Thanks for the review and interest in our measurements. We are pleased with the reviewer's overall evaluation of our manuscript, which acknowledges the novelty and quality of our measurements. However, the reviewer also expressed skepticism about the subsection that describes the smoke case study, based primarily on the fact that similar results have not been observed before. We must disagree with this. There are no other published depolarization measurements of smoke at 355 nm that we are aware of, so there is not enough information available to the science community about the spectral depolarization of smoke, or whether there is significant variability depending on factors like age, composition, combustion efficiency, and humidity. Indeed the reviewer's confirmation that these are unusual measurements makes us all the more eager to publish them and get them out into public discussion. We feel we have provided ample explanation of the measurement technique and uncertainties to support our measurements and stated conclusions. We do partially agree with the reviewer's feelings that the current state of particle modeling is insufficient to thoroughly explain the observed spectral dependencies (either in the more familiar dust cases or in the more unusual smoke case). But again, we do not agree that this means that discussion of these results should be curtailed rather than subjected to open scientific scrutiny. We discuss the current state of modeling because it is the best that is currently available for explaining spectral depolarization measurements. We hope that more researchers with modeling expertise may also become interested in these observations and move this work forward so we as a community will gain a better understanding of the observations. Similarly, we hope that other researchers will publish their 355 nm depolarization measurement of smoke, and multi-wavelength depolarization measurements generally, whether or not they agree with ours, to help move the field forward.

Below, the reviewer's comments are in blue and our responses are in black.

General

This is an exciting contribution to the lidar literature. For the first time, airborne polarization lidar measurements are performed simultaneously at three wavelengths. Several case studies are presented and corroborate that high quality observations with a unqiue lidar setup could be realized. However several points must be improved. The smoke case study triggers many questions that must be checked and answered.

Detailed comments:

Page 24754, second paragraph:

The introduction should be improved. In such an important paper, the field of aerosol depolarization studies must be better reviewed. References to the work of Japanese groups (Sugimoto, Nishizawa and further papers) must be given. The milestone-like work of the SAMUM and SALTRACE groups should be cited (Freudenthaler, Gross, and Tesche papers in Tellus 2009 and 2011). These efforts including the use of the measured particle linear depolarization ratio to separate dust from non-dust aerosol components (Tesche JGR 2009, Ansmann JGR 2011, ACP 2012, Mamouri-Ansmann, AMT 2014) pushed the depolarization lidar work really forward and offered many new opportunities for important quantitative aerosol studies. CALIPSO and EarthCARE benefit from all this basic work (e.g., via Amiridis, ACP 2013). A nice and well balanced introduction increases the importance of the next large step presented in this paper (three wavelength depolarization lidar). Do not hesitate to cite your own recent papers (Burton in ACP and AMT, 2012, 2013, 2014) together with the Gross papers (2011, 2013, 2015: : :). All these aerosol-typing papers are important contributions to the field and should show up in a compact brief introduction.

I understand that a paper from a NASA group wants to put all the spaceborne lidar activities into the center of interest. But polarization lidar is clearly a stand-alone science field and offers exciting possibilities. So, at least in the second paragraph, one should provide a brief introduction into the field of polarization lidar for aerosol studies (not for cloud studies, that is not necessary here).
It should also be mentioned, that there is another group that does these simultaneous three-wavelength depolarization measurements (27 ILRC paper, Haarig et al.). The Leipzig group gave a presentation at the ILRC conference at New York and showed many desert dust cases from their Barbados SALTRACE campaigns which seem to match very well with your results if I remember all the numbers right. Maybe it is possible to compare these results, but I am not sure whether an extended ILRC paper is available or not. Usually they publish 4-page papers in the conference proceedings.

OK, we have revised and reorganized the introduction as the reviewer suggests and included these references. New text:

> *Polarization lidar is an a large and active field, with recent contributions from ground-based networks such as EARLINET (Pappalardo et al., 2014; Mamouri and Ansmann, 2014; Nisantzi et al., 2014) and the National Institute of Environmental Studies East Asian network of lidars (Sugimoto et al., 2005; Nishizawa et al., 2011); directed field campaigns, such as SAMUM (Freudenthaler et al., 2009; Tesche et al., 2011) and SALTRACE (Groß et al., 2015; Haarig et al., 2015); and others.*
>
> *There is also considerable interest in global lidar observations from satellites {…continues with similar text as before…} NASA's airborne HSRL-2 is the first HSRL system making depolarization measurements at three wavelengths. A ground-based Raman system operated by the Leibniz Institute of Tropospheric Research has also been recently upgraded to make three-wavelength depolarization measurements (Haarig et al., 2014).*
>
> *Aerosol classification is one specific application of aerosol polarization measurements (Burton et al., 2012; Groß et al., 2013; Burton et al., 2013; Groß et al., 2014). Aerosol particle depolarization ratio from lidar is of key importance for the detection and assessment of dust and volcanic ash since it is a clear indicator of non-spherical particles. The particle depolarization ratio is also used to infer the amount of dust or ash in a mixture (Sugimoto and Lee, 2006; Tesche et al., 2009a; Tesche et al., 2011; Ansmann et al., 2011; Ansmann et al., 2012; David et al., 2013; Burton et al., 2014; Mamouri and Ansmann,* 2014). It is also sensitive to the size of the non-spherical particles (Ansmann et al., 2009; Sakai et al., 2010; Gasteiger et al., 2011; Gasteiger and Freudenthaler, 2014).
>
> {Continues with original text about dust and smoke measurements specifically.}

Page 24755, second paragraph:
Regarding smoke and dust separation with polarization lidar, I like this Tesche paper in Tellus 2011 very much. They distinguished African biomass burning smoke from desert dust and came finally even up with single scattering albedo values for the smoke part. That paper should also be included in the references here.
It has been added. See above.

Page 24756, Sect. 2, Instrumentation:
Can we have detailed information on the aircraft flight heights a.s.l. for all the case studies discussed later on? I have strong doubts that this strange smoke case with very large 355 nm depolarization ratio is based

on good signals. I speculate that overlap or overloading problems may have caused these strange results. I will come to this point later again.

We added flight altitude to the description here in the instrumentation section ("The typical flight altitude of the B200 during lidar operations is 9 km"), and for the smoke flight specifically, the caption to figure 14 now notes that the flight altitude is 8970 m.

See below for responses regarding the overlap.

Page 24759, Eq.(2), please provide a reference for the equation

Thanks for pointing out this omission. It is Cairo, F., Di Donfrancesco, G., Adriani, A., Pulvirenti, L., and Fierli, F.: Comparison of Various Linear Depolarization Parameters Measured by Lidar, Appl. Opt., 38, 4425-4432, 1999.

Page 24761, Sect.3.1,

Line 22, particle depolarization ratios of 30.4% indicate desert dust backscatter fraction of only 80-90%? I would say the fraction is then close to 100%.

Sugimoto and Lee (2006) give the mixing ratio as

$$X = \frac{(1 + \delta_{d532})\delta_{532}}{\delta_{d532}(1 + \delta_{532})}$$

Plugging in 0.304 for δ_{532} and the same value they use, 0.35, for δ_{d532}, yields 90%. There was no good reason for us to include a wider range for the dust fraction estimate than for the quoted particle depolarization ratio so in the revision it has been changed to "approximately 90% dust".

It would be really nice if we can have a full set of profiles of all the retrieved optical properties, maybe based on 10 sec or 30 sec signal averages or what ever is appropriate in case of aircraft HSRL observations. Then we could best compare your results with other publications in this field. I would recommend: left plot: profiles of particle backscatter at all three wavelengths, center left: profiles of particle extinction coefficient at both wavelength, center right: profiles of lidar ratio at both wavelength, right panel: particle linear depol ratio at all three wavelength. Profiles showing the depol ratio and the lidar ratio together provide the essential basis of all the aerosol typing studies (which includes your own paper Burton et al, AMT,2012, but also Gross et al., 2013 and 2015, even in Illingworth et al., EarthCARE paper in BAMS, 2015, such a plot with lidar ratio versus depol ratio is given).

As mentioned in the manuscript, range dependence of the aerosol and molecular signals in the overlap region prevents us from calculating extinction (and therefore lidar ratio) within the smoke plume (but not particle depolarization ratio, which we discuss below). We prefer not to rely on simulations of the overlap region or perform analytical formulations to correct the overlap of the system, since these would introduce additional uncertainties. However, this is not primarily a typing study but a study of the spectral dependence of depolarization, so we think it should be acceptable to focus only on the depolarization. It's not that we don't agree with the researcher that it would be nice to have the lidar ratio for the smoke case, but in this case, unfortunately, the aircraft altitude was not high enough to allow that. In any case, the broad characterizations of "dust" and "smoke" that aerosol typing methodologies (like Burton et al. 2012, Gross et al. 2013, etc.) allow would not add much information to these particular cases. The two dust cases are not really in doubt; there is no other aerosol type in the typing methodologies with such high 532 nm and 1064 nm particle depolarization ratios (except volcanic ash which is unlikely in the circumstances). For the smoke case, the identification of smoke is well supported by back-trajectories, knowledge from satellite data that smoke was blanketing the entire region, and most importantly, an out-of-the-window visual ID of the smoke plume. Certainly a measurement of lidar ratio would be extremely interesting from the point of view of expanding our understanding of aerosol typing methodologies for an unusual case which may indicate differences between different ages or compositions of smoke, and also for linking 355 nm and 532 nm, which is wanted for future satellite measurements. Unfortunately, that's just not possible in this case, since we cannot calculate high confidence profiles of extinction and lidar ratio within the plume. However, we can provide high confidence profiles of particle depolarization ratio at three wavelengths, since quantities that are the ratio of two channels are not affected by range-dependence in the overlap region, so that is what we discuss in this manuscript. The reviewer says both particle depolarization and particle lidar ratio are necessary for aerosol typing studies, but this is not an aerosol typing study. As the reviewer points out in comments about the introduction, particle depolarization is an important and relevant field of study even by itself.

Page 24763, Sect. 3.2

Line 17, The AOT of the layer on 8 Feb is just 0.02 at 532 nm, and you can still have good particle depol ratios? The 1064nm backscatter and the 532-1064nm Angstrom exponent should at least be rather uncertain. Sakai et al (Appl. Opt 2010) found values close to 39% depol for 'pure' coarse-mode dust. So your results seem to be in agreement with these measurements.

The AOT is low because the layer is so shallow, but the scattering is very strong. Total aerosol scattering ratio at 532 nm is 3.1, higher than the other two cases. This is primarily what drives the uncertainty. We've added total scattering ratio values in the revised text.

Page 24765, line 22, please mention here also Sakai et al. (2010)

We added it in the introduction, where we note that particle depolarization depends on the size of the particles. At this particular spot on page 24765, the text is specifically about spectral dependence, and since Sakai et al. (2010) measured only at one wavelength, the introduction seemed more appropriate.

Page 24766, last two paragraphs of Sect.3.2:

Simulation studies are at all not just trustworthy and should therefore be interpreted with caution The unknown shape characteristics does not allow proper conclusions from simulations of the wavelength dependence of particle linear depol ratio.

We agree that the results should be used with caution. We don't mean to suggest that the theoretical results should be used quantitatively, but we think that these calculations can be reasonably interpreted to suggest that (1) particle depolarization ratio spectral dependence is broadly sensitive to size and (2) specifically, it is reasonable to suppose that a shift in the wavelength of the peak of particle depolarization ratio may be an indicator of increasing particle size. We simplified the discussion of the theoretical papers and tried to make it clear what the limitations are, and that we are drawing only very general conclusions from them. Here is the revision for these paragraphs:

> *Theoretical calculations to date have shown that it is difficult to quantitatively predict the spectral dependence of the particle depolarization ratio for dust (Gasteiger et al., 2011; Wiegner et al., 2009; Gasteiger and Freudenthaler, 2014), due in part to the need for parameterizing the shape of the dust aerosols as spheroids or other simplified shapes.*
>
> *In a theoretical treatment of a particular measurement case, Gasteiger et al. (2011) modeled particle depolarization ratio at multiple wavelengths using size distributions and refractive indices appropriate for SAMUM measurements, parameterizing the shapes of dust particles using spheroids. For their reference distribution, the modeled particle depolarization ratio reflects a spectral dependence with a peak in the middle of the wavelength range. Calculated*

values at 355, 532, 710 and 1064 nm were 0.275, 0.306, 0.311, 0.298, consistent with the measurements we report for the Saharan dust-dominated cases from the NASA HSRL-1 and HSRL-2. However, for the dust-dominated cases in the immediate vicinity of North American sources, the measured maximum shifts to longer wavelengths, and there is no longer agreement with the modeled values at 1064 nm.

Gasteiger et al. (2011) do not show results for size distributions with different size particles, but Gasteiger and Freudenthaer (2014) perform theoretical calculations using spheroids for various size parameters (single particles). These calculations show that the first peak in the spectral depolarization ratio shifts to larger wavelengths as particle size increases. This result, based on highly simplified modeling of dust aerosol, should be used only cautiously, but in general supports the notion that the spectral particle depolarization ratio is sensitive to particle size.

Page 24767, Sect 4.:

You observed a smoke plume at 8 km, and the aircraft was a bit higher. Then you should have an impact of the overlap problem on your measurements. Can you exclude such an impact caused by slightly different beam pathes from the telescope to the photomultipliers of the cross and co-polarized channels. Maybe you made tests at ground and found an almost height –independent depolarization ratio throughout a well mixed PBL and this down to very small ranges to your lidar?

The incomplete geometric overlap is not expected to affect depolarization measurements. We are aware of the range dependent effects of a lidar system and agree that this can be an issue if it is not properly designed or implemented. That is not the case for the system presented here.

As noted in the manuscript, the smoke plume is indeed in the overlap region, which extends approximately 2km below the lidar based our estimates from the extinction calculations when in clear air. The loss of light in the overlap region occurs when the atmospheric target is in the near field and the image is focused beyond the field stop such that the cone of unfocused light overfills the field stop. (Since the system is co-axial, the beam is centered at the field stop even in the near field.) However, there is no range-dependent overlap effect on the ratio measurements such as the volume depolarization ratio (ratio of perpendicular and parallel channels) and the total aerosol scattering ratio (ratio of aerosol and molecular channels), since both channels are equally affected by loss of light from overfilling the range stop. Furthermore, range-dependent effects in the detectors are avoided by imaging the entrance pupil of the telescope (not the field stop) onto the detector, so that the illuminated area on the detector is not range dependent. All the channels have similar optical designs, including pupil imaging, careful optical design to eliminate clipping, and detailed characterization of the detectors and electronics. In addition, the instrument includes an active boresight system to align the incoming beam, and thus all the receiver channels, to the receiver telescope. These considerations mean that ratios of two channels such as the volume depolarization ratio or the total aerosol scattering ratio are not range dependent. This can be checked in cases where the signals are measured in nearly clear air. For example, Figure 5 shows volume depolarization ratios up to approximately 500 m below the lidar and illustrate that there is no range dependence near the top of this profile. (The total aerosol scattering ratio which is not shown also is free of range dependence near the top of the profile). We certainly do not see a significant increase in the 355 nm volume depolarization ratio near the aircraft. In the smoke case shown, aircraft was flying at 8970 m and the top of the smoke layer was 1 km below the aircraft. A range-dependent effect would have to be extreme to see the enhancements in the depolarization shown in Figure 14. Considering this evidence and the design of the system, we are confident the depolarization ratio measurements are not impacted by range dependent effects. To avoid confusion for readers, we added this sentence to the manuscript

“Volume depolarization ratio measurements and total aerosol scattering ratio measurements are ratios of two channels that are equally affected and therefore have no range-dependent overlap function.”

I do not believe this value of 25% depol at 355nm when, at the same time, the other wavelengths show depol ratios below of less than 10 or even less than 3%. There must be something wrong with the 355 nm signals. Are you sure that the signals were well aligned, no overlap problems. Such a strong wavelength dependence between 355 and 532 nm has never been observed). And there are several 355/532nm lidars available now and produced a lot of 355/532 nm depol ratio observations. Simulation studies do not help because of the always unknown shape characteristics.

We don’t share the reviewer’s doubts about the good signals for the 355 nm depolarization ratio. See above for comments about the system, lack of overlap effect on the depolarization ratio, and auto-boresighting to ensure good alignment.

As for the statement that such a strong wavelength dependence between 355 and 532 nm has never been observed, in fact, we were unable to find any published depolarization measurements from smoke cases at 355 nm. Rather we found only published measurements of depolarization from dust at both 355 nm and 532 nm, and published measurements of depolarization from smoke at 532 nm (some cases with “negligible” depolarization and some with linear particle depolarization values up to 8% or so in cases with no evidence of entrained dust or higher for cases that may include some entrained dust or fine mode dust). That is, there are no available measurements that show this strong wavelength dependence *or any other wavelength dependence* for smoke. If we missed some papers on this topic, we’d be happy to have them pointed out to us. In any case, they appear to be rare. We can only speculate on the reasons for the lack of literature about this. Possibly, like the reviewer believes, it is because smoke simply doesn’t in any circumstance create a spectral dependence with larger depolarization at 355 nm, but that is not the only possible explanation or, in our opinion, the most likely one. Based on our own experience, depolarization is a difficult measurement to make and uncertainties at 355 nm are larger than at 532 nm, so it may be not surprising that published measurements at 355 nm are relatively rare. The very fact that reliable three-wavelength observations of depolarization are uncommon is the motivation for this manuscript. We hope that other groups making two and three-wavelength measurements of depolarization will publish more measurements of the spectral dependence of smoke depolarization, because from this first glimpse, it seems that this is a field of study where there is a lot to learn.

The reviewer’s assertion that “There must be something wrong with the 355 nm signals” appears to be based on expectations extrapolated from a sparse data set, and not on the measurements and careful assessment of the uncertainties we have presented. This is unfortunate, but we do believe our measurements and feel that we have presented sufficient information about the instrument and systematic uncertainties, to allow them to be published.

The layer is about 300m in depth and the AOT was estimated to be 0.05 in the green. So it should be possible to compute extinction coefficients and lidar ratios for 355 and 532nm. Based on this optical data set one may be in a better position to discuss this strange observation…

As stated in the manuscript, the optical depth was estimated with an assumed lidar ratio. There is not enough independent information to retrieve a lidar ratio because the overlap prevents a retrieval of lidar ratio or extinction. However, while we have already agreed that lidar ratio would provide additional very interesting information, it’s not clear why the lidar ratio would make the large depolarization in the 355 nm channel more believable to the reviewer. Regardless of whether the lidar ratio is high or low, the unusual spectral dependence of the particle depolarization ratio is still unlike anything previously published, which seems to be the reviewer's main objection.

Page 24769, line 11-28, please include in this discussion (soil dust injection during fire events) the paper of Nisantzi et al., (ACP, 2014).
Nisantzi, A., Mamouri, R. E., Ansmann, A., and Hadjimitsis, D.: Injection of mineral dust into the free troposphere during fire events observed with polarization lidar at Limassol, Cyprus, Atmos. Chem. Phys., 14, 12155-12165, doi:10.5194/acp-14-12155-2014, 2014.
This is interesting because it offers another different speculation about the cause of depolarization (at 532 nm) in smoke, which is "fine mode dust". It's appropriate to add that alternate explanation in the manuscript where we discuss different theories about smoke depolarization, and we do so in the revision. However, the explanation by Nisantzi (2014) is based only on the assertion that "anthropogenic haze, biomass-burning smoke, and marine particles do not produce strong depolarization of backscattered light" for which they reference five lidar papers, four of which do not show any measurements of unmixed biomass burning smoke (three of them concern SAMUM measurements where a significant amount of desert dust, including coarse mode, was considered to be mixed with the smoke). Gross et al. (2012) includes aged biomass burning aerosol with linear particle depolarization ratios of 7$\pm$2% which is not insignificant. It seems to be just a rule-of-thumb that is not supported by enough evidence to warrant it being taken as evidence itself. We don't debate that dust can be entrained in smoke and affect the depolarization; we quote Fiebig et al. (2002) who presents a fairly compelling case which includes coincident chemical analysis. However, we also believe that it can also occur that biomass burning aerosol can exhibit depolarization of backscattered light without necessarily having a dust component, like the observations of Murayama (2004) who observed depolarization at 532 nm but a chemical analysis of the plume showed no mineral content (incidentally, Nisantzi et al. (2014) also reference this case but for some reason discount the chemical analysis and claim that this case also includes fine mode dust.) The lab measurements of dust by Sakai et al. are well done and show that fine mode dust can cause depolarization, but not that it actually causes the observed depolarization in any particular atmospheric observation. The Kahnert et al. (2012) modeling study discussed in this manuscript demonstrates that there is at least one more possible explanation: smoke particles themselves can cause depolarization.

Page 24770, simulations: All the simulations with a spheroidal dust shape model are not trustworthy. The results must be handled with care. Especially the spectral dependence of the depol ratio is rather erroneous.
We cut out most of this paragraph and left only this:

> *Referring back to the theoretical calculations of spectral depolarization for spheroids discussed in Section **Error! Reference source not found.**., the larger particle depolarization ratio at 355 nm compared to longer wavelengths may indicate a smaller size for the non-spherical particles than the dust cases, although of course these results may be only qualitatively applicable to more general particle shapes.*

Page 24771, I am not a friend of speculations as given on this page. Figure 15 is simply useless. I would just remove all this, but leave it open to the authors what to do with this strange case study and all the simulation-based discussions.
The reviewer appears to generally disapprove of current modelling efforts to interpreting lidar observations, which is perhaps a somewhat extreme point of view. We believe that intercomparisons of observations and theoretical studies are useful and necessary. On the one hand, measurements are

indispensable for guiding the development of models. So at the very least, Figure 15 contains useful information for model developers. On the other hand, models are needed in all Bayesian retrieval algorithms to invert observations. We do agree with the reviewer that all modelling studies face great challenges related to the large variability of particle morphologies. We also share his reservations about the use of over-simplified particle models for interpreting depolarization measurements. The model we use for intercomparison in the smoke case is based on a morphologically sophisticated particle model. Its main weakness is that we cannot assume that the morphological parameters used in the model apply to our case study - a point that we clearly pointed out in the text. Given this uncertainty, Figure 15 is a comparison of the current state-of-the-art in measurement technology and modelling capabilities. It illustrates the level of consistency and discrepancies that we can presently expect from comparing observations and model calculations of depolarization. As we understand the reviewer, it is not a requirement that we remove the figure. We therefore decided, for the reasons stated above, that we would like to keep it.

Sect. 5, Summary, second half of this Sect. 5: You make a dangerous conclusion concerning the 355 nm depolarization ratio (EarthCARE) . When looking into the literature (for example Illingworth, 2015, depol vs lidar ratio figure) then there is no doubt that 355 nm depol observations can indeed by used for aerosol typing. Dangerous means here: Your statement is based on the fact that you only observed depol values from 20-25% at 355 nm with your HSRL, disregarding what type of aerosol was present. So these measurements are at least to some extend questionable (at least for me) so that such severe conclusions are not justified, … is my opinion. Sure if this is found by many groups in future, yes then we have to change our mind, but at the moment, one should better leave out such statement regarding EarthCARE.

The reviewer has read a more definitely negative tone in our remark than what was intended. We don't suggest that our observations negate any of the observations in the Illingworth (2015) figure. But any such figure (including the ones for 532 nm and 1064 nm measurements from HSRL-1 in Burton et al. 2012, our own paper) include only specific datasets and, so far, none of these datasets are global and can't be taken to be all-inclusive. We don't suggest that all smoke observations will have a wavelength dependence of the particle depolarization ratio similar to this case that we observe, just that the existence of at least one such case brings up the possibility that there may be aerosol types or subtypes that aren't captured in these figures, or that some of the distributions may be broader than what is shown in these figures. Note that there are two smoke types shown in Illingworth (2015) Figure 8. "Smoke" is shown to have linear particle depolarization ratio approximately from 0-5% and "Aged boreal biomass burning" is shown to have linear particle depolarization ratio of approximately 7-13% for the cases shown. We know nothing about the depolarizing biomass burning cases except that they were observed in Leipzig, from the caption, since there is no publication listed for them; in particular, we do not know the wavelength dependence, since the 532 nm and 1064 nm particle depolarization isn't shown. Could it be that aged biomass burning aerosol of a different age, composition, or combustion type observed on another continent or possibly in different meteorological conditions (temperature, relative humidity) may have different intensive parameters? We believe that our measurements show that it can happen, but we do not know what circumstances drive it or how frequent this occurrence is. The language in the manuscript was actually fairly soft, saying, "*if* this is not an isolated case ... the EarthCARE satellite *may* observe significant particle depolarization in *some types* of smoke as well as in dust." This is such a provisional statement that it seems that the only basis for disagreement with it is that the reviewer disregards the possibility that our observations are valid, or perhaps does not believe that this layer was actually smoke. As we said above, we believe our measurements are valid and we have carefully described potential sources of systematic error and characterized the uncertainties to show evidence for the validity of our measurements. Also, we have no reason to think this observation was anything other than smoke. Given that, we think it is worth calling attention to the potential implications of this observation. Note that our first discussed implication was not even remotely a criticism. Rather we are enthusiastic about the prospect of obtaining global measurements of smoke depolarization in many different circumstances from EarthCARE, which will hopefully help to sort out what circumstances control the incidence of high depolarization ratio values at 355 nm. However, we still do wish to point out the other potential implication, that values of particle depolarization ratio near 20-25% at 355 nm may be more ambiguous than similar values at 532 nm are, in terms of the aerosol classification. We are willing to revise the final paragraph to repeat the caveats again to try to avoid offense, however. Here is the new wording:

> *"On the other hand, the third case study presented here showed that smoke particle depolarization ratio can be significantly larger at 355 nm than at 532 nm, and in fact the particle depolarization ratio at 355 nm for this smoke case was quite comparable to the dust-dominated cases. If this is not an isolated case, and this signature proves typical for some subsets of smoke aerosol in particular conditions, the EarthCARE satellite may observe significant particle depolarization in some types of smoke as well as in dust-dominated aerosol. If this is the case, global observations of smoke depolarization will present an exciting opportunity for improving our understanding of the optical properties of smoke and how they change with age and processing; however, it will also present a challenge. That is, a significant particle depolarization ratio signature at the single wavelength of 532 nm has been sufficient for distinguishing dust-dominated aerosol from smoke aerosol, but at 355 nm this signature by itself is more ambiguous, if the smoke case presented here is not an isolated case. EarthCARE will also measure lidar ratio at 355 nm; this is related to absorption but has significant variability for smoke (Groß et al., 2014). EarthCARE will not have backscatter or extinction measurements at a second wavelength to give an indicator of particle size. Therefore, for any cases where particle depolarization ratio is ambiguous, smoke and dust may not be easily separable."*

Furthermore, to try to meet the reviewer halfway, we changed the abstract so that EarthCARE is not mentioned specifically, saying only "*We note that in these specific case studies, the linear particle depolarization ratio for smoke and dust-dominated aerosol are more similar at 355 nm than at 532 nm, having possible implications for using particle depolarization ratio at a single wavelength for aerosol typing.*"

This brings me to the question: Did you ever observe particle depolarization ratios (in aerosol layers) at 355 nm clearly below 10%?

Yes, of course. For example, see the same case study, but at lower altitudes. Figure 14 shows linear particle depolarization ratio of approximately 1% for most of the boundary layer and the residual layer. Figure 4 shows another example. On 13 July 2014, the upper layer (altitude above approximately 3.5 km) has 355 nm linear particle depolarization ratio of 4-7%.

Final remark: Appenix, Page 24776. line 14 Do you observe any wavelength dependence in the ellipticity angle? If yes, please provide the numbers for the different wavelengths.

Yes, this was partially left out of the manuscript and was confusing. The revision says, *"we have historically measured minimum depolarization ratios in clear air that exceed the theoretical value, namely values of approximately 0.006 in the 355 nm channel, approximately 0.008 in the 1064 nm channel, and 0.0085-0.0135 in the 532 nm channel."*

Table 1: Lidar ratios at 355 and 532 nm in addition would be fine.
Figure 3: There is space to the right... for two more color plots (355nm and 532nm lidar ratios). At least for this excellent dust observation, we need to bring together all the information, we have and on which all the aerosol-typing papers are based on. As mentioned I would like to have an additional four-panel figure: height profiles of backscatter (three wavelengths), extinction (two wavelengths), lidar ratio (two wavelengths), depolarization ratio (three wavelengths).
See above. We cannot derive lidar ratios for the smoke case and, given that, we prefer not to include them for the dust cases either. Lidar ratio for dust is a popular field and it would require a significant amount of work, probably a whole separate paper, to do that topic justice. We feel it would add significantly to the amount of analysis we would have to present in this paper, both to describe the measurement technique to a degree of detail consistent with the depolarization instrument characterization, and also to discuss comparisons with prior published measurements of dust lidar ratio. We chose to focus on the spectral depolarization of dust for this paper and we feel that other measurements are outside the scope of the current manuscript.

Figure 10: smoke is practically invisble, can you use arrows or something else to point to the smoke fields…?
There is a lot of smoke in the image and a circle around the smoke field would encompass a third or so of the image so perhaps not be so useful. It is a little difficult to distinguish the gray smoke from the white clouds at first, though. We added these few sentences to the caption to try to help the reader visually lock onto the smoke: "The bright white is clouds and snow cover and the gray is smoke. Several distinct smoke plumes indicate sources in the U.S. Pacific Northwest and in Western Canada within the cloud-free area on the western part of the continent. Significant smoke layers from these fires blanket the mid-continent cloud-free areas in the northern portion of the image. The HSRL-2 measurements are close to the southern edge of the extensive smoke field.

Figure 13: I am still wondering what the reason for the strange observation is ? Overload in the co-polarized 355nm channel, could be an explanation?
We checked for saturation effects in this case and found no evidence that either channel is saturated.

All in all, the paper is very good and it was fun to study it! I know the group and know that the lidar is excellent, and the data analysis is carefully done by an experienced scientist (professional). So my comments are just to trigger to re-think and to re-check some of the results and to keep the discussion on the very save side ….