Authors' Reply to 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.

Reply. We thank the reviewer for thorough review and providing us encouragement and constructive comments for improving this manuscript.

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.

Reply. Thanks. Upon finalizing the manuscript, we have invited two native speakers of English (graduate students in Atmospheric Sciences) to proof-reading the manuscript during the revision process.

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 some- times 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.

Reply. We conducted calculations for 4 different surface albedos (0, 0.05, 0.2, and 0.5). Because for darker surfaces (such as ocean with albedo around 0.05 or less), past work already shown that the intensity in O2 A band can be used to retrieve height information of aerosol layer. So, our later focus is on the bright surfaces (e.g., 0.2 and 05 albedo) while also keeping the analysis for albedo = 0 to provide the contrast for interpreting the results. So, in the revision, for figure 1 and 2, we show DOLP results for all 4 surface albedos; in the remaining figures, we show the results for 0, 0.2, and 0.5. Some discussions of these are now added in the revised manuscript. In addition, we have shown results for 5 km in the manuscript and for 8 km in the supplementary materials.

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”
Done.

Page 6, line 03: “retrieving aerosol profiles”
Done.

Page 6, line 08: “Fourier” and “. . .carried on the Japanese. . .”
Done.
Page 6, line 09: “. . .is capable of measurement high spectral resolution. . .”
Done.

Page 7, line13: rather than “spectral samplings” maybe you mean “spectral channels” or “bands”
Done.

Page 7, line 26: “. . .retrieving the corresponding. . .”
Done.

Page 8, line 42: “. . .scattering generates. . .”
Done.

Page 12, line 02: “Three types of experiments are. . .”
Done.

Page 12, line 04: “. . .the sensitivity of DOLP and radiance to gamma over. . .” Page 12, line 07: “strongly,” “moderately”, and “strongly”
Done.

Page 12, line 10: “results section (Section 3.4).”
Done.

Page 12, line 17: Databases of non-spherical dust optical properties suitable for in- clusion 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 appli- cation 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.

Reply: We are indeed in the process of developing tools to use these properties for non-spherical particles. A linear model with analytical formula that enables accurate calculation of Jacobins (of aerosol single scattering properties with respect to aerosol physical parameters as such as particle size and index of refraction) is needed for the information content analysis. The database in Meng et al. (2010) can be used to derive these Jacobians through finite difference method (albeit not a analytical solution). We have added this in the discussion in section 2.5.

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.

Reply. Sorry, we fixed the wavelength typo. The change of DOLP with respect to wavelength fluctuates a lot because of fine structure of O2 gas absorption, and so we didn't plot DOLP as a function of wavelength. The large change of DOLP in both O2 A and B bands is primarily due to the change of gas absorption that leads to the change of single scattering albedo in different altitude. If there is no gas absorption, the DOLP will be nearly constant as shown in the Fig. 1e and f where gas optical depth is less than 0.1. Indeed, the changes of Rayleigh optical depth in both O2 A and B band are less than 0.03. The Rayleigh optical depth is 0.026 at 760 nm, 0.024 at 775 nm, 0.040 at 685 nm, and 0.037 at 695 nm. We added this discussion in section 3.1.

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).

Reply. Sorry, we fixed DOLP and 'optical' typo. The calculation is conducted at 0.01nm resolution. So we decide to go to double decimal points. We now show the figures at consistent wavelengths (e.g., 759.98 nm for O_2 A and 689.78 nm for O_2 B).

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.

Reply. Good catch. Sorry for this oversight. The width at half maximum is 2 km, and so, half width at half maxima is 1 km. You're correct, and we fix this in both text and figures.

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?

Reply. Thanks for pointing out this misinterpretation. We clarify the sentences as "It can be seen that ΔDOLP increases as the surface reflectance increases, which is caused by relatively larger increase of multiple scattering between the surface and atmosphere in the continuum band (than in the O_2 absorption band". To further illustrate this, we in Figure 1e and f has added the cases for surface reflectance = 0. See more discussion below.

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.

Reply. We did simulation for 4 cases with surface reflectance of 0, 0.05, 0.2, and 0.5. We now show all four cases for Figure 1 e-f and Figure 2 to further explain the physics behind this study. After this, to avoid the clutter in the figure, we only picked three wavelengths 0, 0.2, and 0.5 for all other figures.

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

Reply. Sorry for the confusion. We reword this as the following: "an increase of surface albedo leads to more depolarization (e.g., decrease the positive DOLP for Θ=120° and make less negative DOLP for Θ=150°) at TOA."

Page 19, line 41: Should refer to Figure 5
Corrected.

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.

Reply. We're sorry for this inconsistency. We run many cases and we select 8 km to present simply because 8 km is the middle value in the range of H (1-16 km) values we studied. In the revision we added the case for 5 km in the main text. Note, '8 km' is just a base case that we use to sequentially select the channels. Once the sequence of channels is fixed, these channels are used as a group to evaluate DFS for peak heights at all levels (not just 8 km, as shown in Figure 7). We have modified the Figure 10 and added figure 11 to show the results for H of 5 km. One paragraph is also added to discuss this. Please see the text for details.

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

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

Reply. This is relatively moderate accuracy. We add this discussion in the paper. “It is noted however, current measurements of DOLP often have uncertainty less than 1% (and sometimes around 0.2%, Zeng et al., 2008)”.

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.”

Reply. This is a good point. We add the following. “It is interesting to note that DFS seems to increase with altitude up to about 10 - 12 km, but decreases very quickly above that because the O_2 absorption is much weaker there.”.

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

Reply. It is not exactly additive, but close. For retrieving two independent variables, the possibly maximum DFS value would be 2.

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.

Reply. Since there are so many scenarios or combinations, we try to present the physics and typical results with limited cases. As discussed, we now selected baseline case as 5 km.

Page 23, line 30: “DOLP”
Corrected.

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?

Reply. This is a good point that we like to study further. As discussed in the introduction of the manuscript, several studies have already used radiance in O2 A band to retrieve cloud top height. We believe cloud contamination would have important impact on the DOLP in O2 A band, but DOLP may also help to help detect cloud and minimize the cloud contamination. We have not done any work in these regards yet. So, instead, we acknowledge this the revised text as the following: “Furthermore, since aerosol and cloud are often co-exist in real atmosphere in the instantaneous field view of a satellite sensor and cloud often has much larger optical depth, it is also necessary to study how cloud contamination may affect the results presented here, and whether or not DOLP in O_2 A and B band can add valuation information into the existing cloud top height retrieval technique that uses the intensity in O_2 A band (Fisher et al., 1991)”.