

Interactive comment on “Doppler lidar at Observatoire de Haute Provence for wind profiling up to 75 km altitude: performance evaluation and observations” by Sergey M. Khaykin et al.

Anonymous Referee #2

Received and published: 5 December 2019

Overall, this is an interesting and well-written paper on one of the very few existing stratospheric / mesospheric wind lidars. Such systems are of interest to the middle atmosphere scientific community, and the paper is well-suited for publication in AMT. I have only a few minor suggestions.

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.

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.

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With only a few radiosondes and ECWMF profiles, the statistics are not very comprehensive yet.

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

Line 56: drop "preparation of"

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.

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

Around line 151: You might want to say here that the uncertainty scales with $1/\sqrt{t_{\text{acquisition}}}$ and / or with $1/\sqrt{\Delta z}$, where Δz is the vertical resolution chosen for data processing.

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

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.

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 Nîmes launches one or two radiosondes every day.

Line 218: "The both" → "both"

Around line 254: By eye, Fig. 4a and 4b seem to indicate increasing standard deviation

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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?

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.

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?

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.

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"?

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.

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

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

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-385, 2019.

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