We appreciate the reviewer’s helpful comments and valuable suggestions to improve the paper. We note that the reviewer focus is too strongly on atmospheric aerosols, denying high resolution wind data which is needed in addition to study Aeolus wind error characteristics.

We did a major revision as recommended, reducing the paper size by about 25%. A detailed point by point reply (in blue) is provided hereafter.

The abstract is confusing when reading it for the first time.

We modified the abstract with the aim to be more clear on the objective and main results of our study.

The introduction does not provide a clear picture of what follows. Only after reading section 6 (summary and conclusions), the motivation and layout of the paper contents became obvious. So please use the first four to five paragraphs of section 6 (Summary, conclusions and discussion) as the road map for the introduction.

We think that some introduction on the Aeolus mission is needed in the Introduction section for completeness. Continuous mode is described in a peer-reviewed paper for the first time, so some paragraphs are needed in the Introduction to describe the consequences for Aeolus horizontal (and vertical) sampling. We updated the Introduction to better explain the motivation and layout of the paper.

It took me a long time of reading before I got an answer to my main question: Why do they use radiosonde data to simulate aerosol and cloud profiles? The motivation is not well described in the present version of the manuscript.

We need a database of high resolution backscatter and WIND. Available databases, such as from Marseille et al. (2011) lack resolution for wind. Co-located radiosonde and CALIPSO data are rare as mentioned in section 4, so CALIPSO data are generally not available for simulating backscatter/extinction variability. The last paragraph of the updated Sect. 1 now mentions that CALIPSO data are not suitable for the purpose of our study with reference to section 3 where this is further motivated.

All in all, the introduction is much too long, ... and confusing. All the technical details in the first, second, third and fourth paragraph should be given in a new section (may be with title ALADIN). And then, as mentioned, please provide a paragraph with a clear motivation, why you use radiosonde data! Provide a clear outline of all the contents which follow. Give an overview of all the sections (why and what is present).

A new subsection on the Aeolus mission has been introduced to better separate mission details and study purpose.

Section 2 is long, are all the details and equations needed to understand the rest of the paper? Please keep the paper as short as possible!

We agree with the reviewer and Sect. 2 has been shortened also following the lines of reviewer #2.

Again, Section 3 just starts without providing any motivating sentence in the beginning. It is strange to read that radiosonde data have been used to estimate aerosol backscatter along the radiosonde path . . . by using humidity parameter. As a lidar, radiosonde, and aerosol expert, I was asking myself, who is using such a strange and completely stupid approach (sorry for these words, but this was my first impression). After 15 years of field campaigns I can tell there is no general relationship between radiosonde profile data (temperature, humidity, winds) and aerosol profiles. Why should that be the case. Aerosol occurrence and particle concentrations depend on source distributions and transport ways, of course, and at a specific place you may see strong correlations in the meteorological profiles and aerosol profiles, but this changes from field site to field site..

Ok, after reading section 6, I began to understand: One can use this radiosonde/cloud/aerosol approach (even if not just state of the art in times with lidar networks and CALIPSO mssion) to characterize the overall error behavior of a spaceborne wind lidar.

We are happy that our purpose became clear in the end, but agree that we should not leave the reader in doubt too long. It is true that cloud and aerosol retrieval from radiosondes do not provide the best estimates (as compared to CALIPSO, ground-based lidar, etc.), but radiosondes do give the best representation of atmospheric dynamics along the Aeolus line-of-sight which was the weak point in previous database studies.

Sections 3.1 is too long. . . The presented long discussion is simply not needed in such a summarizing paper. If you use the Zhang2010 approach to identify cloud layers in radiosnde profiles, then it is sufficient to say that and one may then show one example and state that extended studies show the usefulness of the method. That is sufficient.

We agree and section 3.1 has been shortened by referring to previous papers and avoid repetition, and we will only retain the Zhang2010 method to detect cloud layers from radiosonde profiles.

Section 3.2

The first paragraph is trivial and contains strange references in view of all the publication on EARLINET and CALIPSO work and aerosol field campaign lidar studies (see e.g., Gross et al., ACP 2013, Burton et al., ACP, AMT 2012-2014, all the SAMUM papers in the Tellus B special issues, 2009, 2011).

You state that the reference model atmosphere (RMA) is based on lidar data from 1989, so 25 years old data obtained with simple backscatter lidar!!! Unbelievable for an aerosol lidar expert!

Very nice papers indeed, but where is high-resolution wind in these papers?

I think our choice is clear now, given that CALIPSO-radiosonde co-locations and airborne campaign-radiosonde co-locations are rare, thus denying the generation of a database of substantial size. Also, we explained that model winds are not suitable because they lack resolution for a complete evaluation. The reviewer is too strongly focused on aerosols, but we need high-resolution winds too to evaluate Aeolus wind error characteristics.

A huge number of publications on aerosol profiling with Raman lidars and HSRLs, partly in combination with photometer studies is available, CALIPSO data are avaibable. All these efforts provide a dense description of aerosol distribution around the world in many details especially in terms of backscatter, extinction, lidar ratio, depolarization ratio,. . ..

See above, where are the winds?

And the AEOLUS community uses this rather old, and therefore questionable aerosol model.

No, the reviewer knows that the Aeolus community uses all kind of aerosol sources.

But ok, as said, for an ALADIN errors analysis it is not so important how strange the approach regarding the chosen aerosol profiles is (from my subject point of view).

This is clearly not true. We clearly explained that the location and thickness (both optical and geometric) are important parameters for Aeolus wind error magnitude. We showed this already for the relative simple cloud and aerosol models that guided us to the conclusion that the Mie channel (measuring cloud and aerosols) wind solution should be considered with great care and more confidence to be put in the Rayleigh channel wind solution.

Why do you then give such an extended overview of the hygroscopic behavior of aerosols and consequences for aerosol extinction. This all is well known and exhaustingly discussed in the literature. Keep the paper short, just state what you are using, provide some modern references, not just 45 year old references.

The discussion on aerosol lidar ratios is not state of the art (but sure, I did not expect the opposite). As mentioned above there are meanwhile so many papers on MEASURED lidar ratios (Mueller et al, aerosol type dependent lidar ratios, SAMUM papers in Tellus 2009, 2011, Burton papers on latest HSRL observations, many other papers from EARLINET, see Mona, Amiridis, Mattis etc), and you start the discussion with papers like Evans 1988, Ackermann 1998, Waggoner 1972, and Saleminik 1984. . ... The huge RH dependence from 19sr to 84sr has never been observed with modern lidars. You may find a comparably strong lidar ratio decrease with RH (due to a change in the chemical compostion, more water, changing the refractive index) from 60-70sr of dry absorbing particles to 40-50sr when they are swollen, but usually the decrease with humidity is much smaller and almost not visible in well mixed boundary layers.

But again, the text is rather long again here; please keep the discussion as short as possible. For your study it is just sufficient to know what you assume. Give an example for two lidar ratios (40%, 80% RH) that follow by using this unnecessarily complicated parameters to be considered in equation 28. . .

See above, but we agree that section 3.2 can be shortened, i.e., remove the first paragraph and text about hygroscopic behavior. The main equations (24)-(28) remain with references to the used parameters in these equations.

Section 3.3

Table 3 contains some characteristic backscatter and extinction values, but the most important parameter, just discussed in large detail, is not given: the lidar ratio! Lidar ratios for liquid-water-dominated clouds of 18 to 20sr may be fine, if we ignore multiple scattering. In lidar applications, multiple scattering has to be taken into account. Effective lidar ratios are then around 10sr (effective extinction values are easily a factor of two lower than the single scattering values in the case of a spaceborne lidar). Cirrus lidar ratios of 14 sr are already effective values (they are usually even lower and around 10sr, for an off-zenith lidar as ALADIN). However, in table 3 you provide single scattering extinction and backscatter values, then the respective lidar ratios are too low. Modern papers indicate single scattering lidar ratios around 25-30sr. PSC show lidar ratios aorund 20sr? Is there a reference for this?

See caption of Table 3: Vaughan (2002) which should be Vaughan (1998). We have corrected that.

This is probably again an effective value, right?

Right

So, clouds are water clouds at -17.16C (obviously 256 K) and ice clouds at temperatures if temp is -17.17 C (255.9 K). Note that one can have water clouds at -30C and also ice clouds at -10C as polarization lidar studies indicate. But of course for your study it is simply unimportant how strange (or crazy) the assumptions are. What about mixed-phase clouds?

They are hard to penetrate by Aeolus and the resulting winds are probably suspect as we demonstrated with the theoretical analysis in section 2.

Any choice of introducing backscatter/extinction variability inside clouds is artificial and we decided to take the simplest choice of constant backscatter, indeed underestimating variability.

Section 4.1. . .. please leave out this section completely. This is so long, and is simply

not necessary for an ALADIN error analysis.

We disagree with the reviewer that section 4.1 should be removed completely. The Zhang2010 method was tuned for the Shouxian area with different climatology than The Netherlands. For this reason we applied some changes to the Zhang2010 method which needed some validation. As a side effect, it demonstrates the usefulness of Zhang2010 for other climate zones. Yet, we agree that section 4.1 is long and has be shortened, we removed some contents about WR95 and remain Zhang2010.

We know that there are some references and modern look up tables for CALIPSO data analysis from Omar, but there are several differences: First, the CALIPSO contains

532 nm and 1064 nm, not 355 nm for Aeolus; Second, the study regions are different, one focuses on Aferica and the other is the Netherlands, aerosol type may be different for these regions. In fact, the lidar ratio from the eq.27 and Fig.7 of previous paper is consistent with the following figure from Donovan.

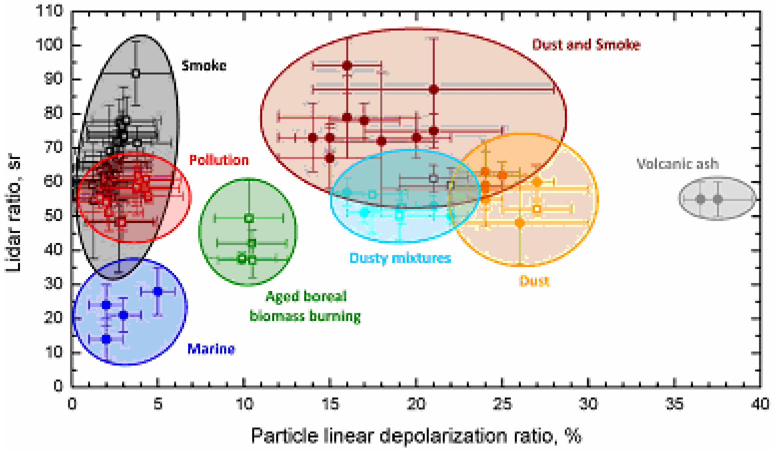

Figure Aerosol classification from measurements of lidar ratio and particle linear depolarization ratio at 355 nm. Ground-based observations were performed with the Raman-polarization lidars POLIS (University of Munich, dots) and Polly^{XT} (Leibniz Institute for Tropospheric Research, open squares) at Cape Verde (dust, marine, dust and smoke, dusty mixtures; dots; Groß et al., 2011), Leipzig, Germany (pollution, aged boreal biomass-burning aerosol, dusty mixtures; open squares), Munich, Germany (volcanic ash; dots; Groß et al. 2012), in the Amazon Basin (smoke; open squares; Baars et al., 2012), and over the North Atlantic (dust, dust and smoke; open squares; Kanitz et al., 2013).

A short summary may be justfied, but is not needed.

Now (and thus too late), you start with comparisons with CALIPSO and CloudSAT. . ..In the case the lidar ratio discussion, there are modern look up tables for CALIPSO data analysis. Why did you at least not look in theses papers (Omar et al., JGR).

Sections 5 and 6 are fine.

All in all the paper is of low quality in its present form. And probably not all coauthors (e.g., Donovan) read the manuscript.

Major revisons are demanded. An important point is to shorten the paper drastically. This will make the paper more attractive.