Final Author Comments

Referee 1

Major concerns

[Comment 1] The authors ignore aerosols above the cloud. It is well known that aerosols can contaminate polarisation measurements above the cloud, and these may result in spurious retrievals noted by the authors. Such contamination may frequently occur in the tropics due to increased anthropogenic aerosol over the Indian Ocean. The authors need to show that aerosols do not bias their results. This can be shown by using standard aerosol profiles in their radiative transfer calculations and perform sensitivity studies or use CALIOP data to show that aerosol was not above the cloud during the period under study.

[Response 1] Our project is in progress, and we are now studying this issue. Similar previous works that attempt to constrain particle shapes and roughness do not consider aerosol and PSC contamination. This is why the impact assessment of the aerosol contamination was not in the scope of the original study.

However, we have some findings to expand upon this issue now. As September 2005, which is analyzed in this study, is before the launch of CALIPSO satellite, we are not able to directly assess the degree of contamination. Instead, we analyzed the collocated POLDER3-MODIS-CALIOP dataset in September 2006 with a warmer brightness temperature threshold (233 K) in the extratropics. According to the CALIOP vertical feature mask, on the CALIOP track, about 22% of pixels are possibly contaminated by either aerosol or a stratospheric feature. However, the histogram of EOF 1 scores changed little after removing these pixels. The estimated roughness parameter (σ^2) changed very little, from 2.23 to 2.26. Therefore, we do not consider the aerosol contamination to introduce a large bias that brings our estimate out of the range of prescribed parameter.

[Comment 2] Determination of cloud-top height. Figure 9 demonstrates the importance of constraining cloud-top height to retrieve surface roughness. Is the cloud-top sufficiently well constrained? It would be useful to show by using an independent dataset provided by the DARDAR product (i.e. combines active radar and lidar to retrieve cloud profiles) as to how well their cloud-top is constrained? The use of passive radiometric measurements provides only a weak constraint as the cloud-top determined using IR or solar measurements depends on the profile of IWC. Thus, over regions of convection, the IWC profiles vary considerably and IWC increases as a function of distance from the cloud-top. With such profiles, the passive IR measurements might appear warmer due to emission from below the cloud-top, which in turn might result in a significant error in the assignment of cloud-top altitude and hence

retrieved surface roughness. Might the error in the cloud-top altitude account for some of the non-retrievals found in the tropics?

[Response 2] We compared the cloud top pressure retrieved from the PARASOL and the CALIOP by using the collocated dataset. It was found that the cloud top height retrieval is less constrained in the tropics than in the extra-tropics. In the extra-tropics, the PARASOL cloud top height is lower than CALIOP cloud top height by about 80 hPa. In this validation, the analyzed data is limited to single-layer clouds. As the referee pointed out, it is possible that some retrieval failures in the tropics are because of the attempts to solve underconstrained problem. We added comments at the end of Sect. 3.2.
P34299, L20
<u>A separate validation using the Cloud-Aerosol Lidar with Orthogonal Polarization (CALIOP) data indicated that our cloud top height retrieval is not in agreement with CALIOP cloud top height data, possibly indicating the limited information content.</u>

[Comment 3] The authors find estimates of surface roughness well beyond the range of their theoretical results. There could be other reasons for this not discussed by the authors. Other reasons could be as follows: (1) Accuracy of the light scattering computations. The authors make use of a physical optics approximation but do not show or cite results that confirm the approximation is sufficiently accurate in calculating the –P12 element. Please show or cite results and quantify errors in the backscattering direction? (2) The method of tilted facets to represent surface roughness may not be sufficiently accurate to represent naturally occurring deep surface roughness? A paper by Liu et al. (2013) [JQSRT 129, 169-185] show that the scattering matrix elements calculated using the tilted facet method becomes inaccurate at backscattering angles when compared against an electromagnetic treatment of idealised surface roughness when sigma=0.2. Is it possible to quantify the inaccuracy of the TF method used to calculate the matrix element when sigma»0.2? They might be able to account for this inaccuracy in their retrievals if there is a systematic bias in the TF results? Please comment and show results.

[Response 3] (1) To the best of our knowledge, we do not have conclusive literature that answers the referee's comment. The light scattering of large complex particles is an active field of research, especially the polarization state.
(2) The referee's comment includes two distinctly different points: (2.a) the appropriateness of the tilted-facet model in representing natural deeply roughened particle, and (2.b) the accuracy of phase matrices computed from the tilted-facet method in comparison to results from rigorous methods.
Our comment for the point (2.a) is the same as the comment for (1): no reliable polarimetric measurement of naturally occurring clouds are available. As for point (2.b), we do not exclude the possibility of potential bias, and the calibration of the tilted-facet method may be helpful. However, to conduct systematic experiments to establish a calibration technique is computationally too

expensive, and beyond the scope of this study. In addition, Liu et al. 2013 uses a particle with size parameter of 100, which corresponds to the maximum dimension of 14 μm. This is significantly smaller than the peak of the natural particle size distribution (50 to a few hundred μm). Comparing an approximation and a rigorous technique at such large actual cloud particle sizes is computationally prohibitive.

Minor points

[Comment 4] Please could the authors proofread their manuscript again to remove errors such as typos, incomplete sentences, etc., etc.?

[Response 4] We would be pleased to learn the specific location of errors that are omitted in our multiple times of proofreading. We checked English grammar and all references are cited.

[Comment 5] Citations.

- **Page 34285 line 6. When discussing microscopic morphology the authors should also consider citing Ulanowski et al. (2006; 2014) [Ulanowski, Z., Hesse, E., Kaye, P. H., and Baran, A. J.: Light scattering by complex ice-analogue crystals, J. Quant. Spectrosc. Radiat. Transfer., 100, 382–392, doi:10.1016/j.jqsrt.2005.11.052, 2006; Ulanowski, Z., Kaye, P. H., Hirst, E., Greenaway, R. S., Cotton, R. J., Hesse, E., and Collier, C. T.: Incidence of rough and irregular atmospheric ice particles from Small Ice Detector 3 measurements, Atmos. Chem. Phys., 14, 1649–1662, doi:10.5194/acp-14-1649-2014, 2014.**
- **Page 34285 line 18. When discussing biases in global retrievals by inappropriate ap- plication of a phase function perhaps they should also cite Macke and Mishchenko 1996 [Macke, A., and M.I. Mishchenko, 1996: Applicability of regular particle shapes in light scattering calculations for atmospheric ice particles. Appl. Opt., 35, 4291-4296, doi:10.1364/AO.35.004291].**
- **Page 34285 line 14. When discussing constraints on IWP, they should cite Sourdeval et al. (2015). In that paper, a technique for directly retrieving IWP from global solar and IR measurements is demonstrated in the presence of multi-layer cloud [Sourdeval, O., C.- Labonnote, L., Baran, A. J. and Brogniez, G. (2015), A methodology for simultaneous retrieval of ice and liquid water cloud properties. Part I: Information content and case study. Q.J.R. Meteorol. Soc., 141: 870–882. doi: 10.1002/qj.2405].**
- **Page 34286 line 7. Two papers published in 2015 are cited to support the applica- tion of surface roughness to improve solar, near-ir, and ir retrievals. I agree with this statement, but the application of this in addition to ice crystal complexity in the form of ice aggregates also leads to more consistent retrievals as demonstrated by Baran and Francis (2004) using very high-resolution solar and infrared measurements [Baran, A. J.**

and Francis, P. N. (2004), On the radiative properties of cirrus cloud at solar and thermal wavelengths: A test of model consistency using high-resolution airborne radiance measurements. Q.J.R. Meteorol. Soc., 130: 763–778. doi: 10.1256/qj.03.151].

[Response 5] Authors appreciate suggestions of additional references. We added these citations to the revised manuscript.

[Comment 6] The authors need to state before page 34299 that their analysis is based on a pixel-by-pixel approach. Perhaps in section 1 or when they first start to use POLDER data?

[Response 6] We added underlined parts to Section 1.
(1) P34286, L29 (Paragraph 9)
and in the conventional "best-fit" approach, even random observational errors can modify the inferred histogram significantly *when it is applied to individual pixels*.
(2) P34287, L17 (Paragraph 10)
This paper demonstrates how a continuous parameter space for the roughness retrieval is constructed and how it can be used to infer the particle roughness of optically thick ice clouds *on pixel-by-pixel basis*.

[Comment 7] The previous best-fit approaches were mostly based on super-pixels derived from the POLDER product. Could the use of super pixels reduce the problem shown in Figure 1?

[Response 7] The problem shown in Figure 1 is a result of two properties: (1) random error of measurements that causes "leakage" of pixel counts to the neighboring bins in the histogram, and (2) the nonlinearity between the phase matrix response and changing roughness parameter that modifies the distribution of "leaked" pixels.
The use of super pixels reduces the magnitude of random errors, ameliorating the problem shown in Figure 1 to some extent. However, the non-linearity remains, and the location of the peak still depends on the intervals of predetermined roughness values.

[Comment 8] The authors use the term "satisfactory" several times throughout the paper when comparing model results. Please quantify this statement? What do they mean? Please provide a quantitative statistical measure to these statements.

[Response 8] These judgments are not based on the statistical hypothesis test. Some of them are redundant. We rephrase the word with more precise expressions.
(1) P34290, L14 (End of the paragraph continuing Eq. (4))
…, but *their agreement justifies the use of the simple statistical model formulated in Eq. (4) to quantify the magnitude of measurement errors*.
(2) P34294, L25 (next paragraph after Eq. (6))

The fast model constructed in this way is accurate enough to solve our inverse problem. A typical difference…
(3)P34314, L2 (Legend in Fig. 7)
Reconstructed $-P_{12}$ (colored solid lines) *agrees with* original $-P_{12}$ (black dashed lines).

[Comment 9] Section 2.2.1 page 34292. Over which size parameter ranges were the IGOM and ADDA methods applied? In the case of ADDA, how was surface roughness represented? The eight-branched hexagonal ice aggregate, is this the same as modelled by Yang and Liou (1998)? If so, please cite the reference as follows [Single scattering properties of complex ice crystals in terrestrial atmosphere. Contrib. Atmos. Phys.,71,223–248].

[Response 9] ADDA is applied when the particle maximum dimension is less than 10 μm (approximately size parameter of 70), and IGOM is applied for larger particles. The surface roughness is omitted in the ADDA method and applied only in the IGOM. The shape of an ice crystal is defined in the suggested reference for the first time, but there is a typographical error in the parameter. Correct values are shown in Yang et al. 2013, which is cited in the original manuscript. We added the suggested publication to clarify this point.
P34292, L21 (Second paragraph of Sect. 2.2.1)
Surface roughness is applied only in the IGOM computation ($D_{max} > 10\ \mu m$), and ten prescribed roughness values …
P34292, L27 (Second paragraph of Sect. 2.2.1)
This particle shape is an aggregate of eight column elements that are solid hexagonal particles with slightly different particle aspect ratios *(originally defined by Yang and Liou 1996, see Yang et al., 2013 for geometric parameters)*

[Comment 10] Section 3. The authors estimate surface roughness parameters well beyond their theoretical limit. Given the techniques employed to represent surface roughness, retrieving sigma values »1 demonstrates a failure in the model, which is pointed out. They do not find any solutions beyond their theoretical limit and must rely on extrapolation to obtain an unphysical surface roughness estimate of 2.82, tending to an upper value of 13.6! Relying on extrapolation to obtain these gross values is very unsatisfying. Surely after a certain sigma value the –P12 element converges until their is no longer information on sigma? They can demonstrate this theoretically by simply showing a figure of the –P12 element as a function of sigma. It could be that they are retrieving unphysical values due to their being no information on sigma? As the –P12 pattern becomes asymptotic at the most extreme values of sigma. Please comment and show results?

[Response 10] The definition of our surface roughness is described in Yang and Liou (1998), and the roughness parameter can exceed 1.0. Please be reminded that the definition is different from Macke (1996)'s work.
We agree with the referee that the shape of $-P_{12}$ becomes gradually insensitive to the change in roughness with increasing degree of roughness. This would results

in the unstable retrieval if it were conducted over the roughness space. Our inference is applied on the EOF space, so the instability is not likely to be the cause of the large roughness parameters.

We do not conclude that 2.82 is unphysical solely due to the magnitude. Our main concerns are that (1) re-entrance of the light into the particle may not be properly represented due to the computational technique used in IGOM for such a large value of roughness parameter, and (2) the actual particle shape that has such a high degree of surface roughness is difficult to come up with.

We do agree with the referee that the extrapolation is problematic, but the extrapolation is still a reasonable guess when assuming the change in phase matrix continues to occur in the same trend. The conclusion drawn from this value is that the addition of a significant degree of roughness is necessary, and the column aggregate particle shape requires unphysically deep roughness to simulate the observed polarized reflectivity in the current IGOM+ADDA framework. It may be more clear by referring to the fraction of pixel containing $\sigma^2 > 0.7$ (Comment 11) to address this point. To clarify our conclusion, we add the following sentence to the manuscript.

P34298, L15 (Sect. 3, Paragraph 3)

The proportion of pixels that contains inferred roughness parameter $\sigma^2 > 0.7$ is 74%, which also indicates the limit of this particle shape.

[Comment 11] It is difficult to see from Figure 13 the proportion of the sample that contains retrieved sigma values > 0.7. Please state this proportion? Is this proportion location specific? Cloud-top height specific? Aerosol above cloud? Please comment and show results?

[Response 11] The proportion of sample that satisfies σ^2>0.7 is 74%. We have not investigated the locality in both time and space. This is our current work in progress.

[Comment 12] Apart from the above, a further possibility is that the particles could be hollow as well as surface roughened. This is mentioned in the case of rosettes, but another form of hollowness is stepped cavities and these appear to be frequently occurring as shown in laboratory studies conducted by Smith et al. (2015) [Helen R. Smith, Paul J. Connolly, Anthony J. Baran, Evelyn Hesse, Andrew R.D. Smedley, Ann R. Webb, Cloud chamber laboratory investigations into scattering properties of hollow ice particles, Journal of Quantitative Spectroscopy and Radiative Transfer, Volume 157, May 2015, Pages 106-118, ISSN 0022 4073,http://dx.doi.org/10.1016/j.jqsrt.2015.02.015]. This is an interesting form of cavity as multiple scattering increases due to the stepped nature of the cavity, and consequently, the asymmetry parameter values decrease relative to the more conventional cavity types. This behaviour is similar to spherical air bubble inclu- sions and will affect the –P12 element in a similar way to surface roughness without having to over prescribe surface roughness values.

Another possibility is that the ice aggregate model might be too compact and as a result the multiple scattering between the individual monomers that make up the aggregate might well over estimate the side scattering and so incorrectly decrease the linear polarization. Natural ice aggregation due to gravitational sedimentation tends to more spatial ice aggregates (by spatial, I mean multiple interactions between monomer particles can be neglected). In this way, the identity of linear polarization is retained, which is eventually removed by hollowness, surface roughness or a combination of both. The authors should also consider the inclusion of more spatial ice aggregates in their future studies.

A further reason for the number of failed retrievals could be due to lack of information. This number might be reduced if other independent measurements were made available to the retrieval through the greater use of radiometric measurements at different wavelengths as well as active measurements to constrain the cloudy profiles of IWC or IWP and cloud-top height through lidar backscatter and linear depolarisation measurements. All these techniques as the authors are well aware will make use of other scattering matrix elements which they so far neglect. They might like to point out the wealth of information that is now available and the relative ease with which it can be incorporated into a PC-based retrieval scheme.

The tropical results are very interesting. I agree that the failure in the tropics is more likely due to errors in the prescribed scattering model. A paper by Baran et al. (2012) [Baran, A. J., Gayet, J.-F., and Shcherbakov, V.: On the interpretation of an unusual in situ measured ice crystal scattering phase function, Atmos. Chem. Phys., 12, 9355– 9364, doi:10.5194/acp-12-9355-2012, 2012] shows that averaged in situ Polar Nephelometer measurements obtained in a convective cloud could only be explained by the inclusion of quasi-spherical particles, in that case represented by Chebyshev particles. Could this be the reason for the retrieval failures in the tropics? This is a further model or variant thereof that is worthy of future investigation by the authors.

[Response 12] The authors greatly appreciate the thoughtful comments by the referee. The retrieval failure in the tropics is possibly because of the weakly constrained height retrieval, as well as particle shapes. We are attempting to use height data from the CALIOP instrument to avoid the underconstrained retrievals. In addition, the modification of IGOM is in progress to improve the approximation. We will add the particle shapes suggested by the referee for our future choices.

We stress that the EOF technique described in this paper is applicable to any set of phase matrices, and reduces the degrees of freedom significantly. Careful treatment of error by using inversion over the EOF-based continuous parameter space will benefit the community to conduct systematic exploration of particle shapes, degrees of roughness, and other types of impurities.

[Comment 13] Figures 12-14. The heights of the histograms exceed the values along the y-axis. Please re-plot so that the histogram heights do not exceed the y-axis values. Otherwise, it is difficult to estimate probabilities from the graphs alone.

[Response 13] We re-plot them in the revised manuscript.

[Comment 14] Along with the retrieved sigma values that are within their theoretical range of sigma, could they provide the corresponding values estimated for the asymmetry parameter? Or is this too much of an extrapolation at the moment?

[Response 14] We believe that it is too much of an extrapolation at this moment. However, we acknowledge that the referee pointed out the possibility of asymmetry factor retrievals.

Referee 2

The comments by referee 2 are insightful, and helpful to contemplate about the obtained result. We summarize the points of discussion and comment for each of them.

[Comment 1] In the tropics, the chi-square is much larger than expected. In the extra-tropics, although the chi-square values are in line with expectations, the roughness parameters that are found are way outside the range of values used for the forward simulations. The discrepancies are not properly analysed. There is no information whether the inconsistency between the theoretical simulations and observations results from the amplitude of the polarization, its spectral variations, or its directional properties.

[Response 1] The complete analysis of the discrepancy between the simulation and the observation is our ultimate goal, and analyzing all physical processes and disentangling source of error are very challenging work. We admit that we do not perfectly understand the cause of inconsistency, but we emphasize that the novel approach proposed in this paper contributes significantly to the quantitative analysis of the polarized reflectivity of clouds.
This study focuses on the spread of retrieved roughness parameter due to measurement errors that are typically ignored by the conventional "best-fit" approach, and outlines a theoretical development that can incorporate this type of error into the theoretical framework. The true value of the EOF technique is that it can reduce the degrees of freedom significantly, and help discern if a specific set of phase matrices fits with observations.
The result shown in this paper indicates that the aggregate of columns shape is not an ideal model. Our current hypothesis is that an unphysically large roughness parameter is due to the weak side scattering of the column-aggregate particle shape. To more clearly state this point, we added the following paragraph to the Sect. 3.3.

P34299, L21 (Sect. 3.3, paragraph 2)

The reconstructed $-P_{12}$ shows stronger side scattering between 80° and 120° than the MODIS Collection 6 particle model. As the increasing roughness enhances side scattering, weak side scattering of the column aggregate shape may be responsible for the unphysically large roughness parameter in the extratropical inferences. By using a shape that has stronger side scattering, it is likely that the degree of roughness that is needed to explain the observations becomes smaller. An example of such a habit mixture is shown by the thick magenta line in Fig. 16.

[Comment 2] It should be very clear that the "surface roughness" of the ice particles (in the title) is only an effective parameter attempting to reproduce the polarization properties of ice clouds with a very simple mono-dispersive crystal shape (a very strong assumption).

[Response 2] This claim is applicable to the conventional "best-fit" approach, but not to our EOF approach. Our parameter space implicitly allows any mixture of differently roughened particles. As a result of natural variation and various error sources, the distribution of retrieved roughness parameter has a large spread. We provide some representative values to facilitate the discussion about the particle model, but we do not intend to claim that the aggregate of column particles with σ^2=2.82 can explain all extratropical data samples. Our conclusion is that the addition of a significant degree of roughness is necessary, and the column aggregate particle shape requires unphysically deep roughness to simulate the observed polarized reflectivity in the current IGOM+ADDA framework.

[Comment 3] In addition, the abstract does not mention that the results are rather inconsistent with the theoretical assumptions.

[Response 3] We included the statement about the inconsistency to the abstract.
P34284, L13
However, the present theoretical results *do not agree with observations in the tropics. In the extratropics, the roughness parameter is inferred but 74% of the sample is out of the expected parameter range.* Potential improvements are…

[Comment 4] At the very least, the authors should extend the range of the roughness parameter used in their theoretical computation to the values that are found by extrapolations in the real data analysis. Does the conclusions of the paper remain the same ?

[Response 4] The extension of the parameter space is technically possible, but it is questionable if the phase matrix for such a large roughness parameter has physical significance. Rather than expanding the parameter space to obtain a pragmatic but unphysical value, more constructive approaches are to look for more appropriate particle shapes in our current scattering library and to improve the computational technique in order to handle deep roughness.
We applied the inference technique outlined in this study with other particle shapes and found that the solid bullet rosette shape requires relatively mild degree of roughness to agree with observations (σ^2=0.7). In addition, even the

column-aggregate shape can simulate the observation well with less intense roughening when an experimental IGOM program is used.

Again, our conclusion is that the addition of a significant degree of roughness is necessary, and the column aggregate particle shape requires unphysically deep roughness to simulate the observed polarized reflectivity in the current IGOM+ADDA framework. Our future work is to identify the particle shapes and degree of roughness that are physically and optically consistent with observations.

[Comment 5] What is the characteristics of these measurements that cannot be explained by the theoretical simulations ?

[Response 5] This is another very good point of discussion. The collocated POLDER3-MODIS-CALIOP dataset shows that the cloud top height retrieval often fails when the cloud is multi-layered. Even for a single-layer cloud, the cloud top height retrieval is less constrained than in the extra-tropics, possibly because of the satellite viewing geometry.

[Comment 6] Also, according to the authors, the results are significantly different in the tropics and extra-tropics. Is it really because the clouds are different or because the viewing geometry characteristics vary with the sun angle and are therefore different in the tropics and extra-tropics ?

[Response 6] The main difference between the tropics and the extra-tropics is a much larger chi-square mean and "tail" in the tropics. This difference indicates no more than that our model is not appropriate in the tropics. As the referee suggests, the viewing geometry is also a possible cause of the difference in two datasets. We revise Section 3.2 as follows.

P34199, L19 (Section 3.2, Paragraph 2)

… cloud heterogeneity, *<u>or the lack of information content due to the limited scattering angle range</u>* are therefore suspected as causes of the …

[Comment 7] The part of the paper that estimates the noise in the PARASOL polarization measurements is based on the very strong assumption that the polarisation is zero for a scattering angle of 170°. Also, the authors mention a study by Fougnie that estimates the polarized reflectance noise. Why not take the value from this analysis. At the very least, the alternative result should be provided and discussed.

[Response 7] We are aware that there is an uncertainty where the polarization becomes zero in the phase function. So the estimated error contains the natural variability of ice cloud scattering property as well as observational noises. This point is clearly stated in P34290, L27. Our goal here is to model the observational noises (including mis-registration noise) by using a simple statistical model, and to demonstrate that the maximum likelihood method formulated for the normal distribution is a reasonable choice. As Fougnie's estimate does not provide the information about the distribution, it does not replace our parametric bootstrapping approach.

Other comments

[Comment 8] It should be more clear that Abstract, line 13-14: "The present theoretical results are in close agreement with observations in the extratropics but". This is a rather surprising statement as the results in the extra-tropics are clearly outside the range of the theoretical simulations.

[Response 8] We agree with the referee. This is a misleading statement. We revised this part of the abstract. Please see Response 3.

[Comment 9] P34286, l 25: It appears that the present study is less advanced than that of Diedenhoven. The study is mentioned in the introduction but the results are not compared. Why ?

[Response 9] The focus of work by van Diedenhoven and ours are quite different. We applied the EOF analysis to the most simplistic case to demonstrate how the theoretical development applies to the actual data. His work is designed to estimate the asymmetry factor by using the simple model, allowing the presence of null space in the model. Therefore, his results cannot be compared.

[Comment 10] P34286, l 25: ". . . it is not suitable for analyzing local variability". This criticism is surprising as the present paper does not analyze the local variability.

[Response 10] We failed to express what we want to deliver. We appreciate the referee's careful review. We rephrase the sentence as follows.

P34286, L27 (Section 1, paragraph 9)
While a conventional "best-fit" approach can constrain the range of the *average* roughness parameter *at the global scale*, it is not suitable for *the pixel-by-pixel inferences*.

[Comment 11] P34289 l 11. I do not quite understand the use of the eta parameter in the equation. Indeed, a signed version of the polarized reflectance is never used in the present paper. Besides, it leads to a rather strange behaviour of the modified polarized reflectivity in the vicinity of 170. (with positive and negative values, but nothing close to zero).

[Response 11] The referee points out the "strange behavior". This strange behavior is what is exploited in our error analysis using the bootstrapping method. To demonstrate that there is no value around zero, we showed this figure about 170°. Our claim is that the gap is caused by the measurement noise.

[Comment 12] P34290 l 1. "where random variables". X_i are not random variables but measurements!

[Response 12] It appears that the referee misinterprets the equation. The equation is not a deterministic equation to obtain L_{np} value but a statistical model to apply distribution theory. We clarify this point in the revised manuscript by editing as follows.

P34289, L25 (Equation 4 and the associated paragraph)
We define a random variable L_{np} that serves as a statistical model of observed L_{np} as follows.

$$L_{np} = \sqrt{X_1^2 + X_2^2 + X_3^2 - X_1X_2 - X_2X_3 - X_3X_1}$$

where random variables X_1, X_2, and X_3 represent the radiances of a pixel in the original three images with different polarizers (not available in a product). *With the statistical model outlined* in Eq. (4), we *first* assume that X_1, X_2, and X_3 follow the same normal distribution

[Comment 13] P34290 l 2. "because the average polarization". The value of importance here is not the averaged polarization but the actual value.

[Response 13] This was an incorrect wording. The statistical model predicts the distribution of observations about zero polarization. The predicted and observed distributions are compared in Fig. 3. We revise this part of the manuscript as follows.
P34290, L4 (Same paragraph as above)
because the *expectation of modified polarized reflectivity L_{nmp}* is assumed to be zero

[Comment 14] P34291 l1-2: We apply the same variance to all three POLDER channels used in the analysis (0.865, 0.67, and 0.49 µm). Why not provide the results of this analysis. Are similar values found ?

[Response 14] This is because the 0.865 µm channel is expected to be least contaminated by other sources of variability, such as ozone content (0.67 µm) and cloud top height (i.e. Rayleigh scattering, 0.67 µm, 0.49 µm). We add the following sentence.
P34291, L1 (Last paragraph of section 2.1.1)
We estimate the magnitude of error using 0.865 µm channel because the channel is likely to be the least contaminated by other sources of uncertainty such as ozone absorption (0.67 µm) and Rayleigh scattering (0.49 µm, 0.67 µm).