Responses to Review #1

The authors would like to thank the reviewer for his valuable comments which helped in improving the quality of the manuscript. Our point-by-point responses to the reviewer's comments appear in bold below.

Title: what is "HPE"? You mention the comparison to IASI, but isn't the comparison to ERA5 at least equally important?

HPE stands for 'High Precipitation Event'.

In this article, ERA5 is used as a reference to adjust the lidar calibration coefficients and obtain a consistent dataset. For this reason, the lidar data are not entirely independent of the ERA5 reanalyses. However, only a slight correction is made to the lidar calibration coefficient, and not for all ground-based stations. This is why statistical results are also provided for the lidar/ERA5 comparison. Furthermore, the ERA5 data are not completely independent of the IASI measurements, since the latter are assimilated into the model above 2-3 km.

The title has been modified as "To what extent are the IASI and ERA5 water vapour profiles representative of the conditions in the autumn 2022 during the WaLiNeAs campaign"

This paper seems to miss in the introduction: https://www.mdpi.com/2072-4292/17/20/3473

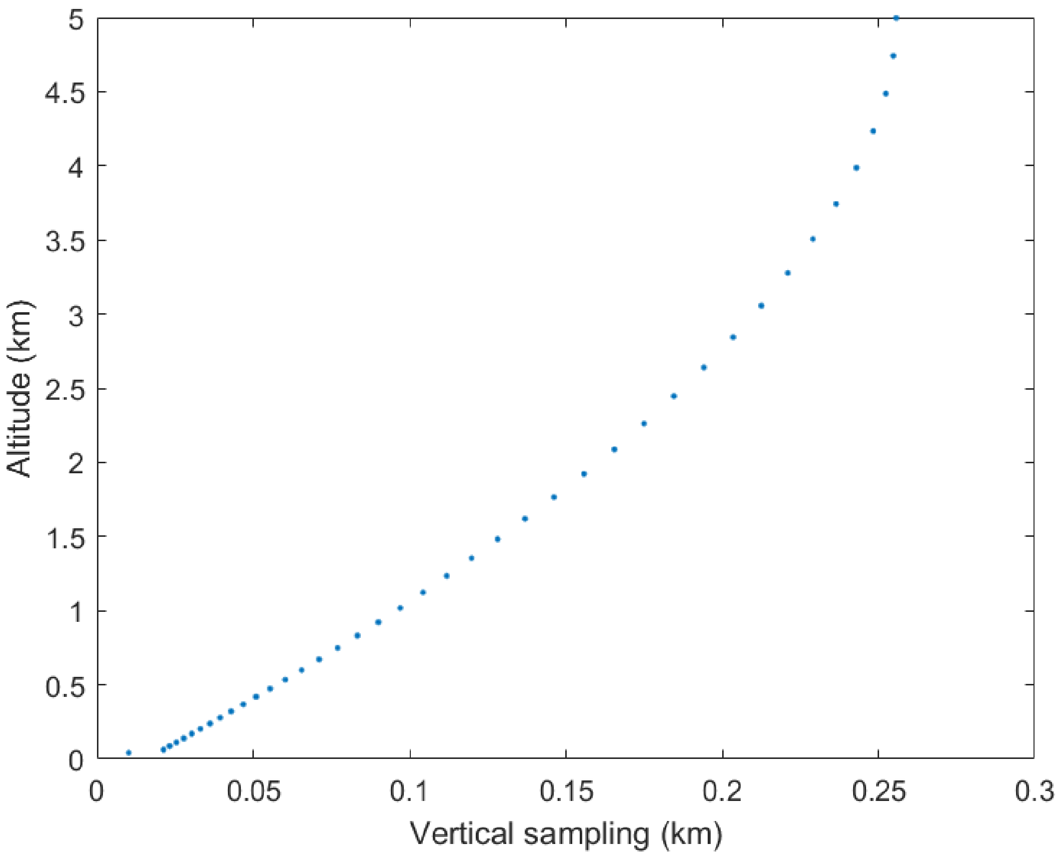
The reference has been added.

This is a bit pity because these results could have been used to separate the free troposphere from the PBL – the authors fixed threshold at e.g. 1.5 km altitude seems a bit rough.

We have set a boundary at 1.5 km. This ensures that the entire atmospheric boundary layer, along with any potential residual layers, is included. It was the lidar observations that led us to define this limit. To ensure consistency, we have chosen the same altitude for all ground-based stations.

L 120 and L 128 why do you interpolate the lower resolution (IASI; ERA) to your lidar data? Why don't you average the lidar to the vertical resolution of the former? Suppose it is getting drier with altitude, a part of this drying signal is in the IASC / ERA, not in the lidar.

The IASI weighting functions are very broad, creating a correlation between neighboring points in the operational product. The profiles are already oversampled in relation to the vertical resolution of the weight functions. This stems from the inversion process based on nearest-neighbor recognition. Consequently, linear interpolation at a sampling interval of 100 m has little effect on the result. The figure below shows an example of the vertical sampling of IASI products, which is similar to lidar sampling. In the text, we state that there are 40 levels between ground level and 5 km, with vertical sampling at intervals of 20–250 m (see the figure below).



L167 slope [to compare with ERA] just to be sure: this slope “treatment” is not applied later in Fig 4, isn’t it? So this is just a hypothetical overview on a mismatch, supposed a linear function between water vapor in lidar and EWRA existed? Therefore, you write in line 170 that a bias remains. /Did I understand this correctly?)

All lidars were calibrated independently. This calibration was cross-checked against ERA5 reanalyses to ensure consistency. This is because most lidar stations are located near radiosonde stations, and the reanalyses incorporate these observations. The results of the comparisons between radiosondes and reanalyses are very good in the lower and middle troposphere (see Figure S1 in the supplement). Given that the lidar measurement is unbiased, we therefore used the reanalyses solely to correct the slopes. Broadly speaking, a measurement of 0 V indicates the absence of water vapour (barring noise). It is the slope-corrected data that are subsequently used. It should be noted that this correction is small and negligible for certain lidar (see Table 2).

Table 2: I am not sure whether I understand this. Do you mean: WVMR_Lidar = calibration_correction*WVMR_ERA5 *Intercept and coefficient of determination is the statistical soundness of the fit? Which altitude interval has been considered and how many data sets? Maybe provide the equation.

As explained, this is a simple linear interpolation of the datasets shown in the supplementary material (Figures S2–S9). Indeed, the coefficient of determination reflects the robustness of the fit (it is the R of the R^2). The regression lines are plotted in Figures S2–S9.

Line 197 and Fig 3: the approach is alright. But not very specific. One could have removed or weighted boxes depending on wind direction. Or do you have the impression that P1, P3, P7 and P9 match equally well as P5 to lidar?

One of the major difficulties with this type of exercise is using a localised measurement (from the lidar) as a reference for a measurement with much lower spatial resolution. It is necessary to ensure that the horizontal homogeneity across the pixel size allows for this comparison, and that the transport conditions permit it. In theory, the nine pixels do not have the same representativeness in relation to the lidar measurement. Nevertheless, wind speeds were below 20 m s^{-1} during the IASI overpasses and there was low variability in vertical humidity profiles across the significant IASI pixels for each site.

We have completed our explanation by

"Due to variations in horizontal wind speed, the nine pixels do not provide the same level of representation at different altitudes. During the period under consideration, wind speeds were below 20 m s^{-1}, ensuring that observations above the sites were accurately captured by at least one of the pixels. Furthermore, variability in vertical humidity profiles across these IASI pixels was low, averaging less than 0.2 g kg^{-1}. Additionally, as the stations are situated close to the coast, at least one of these pixels consistently provides data on the lower troposphere."

Line 202 500m … minimal terrain obstruction This sounds somehow vague. Do you need this information? If it is on comparing lidar to ERA5 I would have started in the free troposphere and then considered the BL (the latter one being more complicated, due to small scale effects, parameterizations behind the ERA data and ….the topography.)

Yes, the idea is to exclude the effects of topography from the lower parts of the profiles to ensure that sufficient information is available in the boundary layer. The original term was not well chosen, so we have replaced it with:

"…minimal topographic effects in the low troposphere including the PBL".

After Eq. 7: as I did not find a free pdf of the Foudali, Steiger (2008) quote: could you briefly justify the number "3" in nominator and denominator of F= …

Deriving this equation requires quite some work. The purpose of this article is not to delve into the theoretical aspects of correlation combination. The relevant references can easily be found in university libraries. N(z)-3 is an approximation used when there are only a few samples (Kemp et al., 1994).

We have added:

"N(z)-3 is an approximation used when the number of samples is less than or around 50 (Stuart and Ord, 1991) ."

Stuart, A. and Ord, J. K.: Kendall's Advanced Theory of Statistics, Edward Arn., London-Melbourne-Auckland, 1991.

Table 3: why the MB for station 6 (Toulon) is so much smaller and why this effect is not seen in RMSE?

The Lidar-ERA5 Mean Bias are small not only for station 6, but also for other stations (e.g. RL1 and RL8). Values of RMSE are indeed larger than those of MB. In the case of station 6, as also for other stations, the RMSE is dominated by the mutual differences between the LIDAR and ERA-5 mixing ratio profiles, most of this variability probably having an atmospheric origin. Differences between the single lidar and ERA-5 profiles have a larger impact on RMSE, which make use of absolute differences (root-mean square), than on bias, which accounts for the sign of the differences, and mostly cancels out.

We have added in subsection 4.1:

"Note that differences between the single lidar and IASI or ERA5 profiles reverberate more on RMSE, which make use of absolute differences (root-mean square), than on bias, which accounts for the sign of the differences, most part of those cancelling out."

L 241 (PBL) "more noticeable at some sites" be more specific. Do you mean that one would expect MB and RMSE to decrease with altitude, because there are more stationary conditions in the free troposphere, which is not seen at some sites like e.g. station 4? Is this due to strong winds in Rhone delta?

We do not definitely observe all of this. In terms of correlation, we see a decrease at stations in the Rhône delta and in Barcelona. This decrease occurs at the transition between the boundary layer and the free troposphere, and it may indeed be linked to stronger winds. We have added with cautious wording:

"…The resulting synthesis across all lidar stations shows that the vertical profiles of the three statistical parameters exhibit relatively smooth continuity between adjacent layers, highlighting that the variability of the PBL height has on average a moderate influence. This is more noticeable at sites expose to strong winds, such as the Rhône delta and Barcelona. At these sites, a decrease in correlation is observed at the interface between the PBL and the free troposphere."

Fig.4 is convincing as it is. However, this result states that ERA is systematically too dry and your water vapor is systematically larger than IASI. Can you think of reasons for this? This is an important result. I would ask the authors to repeat this analysis (if not here in a follow up study) with a few golden cases only, dry versus wet, only free troposphere and using the wind speed from model and obs to weight the pixels from Fig 3 to provide a robust estimation for a possible bias in ERA5.

We analysed the wettest and driest periods. The results are shown in Figure 5. The conclusion drawn for all stations remains unchanged. Indeed, the profiles produced using either ERA5 or IASI are drier. ERA5 is slightly wetter than IASI because it assimilates additional data, such as radiosonde measurements. It is the assimilation of IASI radiance data that could potentially lead to drier reanalyses. However, it is difficult to answer this question definitively. The positive bias in IASI may be linked to the radiance inversion procedure, but once again, we lack the evidence to draw a definitive conclusion. What is certain is that the lidar instruments were developed independently, yet they all lead to the same conclusion: an underestimation of water vapour content by IASI and ERA5.

L 255 "only lidar data with statistical error < …" Maybe it is worth mentioning how many valid profiles for each station were included in your analysis.

The number of relevant profiles is given in the figures showing the correlation, e.g. Figure 5a and c.

Fig 5: The reason for showing Toulon and Ajaccio separately is that for these stations you have the best data for both wet and dry conditions, isn't it? Maybe you state this more clearly, e.g. in figure caption

Yes, we have added:

"These two stations provide the most reliable data in both wet and dry conditions."

S3 (versus S2): To me, S3 looks "worse" than S2. Yet you give in Table 2 no calibration correction factor for site 3. Is this correct?

Figure S2 refers to RL1 and Figure S3 to RL2. A correction coefficient (Table 2) has been applied to both figures. There is much more data available for RL2 (Barcelona) than for RL1 (Valencia), which explains the greater dispersion for RL2. Dispersion mainly affects the RMS more than the slope of the linear fit.