We thank the referee for the useful comments. We have attempted to address each below.

Specific Scientist Comments:

1. P6452, lines 19-20. More appropriate reference has been included in the manuscript.
    a. Whiteman et al., JAOT, 2010
2. P6453, line 15. More appropriate reference has been included in the manuscript.
    a. Vaughan et al., QJRMS, 1988
    b. Sherlock et al., AO, 1999
    c. Venable et al., AO, 2011
3. P6453, line 20. We agree with the referee comment and the term "more" as qualification on the use of the term independent has been removed. Moreover, a more appropriate reference has been included concerning the independent calibration (Sherlock et al., AO, 1999)
4. P6453, line 25. The sentence regarding the calibration stability has been removed and has been replaced by the question of the range of tolerable measurement uncertainties for upper tropospheric trend detection addressed by the referee. Trend detection of water vapor in UTLS being one of core objectives for the future system, it is important to address this point and the reference which addresses this question has been included. Concerning the calibration aspect which is also an important issue for water vapor long-term monitoring, it is addressed later in the manuscript.
5. P6454, Section 2.2 (Comments 5.1 and 5.2). As mentioned in this section, SR is derived from the Rayleigh-Mie and nitrogen Raman signal; however the nitrogen signal is too noisy for a good optical depth measurement that is why we decide to use the described methodology in this section. In many measurement cases, the direct calculation was not appropriate in our case. To be able to retrieve with the same confidence all cases including thin or thick clouds, we choose to use a lidar ratio of 18.2sr (Platt and Dilley, 1984) which is in quite good agreement based on Raman lidar measurements yield LR values (Reichardt et al., 2002; Cadet et al., 2005). Furthermore, it permits directly to compare our results to some cirrus clouds analysis obtained by Cadet et al. (2003) in which analysis were performed using a LR value of 18.2sr.
6. P6455, paragraph starting on line 7. (Comments 6.1 to 6.3). The text regarding the use of optical fiber has been incorporated in the discussion, the figure 2 has been introduced earlier in the text and the term α-ε removed. Indeed this expression perhaps is not very useful information. In fact, the first term α is used for Alpha Technology and the second term design the slope factor which is a measure by which the sharpness of transition between attenuation and transmission is achieved. Also we use alpha-epsilon which is a superior filter for Raman spectroscopy. Regarding the comment 6.4., the statement about the use of cooled PMT for N_2 signal, as sky background is low during nighttime; the amount of noise from this source is quasi-similar to the detector noise. With a FOV used in the old system (1mrad) and the wide bandpass filter of 1nm, the value indicated for sky background is around 5.6 photons for 30min integration and of 5.4 photons for dark noise detector considering a dark count rate of

100cps.s^{-1}. When PMT is cooled, the background noise is ~5.9 and ~11 when it is not cooled. So the background noise is reduced by a factor ~2. The text has been modified accordingly.

7. P6457, section 4.1. Corrections about differential transmission due to molecular scattering have been applied however no corrections about differential transmission due to aerosols have been applied. Resulting from the calibration used here (ECMWF); this is less of concern that the molecular scattering correction and the disregard for aerosol scattering is justifiable as underline the referee.
8. P6459, section 5.1. Discussion about the methodology used for data sampling has been extended. Please, refer to the new version of the manuscript.
9. P6460, section 5.2. Months relating to the two seasons have been added in the text – Dry season (May to October) – Moist season (November to April)
10. P6461, lines 8-10. As mentioned by the referee, in general many figures have shown with sometimes little text devoted to them. We decide to remove the figure 9. Indeed, it can appear not clear for the readers. Relating to this figure, we have concluded that the atmospheric variability show larger variability than diurnal cycle due to the absence of diurnal cycle variation expected in the long acquisitions. If these variations could have been present and meaningful, following the beginning time for the acquisition, a bias could have been introduced in measurements analysis. However as no diurnal cycle variation is visible, it suggests that the atmospheric variability seems greater than diurnal cycle which allows to keep a measurement protocol during nighttime independently of time. Nevertheless, we decide to remove these explanations and the related figure because more important and long acquisitions have to be performed over the year to ensure this affirmation.
11. P6462.More explanations have been added concerning the cluster analysis. In fact, the method consist to use geometrical macrophysic cirrus clouds parameters derived from lidar such as the optical and geometrical depth, mean altitude and top altitude in a hierarchical ascendant classification. The discriminant factor analysis is a complementary method to insure the HAC results. The methodology used is similar to that described by Keckhut et al. (2006).
12. P6464. Line 26. The statement is made "A narrow FOV of 1mrad is used to reduce as little as possible sky background". In fact, more appropriate sentence should have been "…to relatively reduce the sky background. However, we agree with the referee when he said that a FOV of 0.2-0.25mrad achieved a reduction between 16 and 25 times in skylight background. Indeed, a FOV of 0.25mrad has been tested in our simulations achieving a reduction in skylight of around 16.4. So we will probably use this FOV value in the future system. For the old lidar system, the use of a 1mrad FOV has been preferred even if it limits the measurements in the UTLS. The use of a smaller FOV, given the emission-reception parallax, should not permit one to perform measurements in lower altitude for the old lidar system.
13. P6465. Line 5. The sentence "…constant illumination conditions at the optical fiber output" is not correct. The term constant has been replaced by quasi-constant. Indeed, in the case of the old lidar system, fibers as long as 20m were used. In same way, a

word of caution has been included in the discussion about fluorescence that is not limited to optical fibers in agreement with Sherlock et al. (1999).

14. P6465. Section 6.3. Figure 16 have been introduced earlier in the text.
15. P6466. Line 12. Effectively, as mentioned in point 6 of this document, some information between Raman and Rayleigh-Mie channels has been mixed. Indeed, gated tubes will not be used on Raman channels and the statement done about a measure which down to the ground keeps true.
16. P6466. Line 13. In fact, no more tests are need concerning the PMTs, however we will need to check which one between both PMTs give better performance with different emitted energy per pulse. This section has been modified accordingly.
17. P6466. Line 15. About the Licel Transient Record, we will use the TR20-16 as specified in the text; this transient permits both analog (lower altitude) and photon-counting (upper altitude) combination increasing the dynamical range of acquired signal compared to conventional system. The PR10-160-P transient recorder is a photon counting system used in the old lidar system and will be available if need.
18. P6466, line 10. More explanations regarding the use of R7400-03 or R7400-20 have been included. Please refer to the revised manuscript.
19. P6466, Section 6.4. In this section, dedicated to the PMTs, we discuss the criteria relating to the choice of the PMTs and the limiting factor for a PMT in photon counting mode. This statement does not disregard the influence of skylight background. We agree with the referee that the skylight background is a larger source of noise than the PMTs themselves but the statement made here related to measurements performed during nighttime. Perhaps this statement was not clear for the referee because it was not mentioned in this part of the text. Indeed, the sky spectral radiance between both night and day vary around an order 6. That is explain why the PMTs noise is negligible and not of interest compared to the sky background in daytime. However as shown in Table 3, the night sky background to detector noise ratio (in photons) indicated values close to 1, so both noise sources are quasi-similar in nighttime.
20. P6467. Section 6.5. Effectively, this section which the calibration aspect was not very clear. It has been modified. To just give an explanation here, it has been decided to use a H_2O total column measurement to calibrate the water vapor profile from lidar. The use of the radiosonde data will be compared to the lidar profile but as an independent and systematic measurement. The GPS and lidar will be collocated, only the radiosonde will be not.
21. P6476. Line7. See point 15 of this document. As gated tubes will be not used for Raman channels. The lidar will measure down to the ground.
22. P6476. Line 8. The sentence relating to the NDACC recommendation concerning the mentioned hybrid technique has been removed.
23. P6469. Line 8. Separate values concerning the dark count rate of the detector and the skylight background have been included in the text.
24. P6469. Line 22. Effectively, here we are just comparing the water vapor signal strength between old and new system. However as the old and new station are not

located at the same altitude (2100m difference), the altitude-square dependence is not the same which imply a difference even if both systems are the same.

25. P6470. Line 7. Simulations for the future system have been adjusted by the same factor 4 to better correspond with real-world expectations and results have been added in the text.

Scientific Comments concerning Figures.

Some figures have been removed and others have been modified using color as recommended. Concerning the figure 8, showing bimodal distribution in lower altitude, it has been indicated that the bimodal distribution has been observed up to ~4km using all water vapor profiles of the database. However we choose to show an example.

Technical corrections

Technical corrections have been considered.