Reply to Referee 2 (RC1)

We thank you for the detailed review you provided. We tried to take into account your suggestions and think they will help to clarify and improve the paper. Your comments are in italic. We highlighted in red the modifications we propose to do in the manuscript.

'1) Section 2. The symbol q is usually used to imply the specific humidity (i.e. the mass of water vapour per unit mass of moist air) rather than the mixing ratio (r, the mass of water vapour per unit mass of dry air) as is stated at the beginning of this section. I suspect that q has simply been misnamed since the formulae in this section all appear to be consistent with it implying specific humidity. The abbreviation WVMR is used throughout the manuscript.'

Although r and q are very similar, we changed WVMR for q, specific humidity, throughout the figures and whole manuscript. Concurrently we propose to change the q definition (top of page 3, at the beginning of the 'Theoretical background' section) as: ' We can also express N in terms of specific humidity, q (kg of water vapor per kg of moist air). q is the parameter we aim at retrieving in the present work. Using the approximation q=0.622 e/ (P-0.378e) ≈ 0.622 e/P, Eq. (1) becomes:…', and to insert : 'In the following, we will use symbol q for specific humidity and water vapor mixing ratio equally, since the percentage deviation between both is rarely exceeding 1%, even in case of large humidity concentrations, which remains far less than the systematic and statistical uncertainties affecting the lidar mixing ratio measurements.' in section 3.2 (p9, line 6), where we describe the lidar measurements. Anyway, the approximation that is done above (where 0.378 e is considered as small relative to P), implies that the accuracy of the humidity retrieval by the radar cannot be better than the difference between q and r, since 0.622 e/P is also an approximation of r.

'2) Although the figures are all sufficiently large to allow the important features to be seen, the labels are quite small on some of them - particularly Figs 9 - 12. It would be useful if the labels were made larger.'

New figures will be provided with larger labels.

'3) In connection with Figure 6, use of the gradient Richardson number will only identify regions of (dry) convective instability (which are expected to be confined to the boundary layer) and dynamic instability. I would expect moist convective processes to be a more significant contributor to turbulence at these altitudes, although such regions will not be identified in this way.'

We agree that the link between the gradient Richardson number and the calibration coefficients is relevant for the dry conditions of BLLAST, but not for those of HYMEX SOP1, for which the boundary layer was not very deep (most of the time), nor for those of HYMEX SOP2, for which dynamic turbulence systematically prevailed over thermal turbulence.

To be clearer in the manuscript, after the sentence: ' The same analysis was applied to the HyMeX datasets from which no significant result arose, neither during SOP1 nor SOP2 (not shown)' (p16, line 29), we removed the sentence: 'Unstable conditions were too occasional to show any tendency. Usually, the upper layer exhibited a stronger stratification than the lower layer, but the calibration coefficients varied irrespectively of this variation.' and replaced it by: 'During HyMeX, the development of the boundary layer was most of the time generated by mechanical turbulence (due to the wind intensity or to the roughness change at the sea/land transition). This can explain why the gradient Richardson number is not a good indicator of the variability in the calibration coefficients under the HyMeX conditions. There is also no clear difference in the HyMeX coefficients between the lower and upper parts of the profiles, probably because moist convection equally affected all levels.'
The fact is that the calibration coefficients for HYMEX during both SOPs do not vary as much as those during BLLAST. It is an hypothesis to consider that the difference in the variability could result from the buoyancy contribution due to the temperature fluctuations. The point is that turbulence is also taken into account in C_n^2 through ε (Eq. 15), which includes contributions from temperature, wind and moist

(through the release of latent heat). We recognize we did not address this issue and kept a practical point of view by underlining that the calibration coefficients may vary, and recommending the use of a calibration coefficient that varies with time and space.

This conclusion appeared in the previous version of the manuscript in section 4.3, p17, line 8 ('The main conclusion we can draw from these results, coming primarily from the BLLAST data, is the necessity of distinguishing between the mixed layer and the free troposphere in case of unstable conditions in the low troposphere.') and we added in the final section of the proposed new version of the manuscript : 'We highlighted the necessity of calibrating the vertical gradient of refractivity provided by the radar, with calibration coefficients likely to vary in time and space.'

'4) Page 17, line 27. "We checked the distributions of C_nˆ2 for the 3 datasets and found that the logarithmic averages of C_nˆ2 (close to the median values) gave 1.4, 31, and 1.0 10ˆ-14 mˆ-2/3." Should the middle value be 31 or 3.1? The shown value of 31 is much larger then the two other values quoted.'

We maintain 31. This high value is the reason why we explained (p17, lines 29-32) that HYMEX SOP1 conditions are borderline to apply the method (Bragg and not Rayleigh conditions). We are aware that the application of the method under these conditions is questionable, but these conditions are characteristic of the whole Mediterranean western basin, during at least 2 months every year, and probably characteristic of other regions in the world. It was worthy to assess the method under these specific conditions, especially because models had difficulties to retrieve accurate values of humidity under these conditions, which was one of the motivation to organize the experiment.

'5) Page 19, second paragraph. The authors discuss the possibility that the cloud connected with Fig. 9 may be virga. If so, the radar might be seeing hydrometeor scatter rather than clear-air scatter and the humidity retrieval algorithm would not be valid. This possibility should be discussed. This point is also relevant for the discussion of Fig. 10.'

We agree with you: they cannot be virga, since the turbulence structure parameters, as well as the vertical velocity measured by the radar, would be higher if it was a virga, and we could not have applied the method. We intend to replace 'virga' by 'cloud' in the new version of the manuscript.

'TECHNICAL CORRECTIONS
C2
6) Page 18, line 18. The word "criterion" is misspelled as "criterium" twice in this section.'

It will be corrected in the new version of the manuscript.