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

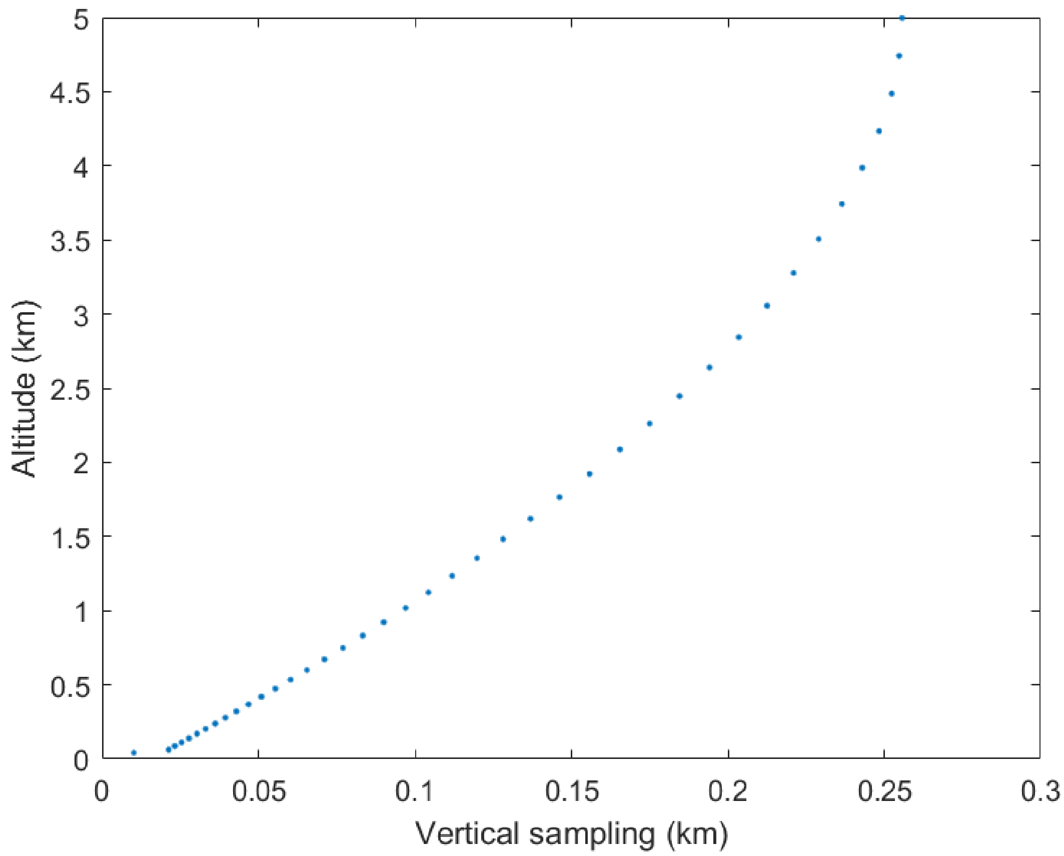
The reference has been added.

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.

Responses to Review 2

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.

Major Comments

1. Consistency between lidar calibrations.
    1. From the text, I was left wondering if the various lidar systems are calculating the particle component of the differential transmission calculation. It is not quite clear from the text even though it is addressed at some level. Is it true that the analysis of data from all systems has neglected to calculate the particle component of differential transmission? Please address this explicitly. Even though the AOT may not have been large (something that needs more clarification as noted later), if this calculation is not included in the data reduction it results in a bias in the derived products, not a random uncertainty that could be reduced by averaging. The point is made in the manuscript that neglect of the particle component of differential transmission results in a bias but it seems to be small compared with the magnitude of differences that are found in the analysis. But when lidars are being used as a reference, should any component of the data reduction be neglected? I would argue that it is long past time that the full differential transmission, including both molecules and particles, is routinely accounted for in the analysis of Raman water vapor lidar data and reviewer comments such as this become a thing of the past. Please see the recent paper by DiazZurita et al. (Remote Sensing, 2025), for an example of a way to automate the calculation of the particle contribution to the differential transmission.

Authors indeed agree that the full differential transmission, including both molecules and particles, should be accounted for in the analysis of water vapor lidar data. However, an effort was made to minimize algorithm-induced sources of uncertainty in the data analysis procedure, with the authors being aware that this may become an important source of uncertainty, especially when a non-completely homogenized data processing chain is applied to the different lidar systems and a dedicated intercomparison effort between the different lidars preceding the field deployment was not carried out. Authors tried to minimize biases associated with the application of the data processing chain, trying to simplify this as much as possible.

As a result, data have not been corrected for the differential transmission due to particles, but they have been corrected for effects on the differential transmission associated with molecular scattering.

1. To address this, please be explicit if the differential transmission due to particles has been included in the data reduction and, if not, state the estimated magnitude of the bias that can result. Better yet, of course, would be for the groups to include this in their analysis but I realize the difficulty of getting that done in a timely manner with 8 different lidar groups since some sort of approximation is likely needed such as done in Diaz-Zurita et al mentioned above. So a clear statement of the estimated bias, lidar by lidar, would suffice although I sincerely hope the community makes progress

toward routinely including this in their analysis of Raman water vapor lidar data.

Indeed, for all lidars, the differential transmission correction related to aerosols was not taken into account. The reviewer is correct in noting that a discussion justifying this choice was missing. We have therefore added this discussion in Appendix B in order to preserve the original structure of the manuscript.

"B2.1 Bias linked to the differential aerosol transmission

This bias is due to the difference in wavelength between the H_2O Raman channel (~407 nm) and the channel used as a reference, which is either a rovibrational channel on dinitrogen (N_2) (387 nm) or a rotational channel on dinitrogen and dioxygen (N_2 + O_2) (~355 nm).

During the WaLiNeAs field campaign, we observed two contrasting scenarios regarding aerosol loading in the lower and middle troposphere, as recorded by all the lidars. The first scenario corresponds to a typical boundary layer load, with an AOT of less than or around 0.1 at 355 nm. AERONET data (https://aeronet.gsfc.nasa.gov) show variable Ångström exponents of up to ~1.5, averaging around 1. The second scenario is similar to the first, but with the addition of layers of Saharan aerosol plumes, which can lead to AOT of around 0.5 and Ångström exponents of around 0.5 or lower. Based on these two situations, we estimated the bias associated with aerosol-related differential transmission in the lower (≤1.5 km a.m.s.l.) and middle (≥1.5 km a.m.s.l.) tropospheres. The values are shown in Table B1, and the corresponding vertical profiles of bias are given in Figure B1. In the PBL, the bias remains below 0.7% (2%) for lidars using a rovibrational (rotational) channel. In the presence of dust aerosol layers, biases of less than 0.5% (1%) can be observed in the PBL and 1.3% (4.7%) in the free troposphere. Above 3 km a.m.s.l., aerosol-related bias significantly influences lidars using a pure rotational channel, where WVMRs predominantly remain below 10 g kg^{-1}.

Table B1: Biases for each Raman lidar due to i) the temperature sensitivity of the backscatter cross section of the H2O-Raman channel and ii) the differential transmission of aerosols. Two aerosol loads are considered. The first has an aerosol optical thickness (AOT) of 0.1 and an Ångström exponent of 1.5. The second corresponds to the presence of Saharan dust layers, with an AOT of 0.5 and an Ångström exponent of 0.5.

<table>
<tr><th colspan="2"></th><th>RL1</th><th>RL2</th><th>RL3</th><th>RL4</th><th>RL5</th><th>RL6</th><th>RL7</th><th>RL8</th></tr>
<tr><td colspan="2">Emitted wavelength (nm)</td><td>~354.7</td><td>354.69</td><td>354.71</td><td>354.83</td><td>354.72</td><td>354.74</td><td>354.74</td><td>354.70</td></tr>
<tr><td colspan="10">H_2O-Raman channel</td></tr>
<tr><td colspan="2">Central wavelength of filter (CWL) (nm)</td><td>407.46</td><td>407.56</td><td>407.65</td><td>407.62</td><td>407.50</td><td>407.54</td><td>407.58</td><td>407.5</td></tr>
<tr><td colspan="2">Filter bandwidth (FWHM) (nm)</td><td>1.06</td><td>0.30</td><td>0.35</td><td>0.37</td><td>0.25</td><td>0.30</td><td>0.35</td><td>0.30</td></tr>
<tr><td colspan="10">Bias</td></tr>
<tr><td colspan="2">Bias due to aerosols AOT = 0.1 (PBL)</td><td>< 0.7%</td><td>< 2%</td><td colspan="5">< 0.7%</td><td>< 2%</td></tr>
<tr><td rowspan="2">Bias due to aerosols</td><td>≤ 1.5 km</td><td>< 0.5%</td><td>< 1%</td><td colspan="5">< 0.5 %</td><td>< 1%</td></tr>
<tr><td>> 1.5 km</td><td>< 1.3%</td><td>< 4.7%</td><td colspan="5">< 1.3%</td><td>< 4.7 %</td></tr>
</table>

AOT = 0.5 (with dusts)								
Bias due to temperature (DT = 30 K)	< 0.3%	2.70%	2.7%	0.5%	2%	1.8%	2.8%	2.2%

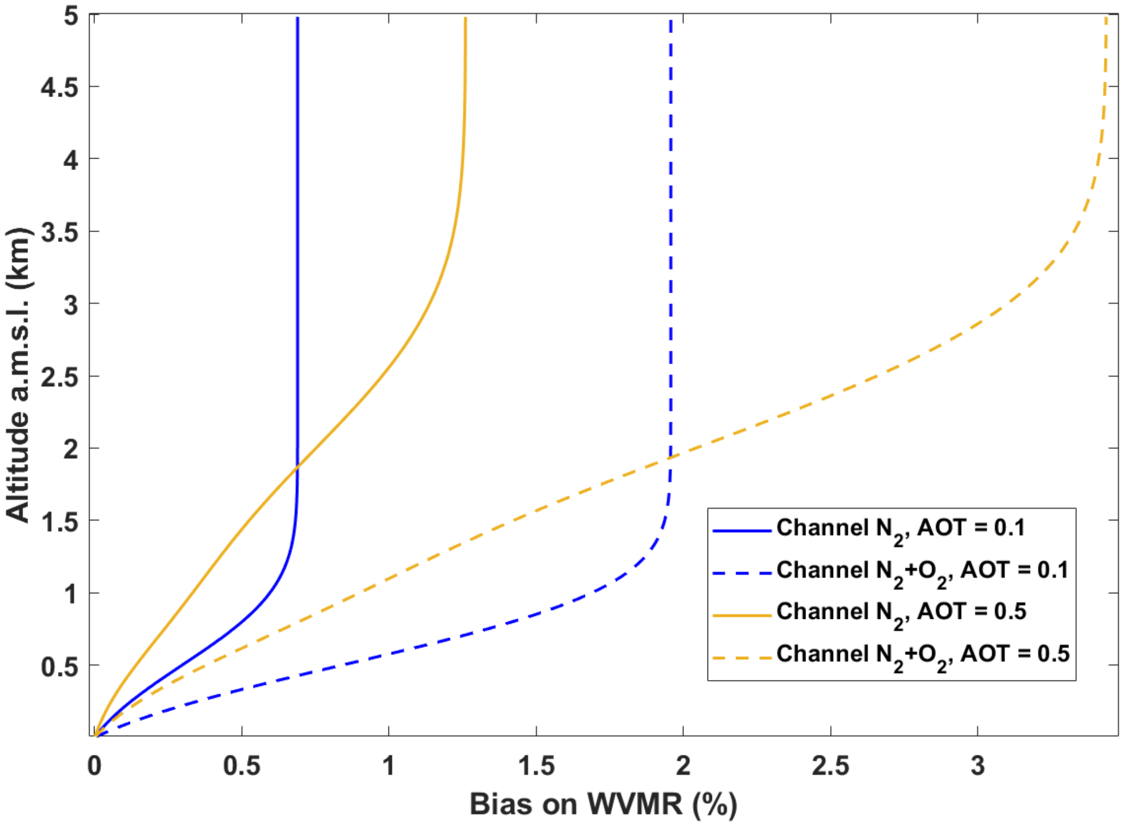


Figure B1: Bias on the lidar-derived water vapor mixing ratio due to the differential aerosol transmission between the water vapor Raman channel and the rovibrational nitrogen (N2) or the rotational (N2+O2) channels. Two typical aerosol loads encountered during the field campaign are considered: i) an aerosol optical thickness (AOT) of 0.1 with an Ångström exponent of 1.5 (shown in blue) and ii) an AOT of 0.5 with an Ångström exponent of 0.5 (shown in orange), which corresponds to the presence of dust aerosols."

2. Not addressed in the paper is the influence of the temperature sensitivity of Raman scattering in the evaluation of the mixing ratio equation. Since these systems offer daytime measurements one can assume that the water vapor filters are quite narrow (this is quite important information for the consideration of this temperature sensitivity so should be included for all systems). I could only find the filter width of 0.3 nm listed for the RL2 system. This filter width (assuming optimal positioning of the filter) was analyzed in Whiteman, 2003 and found to result in ~4% increase in mixing ratio between the surface and 11km. So for the purposes of the evaluation done here the effect might be about half of that given that comparisons are done up to an altitude of 5 km. It is very important to note that this temperature effect also results in a bias that

is in the same direction as the particle contribution to differential transmission. In other words, these effects are additive and both create an apparent increase in mixing ratio with height. Given that the authors state that neglecting the particle component of differential transmission can result in biases up to ~4% and that the temperature dependence adds another ~2% to that, the combination of the two can create more than a 5% bias in the lidar profiles, which are treated here as reference profiles. Should reference profiles contain 5% biases when it is well known how to do the calculations to reduce or eliminate these biases?

1. To address this, please consider the filter widths of the various systems in use and give the reader information on the magnitude of the unevaluated temperature sensitivity of the Raman lidar measurement of water vapor mixing ratio.
    1. Better yet, of course, would be for the community to also routinely perform these temperature dependence calculations which are not more difficult than those of rotational Raman scattering. I understand that this is also unlikely to be accomplished in a timely fashion when dealing with 8 different lidar groups. But, as with the particle component of differential transmission, the community should strive to make these calculations a routine part of the data reduction so reviewers cannot pick at incomplete data analysis in papers like this.

Although sensitivity studies had already been conducted for each lidar used in this study, the reviewer rightly points out that we did not address the biases associated with temperature variability within the atmospheric column. This discussion has now been included in Appendix B.

"B2.2 Bias linked to the temperature

Lidars are calibrated using radiosondes or ground-based weather stations when equilibrium has been reached in the first few hundred metres of the atmosphere (Laly et al.). The spectral widths of the interferometric filters for the N_2 and N_2+O_2 channels are optimised so that the associated backscattering cross-sections vary little with temperature. The measurements carried out for this article range from the surface to 5 km above mean sea level, with a temperature variation of less than 30°C between these two altitudes. This results in biases of less than 0.15%.

The biases are greater for the H_2O–Raman channel. They are highly dependent on the central wavelength (CWL) of the interference filter used and its full spectral width at half maximum (FWHM). These vary from one lidar to another, as shown in Table A2. Bias calculations were performed based on roto-vibrational Raman spectroscopy of water vapour (Avila et al., 2004; Adam, 2009). The temperature dependence of the Raman backscattering cross-section can be considered linear within a temperature range of 30°C. Biases associated with a temperature variation of 30°C are shown in Table A2. They also vary significantly depending on the lidar but remain below 3%. This is a conservative estimate, as the majority of the data used for this study were collected below 3 km a.m.s.l., and therefore for smaller temperature variations. Furthermore, IASI overpasses occur at fixed times in the morning and evening, with temporal temperature variations remaining below 10°C.

3. An argument is made that due to uncertainties in the mixing ratio that are calculated to be <= 0.1 g/kg, and due to regressions between lidar and IASI that yield coefficients of determination of 0.71 – 0.91, that in calculating the consistency factors using comparisons of lidar and ERA5, only the slopes of the regressions between lidar and ERA5 need to be considered. It is also stated that the biases of the regressions are more likely to be due to modeling errors than to instrument errors thus supporting dropping the calculated biases (offsets). I don't believe that either of these justifications is convincing.
    1. I believe the authors need to take a different approach with this part of the analysis. Is any filtering of the datapoints done prior to regression? Common sense considerations can be used to yield a more representative population of points. For example Whiteman et al., AMT, 2012 show a technique where outliers in the regression between sonde and lidar are filtered out by considering how geometrically similar portions of the profile are. In the 2012 paper, choosing geometrically similar portions of the curve helped to deal with the issue of the radiosonde drifting with the wind and thus sampling a different atmosphere than the lidar. Perhaps part of the disagreement in the comparisons as shown in Table 2 comes from the sampling of different atmospheres between the lidar and IASI. A filtering procedure could remove portions of the profiles that show poorer agreement with each other thus improving the regression.
        1. BUT, and this is very important, the calibration factor determined from such a comparison is not just the slope of the regression obtained. The calibration factor can be taken as the mean (or median if there remain some outliers in the population) of the ratio of the points selected for regression by the filtering technique. I would argue that the same type of procedure should be performed here in this analysis so that there are no questions about selectively choosing the slope alone from the regressions of lidar/ERA5 and neglecting the offsets.
            1. Please redo this analysis and either use the full regression equation, including offset, to compare systems (not recommended) or use the mean/median of the ratio of points selected by a careful filtering algorithm (recommended).

As we explained in our response to Reviewer 1, the lidar-ERA5 mean bias is highly correlated with the intercept. We decided to correct only the slope using ERA5 to avoid differences in the way all lidars were calibrated. Meanwhile, the offset remained uncorrected in order to maintain natural differences in the data independent of calibration. This robust approach avoids issues such as radiosonde drifting, as mentioned by the reviewer. As explained, the comparison between LIDAR and ERA5 uses only WVMR with an RMS lower than 0.4 g kg^{-1} to avoid outlier data. Initial calibrations were performed using ground-based meteorological probes in a well-mixed atmosphere or near radiosondes. Using ERA5 enables us to verify the relevance of the calibrations.

Now that we have included a more detailed discussion of the causes of bias, we have amended the text accordingly in subsections 4 and 5:

“The vast majority of biases (lidar–IASI or lidar–ERA5) are positive (see Figure 4b), indicating a consistent IASI underestimation of the water vapour content by ~+0.7 g kg⁻¹. This must be corrected by the bias relative to lidar measurements (Appendix B), leading to an underestimation by IASI (ERA5) of ~+0.4 g kg⁻¹ (~+0.2 g kg⁻¹).”

It should be noted that the synthesis is now performed while accounting for the number of valid samples at each site.

4. It is difficult to assess the magnitude of the effect of the neglected height dependent calculations that are being requested above since nowhere in the paper nor in the supplement are the mean lidar, IASI, ERA5 profiles of water vapor mixing ratio shown. Please add mean mixing ratio profiles to figures 4 and 5 (and to figs S18 – S25 in the supplement), perhaps as a third panel. This will greatly aid understanding the measurements in general.

We have added the mean vertical profiles of the water vapor mixing ratio as a third panel. An example is given below.

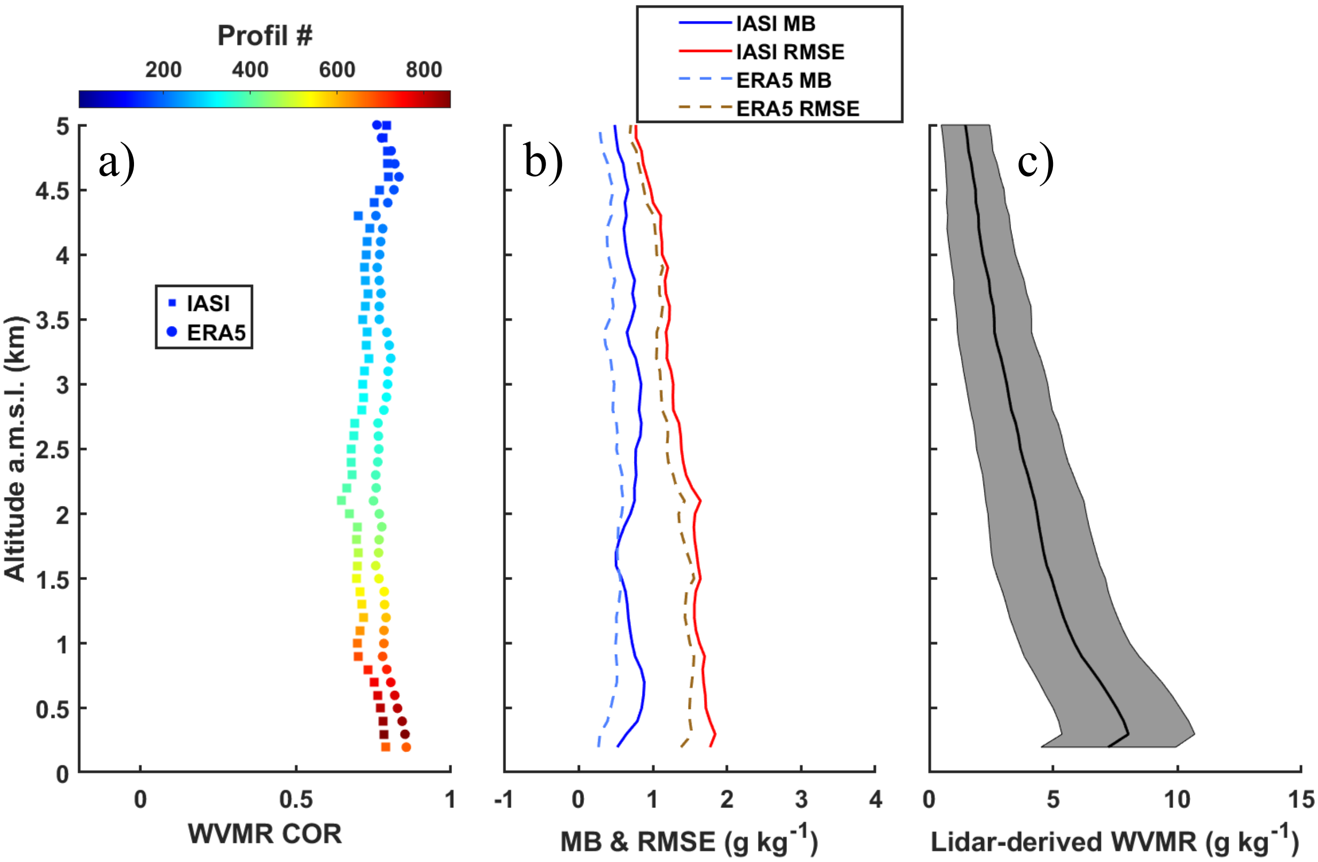


Figure 4: Synthesis of the statistical coefficients across all lidar stations when comparing lidar measurements with IASI and ERA5 products. the vertical profiles show a) the correlation coefficient (COR); b) the mean bias (MB, lidar - IASI and lidar - ERA5) and the root mean square error (RMSE); c) the mean water vapour mixing ratio (WVMR) profile of all the overpasses of IASI. The synthesis profiles have been obtained by combining statistical profiles derived from individual lidar systems.

Minor Comments

1. Line 2: please spell out the meaning of HPE in the title. It is not defined in the paper until line 284 so I also suggest mentioning this in the abstract if the relationship of the measurement period to when HPEs usually occur is important to the paper. In fact, it is not clear from the text of the paper that this is an important consideration and the authors may want to consider changing the title of the paper to more agree with the work presented in the body of the manuscript.

We agree and we have changed the title as "To what extent are the IASI and ERA5 water vapour profiles representative of the conditions in the autumn 2022 during the WaLiNeAs field campaign".

2. Lines 39-48: a description of IASI is given. It is described that IASI radiances are assimilated into forecast models but the focus here is on validating the IASI inversions. It would be very helpful to know what IASI profiles of water vapor are currently being used for as it would help to motivate the current work.

As with AIRS products, vertical WVMR profiles derived from IASI are particularly valuable for process-oriented studies, such as those presented in Chazette et al. (2015) and Lacour et al. (2018). Maps such as Figure 2 in this article provide clear scientific value by extending the interpretation of localized measurements, such as those obtained during field campaigns. We have added the following statement to the introduction:

"The operational IASI product offers significant scientific value for near-real-time monitoring of humidity field evolution. In particular, it is well suited for process studies of extreme precipitation events (Chazette et al., 2016) and for investigations of deep convection (Lacour et al., 2018)."

3. Line 109: statement is made "...the planetary boundary layer is therefore less directly constrained by IASI measurements." From the discussion of the paragraph, you are referring to products that use IASI radiances as one of many inputs. But the paper is about inversion products such as the profile of water vapor which are based mainly on IASI radiances, I believe (perhaps with some machine learning to improve the boundary layer?). So is this statement about the use of channels that peak above 2 km pertinent to the main subject of this paper which is comparing IASI and lidar profiles?

The radiance inversion procedure used to derive the operational products analyzed here is influenced, similarly to data assimilation methods, by the vertical positioning of the weighting functions. The key difference between the operational algorithm and the assimilation is that it relies on a proximity-based algorithm prior to a Bayesian inversion that explicitly accounts for the Jacobians (i.e., weighting functions). Even when machine learning approaches are employed—effectively emulating the Jacobians—the sensitivity to the lower troposphere remains limited. In this context, the proximity algorithm can be viewed as being effectively replaced by the training phase. We therefore consider it essential to provide information on the vertical distribution of the weighting functions, which reflects the instrument's effective vertical resolution.

4. Line 151: Figure 2. Gradients in IPW appear to be strongly correlated with land-ocean boundaries raising the question of whether the IASI retrieval may have emissivity dependencies that are influencing the images shown in Fig 2.

1. Can the authors supply any references that serve to address that concern about the IASI inversions?

Relevant references are already provided in subsection 2.2.1. The IASI retrieval accounts for surface emissivity, which is better characterized over oceanic surfaces. Consequently, greater variability is expected over continental regions, particularly in areas with complex topography. However, the infrared/microwave channels are selected to mitigate this variability, especially by favoring channels that are less sensitive to surface temperature. Nevertheless, this remains a significant source of uncertainty in the retrieval of vertical WVMR profiles from IASI radiances.

2. Are there known issues in the inversions over land vs water? If those inversion biases are known to exist, do they influence the comparisons shown here since all stations are located near the land/water boundary? If so, can you estimate how much? Can you speculate if this intercomparison might have yielded better results (reduced IASI dry bias) if the lidar systems had been situated away from the coastlines?

All sites involved in the WaLiNeAs field campaign are coastal, as one of the primary objectives was to investigate precipitation processes associated with the advection of air masses from the Mediterranean Sea. Coastal locations ensure that a majority of IASI pixels are located over the sea, thereby reducing sensitivity to emissivity variability. Some pixels over land remain, but topographic effects were minimized by retaining only those with local elevations below 500 m (see subsection 3.2). Based on previous results obtained over open ocean (Chazette et al., 2014a), no significant differences are observed. This may be explained by the additional selection of sea pixels induced by surrounding topography. Over land, a comparative lidar/ERA5 analysis has already been performed (Laly et al., 2025), yielding results consistent with those presented here.

5. Line 168: corrections covering a range of 17% are referred to as “small”. In the context of a validation campaign I would not describe such a range as small. Please use more appropriate wording.

The sentence has been replaced by

“As shown in Erreur ! Source du renvoi introuvable.**, the correction factors range from 0.93 to 1.1 with values mostly close to 1.”**

6. Line 170: Statement is made: “In some cases, these may be associated with imperfect corrections for molecular and particulate contributions in the lidar data.” I believe using the term “corrections” here is misleading. A correction is something that is done because of a known error/mistake. The error or mistake in this case would be to *not* calculate the molecular or particle transmission. (Authors state that the molecular component of differential transmission is being calculated the same by all groups. The major question above addresses what the different groups are doing regarding particle transmission.) So calculating the differential transmission is not a correction; it is simply a calculation of what is explicitly in the lidar equations. I suggest instead referring to “calculations of the differential transmission due to molecules or particles”. Full disclosure ... in the past I have also inappropriately used the term “correction” in this context.

We have replaced “correction” by “calculation”.

7. Line 173: “...either because aerosol optical thicknesses (AOTs) values are below 0.15”

1. please make an explicit statement that at all sites for the entire campaign the AOT remained below 0.15. Please specify the wavelength at which this statement pertains and what the source of information is for AOT.

Appendix B has now been introduced to document uncertainties related to aerosols, and appropriate references to this appendix have been added throughout the manuscript.

2. Note the issue in the quoted text if it remains: either strike "values" or change "thicknesses" to "thickness"

The correction has been done.

8. Lines 177-180: Very good. This is helpful. But as noted above please be explicit about which systems have made a correction for particle transmission. It is not optimum for the reference systems (lidars in this case) to have biases of 4% if they are to be used for assessing IASI profiles.
   1. Please note that here you refer to a "typical AOT of 0.2 (355 nm)" but earlier you stated that AOT is always below 0.15. Please reconcile these statements as they bear exactly on the issue of uncalculated differential transmission.

Indeed, we have corrected this passage taking into account the new Appendix B.

"As shown in Appendix B, for a typical AOT of 0.1 (at 355 nm) and an Ångström exponent of 1.5 (corresponding to pollution aerosols), the overestimation could reach approximately +1% for the use of N_2–channel and +2% for the use of N_2+O_2–channel, decreasing progressively closer to the lidar. It is less than +1% (between +1.3% and +5%) for dust aerosol mixed and above the marine boundary layer for the altitudes lower than 1.5 km a.m.s.l. (above 1.5 km a.m.s.l.)."

2. Please provide a citation for the statement "...aerosols are primarily desert dust with Angstrom exponents less than 0.5".

The origin of the data is specified in the new Appendix B and corresponds to AERONET sites.

9. Line 191: Table 2. I don't understand what you have done here. Please provide an equation so it is clear how this correction is applied. It is also confusing (and not really correct) to give a correction factor in units of percent. I assume that what you mean is if you have a correction of +10%, the factor is 1.1 and one of -7% is 0.93. An equation showing how the correction works with the actual factors provided (not percentages) would help clear up this confusion.

The correction has been done in Table 2:

	RL1	**RL2**	**RL3**	**RL4**	**RL5**	**RL6**	**RL7**	**RL8**
Calibration correction factor	**1.10**	**1.02**	**1**	**1.01**	**1.06**	**0.93**	**0.99**	**1**
Intercept ($g\ kg^{-1}$)	**0.03**	**0.54**	**0.67**	**1.00**	**0.62**	**0.10**	**0.45**	**0.19**
Coefficient of determination	**0.88**	**0.83**	**0.91**	**0.87**	**0.75**	**0.82**	**0.91**	**0.87**

Finally, the relationship is described by a linear equation which, after application of the calibration correction factor, has a unit slope. The intercept is provided in Table 2. As

stated in the manuscript, the slope correction is applied to the calibration coefficient independently determined for each lidar.

10. Line 200: "The number of IASI pixels …. ensuring minimal terrain obstruction." Please expand on this by giving an example. Are pixels completely removed or is the retrieval only used above mountain height?

The sentence has been modified to make it clearer:

"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 four of these pixels consistently provide data on the lower troposphere."

11. Line 206: "These metrics are used …". Please tell the reader how many points are used at each z to determine MB, RMSD, etc.

The number of points is indicated by the colour scale in Figures S18a to S25a, as well as in all figures of the same type. We have changed the colour scale to better highlight the number of points.

12. Equation 3: Please add z dependence to RMSD and MB.

The correction has been done.

13. Equation 5: MB and RMSD are not continuous functions. Please express these equations using summations instead of integrals.

The reviewer is right; we have made the correction.

14. Line 225: "...temporal dynamics differ between stations." I believe that "among" is more appropriate than "between".

The correction has been done.

15. Line 227: "Low clouds can easily … lidar measurements." This sentence seems to not have a real place in the flow of the text. Can you prepare it better with sentences before or after?

We have rephrased it as follows:
"The presence of clouds limits the range of lidar systems and increases measurement dispersion. They can be identified on the lidar profiles, and only the data beneath the clouds is considered. The low clouds can be easily identified in the figures by the altitude range of the lidar measurements."

16. Line 239: "The vertical profiles of the three statistical parameters…" Please revise to add the figure numbers to which this statement pertains.

The correction has been done.

17. Line 250: "This is due both to … within the PBL." I suspect this is also influenced by the fact that ERA5 was used as part of the cross calibration of the lidars. This should be mentioned here.

The correction applied relates to the slope and is very small. In fact, it is virtually zero for some lidars. The main causes are those given in the text.

18. Line 351: Table A1. Reception channels for vibrational N2 are noted as 387.5 nm. This disagrees with the text in the paper and is likely just a typo. Also, there was confusion earlier about how many of these systems are using the RR signal to normalize the water vapor in the MR calculation. Please make sure the paper is consistent in this regard between the paper text and this Table.

The correction has been done.

19. Line 360: N2 wavelength is given as 387.5. Please correct.

It has been corrected to 386.6 nm.

Supplemental material

1. Figures S1 - S9: please provide regression equation as an inset to the figure itself. This will greatly aid ease of interpretation.

The equation is provided in the caption for Figure S1. For Figures S2 to S9, the captions give a slope of 1, while the intercept is provided in Table 2. Therefore, we do not consider it necessary to include the regression line equation in the figures.

2. Figure S18 – S25: as a reminder, please add the pertinent mean mixing ratio profiles perhaps as a third panel to all figures.

The third panel with the profile of WVMR has been added (see Figure 4).