The authors wish to acknowledge the anonymous reviewer #3 for his/her detailed review of the manuscript that contributes to improve its quality.

In the following, a detailed reply to the general and specific issues pointed out by the anonymous reviewer #3 is provided. In the following the reviewer's comments are reported in normal style while in italic is reported the corresponding authors' reply.

This paper presents a relevant infrastructure (the CIAO observatory) for ground-based observation of the atmosphere, devoted in particular to the measurement of aerosol, water vapor and clouds on a long-term basis permitting to build up a climatology data base of their properties.
The paper is structured in two parts. The first one (sections 2 and 3) presents the instruments and installations constituting the infrastructure, as well as the employed measurement strategies aimed to the exploitation of the synergies between the different instruments. The second one (section 4), while still description-oriented, focuses on the example of water vapor measurements through radio soundings, Raman lidar and microwave radiometers, going down to the description of the Kalman-filter algorithm used to merge lidar and radiometer data for enhanced water-vapor profiling.
Although possibly not containing ground-breaking results, the quality of the paper is good, and it certainly includes information that can be used by other researchers to improve their atmospheric measurement techniques.
In addition to minor issues, typos and English writing remarks listed at the end of this review report, I have the following remarks and suggestions:
1. In the present organization of section 2.1, the description of the Raman lidar system for water vapor measurements is placed between those of the two multiwavelength aerosol systems. I suggest reordering the paragraphs so that the descriptions of the aerosol systems are placed next to each other.

The instrument description in section 2.1 has been modified according to the reviewer suggestion.

2. I have some concerns regarding the description of the Kalman filter algorithm in section 4.3:

a) There is probably a typo in Eq. (5), where $\mathbf{R}^{-1}$ should be $\mathbf{R}$, i.e. the equation should read

$$K_i = \mathrm{P}_\mathrm{i}^\mathrm{b} H_i^T \left[\mathrm{H}_\mathrm{i} \mathrm{P}_\mathrm{i}^\mathrm{b} H_i^T + R_i\right]^{-1}$$

instead of

$$K_i = \mathrm{P}_\mathrm{i}^\mathrm{b} H_i^T \left[\mathrm{H}_\mathrm{i} \mathrm{P}_\mathrm{i}^\mathrm{b} H_i^T + R_i^{-1}\right]^{-1}$$

The authors acknowledge the reviewer for the detection of this typo. The matrix is R and not the inverse as clearly visible from dimensional analysis of Eq. 5.

b) The authors seem to use the same symbol, $_\mathrm{i}\mathbf{x}$, to denote the state variables and their estimates. This is apparent in Eq. (3), which, the authors say, corresponds to the ñimproved a posteriori estimateò. Previously it has been said (line 19 of page 5275) that the state variable is represented by x (by the way, shouldnôt it be boldfaced?). Different symbols should be used to distinguish the state variables, which never are perfectly known, from their estimates, which are the measurement product.

In order to avoid any confusion between the state variable and its estimation obtained from the retrieval algorithm and in order to enhance the quality of the mathematic formalism of the

equations described in the manuscript, the state variable x is replaced by $\hat{x}$. Moreover, Kalman filter theory is briefly described in the Appendix A, added to the previous version of the manuscript.

c) Likewise I think there is a problem with Eq. (1), where the left side should be the state variable at time step **i**, instead of the background estimate x_i^b (see Eq. (2) in ref. Han et al. (1997) given in the paper). In my understanding, the background estimate at time step **i** is obtained from the projection through the transition matrix **A** of the estimate at step **i-1**, not of the state variable, which is, strictly speaking, unknown (cf. Eq. (3) and the previous paragraph in ref. Han et al. (1997)), like the transition error, for which only its covariance matrix **Q** is known.
In summary, I think the authors should revise the description of the Kalman filter and its notation.

Here, that authors beg to disagree with the reviewer. The eq. 2 in Han et al. only describes a Markow stochastic process that is used as possible model in the update equations of the Kalman filter as done also in our manuscript. In the general context of the Kalman filter the Eq.1 is well reported if the state variable x is replaced by its estimate $\hat{x}$, as already mentioned in the previous comment.

In order to get further information and a detailed formal description of the notation used in the Kalman filter as well as of the definition of the background estimation, please, see also the presentation at this link:

http://www.ecmwf.int/newsevents/training/meteorological_presentations/pdf/DA/AssAlg_4.pdf

This provides a more general description of the Kalman filter equations, while equations reported in Han et al. 1997 is much focused on a specific application.

3. The discussion of the case study summarized in fig. 4 would be clearer if the way in which the Kalman filter has been initialized (through a radiosounding previous to the time series?; through a lidar measurement also previous to the time series, before the cloud cover blocked the lidar data above 3km ï 3.5 km?) were explained. If Iôm not wrong, this initialization would be the reason for the Kalman filter output yielding better results above the cloud base than the microwave radiometer alone, as stated in the sentence on lines 19-24 of page 5279, since in the time interval shown in fig. 4a and b the lidar is blocked by the cloud and cannot contribute to improving the estimate above the cloud base. This remark applies as well to the sentence on lines 4-7 of page 5281:
ñThe reported case study also shows that the proposed integration approach is in better agreement with the co-located radiosounding profile with respect to the neural network retrieval applied to the microwave Tbs onlyò.

The authors wish to underline that the application of the proposed Kalman-based retrieval scheme during daytime or cloudy conditions allows us to preserve the high resolution lidar information, where available, and to integrate it with the microwave observations. Obviously, below the maximum altitude level available from lidar data, a real data integration will be performed, while above we will have a passive retrieval only. However, since the Kalman filter is an iterative filter in time, also the daytime/cloudy profiles will benefit from the previous measurements assimilated in the Kalman scheme and used as a guess for the following temporal steps, i.e the last lidar profile in night time and clear sky conditions or the last co-located radiosounding available. Moreover, both the transition error matrix retrieved from climatologically long time series of lidar data and the cross-covariance error terms optimize the physical consistency of the profile portion retrieved

below the lidar profile with that retrieved above the lidar profile and based on microwave observations, potentially reducing the bias between the ***ñ****true****ò*** *water vapour mixing ratio profile and the microwave retrieval.*
In the revised version of the manuscript, the authors also mention the first guess used in the presented case study, that is a co-located radiosounding started at 15:45 UTC on 20/02/2004.

4. The sentence (starting on line 25 of page 5278) ñThe comparison with the time series of Fig. 4a shows a slight degradation of the water vapour profile resolution in the PBL with respect to the original lidar measurements. However, this can be compensated for performing a final merging between the filter estimation and the original water vapour lidar profileò prompts the following question: if the filter output has to be merged again with the lidar profile, which was one of the filter inputs, to recover the lost resolution, couldnôt a procedure be devised in which modifications are made to the filter to avoid since the beginning this loss of resolution and the consequent proposed feedback to recover the original one in the lidar profile? Could the authors comment on this? At another level, please note that ñbyò is probably missing between ñforò and ñperformingò.

Retrieval techniques applied to ill-posed problems provides a solution set that is intrinsically characterized by large errors in the determination of the final solution and low vertical resolution respect to the first guess used for constraining the problem and reducing its dimensionality. In order to preserve the lidar resolution in the final output of the filter, the use of diagonal covariance matrices is recommended. This would exclude all the potential benefit from the cross-covariance terms of the error matrices in the Kalman integration retrieval. Therefore, to preserve the lidar vertical resolution, part of the benefit from integrating lidar and microwave observations using this approach would be suppressed. However, a new integration strategy not based on the Kalman filtering, currently under evaluation, also aims at avoiding the further merging of the retrieval estimation and of the original lidar profile.

5. Eq. (7) should be further explained: whatôs δz in that equation? What does $diag(K_i H_i)$ mean?

In order to clarify the meaning of the terms δz *and* $diag(K_i H_i)$*, in the revised version of the manuscript at page 5279, line 5, the following lines have been added after the Eq. (7):* ***ñ****... where* δz *is the vertical step of the output solution and* $diag(K_i H_i)$ *are the diagonal elements of the matrix* $K_i H_i$*.****ò***.

Other issues

1. Page 5264, lines 24 and 25: the sentence ñCT25K ceilometer is able to detect three cloud layers simultaneouslyé ò is not very clear. Whatôs the reason limiting the number of layers that can be detected? If it was a hardware reason, probably the number of layers that can be detected would depend on their optical thicknesses. Is it because of the associated software? By the way, ñTheò should probably be inserted before ñCT25Kò. The same remark applies to the sentence on lines 7 and 8 of page 5265 referring to the CHM15k ceilometer: ñAs for the CT25K ceilometer, it is able to detect three cloud layers simultaneouslyò.

CT25K and CHM15k are operated at CIAO using the acquisition and processing software delivered by the respective manufacturers. These software are pretty different but both are designed for retrieving the height of a maximum of three cloud layers. Three layers is an upper limit, but the number of cloud layers is obviously depending on the extinction of the laser beam and, therefore, on the cloud optical depth, that could limit the number of observable cloud layers. In the case of

CT25K, the software provides only the profile of an attenuated/uncalibrated backscattering coefficient and the retrieval of the height of the first three layers. Even if this matches with the needs of the observatory related to the use of CT25K cloud profiling data, considering also that its range is limited to 7.5 km above the ground level, there is no chance to get or reprocess the raw data since they are not available (VAISALA stated that raw data are commercially protected). The situation for the CHM15k is different. Actually, the CHM15k acquisition software provides the vertical profile of the 1064 nm raw backscattering signal (in terms of number of photons). The processing software provides several products, such as the retrieval of the height of a maximum of three cloud layers. At the moment, the manufacturer software is used at CIAO and its performance versus other cloud base retrieval methods, reported in literature, will be assessed taking also advantage of co-located Raman lidar data.
*However, in order to be more clear, the text at page 5264, lines 24 and 25, in the revised version of the manuscript has been modified as follows: **ñ**Depending on the cloud optical thickness, CT25K processing software, designed by VAISALA, is able to provide up to three cloud layer heights simultaneously**ò**. Similarly, at lines 7 and 8 of page 5265, the revised version of the manuscript has been modified as follows: **ñ**As for the CT25K ceilometer, CHM15k processing software, designed by Jenoptik, provides up to three cloud layer heights simultaneously, but it also provides the retrieval of the boundary layer height and of the cloud penetration depth**ò**.*

2. Page 5265, line 23: the abbreviation ñlv2.0ò is used before its definition is given on line 19 of page 5267.

In the revised version of the manuscript lv2.0 definition is done before its use.

3. Page 5271, line 12: ñAn optimal agreement is observedò. ñOptimalò has a very strong meaning that I think does not correspond to the sense of the sentence. I would suggest replacing ñoptimalò by ñvery goodò or something similar.

*In the revised version of the manuscript **ñ**optimal**ò** has been replaced by **ñ**very good**ò**.*

4. Page 5272, line 16: ñresulting stable within 5%ò. I suggest explicitly stating the period during which this stability has been observed, even if indirectly it can be inferred that it lasted from 2002 to 2008.

*The **ñ**stability**ò** period the authors are referring to is from March 2002 to June 2006 and it is related to the assessment of the performance of the CIAO water vapor Raman lidar only. This is in agreement with the paragraph reported on page 5272, at lines 17-19. After this period, lidar measurement configuration has been modified and systematic water vapor measurements has restarted in November 2008. Moreover, a more recent reference (Mona et al., 2007), dealing with the stability of Raman lidar calibration has been added in the revised version of the manuscript.*

5. Page 5276, lines 3-5: ñFinally, **Q** is the covariance matrices of **w**, assumed as white Gaussian noise processes with zero-meanò. Is it required in the assumptions of the Kalman filter that the noise is white in addition to Gaussian? Is not the Gaussian assumption sufficient? Note as well that ñmatricesò is used instead of ñmatrixò and that the hyphen between ñzeroò and ñmeanò should be dropped. The same question and last remark apply to the sentence on lines 7-8 of page 5277: ñassumed as white Gaussian noise processes with zero-meanò.

*White noise is a random signal (or process) with a flat power spectral density. In particular, if a time series R_t is normally distributed with mean zero and standard deviation **ů**, the series is called a Gaussian white noise. This is a basic assumption for using the Kalman filtering scheme. In addition, the mistake **ñ**matrices**ò** has been corrected in the revised version of the manuscript.*

6. Page 5277, lines 28-29, and page 5278, line 1: please check the correspondences between the rms deviations stated as mixing ratio (g/kg) and mass concentration (g/m3). As the air density appears in the conversion from mixing ratio to mass concentration, shouldnôt the assumed air density for the conversion be specified?

The air density used in the reported case study is obtained from temperature and pressure profiles obtained by the co-located radiosounding started at 15:35 UTC on 20/02/2004. This information has been included in the revised version of the manuscript.

7. Table 1. a) General remark: could the font size of the table contents on page 5288 be increased? The font size of the part of the table on page 5289 looks larger and easier to read. b) In the lv2 products from PEARL the wavelengths at which α is measured are missing. c) At the end of the lv2 products for PEARL and MUSA ñat ŭ at 532 nmò should be ñand ŭ at 532 nmò. d) In the text of the paper, an algorithm for retrieving the aerosol backscatter coefficient at 1064 nm is referenced (page 5262, line 27, Di Girolamo et al., 1995), but it is not mentioned in the algorithm column for PEARL and MUSA; should it be included?

The reference Di Girolamo et al., 1995 has been included in Table 1 of the revised version of the manuscript.
Regarding the font size of the table contents on page 5288, this is also the result of the manuscript typesetting carried out by Atmospheric Measurement Techniquesô staff. The original version of the Tab. 1 had a font size similar to the part of the table on page 5289. In order to improve the readability of Tab. 1, if the manuscript will be accepted for publication, the authors will find an agreement with the editing staff to preserve the original font size.

8. The caption of fig. 3 mentions a ñlower panelò. However the figure only contains two panels on the same line, labeled (a) and (b). Please check that there is not a panel missing and the consistence of the caption. Also, on the third line from the end of the caption ñare reportedò should be read instead of ñis reportedò.

The reviewer is right. In the revised version of the manuscript the caption are referred only to panels (a) and (b), respectively.

9. Fig. 4: the sizes of the panels and of their legends should be increased for enhanced readability.

Fig. 4 has been revised and in the revised version of manuscript the size of the panels and of their legends has been increased to improve the manuscript readability.

Typos and minor suggestions

1. Page 5254, line 9: probably ñandò is missing before ñradarò
2. Page 5255, line 2: inserting commas before and after ñas well as their reciprocal interactionsò is suggested.
3. Page 5257, line 7: ñinfrastructureò should be ñinfrastructuresò
4. Page 5257, line 13: the hyphen between ñphenomenaò and ñlikeò seems to be surplus.
5. Page 5259, line 21: ñparticipation in GAW-GALIONò is probably better than ñparticipation to GAW-GALIONò
6. Page 5260, line 12: ñin orderò is probably not necessary.

7. Page 5260, line 18: ñwith second and third harmonic generatorsò probably better than ñwith the second and third harmonic generatorsò
8. Page 5260, line 25: ñdevoted toò probably better than ñdevoted inò
9. Page 5261, line 4: ñsplit intoò maybe better than ñsplit inò
10. Page 5261, line 8: ñinterferential filterò: there are several occurrences of this term throughout the paper. Consider if you might prefer using ñinterference filterò, which seems to be more common usage.
11. Page 5262, line 14: ñN_2ò instead of ñN2ò.

12. Page 5262, lines 18-19: the sentence ñFor MUSA the calibration of depolarization channels is made automatically using the ±45 method (Freudenthaler et al., 2009)ò is somewhat redundant with that found on lines 28-29 of the same page: ñThe aerosol linear depolarization ratio measurements are obtained according to Freudenthaler et al. (2009)ò. Consider if it is worth keeping that redundancy.

13. Page 5263, line 28: consider if a hyphen should be inserted between ñmagnetronò and ñbasedò.
14. Page 5264, line 14: ñBefore the end of 2010é ò. This sentence should be updated.
15. Page 5264, line 20: ña CT25K ceilometerò, instead of ñCT25K ceilometerò.
16. Page 5264, line 22: ñThe ceilometer is basically a Rayeligh lidar systemé ò. Do the authors mean a Mie lidar system?
17. Page 5266, line 9: ñpyreliometerò should probably be ñpyrheliometerò.
18. Page 5266, lines 13 and 14: ñA Trimble GPS antenna/receiver station is already operative, even if its use for providing the integrated water vapour estimation will start by the end of 2010ò: please update.
19. Page 5266, line 22: ñGPS radar heightò. Please check if that the intended term.
20. Page 5268, lines 2 and 3: referring to Table 1 it is said ñFinally, the list of the advanced products obtained from the integration of the data provided by different instruments is reportedò. However, in the case of the multi-wavelength lidars the advanced products are obtained from the data provided by a single instrument.
21. Page 5269, line 5: ñdifficult for the provisionò: probably ñforò is surplus.
22. Page 5269, line 12: a semicolon instead of a comma after ñwithin cloudsò is suggested.
23. Page 5269, line 13: deleting the comma after ñfeedback processesò is suggested.
24. Page 5269, line 23: ñmean aspectsò; please check if ñmain aspectsò was meant.
25. Page 5270, line 13: ñeitherò should probably be moved from before to after ñseveral examplesò.
26. Page 5271, lines 5-6: ñIn both the considered regionsò. Probably ñIn both considered regionsò is better.
27. Page 5274, line 16: probably ñwithò is missing between ñthanò and ñtheò at the end of the line.
28. Page 5276, line 23: probably the hyphens in ñpoint-of-viewò can be dropped.
29. Page 5280, line 27: check the construction of the sentence starting in that line: ñThe integration retrieval, though provides a description of the water vapour field with a coarser resolution with respect to the lidar, it is able to provide a more operational product that allows us to override possible limitations in the Raman lidar measurementsò.
30. End of Fig. 2 caption: ñBoth couplesò instead of ñBoth the couplesò.

Regarding to the other typos and minor suggestions provided by the reviewer, the authors have modified the text of the revised version of the manuscript accordingly.
In particular:

18. Page 5266, lines 13 and 14: ñA Trimble GPS antenna/receiver station is already operative, even if its use for providing the integrated water vapour estimation will start by the end of 2010ò: please update.

*GPS data processing is still pending but the following sentence has been included in the final version of the manuscript to update the status of the GPS processing: **ñ**A Trimble GPS antenna/receiver station is operative and CIAO is going to be part of the NOAA GPS network by the end of 2011. As soon as the station will be formally included in the NOAA network, the NOAA GPS products from CIAO station, including the integrated water vapour estimation, will be available to the users in near real-time.**ò***

20. Page 5268, lines 2 and 3: referring to Table 1 it is said ñFinally, the list of the advanced products obtained from the integration of the data provided by different instruments is reportedò. However, in the case of the multi-wavelength lidars the advanced products are obtained from the data provided by a single instrument.

*The authors agree with the reviewer. In the final version of the manuscript, the last column has been renamed **ñ**Advanced and Synergetic products**ò**. This classification is suitable for the possibility to provide advanced products resulting from the use of observations from single advanced sensors, like multi-wavelength Raman lidar, as well as from multiple sensors.*

29. Page 5280, line 27: check the construction of the sentence starting in that line: ñThe integration retrieval, though provides a description of the water vapour field with a coarser resolution with respect to the lidar, it is able to provide a more operational product that allows us to override possible limitations in the Raman lidar measurementsò.

*According to the reviewer suggestions we revised the sentence at Page 5280, line 27 as follows: **ñ**Though the retrieval provides a description of tropospheric water vapour with a resolution coarser than the lidar, the integration retrieval is able to provide a more operational product that allows us to override possible limitations in the Raman lidar measurements due to the presence of thick clouds or daytime solar background.**ò**.*

*Finally, in the revised version of the manuscript the authors renamed sub-section 4.2 as **ñ**Calibration of water vapour Raman lidar**ò** in order to better describe the content of the sub-section itself.*