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Interactive Comment

Interactive comment on "Probing the sensitivity of polarimetric O_2 A-band measurements to clouds with emphasis on potential OCO-2 and GOSAT retrievals" *by* S. Sanghavi et al.

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The authors thank the reviewer for their insightful comments and suggestions. In the following, we respond to them in a step-by-step fashion.

1. The subject of the manuscript is very interesting and original and totally correspond to the topics of the Atmospheric Measurements Techniques journal. The authors discuss the sensitivity of O2 A-band polarimetric measurements at the top of the atmosphere to cloud parameters such as optical depth, effective ra-

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dius, layer altitude and geometrical thickness, which is to my knowledge the first paper on this topics.
AUTHORS: We thank the reviewers for their positive assessment of the suitability of this subject for publication with AMT.

2. Unfortunately the paper suffer from a lack of rigor, there are a lot of errors or misleading affirmations and conclusions. There are too many figures which are for most of them usefulness, and even not really discussed in the text. If I refer to the evaluation criteria of the AMT journal I recommend not to publish the paper in his present form. The manuscript needs a significant revision before reaching the scientific level of the journal.
AUTHORS: The text as well as the figures have been revised accordingly. The main changes can be summarized as follows:

    (a) The original Fig. 1 and Fig. 4 have been removed. The original Fig. 2 (now Fig. 1) has been modified so that both y-axes refer to the same plot.
    (b) Spectra have been recomputed at a resolution of 0.005 nm followed by convolution with a Gaussian slit function of FWHM 0.04 nm to be comparable to GOSAT and OCO-2 measurements.
    (c) The spectral plots (the new Figures 5, 6 and 7) are now based on simulated measurements of I and Q rather than I and $p = |Q|/I$. The discussion of Section 4 has been modified accordingly.
    (d) All spectral plots now include the pure Rayleigh case and the case of reflection by a white Lambertian plate (WLP, introduced in Section 2) as reference.
    (e) The new Fig. 8 (originally Fig. 17) has been modified to include the response of Q to changes in cloud geometrical thickness.
    (f) All other Figures (spectral plots) dealing with sensitivity to cloud geometric thickness Δz have been removed.

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(g) Angular dependences on size of I, Q and the corresponding p have now been condensed into one figure each for optically thin and thick cloud (Figures 10 and 11). The pure Rayleigh case has been included as reference.

(h) All 2D plots comparing the I, Q and $I \pm Q$ response to pairs of cloud parameters τ_{cloud}, z_{top} and r_0 have been eliminated. Instead, a simple table comparing the characteristic responses of I and Q to each parameter has been introduced to motivate the possibility of identifying the three parameters simultaneously using methods like optimal estimation in Section 5.

3. Some results shown on different figures make myself wondering on the validity of the overall paper. It is for example well known that as the particle radius increase (or the mie size parameter x increase), the scattering phase function of spherical particles (element F11 of the scattering matrix) increase in the exact back-scattering direction or at least it do not decrease (e.g. at scattering angle = 180 degrees), which is note the case on figure 18!. . .
AUTHORS: The authors do not see any contradiction concerning the behavior of the phase matrix with respect to particle size. If the strength of the backscatter peak F_{11} were to have a monotonic relationship with respect to particle size, then pure Rayleigh scattering at $\theta_{\text{scatt}} = 180°$ would be expected to be the weakest, but it is well-known that this is not the case.

4. Concerning the F12 element the result shown on this figure 18 are questionable, indeed in the rainbow region (scattering angle around 140 degrees) the F12 element decrease as the particle size increase, which should be the opposite (see Hansen and Travis (1974))!!! There are also strange behavior on figures 20 and 22 in regards of figure 18, especially for view angle between 0 and 40 degrees. Why $Q = -0.01$ at theta view= 0 whereas figure 18 shows that at this angle the cloud F12 is almost null (or at least positif) and the rayleigh F12 is positive, it would be interesting to see the same figure for a pur rayleigh atmosphere and a cloud without rayleigh.

AUTHORS: You are right - thank you very much for pointing this out to us! We recomputed our angular data and have represented I and Q together for COT=1 and 10, respectively. As requested, we have included the behavior of a purely Rayleigh scattering atmosphere in each subplot. In each case, we have tried to make the plots more focussed by retaining only one cloud height level and have chosen a non-absorbing wavelength, since it bears maximum angular sensitivity to particle size.

5. The last section, section 5, greatly disappointed me because of many wrong conclusion especially in regards of Q (see specific comments below), the authors really need to raise the scientific level of this section.
AUTHORS: This section has been replaced by a comparison shown in Table 1 of the characteristic response of the O_2 A-band to the three cloud parameters considered.

6. Some other figures can lead to wrong conclusion because the studied quantity mix two different effects. For example for figures 7 to 16, authors looked at the effect of different cloud parameters on the measured I (intensity) and Q/I (degree of polarization). The problem of looking at Q/I is that they mixed the effect of cloud parameter on polarization Q and intensity I which can be opposite. By looking such quantity one can have wrong deduction on the polarized signal due to I. The author should look only at Q!! especially for consistency because in the last part of the manuscript they do not analyse Q/I but Q!!!
AUTHORS: p has now been replaced by Q throughout the paper - thank you for your very constructive comments and suggestions in this regard.

7. The authors do not talk about measurement noise at all, neither about the effect of the radiative transfer model accuracy on the sensitivity study. For example it is well known that when dealing with large particle in the visible part of the spectrum (large mie size parameter), to reduce computational time one need to

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truncate the element of the phase matrix (there are different method for that), this truncation affect the accuracy of the computation, especially when looking at polarization. What are these effects on the results presented in the manuscript?
AUTHORS: We intend to introduce noise and error sources as part of the information content and error analysis based on Jacobian matrix computation in a follow-up study.

8. The authors argued that they studied the entire cloud state space which is far to be true, first because the discretisation of each cloud parameters is too large (5 median cloud droplet, 4 cloud optical depth, 6 top layer altitude and 3 geometrical thickness), and second because the range of some cloud parameter are not representative of the natural variability (e.g.the geometrical thickness for example). The cloud layer altitude range between 2.4km to 12.4 km, but are the particles still liquid (and then spherical) at 8.4, 10.4 and 12.4 km? Moreover the author should address the effect of the width of the size distribution on their results, because this parameter has an important effect on polarization.
AUTHORS: The statement about state space of the cloud has been removed. We are grateful to the reviewer for pointing out the sensitivity of polarization to the width of the size distribution. We will consider this in more detail in future studies with greater focus multi-angle measurements, but have left this out of this work because the sensitivity to r_0 is already very weak for nadir measurements in the geometry considered.

9. I am wondering what section 2 bring to this study, especially when the authors conclude "While this served as a good assumption to study the nature of the individual lines constituting the O2 A-band, it does not suffice to describe backscattering by actual clouds, which are better represented by a vertical distribution (ignoring variations in the two horizontal dimensions) and as a distribution of differently sized droplets. In the following section, we explore the scattering characteristics of a (liquid) cloud as a function of the size of its constituent dropletsÂż.

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AUTHORS: Section 2 deals with the intrinsic sensitivity of the O_2 A-band on the height of scattering without any dependence on cloud optical/microphysical properties or geometric thickness. Using the white Lambertian plate (WLP) introduced in Section 2 as a reference in Figures 5, 6, and 7, we are able to specifically attribute the difference between WLP and cloud of the same height to cloud parameters like τ_{cloud}, Δz and r_0.

10. page 9604, line 8: what is the cloud parameter called Âńviz.Âż?
AUTHORS: http://en.wiktionary.org/wiki/viz.#Usage_notes

11. page 9609, line 7: we do not see the altitude 30km on figure 2, by the way this figure is very hard to understand. you missed ÂńkmÂż in the legend.
AUTHORS: 30 km has now been replaced by 20-25 km. The discussion of the figure now explicitly describes each axis in order to make it more clearly understandable.

12. page 9609, line 24: When looking at figure 3 the the critical value of tau_abs is 0.3 and not 1.15, why?
AUTHORS: This was because the scaling on the y-axes for two plots (red for z_{min} and grey for τ_{abs}) was each independent of the other. This has now been changed, with only one curve, showing z_{min} on the left and corresponding values of $\log(\tau_{\text{abs}})$ on the right. It can be verified that $z_{\text{min}} = 0$ corresponds exactly to $\exp(0.141) = 1.151$.

13. page 9610, line25: Why choosing these values for the imaginary part of the refractive index? I do not see what the discussion about these imaginary part of the refractive index bring to this study? Do we need figure 4, it is a well known figure that everybody can find in different book (e.g. Van de Hulst, Lenoble, Hansen and Travis, ...).
AUTHORS: The idea was to first examine the response of the O_2 A-band to a pure laminar reflector at different heights, and then to examine how the cloud

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differs from such a reflector due to its microphysical and hence optical properties. By examining the dependence of the optical properties on the microphysical properties, we could show that for a pure water cloud, the only optical property dependent on the size distribution is the phase matrix.

Section 3 has now been simplified, and details on the effect of different imaginary parts of the refractive index have been removed.

14. page 9611, line 10: Why choosing this particular value of the width sig0=1.13?
AUTHORS: This value has been chosen to comply roughly with the findings of Mayer et al. (2004); Nakajima et al. (2010); Stephens and Platt (1987); Stephens (1978), as added to the manuscript text.

15. page 9611, line 12: $\rho(r)$ is not a probability but a probability density function (PDF), $\rho(r)dr$ is a probability!
AUTHORS: Corrected

16. page9612, line 1: The authors choose to plot Pv and Ph, because OCO-2 is measuring Iv and Ih, but to understand what polarization bring in the O2 A-band, one has to work with I and Q. In the manuscript the authors use both (Iv,Ih) or (I,Q) convention, which make the manuscript sometime confusing, it would help and clarify the manuscript if the authors choose to use only I and Q, which are well known in the community. This would reduce the number of figures and would make the manuscript more consistance. So I would recommend to show directly F11 and F12 instead of Pv and Ph.
AUTHORS: F_{11} and F_{12} have been shown in Fig. 9. We decided to retain the Figure showing Pv and Ph since it directly governs the scattering of the Iv and Ih component: that they are orthogonal to each other has relevance to GOSAT measurements, and the inclusion of Ih has relevance to OCO-2 measurements.

17. page 9612, line 24: Why choosing a solar zenith angle of 60 degrees?

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AUTHORS: We choose SZA= 60° simply to get a representative value of its cosine $0 \leq \mu_0 < 1$ at $\mu_0 = 0.5$ for SZA=60°.

18. page 9613, line 7 and 8: Be consistent, Q/I is called some time polarization response or degree of polarization. Again when looking at figure 7, one could think that high absorbing wavelength are highly polarized which is not the case, it is just due to very small value of I (see major comments).
AUTHORS: We have followed the reviewer's previous advice and have replaced discussion of polarization with that of Q. All spectral Figures have been modified to include Q instead of p.

19. page 9613, line 25 to the end: The explanation of why Q is only sensitive to the first fews order of scattering is confusing. Q is not diminishing with high order of scattering, but the contribution of higher order of scattering to Q at the top of the atmosphere. I do not understand the sentence Âń . . .due to the blurring of the differences between the components Ih and IvÂż. The authors should read Chandrasekhar (1960) to understand why after few order of scattering the light becomes unpolarized.
AUTHORS: We have tried to reframe our argument as: "$Q = I_h$-I_v becomes increasingly diminished for higher orders of scattering due to the blurring of the differences between the components I_h and I_v. This is because each new scattering event involves a different scatter- ing plane and, consequently, a different rotation of the scattering matrix according to Hovenier (1971)."

20. page 9614, line 20 to 22: What is happening if the geometry of observation fall in the rainbow region where the liquid cloud polarized the light? You should discuss this case.
AUTHORS: This has been discussed in detail in Section 4.3.1 with reference to Figs. 9, 10 and 11.

21. page 9614, line 24: On figure 8 I like the representation of the radiance as a function of total column absorption, but could you explain what is the large discontinuity around tau_abs = 15 or 25, especially for ztop = 12.4km.
AUTHORS: At the beginning of Section 4 in the revised manuscript, we have provided the following explanation for the discontinuities referred to by the reviewer:

"The dependence of both I and Q on $\tau_{\mathrm{abs},0}$ is expected to be monotonic. Deviations from the monotonicity are observed primarily because of convolution, which causes a spread of absorption strength from the individual lines to the neighboring wavelengths. A weaker role is played by the vertical variation of absorption cross-section of O_2, which disturbs a $1:1$ relationship between the total column absorption strength and the measured value of I or Q."

22. page 9615, line 10 to 17: In the text the authors explain the physical difference between I and Q, which is interesting, but the figures do not depict I and Q but I and Q/I, which make difficult to relate the text with the figures. Again by looking Q/I instead of Q one can miss interpret the polarized signal due I (see major comments).
AUTHORS: Again, we have followed the reviewer's previous advice and have replaced discussion of polarization with that of Q. All spectral Figures have been modified to include Q instead of p.

23. page 9615, line 24: Do we still have spherical particles for a cloud top layer at 12.4km?
AUTHORS: Probably not, but we have only shown it here as a hypothetical example.

24. page 9616, line 7 to 12: The authors should explained what is a Âńmaximum sensitivityÂż, again here we are not looking at sensitivity, but absolute value. Is this Âń sensitivityÂż be the same if the geometrical thickness is greater that 0.6km, which is often the case? Nothing to say about the effect of deltaz on the

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polarized signal?
AUTHORS: “Maximum sensitivity" here simply implies the largest change in I as a result of a change in Δz. The sensitivity of Q has been added to both the Figure and the discussion.

25. page 9620, line 7 to 10: The authors says exactly the inverse of what we can see on figure 19 and 21. Figure 19 shows that for low optical depth there is more ÂńsensitivityÂż to r0 mainly for $\theta_{view} < 0$, whereas for larger optical depth (fig. 21) both backscattering and forward scattering seems to be ÂńsensitiveÂż to r0. Which is something I do not understand . . . are there the right figures?
AUTHORS: There seems to be a misunderstanding. . . we have recomputed our plotted data, and reworded our discussion in the revised manuscript.

26. page 9620, line 16-17: Why would the size dependance intensity diminish with increasing theta view for the small optical depth case? The intensity of such cloud is driven by single scattering, which is driven by F11, and F11 shows increasing size dependance at such angle!!!!
AUTHORS: We have explained the behaviour of I at $\theta_{\text{view}} > 0$ as follows: “For the backscatter angles ($\theta_{\text{view}} < 0$), the sensitivity to r_0 of the scattered intensities closely resembles the size-dependence of the phase function, revealing a largely low-order scattering signal. As a result, this angular range is well-suited to the detection of droplet size, with good resolution near the angle of exact backscattering $\theta_{\text{scatt}} = 180°$ occurring at $\theta_{\text{view}} = -60°$, and at the rainbow occurring near $\theta_{\text{scatt}} = 146.1°$, seen at $\theta_{\text{view}} = -26.1°$.

    At larger forward scatter angles ($\theta_{\text{view}} \gg 0$), however, the size-dependence of the intensity either diminishes with increasing θ_{view} or becomes reversed (diamonds at the top, squares at the bottom). The high order of multiple scattering within the forward peak causes the original size-dependence of the scattered signal to be blurred. Smaller droplets (e.g., the smallest $r_0 = 5\,\mu$m represented by squares) show a more isotropic distribution of light than larger ones (e.g., the largest $r_0 =$

$15\ \mu$m represented by diamonds), leading to a greater intensity spread around the forward peak but less concentration within the forward peak for smaller droplets, and vice versa.

This "convolution" due to multiple scattering causes the reversal in the size sensitivity at large positive view angles and generally diminished size-sensitivity: increasing cloud optical thickness gives rise to more multiple scattering, and thus greater mixing of the forward peak at $\theta_{\mathrm{scatt}} = 0°$ (stronger for larger droplets) with surrounding angular regions at $0° \lesssim \theta_{\mathrm{scatt}} < 90°$ (stronger for smaller droplets). These angular regions are represented by the positive view angles of the geometry chosen in this study. As a result, small droplets dominate the scattered signal in the low order scattering regime due to less mixing between the forward peak and the rest of the angular range, while larger droplets dominate the strongly multiple scattered signal. Intermediate orders of scattering, e.g., at larger positive angles in the thin cloud case, do not show a clear dependence on size."

27. page 9620, line27 to page 9621, line 6: I do not understand this explanation, cloud you rephrase it. Which aureoles are you talking about?
AUTHORS: Rephrased

28. page 9621, line 14: why are we studying Q now and not Q/I ??
AUTHORS: Again, we have followed the reviewer's previous advice and have replaced discussion of polarization with that of Q. All spectral Figures have been modified to include Q instead of p.

29. page 9621, line 18: First time I see the peak around 90 degrees in scattering angle on Q! Can the authors explain why Q would be negative at theta view = 0 degree, whereas neither the cloud or molecules show negative F12?? By the way Q is positive in the rainbow region as the F12 for cloud and rayleigh! The authors need to give a robust explanation on this behavior around 90 degrees in scattering angle. I need to see the same plot but for rayleigh alone and cloud

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AUTHORS: Our I and Q values have been recomputed, and can be seen to eliminate the issues identified by the reviewer. A Rayleigh plot has been included in the Figures, which shows large p close to $\theta_{\text{scatt}} = 90°$. However, we realize that our description was misleading and has been rephrased.

30. page 9621, line 23: The authors has to give a robust explanation on this behavior, why the polarization in the rainbow is stronger for small particle???
    AUTHORS: We do not see a problem here. The behavior of I and Q in Figures 10 and 11 near the rainbow seem to be fully compatible with that predicted by the phase matrix (Fig. 9).

31. page 9622, line 5: Be careful to what you say, it is not the nadir view which is insensitive but the geometry of observation for which the scattering angle is equal to 120 degrees, for another solar zenith angle the nadir view would be sensitive.
    AUTHORS: We make explicit mention of the fact that our statements apply only to the specific geometry considered in our study.

32. page 9623, section 4.3: Nothing is said on the advantage of high resolution measurement to retrieve cloud optical depth . . . for exemple when the ground contribution is not well known.
    AUTHORS: The authors are grateful to the reviewer for bringing up this important observation. We have now included it at the end of Section 4.3.1.3 as follows: “Further, it should be noted that the decreasing sensitivity to lower atmospheric layers and the ground surface in the pres- ence of absorption is useful to determine the optical thickness of a cloud even in the absence of sufficient information about the brightness of the ground surface."

33. page 9624, line 1: First, on figure 23 the caption seems to me very short! Second, I think that
    AUTHORS: This figure has been eliminated from the revised manuscript.

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