Response to Anonymous Referee #1.

We thank the Anonymous reviewer #1 for a thorough review and useful suggestions, which have been carefully implemented in the revised manuscript. The detailed answers to all comments are provided below.

General comments

1) The manuscript lacks some details on the upgrade and development of the lidar and retrieval algorithms (including calibration) during the recent years since 2012 (after the last publications from this group by Souprayen et al. 1999). E.g. please state clearly, which part of the instrument design in ch. 2.2. is new and also provide more details of the upgrade. Please be more specific on instrument details (see also my specific comments) to this part, e.g. in ch. 3.1 it is stated that FPI plates were reconditioned, but not further explained. I would also recommend providing more details on the calibration (L79-L83), as this is essential for wind retrieval and wind bias. E.g. is the spectral tuning of the FPI only used for monitoring, or is it used during the wind retrieval (as mentioned in L125). If yes how are these functions used (measured, fitted), and used for wind retrieval from the actual measurements of the same day. Also a short description on how calibration constant C in equation (1) is obtained is missing.

The Section 2 regarding the instrument design, measurement principle and instrument calibration has been entirely reworked, please see the revised text and the answers to specific comments below.

2) I have two comments to the statistical comparison approach. I am wondering about a justification of using weighted distances for deriving bias and standard deviation in Ch. 3. I would like to see a clear justification of this approach, because I consider this as unusual for instrument intercomparisons, and provide a short description (e.g. equation), how this was implemented. But overall I would recommend deriving these statistical numbers on bias/std. with and without this weighted approach.

As a matter of fact, the comparison statistics figures in Table 1. are obtained without the horizontal offset weighting. Besides, as mentioned in Sect. 3, the weighting only affects the average standard deviation, whereas the average bias and correlation coefficient are not affected. Please see the answer to specific comment below.

My second comment here is related to a missing statistical comparison of the horizontal wind speed (from u and v-components, and possibly wind direction). I would propose to add this quantity to chapter 3, and specifically provide a scatterplot (as Fig. 4d) and statistical numbers (as part of Table 1). I would also propose to add the statistics of all radiosonde comparisons to Table 1 as an additional row, and discuss these numbers in the text.

The results and discussion of statistical comparison for the total wind and wind direction have been added into Sect. 3 and Table 1.

Specific comments

Lines 12, 523 Provide numbers for vertical and temporal resolution; "high resolution" is different for several application areas

The respective fragment in the abstract and the summary have been modified: "**After a recent upgrade, the instrument gained the capacity of wind profiling between 5 and 75 km altitude with vertical resolution up to 115 m and temporal resolution up to 5 minutes."**

Line 29 Provide a reference for deriving wind speed on regular bases from space-borne temperature measurements using geostrophic assumptions.

Reference provided: **Oberheide J, Lehmacher G A, Offermann D, Grossmann K U, Manson A H, Meek C E, Schmidlin F J, Singer W, Hoffmann P. Vincent R A. Geostrophic wind fields in the stratosphere and mesosphere from satellite data. J. Geophys. Res. 107(D23):8175. doi: 10.1029/2001JD000655, 2002.**

Line 38 I would propose to add some more references in the introduction of wind lidars using molecular backscattering, especially here also mention ALADIN and its airborne demonstrator

The following text has been added: "**The direct-detection technique for wind profiling has been successfully realized in an airborne Doppler lidar – A2D, Aeolus Airborne Demonstrator (Reitebuch et al., 2009). A2D instrument served a prototype for the most ambitions endeavor in the context of lidar wind profiling – the first ever satellite-borne Doppler lidar ALADIN (Atmospheric Laser Doppler INstrument) (ESA, 2008; Stoffelen et al., 2005), that has been successfully launched by European Space Agency (ESA) in August 2018 (Kanitz et al., 2019).**"

Line 58 Add 1-2 references for Aeolus here

Done

Line 70 Parameters of the FPI are introduced here, while the operating wavelength is not stated (at this place of the manuscript).

Line 91, eq 2 The introduction of parameter P(z, 40°) is missing in the text. 95-99 The vertical pointing beam is used to compensate for laser frequency drifts, with a value, which is constant for each altitude (average over 15-25 km). Please discuss, if there are or not altitude dependent effects in the calibration, which need to be compensated.

Line 100ff Please provide more instrumental details, as laser frequency stability (shot-to-shot), laser divergence (at output of beam expander) and laser linewidth. Also FOV of telescope should be provided, as well as diameter of multimode fiber. The method of mode scrambling should be shortly introduced. Also the "reconditioning" of the FPI plates (as mentioned in ch. 3.1) should be explained here (new coating? New polishing?)

All of the above comments have been carefully implemented in the Sect. 2, please see the revised text. The reconditioning of the OHP wind lidar FPI has not actually been carried out. It was not necessary since the auxiliary experiments have shown that the spectral characteristics of the FPI have remained unchanged. The reconditioning of FPI has only been done for the La Reunion wind lidar.

Line 150 Is this equation of the error in units of m/s? Is C the same constant as introduced in eq. (1)?

This equation describes the error in the response profile *R,* which is unitless. *C* is the same constant as in eq. (1). This has been clarified in the text.

Line 200 Figure 3: black circles are hardly visible, e.g. use different colour

The colour of the circles in Fig. 3 (now Fig. 4) has been changed.

Line 220 Do you provide numbers for correlation coefficient as r or r^2. Please state explicitly in the text and in Table 1.

We provide the Pearson's correlation coefficient *r*. This has been specified in the text and in the Table 1 caption.

212 Please explain the rationale to compute the comparison statistics, by "weighting" the difference with the horizontal offset between the measurements. I think this is very unusual. I would propose to provide statistics without weighting, or at least show both the non-weighted or weighted results. The weighting should be shortly explained (e.g. via an equation).

We have used the horizontal offset weighting for computing the averages of comparison statistics in order to evaluate the effect of the spatial variability of the horizontal wind components. The results of intercomparison in Sect. 3 and Table 1 are computed as ordinary arithmetic averages. The following text and equation have been added:

"For evaluating the effect of the horizontal offset between the lidar and RS measurements we computed the offset-weighted averages of the intercomparison statistics and compared them with the ordinary averages. The weight for each individual value is defined as $w=1-\bar{D}/D_{max}$, where $\bar{D}$ is the mean distance between the lidar and RS sampling locations and D_{max} is the maximum distance amounting to 69 km (Table 1). We note that the horizontal-offset weighting of the differences neither affects the mean difference nor the mean correlation but reduces the standard deviation for the wind components and total wind by about 0.2 m/s."

Line 240 Figure 4: y-intercept also in units of m/s

The figures' legend has been modified accordingly

314-316 Please explain, how a possible Mie-induced bias would be recognized in the profiles, e.g. too high or too low values? Do you correct for the Mie-induced bias in the wind retrieval (or any QC), or is it only compensated by the FPI spectral configuration (spacing, FWHM)?

The Mie-induced bias would appear as sharp enhancement in the wind profile towards higher absolute values. Such a bias may appear in case of the spectral detuning of the FPI bandpasses with respect to the laser backscattered line. The Mie bias can be corrected for, however in reality this is required only in the case of cirrus clouds with scattering ratio above 20 or so. Otherwise, the correction is unnecessary as we demonstrate in the article.

The following paragraph has been added in the beginning of Sect. 3.1:

“Although the Mie-backscattered line is narrow (0.08 pm) compared to the thermally-broadened Rayleigh line (2 – 2.4 pm) the intensity of the former may be substantially higher and thereby alter the spectral shape of the return signal. In this case, a disproportionally larger flux would be transmitted through one of the FPI bandpasses, affecting its calibration function and introducing a bias into the wind retrieval within the particle layer. The sensitivity to Mie scattering can be reduced by increasing the FPI spectral spacing, however this also reduces the sensitivity to the Doppler shift. The optimal spectral configuration of the FPI has been established on the base of a theoretical model carried out by Souprayen et al. (1999b). They found that for observable stratospheric wind velocities, the residual Mie-induced error is less than 1 m/s for the scattering ratio *R*=10, which is characteristic of a cirrus cloud readily visible to an unaided eye.”

Line 505 Fig caption 10; provide date of comparison and mean distance of Aeolus observations to OHP; it would be also good to include Aeolus track in Fig. 3

Date and mean distance have been added to the figure caption. The Aeolus track has been added into Fig. 3 (now Fig. 4).

Line 515 Please provide distance for altitudes below 5 km of OHP and Aeolus track for spatial variability. Causes could be also related to preliminary nature of Aeolus observations. Have you checked error estimates within Aeolus data products, and potentially exclude data with too high errors (e.g. 8-10 m/s)? Have you checked presence of aerosol or cloud layers, which might influence Aeolus Rayleigh wind retrieval?

The distance between RS and Aeolus (91 km) has been provided in the text. The preliminary nature of the Aeolus data is clearly articulated in the text and we refrained from a further discussion on the ALADIN data quality. Indeed, the variability of lower-tropospheric winds on a scale of 100 km in the vicinity of Alps can be much larger than the measurement errors. The Rayleigh-clear profiles that were used for this particular validation case did not feature any error anomalies. From the ground-based lidar and AERONET measurements, we noted an absence of clouds and tropospheric aerosol layers during the period of measurements.

526 Please state that this number of 6 m/s refers to random error.

Done.

553 Could you be more specific, how this finding should be considered for spatial and temporal collocation requirement for performing comparisons for space-borne wind lidars as Aeolus

The respective sentence has been modified: **“This finding is to be considered for Aeolus wind validation activities in a sense that a precise temporal collocation may be more important than the spatial collocation of the measurements.”**

527 Please specify which optics could be replaced to improve performance

The respective sentence has been modified: **“We note that the vertical range can potentially be extended to 3 – 80 km through replacement of the beam-commuting and beam-splitting mirrors, for which the resources are available.”**

533 The std of 2.2. m/s refers only to 1 component and not the horizontal wind speed. This should be clarified. I would also propose to add statistics for the horizontal wind speed in the conclusion (see my general comment 2).

We have provided the statistical figures for the total wind speed and direction in the summary section.

605, Table 1 Please add in Table caption if you use R or R=r2 as correlation coefficient; I would also propose to add at least columns for mean difference and standard deviation for horizontal wind speed (squared sum of u,v; and possibly wind direction) and also another row with mean quantities over all days of comparison.

All done.

References: I would propose to add a few more references related to Aeolus (ESA 2008, Stoffelen et al. 2005) and its actual performance (Kanitz et al. 2019, Reitebuch et al. 2019).

All the suggested references have been added into the introduction and Sect. 5.

Editorials

All done.