Response to Anonymous Referee #2.

We thank the Anonymous reviewer #2 for a positive review and useful comments. We provide below the detailed answers to them.

Line 12: It would be good to get some numbers for the improvement, e.g. from typically xx m/s uncertainty to yy m/s uncertainty. Or were the improvements just technical - then also indicate what has improved.

The abstract has been modified to clarify the improvement: **"A direct-detection Rayleigh-Mie Doppler lidar for measuring horizontal wind speed in the middle atmosphere (10 to 50 km altitude has been deployed at Observatoire de Haute Provence (OHP) in southern France since 1993. 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 15: Instead of "The evaluation" I suggest to write "An initial evaluation". The present paper, in my opinion, does not provide a full and comprehensive evaluation. With only a few radiosondes and ECWMF profiles, the statistics are not very comprehensive yet.

We believe that 12 spatiotemporally-collocated radiosoundings conducted in various atmospheric conditions over a period of 4 years is sufficient for the instrument performance evaluation. Note that all the intercomparison measurements have been conducted in a "campaign" regime, which is resourceful and costly.

Line 39: drop "there exists"; "with" → "have"; "which" → "and";

Line 56: drop "preparation of"

Line 218: "The both" → "both"

All done.

Section 2, 2.1: I think it is necessary to give a bit more background on the wind-lidar measurement principle. I strongly suggest to add a schematic Figure showing the two (A and B) Fabry-Perot band-passes spectral shapes, as well as the spectral shape of the backscattered Rayleigh and Mie radiation. Also explain that a Doppler shift of the return signal will enhance one channel (A or B) while reducing the other (B or A). How is spectral calibration obtained? I assume by de-tuning the laser with a wavemeter, and observing the zenith pointing return channel. Please also explain.

The Section 2 regarding the instrument design, measurement principle and instrument calibration has been entirely reworked, please see the revised text. A figure showing the spectral shapes of backscattered line and FPI bandpasses has been added.

Around line 70: Please give the manufacturer of the Fabry-Perot interferometer.

It is StigmaOptique, a small French company that does not exist anymore, we mentioned the name in the text.

Around line 151: You might want to say here that the uncertainty scales with 1/ √ tacquisition and / or with 1/ √ Δz, where Δz is the vertical resolution chosen for data processing.

Thank you for the suggestion. Done.

Fig. 2a: It would be good to show both the NA and NB profiles (or their difference if they are very similar). Maybe also show a raw R(ϑ, z) profile?

The NA and NB profiles look identical on such a plot, whereas the difference between them in MHz doesn't have much physical meaning. Meanwhile, the response profile *R* is not much different from the horizontal wind profile as the latter is obtained by multiplying *R* by the instrumental constant, which only slightly varies with temperature.

Line 172: By "noise level" you probably mean the "background noise level"? If yes, change text. I would assume that the total noise level would increase a bit at lower altitudes, e.g. at the altitude where low and high gain channels are spliced together.

Yes, we mean the background noise level. The splicing of low and high gain signals is done where the former is orders of magnitude above the background level.

Around line 177: 12 Comparisons over a 4-year period are not a lot. Please add some statement why only so few RS comparisons are made, especially since nearby Nimes launches one or two radiosondes every day.

The wind lidar validation experiment was conceived to rely exclusively on the reference measurements by GPS radiosondes collocated with the lidar acquisition in time and, as close as possible, in space. The Nimes radiosoundings are too far away (>100 km) and not always collocated in time, which would make the attribution of the discrepancies in the wind profiles ambiguous.

Around line 254: By eye, Fig. 4a and 4b seem to indicate increasing standard deviation from about 10 to 30 km. How do standard deviation profiles compare to the estimated uncertainty profile from shot noise? Always a good idea to check such estimates. Maybe this warrants an additional Figure?

This aspect is discussed in the original version of the article around line 255. We have included the statistical error profile in Fig. 4 (now fig. 5).

Section 3.1: Please add some explanation, that the very narrow Mie line alters the spectral shape of the return signal, and that this might affect/ alter the calibration function in Section 2.1.

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."

Figure 6: I suggest that the authors be more critical here. The largest differences between RS wind and Doppler lidar wind do occur near 12 and 17 km, very close to the aerosol / cirrus layers. I don't think the authors should ignore that and simply claim no effect. Could the Mie effect be reduced / quantified by wavelength scanning the zenith return signal in the presence of aerosol layers, and assume negligible vertical wind?

The Mie-induced bias would appear as sharp enhancement in the wind profile towards higher absolute values. It would be closely correlated with the scattering ratio, which is obviously not the case here. 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.

Line 366: But ECWMF also assimilates stratospheric and mesospheric radiance measurements from satellites, providing a large amount of information on the temperature fields. Since the atmosphere is close to a geostrophic state in the stratosphere and mesosphere, it is not surprising to me that ECWMF winds are quite realistic up to 60 or 70 km.

To the best of our knowledge, there are no operational radiance measurements in the mesosphere that are assimilated into ECMWF model. The highest channel of AMSU (ch14) peaks at around 43 km.

lines 386/386: Is it the mirrors, or is it the darker sky in the North? Should "due to a better condition . . . mirrors of" be replaced by "due to the darker sky seen by "?

It is the mirrors and the alignment issues that lower the signal strength for the East line-of-sight. The sky background is the same for both directions

Line 534: I am not sure if you have really demonstrated that results are "insensitive" to aerosol. I think "not very sensitive" would be a better statement.

A statement "not very sensitive" would have to be quantified, whereas this is not possible as we did not see any measureable effect.

Around line 535: Can you not measure the temperature profile as well (using the Chanin Hauchecorne method)?

To measure the temperature profile using the Chanin Hauchecorne method it is necessary to acquire the full spectrum of the backscattered signal proportional to the atmospheric density. In the case of our Doppler lidar the signal is convoluted with the spectral transmission of the Fabry-

Pérot interferometer. The temperature retrieval would thus be prone to a much larger error than using the dedicated LTA lidar instrument at OHP.

Line 538: I don’t think the authors have provided "insight". They only showed "examples" . Replace the word?

Done.