumed DOLP accuracy was 0.05. Is that a high accuracy or low accuracy?

Page 20, line 76: DFS seems to increase with altitude up to about 10 - 12 km, but decreases very quickly above that, so this statement about Figure 8 seems no correct. Also, line 77 misspelled the word “larger.”

Page 21, line 85: Is DFS additive? Why does it approach 2 here?

Page 21, line 94: why is now a different aerosol altitude assumed (5 km) than for earlier discussion using 8 km? This is at least a more reasonable choice of aerosol height.

Page 23, line 30: “DOLP”

Page 23: in this paper nothing is said about how clouds might affect this analysis. Can you at least mention the implications of dealing with a cloudy atmosphere?

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