Reply to Interactive comment to amt-2015-339

Dear Dr. Miffre.

We thank you very much for your comments, which give us the chance to elaborate some peculiarities of our approach for the numerical error calculation.

This manuscript, published on 08 Feb 2016, uses the theoretical model as described in detail in the companion manuscript by Freudenthaler, 2016, doi:10.5194/amt-2015-338, which was published on 11 Feb 2016 as discussion paper together with this manuscript in the same journal and the same special issue, for the calculation of polarisation dependent systematic errors of a variety of lidar systems. We think that it is appropriate to split the long theoretical part and the part with the numerical error calculation in two papers in the same issue, so that one can refer to the other and not all details and all references and related discussions have to be repeated again. However, the basic theory, which is necessary to follow the concepts of this manuscript, is included.

This manuscript just uses the theory of amt-2015-338 for a comprehensive numerical error assessment with a complete search of the systematic, polarisation dependent error space. In this error assessment we include the error space of the calibration factor depending on the uncertainties of all model parameters. Such an error assessment, which includes simultaneously all the error sources of our model, seems not possible by means of analytical error analysis; at least nobody showed it yet.

Furthermore, in the manuscript we compare the complete uncertainty of the volume linear depolarisation ratio of eight different lidar systems with various setups and calibration techniques on the basis of the proposed model in amt-2015-338. These results can be used as a reference in scientific works employing the investigated lidar systems.

We have the impression that the majority of your comments rather pertain to the theoretical part amt-2015-338. In the view, that the two manuscripts belong together, we will answer your comments below.

1. In their manuscript, Bravo-Aranda et al. indicate that they propose a new tool to assess the lidar polarizing sensitivity and to quantify the error on the depolarization due to the unknown systematic errors. To my knowledge, such a discussion on the sensitivity and accuracy of polarization lidar measurements was proposed in (G. David et al., Appl. Phys B, 108, 197-216, 2012) through laboratory and field experiments, by quantitatively discussing all possible systematic errors, including the following ones that are here analyzed: a small-unpolarized component in the emitter laser beam, an imperfect separation of polarization components, a misalignment between the transmitter and receiver polarization axes. Could the authors situate their work in regards to other published works and identify the novelty of their work? I feel quite surprised that this paper is not quoted.

**Answer**: As it is stated in the abstract, ‘this work presents a new tool to assess the lidar polarizing sensitivity and to estimate the systematic error of the volume linear depolarization ratio (δ), combining the Stokes-Müller formalism and the Monte Carlo technique’. The mentioned citation has been included in the manuscript (page 2, line 17). David et al., 2012, is referenced in amt-2015-338. It discusses the influence of several error sources individually, not in combination, and not including the rotational errors between them or the retardation of the beamsplitters. Furthermore, the neglects are special for the instrument described there.

2. As a reader, it seems to me that the term of "polarizing sensitivity", as proposed all along the manuscript, needs some clarification. Could the authors add precision?
**Answer**: this phrase has been removed or replaced all along the manuscript.

3. The Stokes-Mueller matrix formalism is here used by introducing the Mueller matrix of the emitter optics and of the receiver optics with reference to Freudenthaler (2016a) all along the manuscript, a reference that is not yet published and hence not yet reviewed. For the sake of clarity, I propose to the authors to use reference to the publication by G. David et al. (Opt. Exp., 21, 16, 18624-18639, 2013) where with my co-authors, we stated the Stokes-Mueller matrix formalism for a pulsed laser source in the lidar backscattering geometry by introducing the same emitting and receiver optics Mueller matrices and the corresponding formalism. Could the authors situate their work in regards to this published work? I feel quite surprised that this paper be not quoted.
**Answer**: Please see the general comment above.
Furthermore, there was a mistake in the References of the manuscript using a title of Freudenthaler (2016a) which had been changed in the meantime. We apologize for that.
David et al., 2013, is referenced in amt-2015-338. As explained above, we believe that Freudenthaler (2016a) (i.e. amt-2015-338) can be used as a reference. In fact, it has to be used, because David et al., 2013, or any other paper do not provide the complete model presented there. Furthermore, David et al., 2013, are not the first to propose the use of the Müller-Stokes formalism. However, Freudenthaler (2016a) cannot be replace by the other one because the simulator is based on the new general formula presented inside.

4. When reading the manuscript, the reader gets the impression that the approach that is here proposed (the polarization lidar simulator) is the only possible one to address the sensitivity and the accuracy of lidar depolarization measurements. In David et al. (APB, 2012), we used a somewhat different approach by addressing the detector transfer matrix, which allows, as being diagonal, to perform a robust calibration, after minimizing all possible effects on the emitter optics. What additional information(s) do the authors get by introducing the Stokes-Mueller matrix formalism compared to our contribution? Whether this formalism is necessary or not should be clearly stated for potential readers.
**Answer**: As described above, we present a complete numerical search of the error space including all parameters of the model. We believe it is sufficiently clear for the reader that this approach excels analytical error calculations which consider only a few error sources individually.

5. The manuscript only deals with 1_-polarization lidars while many papers have been published with 2_-polarization lidars such as Shimizu et al., 2004 or Tesche et al., 2009, as well-known. Such polarization lidar stations rely on wavelength-dependent components, that may attenuate the backscattering intensity and also modify the polarization state of the backscattered radiation in agreement with Fresnel's formulas. How do the authors account for this important contribution? Is their conclusion similar to that published in David et al. (APB, 2012)? Different?
**Answer**: In our manuscripts we analyse the possible errors of each individual signal channel of the lidar model. Each channel is only used at one wavelength. The channels used at other wavelengths have to be described with

the optical parameters for that wavelength. We do not deal with errors arising from a combined analysis of signals at different wavelengths.

6. Using a polarization simulator may be interesting but it is not to my knowledge the only possible approach. To address accurate lidar depolarization measurements or to calibrate the lidar depolarization, laboratory experiments at exact 180_ are nowadays available and can be used to quantitatively address this quantity, even for non-spherical particles such as mineral dust particles, and at two-wavelengths, as we recently published (Miffre et al., JQSRT 2016). I think adding such information may be useful for potential readers of your manuscript, as it complements your approach. Indeed, a robust calibration has to rely on accurate laboratory measurements. That's why, I am proposing to add this reference that complements your work.

**Answer**: This manuscript is about error calculation, not about calibration. In the theoretical part, which describes several polarisation calibration techniques, we reference some publications describing alternative calibration techniques. However, Miffre et al., JQSRT 2016, do not describe a new technique but use the well-known method of Alvarez et al., J. Atmos. Ocean. Technol. 2006. Furthermore, a single laboratory calibration would not be sufficient. The actual calibration factor of a lidar has to be determined repeatedly over time in order to detect or exclude temporal changes.