*Author responses to Reviewer Comments for*

*Journal: AMT*
*Title: Development and testing of the Active Temperature, Ozone and Moisture Microwave Spectrometer (ATOMMS) cm and mm wavelength occultation instrument*
*Author(s): E.R. Kursinski et al.*
*MS No.: amt-2010-218*
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*Special Issue: Observing atmosphere and climate with occultation techniques - results from the OPAC 2010 Workshop*

We thank both reviewers for their time spent in reviewing our paper. There were many helpful comments and suggestions. Addressing these issues has improved our paper. There is a separate response to each of comments. In each case, the original comment is provided followed by our response. Many figures were redone based on the reviewer's comments. These figures and their captions are included at the end of this document.

## ***Responses to Reviewer 1***

### ***Reviewer 1, General comments***

*1) The 22 GHz measurements along the 5,5 km path are obviously worse than those at 183 GHz along 800m. Many instrumental questions, as well as they are stated correctly by the authors in sec. 3.3, leave some open doubts in the measurement interpretations together to some big atmospheric questions like scintillation and turbulence effects (see. "Impact of tropospheric scintillation in the Ku/K bands on the communications between two LEO satellites in a radio occultation" by E. Martini et al., on Geoscience and Remote Sensing, IEEE Transactions on , Volume: 44 Issue: 8) that authors should be accounting for in measurement interpretation.*

To estimate the effects of turbulence, we use a Kolmogorov 3D turbulence spectrum in eq. (8) of Martini et al. (or equivalent expressions like Wheelon, D. A. (2003), Electromagnetic Scintillation, II. Weak Scattering, Cambridge Editorial) with a signal frequency of 183 GHz along a 840 m horizontal path. We can estimate the $C_n^2$ from the local variability of the humidity field to be in the range $C_n^2$ = 5e-15 $m^{-2/3}$ to 1e-14 $m^{-2/3}$. The resulting predicted standard deviation of the log-amplitude fluctuations due to turbulence is 0.23% and 0.33% respectively. The higher of these values is comparable to the residual amplitude discrepancies we see in Figure 5. Therefore a good part of the residuals in Figure 5 could be due to turbulence.

For the 5.4 km path, we assume a value for $C_n^2$ of 2.5e-15 $m^{-2/3}$ based on subsequent measurements we made in experiments in summer 2011 by which time we have achieved substantial improvement in the performance of the 22 GHz system. (These will be published in another paper). The resulting predicted log-Amplitude variations are 0.27%. These variations are much smaller than the variations we see between the 22.6 GHz and 23.5 GHz channels shown in Figure 10. Therefore the observed variations are not due to turbulence.

*2) There are many calibration and instrumental open issues that are clearly stated by the authors along the papers besides problems of comparison among complete different measurement instruments and methods, especially for the 22 GHz experiment cases. Therefore some statements about better or worst reliability of the propagation models seem to be too much risky. I suggest to give some more details on the reliability of the amplitude ATOMMS measurements.*

We agree that the 22 GHz data described in this paper is not good enough to draw any conclusions about the relative accuracy of propagation models. The following changes to the text are suggested:

Page 4679, line7 through the remainder of the paragraph be changed to
"A comparison of the results in Fig. 10a and b reveals that the derived 22.6 GHz and 23.5 GHz changes in specific humidity agree better with each other when the AM 6.2 model is used compared with MPM93. This suggests that the AM model may better characterize the 22 GHz absorption line. As we improve the ATOMMS 22 GHz system and acquire more data, we expect to be able to make definitive statements about the accuracy of microwave propagation models near the 22 GHz absorption line."

*3) Can you give some more technical parameters of the transmitter and the receiver (i.e. some antenna parameters, beamwidth, transmitting and receiving gain, transmitter power, kind of frequency generator and amplifier)?*

Not here. We will do this in a separate paper on this subject which is in preparation.

*4) How much is the sensitivity and the resolution of the receiver for both 183 and 22 GHz bands? How much is the amplitude measurement accuracy?*

According to Figure 5, the 183 GHz amplitude error is 0.3% 1 sigma. This includes the effects of the instrument and scintillations due to turbulence.

The results presented here represent the first effort to use the 22 GHz. The 1 sigma level of amplitude noise for the 22.6 and 23.5 GHz channels was approximately 1%. This translates to errors in specific humidity of 0.6 g/kg which corresponds to fractional specific humidity errors of approximately 7%. The small optical depth at these wavelengths over a 5.4 km path causes the 1% amplitude error to result in a 7% specific humidity error.

We have improved the 22 GHz performance over the past year so that all 8 channels are working with better stability.

5 )183-187;200 GHz experiment. You have one amplitude measurement sample for each second (so about 1Hz of noise band), 27 amplitude measurements for a complete amplitude spectrum in 27 sec and a repetition time of 224 sec.
You compute a single absorption line fit each 224 sec and, correctly, Fig. 7 shows about 18 markers per hour. Do you account for the amplitude measurement accuracy in the fitting procedure?

We compute a fit to the ratio of the absorption lines and thus estimate the change in water vapor content relative to the reference time. The amplitude ratio fit is performed for 27 discrete, measured frequencies over the range from 183.60 to 187.50 GHz in steps of 0.15 GHz. The amplitude ratios are all given equal weight in the fitting procedure. For this experiment we do not have enough information to be able to assign differential measurement uncertainty based the signal frequencies, however, this is something that we are planning to estimate and include in future retrievals.

6) for the 180-200 GHz experiment you use the partial pressure e for the water vapour while for the 22 GHz you use the specific humidity q. I suggest to uniform the measurement units.

We will attempt to change all references to vapor pressure in Section 2 of the paper to specific humidity. Changes to the text are indicated below. This request also requires the replacement of figure 4 and figure 7, which will be separately uploaded.

| | |
|---|---|
| Page 4671, | line 6, equation 3, change "e" to "q" |
| | Line 7, column 1, "where q is the specific humidity" |
| Page 4672, | line 6, equation 4, change all "e" to "q" |
| | Line 12, equation 5, change all "e" to "q" |
| | Line 17, equation 6, change all "e" to "q" |
| | Line 18, near end of line, change "e(t)" to "q(t)" |
| | Line 20, middle of line, change "$e(t_0)$" to "$q(t_0)$" |
| | Line 21, equation 7, change all "e" to "q" |
| | Line 24, equation 8, change all "e" to "q" |
| Page 4673, | line 11, change "water vapor pressure" to "specific humidity" |
| | line 15, 16, change "vapor pressure" to "specific humidity" |
| | line 16, change "4.15 mb" to "2.80 g $kg^{-1}$" |
| | line 16, change "vapor pressure" to "specific humidity" |
| | line 16, change "6.38 mb" to "4.34 g $kg^{-1}$" |
| | line 20, equation 9, change all "e" to "q" |
| | line 24,25, change "water vapor" to "specific humidity" |
| Page 4674, | lines 9, 12, 19, and 20, change "$e_{max} – e_{norm}$" to "$q_{max} – q_{norm}$" |
| | Line 9, change "water vapor" to "specific humidity" |
| | Line 20,21, change 2.23 mb to 1.51 g $kg^{-1}$ |
| | Line 23, change 2.15 mb to 1.45 g $kg^{-1}$ |
| Page 4675, | line 1, column 1 to end of that sentence, change to "for changes in specific humidity." |
| | Line 2, change "water vapor pressure" to "specific humidity" |
| Page 4700, | Caption for figure 4, change "partial pressure" to "specific humidity" |
| Page 4702, | Caption for figure 6, change "$e_{max} – e_{norm}$" to "$q_{max} – q_{norm}$" |
| Page 4703, | Caption for figure 7, There are 3 places where "vapor pressure" needs to be changed to "specific humidity" |

*7) the 197 GHz cloud estimation in sec. 4.3 is completely unreliable due to the huge uncertainty and approximation of 197 GHz rain computation besides the uncertainty in 197 GHz measurements. Too much errors source are present in the proposed procedure for the estimation of 197 GHz cloud: the dimension and the position of the volume radar cells with respect the propagation path, ground clutter, the very know conversion error of the Marshall palmer among R Z and K (why 300 and 1.4 ??). Therefore the section 4.3 without at least a rough error quantification is not acceptable.*

While we agree with the reviewer on overall philosophy that error bars are good, to call the results, "completely unreliable", is an overstatement of the uncertainty as we show below. Below we discuss the changes we made in the analysis and description.

The rationale for the exponential distribution now reads

"The attenuation of the 197 GHz observations and the radar backscatter depend on the particle size distribution. Researchers have described the size distribution of raindrops by a number of analytic expressions including the exponential, gamma and log-normal distributions. Williams and Gage (2009) examined seven distributions consisting of 2 versions of exponential including the original distribution of Marshall and Palmer (1948), 4 versions of the gamma distribution and one log-normal distribution. They concluded that other than the original Marshall Palmer distribution which was systematically in error for the tropical conditions they were considering, the other 6 distribution were essentially equivalent in terms of accuracy. Therefore, we choose to use an exponential size distribution for raindrops as defined in (16) to provide a simple, realistic size distribution with only two degrees of freedom, $n_0$ ($m^{-3}$), the number density of droplets, and $D_0$ (m), the mean droplet diameter.

$$n(D) = \frac{n_0}{D_0} e^{-\frac{D}{D_0}} \quad (16)”$$

The reference is

Williams, C. R., and K. S. Gage (2009), Raindrop size distribution variability estimated using ensemble statistics, Annales Geophysicae., 27, 555–567, 2009, www.ann-geophys.net/27/555/2009/.

Regarding the Z(R) relation, the text in Section 4.3 now reads

**“4.3 Estimating rain mass and opacity from the weather radar**

The radar provides a time sequence of one variable, *Z*. To obtain a time sequence of the two variables, $n_0$ and $D_0$, in the exponential distribution from the radar data, we need at least one more constraint. As that constraint, we use the radar-rain rate relation, $Z=aR^b$. The obvious *a* and *b* constants are the U.S. National Weather Service’s standard conversion between the reflected power measured by a WSR-88D radar and rainrate given in (18)

$$Z = 300\, R^{1.4} \quad (18)$$

where *Z* is in $mm^6/m^3$ and rain, *R*, is in mm/hr (Fulton et al., 1998). Researchers have found some variation in these constants with conditions. Morin et al. (2005) used $Z = 655R^{1.4}$ in the Walnut Gulch region in Southeast Arizona, arguing that large evaporation of the droplets was responsible for much larger value of *a*. Because our ATOMMS measurements were taken approximately 1,500 meters higher than the rain gauge measurements in Walnut Gulch and close to cloud base such that evaporation was much smaller than the conditions examined by Morin et al., we use the standard relation (18). We have also examined different constants. Ochou et al (2011) found the variations in the $Z=aR^b$ relation were primarily in the constant, *a*, finding 1 sigma variations in *a* of 30% and *b* of 7%. We therefore also included +/-30% variations in *a* specifically with *a* = 231 and 390.

To determine how $n_0$ and $D_0$ vary with *Z* for a given $Z=aR^b$ relation, we calculate the reflectivity from (17) and the rain rate as a function of $n_0$ and $D_0$ according to (19)

$$R = \frac{6\pi}{10000} \int_0^{D_{max}} N(D) D^3 v(D) dD \quad (19)$$

where *v* is the droplet terminal fall speed determined from laboratory measurements and approximated as

$$v_{fall\,speed}(D) = \left(9.65 - 10.3 exp(-0.6D)\right) \left(\frac{\rho}{\rho_0}\right)^{-0.4} \quad (20)$$

where ρ is the air density in the current conditions and $\rho_0$ is the air density at sea level (Gunn and Kinzer, 1949; Atlas et al., 1973). From these we determine the relation between $n_0$ and $D_0$ that results in $Z = aR^{1.4}$ for $a$ = 231, 300 and 390."

The 5 new references are

Atlas, D., Srivastava, R. S., and Sekhon, R. S. (1973) Doppler radar characteristics of precipitation at vertical incidence, *Rev. Geophys*.,11, 1–35, 1973.

Fulton, R. A., J. P. Breidenbach, D. J. Seo, and D. A. Miller (1998): TheWSR-88D rainfall algorithm. *Wea. Forecasting*, **13,** 377–395.

Gunn, R., and Kinzer, G. D. (1949), The terminal velocity of fall for water droplets in stagnant air, *J. Meteor*., **6**, 243–248.

Morin, E., R. A. Maddox, D. C. Goodrich and S. Sorooshian (2005), Radar *Z–R* Relationship for Summer Monsoon Storms in Arizona, *Weather and Forecasting*, **20**, 672-679.

Ochou, A. D., E-P Zahiri, B Bamba, M. Koffi (2011), Understanding the Variability of *Z-R* Relationships Caused by Natural Variations in Raindrop Size Distributions (DSD): Implication of Drop Size and Number, *Atmospheric and Climate Sciences,* 2011, 1, 147-164, doi:10.4236/acs.2011.13017 Published Online July 2011 (http://www.SciRP.org/journal/acs)

After line 6 on page 4684, we inserted the following text

"As noted, we considered 3 different values of the constant, *a,* in the $Z = aR^{1.4}$ equation. Of the 6 possible combinations of the three *a* values and the two elevation angles, 0.88° and 1.28°, Figure 17 shows the three combinations that provide the most reasonable results and provide some indication of the uncertainty. Based on (1) the nominal $Z(R)$ relation of Fulton et al., (2) the $a$=655 result Morin et al. found under conditions of large evaporation and (3) the close proximity of the ATOMMS beam to cloud base, we anticipate a best value of $a$ = 300 or perhaps a bit less. The similarity of the measured 197 GHz and visible wavelength optical depths at the onset indicates the rapid increase in opacity is due to rain such that the radar-based optical depths at 197 GHz should be close to the measured 197 GHz values. However, when optical depths at the storm onset are estimated using $a$=390, they were too small at both 0.88° and 128° indicating the $a$=390 value is too large. The results for $a$=300 at 0.88° elevation angle and $a$=231 at 1.28° elevation angle were very similar to one another and to the 197 GHz measurements. The $a$=300, 1.28° elevation opacities were a bit smaller but still credible. The peak rain opacity during the onset for $a$= 231 and an elevation of 0.88° was 2.2 which is 50% larger than the measured 197 GHz opacity and therefore too large. We suspect the best value of $a$ is somewhat smaller than 300 but not as small as 231. These results further suggest that a systematic evaluation of the $Z(R)$ relation could be accomplished using a combination of radar, ATOMMS, visible wavelength measurements and disdrometers."

Our refined approach to converting the rain to 197 GHz opacity resulted in a new Figure 17 which is shown here.

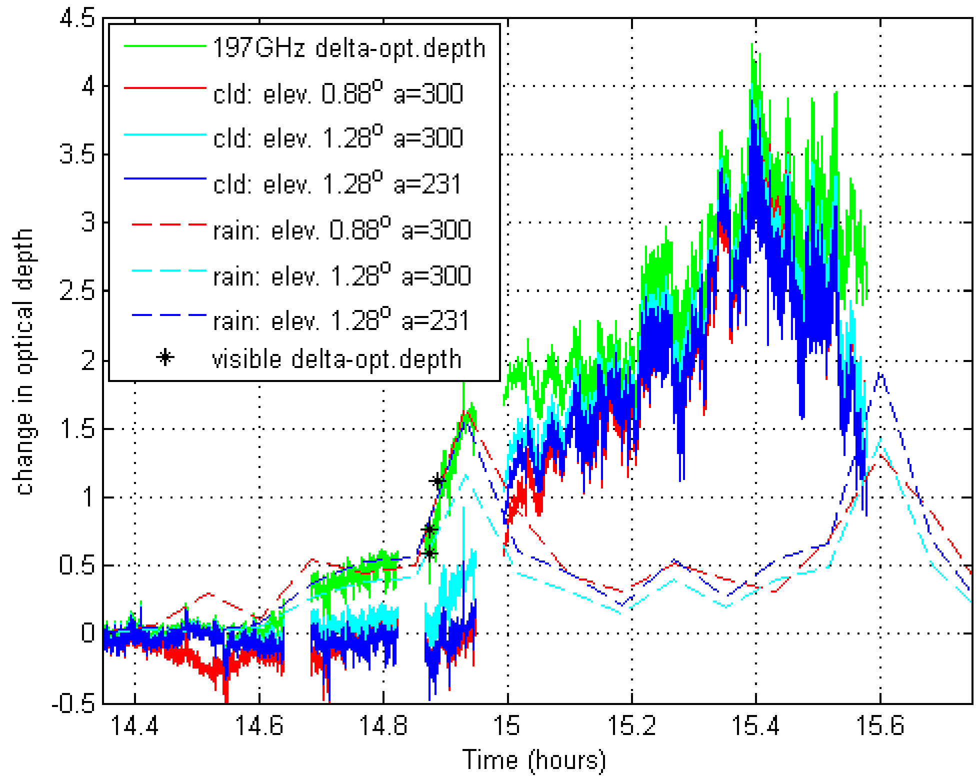



Figure 17. Plots of the change in optical depth during the storm. Green is the optical depth measured at 197 GHz. The optical depths due to rain estimated from the WSR-88 radar are dashed lines at 0.88° and 1.28° elevations, The estimated optical depths due to clouds are shown as solid red, cyan and blue lines which are calculated as the difference between the 197 GHz measurements and that due to rain. The asterisks are changes in optical depth at visible wavelengths derived from photographic images taken during the storm. See text for details.

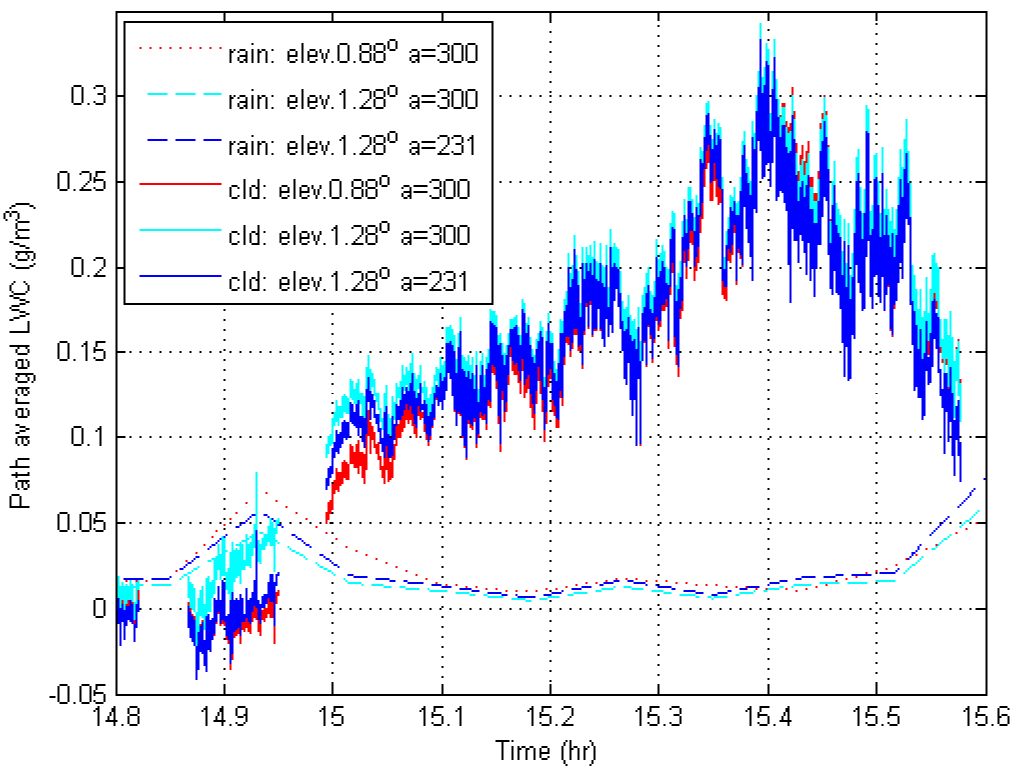
At the end of Section 4.6.2, we added the following text

"This rapid onset period therefore gives us the additional constraint that the radar-derived rain opacity at 197 GHz must equal the measured 197 GHz opacity at the storm onset. We varied the constant, *a*, in the *Z*(*R*) relation by +/- 30%. In converting the radar measurements at 0.88° to 197 GHz opacity, the nominal value of *a* = 300 yields a rapid rise in opacity very similar to the measured 183 GHz opacity. The conversion of radar measurements at 1.28° to 197 GHz opacity is closest to the measured 197 GHz opacity using *a* = 231. The 0.88° radar measurements could be too high because of ground clutter. The 1.28° measurements could be an underestimate because the measurements are a few hundred meters above the ATOMMS signal path. Because the ATOMMS measurements are so close to cloud base at the storm onset, therefore with very little evaporation, one could imagine that the best estimate of *a* is somewhat smaller than the NWS nominal value of 300. This suggests that visible/near-IR measurements simultaneous with ATOMMS and weather radar measurements can be used to assess and refine the *Z*(*R*) relation under different conditions. Further combining these with a drop distrometer would provide further information on the drop size distribution and its evolution as the drops evaporate as they fall from the cloud to the ground.

Furthermore, a fully functioning ATOMMS system would provide sufficient spectral information to simultaneously measure the variations in water vapor and condensed water and perhaps temperature (via lineshape) to measure conversion between the phases during the rain/virga. This would have been useful here because as rain fell into the subsaturated clear air below cloud base, some evaporated, cooling the air and lowering the saturation vapor pressure of the air which could then cause some of the evaporated moisture to recondense into cloud droplets. This is qualitatively consistent with the rapid decrease in temperature and increase in dew point measured by the hygrometer as the storm approached."

*8) for the same reasons reported in the previous comment also quantitative results of LWC is sec 4.5 are unreliable.*

See response to Question 7. We would disagree that the LWC is unreliable. The new LWC Figure is shown below. There is some uncertainty in the rain LWC and therefore the cloud LWC near the storm onset. There is little uncertainty in the cloud LWC after 15:10.




*9) Authors use amplitude ratio as base measurement parameter. I suggest to try to consider also the normalized amplitude ratio parameter (i.e. the sensitivity approach as analyzed in "Normalized Differential Spectral Attenuation (NDSA) measurements between two LEO satellites: performance analysis in the Ku/K bands" by F.Cuccoli et al. on Geoscience and Remote Sensing, IEEE Transactions on , Volume: 46 Issue: 8). This should offer a more "cleaned" and low-sensible parameter to be used in estimating the water vapour content along propagation paths.*

We have decided to add the following sentence and references. This will be added as the second sentence in the paragraph beginning on line 2 of page 4671:

“The merits of a microwave differential absorption approach for the retrieval water vapor are also discussed in Kursinski et al. (2002) and Cuccoli and Facheris (2006).”

The following will be added to the reference list:

Cuccoli, F.; Facheris, L.: Normalized differential spectral attenuation (NDSA): a novel approach to estimate atmospheric water vapor along a LEO-LEO satellite link in the ku/K bands, *IEEE T.* Geosci. Remote Sens., 44(6), 1493-1503, 2006. doi: 10.1109/TGRS.2006.870438.

**Reviewer 1, Specific Questions**

*1) Pag. 4671, line 6, eq. (3)*

*What do you really mean with e, T and P in F(e,T,P)? the optical depth should be* function of the whole temperature and pressure profiles along the propagation path. Therefore e, T and P should be like e(r), T(r) and P(r) with r the position along the path. Please clarify this aspect.

The quantities q, P, and T represent path averaged values. To make this clear, we suggest adding these two sentences.

Page 4671, line 8, after the period in that line, add the following, “Note that q, T, and P in Eq. (3) represent path-averaged values, which is consistent with our subsequent data processing. For precise calculations, the optical depth is a path-integrated quantity and explicitly depends of the variation of q, T, and P along the propagation path.”

*2) Pag. 4671, line 25*

*In order to well understand the multipath effects, are they due to ground reflection? Or atmospheric multipath?*

The multipath we are referring to is a reflection or scattering off structures generally near the receiver.

*3) Pag. 4672, line 1-2 “By holding the geometry fixed, the multipath effect remains constant and common to all measurements: : :”. Are you sure about this? The 2 frequencies are separated (in sec. 2.2 you have about 200 GHz as ref and 183-187 GHz the others) so the propagation multipath can be different in space and change in time. This comment is connected to the previous one.*

We have written the following explanation of the multipath effect for the reviewers**. We ask the reviewers whether we should add this write-up as an appendix to the paper?**

Multipath refers to the situation where the transmitted signal arrives at the receiver via multiple paths. The received signal is the sum of these multiple signals. When the geometry is held rigidly fixed, the multipath effect will remain constant and cancel when the amplitude ratio is formed, with a negligible residual error as we show below. To understand this, it is easiest to consider two signal paths and then generalize to more multipath signals.

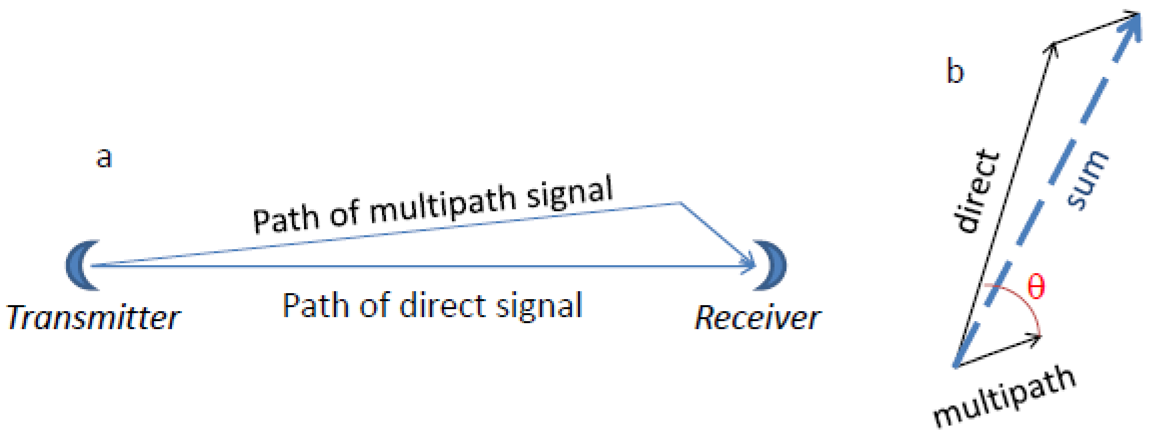



Figure A1. a. Multipath geometry. b. Vector sum of direct and multipath signals. The lengths of the vectors of the direct, multipath and summed signals represent the amplitudes, $A_1$, $A_2$ and $A_{12}$ respectively. $\theta$ is the phase angle between the direct and multipath signals

In the multipath geometry shown in Figure A1a, the signal from the transmitter arrives at the receiver via two different paths, the direct path and a second path along which the signal has scattered off of a surface to the receiver. The amplitude of the sum of the two received signals, $A_{12}$, with the subscript 1 for the direct and 2 for the multipath signal, is

$$A_{12}(t) = A_1(t)\sin(\omega t - kL_1 + \phi_1) + A_2(t)\sin(\omega t - kL_2 + \phi_2) \quad \text{(A1)}$$

where $A$ is the received signal amplitude, $\omega$ is the frequency of the signal, $t$ is time, $k$ is the wavenumber, $\phi$ is a phase offset and $L$ is the optical path length defined as

$$L_1 = L_{\text{geom1}}(1+c/v) = L_{\text{geom1}}(1+n(\text{t}))$$

$$L_2 = L_{\text{geom2}}(1+c/v) = L_{\text{geom2}}(1+n(\text{t}))$$

where $L_{\text{geom}}$ is the geometric path length, $c$ is the speed of light in a vacuum, $v$ is the speed of light in the medium and $n$ is the index of refraction. $n$ varies slightly with time because of changes in air density and water vapor concentration. As shown in Figure A1a, the geometric length of the multipath path, $L_{\text{geom2}}$, is slightly larger than the that of the direct signal length such that

$$L_{\text{geom2}} = L_{\text{geom1}} + \Delta L$$

As shown in Figure A1b, the time varying amplitude, $A_{12}(t)$, is the sum of two complex phasers of different amplitudes that are oscillating or rotating at almost the same rate. The only difference in angular rate is due to the subtle changes in differential path delay associated with changes in the index of refraction with time

The amplitude of the vector sum of the direct and multipath signals is given by the law of cosines

$$A_{12}(t) = [A_1(t)^2 + A_2(t)^2 + 2A_1A_2\, cos\theta]^{1/2} \quad \text{(A2)}$$

where $\theta$ is the angle between the two vectors as shown in Figure A1b. The two amplitudes are affected almost identically by absorption because their paths are very close. The optical depths along the two paths are

$$\tau_1 = \int_{path1} k_{ext}\, dL \qquad\qquad \tau_2 = \int_{path2} k_{ext}\, dL$$

such that

$$A_1 = A_{01}\exp(-\tau_1/2) \qquad\qquad A_2 = A_{02}\exp(-\tau_2/2) = R\, A_{01}\exp(-\tau_2/2) \tag{A3}$$

where $A_{10}$ and $A_{20}$ are the signal amplitudes that would have been received in the absence of absorption, $R$ is a coefficient that accounts for the changes in amplitude associated with the multipath scattering and any difference in antenna gain experienced by the multipath signal relative to the direct signal.

The presence of multipath will not adversely affect the derived optical depth ***if*** the variations in $A_{12}$(t) capture the time variations in the absorption along the path according to (2) such that

$$2\, ln\left(\frac{A_{12}(t_0)}{A_{12}(t)}\right) = \tau(t) - \tau(t_0) \tag{A4}$$

To the extent that the individual amplitudes, $A_1$ and $A_2$, both track the time varying optical depth and the phase angle, $\theta$, between the two signals remains constant, then (A4) will be satisfied. Forming the amplitude ratio in (A4) and substituting (A3) in yields

$$\frac{A_{12}(t_0)}{A_{12}(t)} = \frac{[A_1(t_0)^2 + A_2(t_0)^2 + 2A_1(t_0)A_2(t_0)\, cos\theta(t_0)]^{1/2}}{[A_1(t)^2 + A_2(t)^2 + 2A_1(t)A_2(t)\, cos\theta(t)]^{1/2}}$$

$$\frac{A_{12}(t_0)}{A_{12}(t)} = \frac{A_1(t_0)[1+R^2\, exp(-\Delta\tau(t_0))+2R\, exp(-\Delta\tau(t_0)/2)\, cos\theta(t_0)]^{1/2}}{A_1(t)[1+R^2\, exp(-\Delta\tau(t))+2R\, exp(-\Delta\tau(t)/2)\, cos\theta(t)]^{1/2}} \tag{A5}$$

where $\Delta\tau$ is $\tau_2 - \tau_1$. If the ratio of the bracketed terms is 1, there is no error. We assume the geometry is fixed and the coefficient, $R$, does not change with time. Since $R$ is typically no larger than 0.1, the $2R$ term is generally much larger than the $R^2$ term. We can therefore approximate the ratio of the bracketed terms as

$$\frac{[1 + R\, exp(-\Delta\tau(t_0)/2)\, cos\theta(t_0)]}{[1 + R\, exp(-\Delta\tau(t)/2)\, cos\theta(t)]}$$

$$\cong 1 + R\left[exp\left(-\frac{\Delta\tau(t_0)}{2}\right)\, cos\theta(t_0) - exp\left(-\frac{\Delta\tau(t)}{2}\right)\, cos\theta(t)\right] = 1 + \varepsilon_{Aratio} \tag{A6}$$

where $\varepsilon_{Aratio}$ is the error in the amplitude ratio in (A4). The two terms that can change with time and cause this ratio to be non-unity are the phase angle, $\theta$, and the optical depth difference, $\Delta\tau$. The optical depth difference is the extinction coefficient times the difference between the multipath and direct path lengths,

$$\Delta\tau = k_{ext}\Delta L$$

which is quite small. The temporal change in $\Delta\tau$ is

$$\Delta\tau(t) - \Delta\tau(t_0) = \Delta k_{ext}\, \Delta L.$$

caused by changes in temperature, and pressure and amount of water vapor in the air. We estimate $\Delta k_{ext}$ from Figure 6. The change in optical depth associated with the measured amplitude ratio of 0.55 for a frequency of 183.6 GHz in Figure 6 is 1.2. The corresponding change in the average extinction coefficient, $\Delta k_{ext}$, along the 800 m path length is 0.0015 $m^{-1}$. The likely source of multipath is the overhanging beam in Figure 3 that sits approximately 1 meter in front of and 1 meter above the receiver. The path of the signal scattering off that overhanging beam will be longer than the direct beam by about $\Delta L$ = 0.4 m. The resulting $\Delta k_{ext}$ $\Delta L/2$ term in (A6) is 0.0003. From (A6), the largest error in amplitude occurs when $\theta$=0 or $180^o$. Using a value of $R$ = 0.1 which is representative of large multipath we have observed, the resulting amplitude ratio error in (A6) is 0.003% and optical depth error of 6e-5 which is insignificant.

We now turn to the impact of variations in the phase angle, $\theta$, which can result in large optical depth errors. For example, a change in $\Delta L$ of only 0.75 mm rotates $\theta$ by 180 degrees, changing the received amplitude by 2 $R$ which could be as large as 0.2. This amplitude variation would be interpreted via (A4) as an erroneous change in optical depth of 0.36. If the true optical depth were 1 then the error in the estimated water vapor concentration would be 36%. Thus, controlling this potentially large error requires that the observing geometry be held quite rigidly fixed.

Because of the dependence on the index of refraction, small errors can still result when the geometry is fixed. From (A1) and (A2), the phase difference, $\theta$, between the direct and multipath signals is

$$\theta(t) = k\,[L_1 - L_2] + \phi_1 - \phi_2 = k\,[L_{\text{geom1}}\{1+n(\text{t})\} - L_{\text{geom2}}\,\{1+n(\text{t})\}] + \phi_1 - \phi_2$$

Given a fixed geometry, the change of the phase difference with time that causes an amplitude error is

$$\Delta\theta(t,t_0) = \theta(t) - \theta(t_0) = k\,(L_{\text{geom1}} - L_{\text{geom2}})\,[n(t) - n(t_0)] = -k\,\Delta L\,\Delta n(t,t_0) \qquad \text{(A7)}$$

(A7) is the change of the index of refraction integrated along the extra path length of the multipath signal path relative to the direct signal. This term is the only portion of the phase difference that is not constant when the geometry of the instruments and multipath surfaces are fixed.

The change in the index of refraction associated with measured 2 mb change in water vapor in Figure 7 is about 1e-5. The resulting change in the phase angle via (A7) is about 0.017 radians or 1 degree. Using the approximation of $\exp(-\Delta\tau/2)$ as 1, the error in the amplitude ratio in (A6) is given approximately as

$$\begin{aligned}\varepsilon_{Aratio} &\cong R[\cos\theta(t_0) - \cos\theta(t)] = R[\cos\theta(t_0) - \cos\{\theta(t_0) + \Delta\theta(t,t_0)\}] \\ &\cong -R[\Delta\theta(t,t_0)\,\sin\theta(t_0)]\end{aligned} \qquad \text{(A8)}$$

Clearly the largest error in (A8) occurs when $\theta(t_0) = 90^o$. With $R$ = 0.1, the largest resulting change in amplitude would be 3e-5 with a corresponding optical depth error of 6e-5 which is negligible.

Given a fixed geometry, how large can this phase angle error be? The change in water vapor can be 7 mb as in Figure 9. The antenna response limits the range of angles at which the multipath signal arrives at the receiver which limits the geometry of significant multipath. The largest possible multipath amplitudes occur from specular reflections off extended surfaces in the main lobe of the antenna pattern, a geometry that we design our experimental configurations to avoid. Regarding scattering from complex surfaces, the solid angle of the scattered signal subtended by the receiving antenna decreases as the square of the scatterer-to-receiver distance such that the amplitude of the received multipath signal and the factor, $R$, decrease approximately inversely with that distance. Thus, in general, as the multipath scatterer to receiver distance increases, $\Delta L$ increases and $R$ decreases approximately linearly with the distance so the resulting changes in $\Delta L$ and $R$ in (A8) cancel approximately. Thus, the impact of changes in the phase angle will generally be less than 0.1% and can be ignored.

4) Pag 4673, line 20, eq(9)
Do you use A symbol both for the signal amplitude at a single frequency as in eq (1) and for the amplitude ratio? I understand A(f,e,P,T) the signal amplitude received but A(f1;f2;e,P,T) is A(f1,e,P,T)/ A(f2,e,P,T)?? and A(f:emax-enorm, P,T) is amplitude ratio / amplitude ratio ?? It is confusing. I suggest to use different symbols for the amplitude ratio (ï ¸ AˇDA ??) and the quantity of eq.(9)

We suggest the following changes to help clarify the meaning of equation (9).

Page 4673, line 20, Equation 9. The quantity of the right side of the equation be changed to

$A_{SR}(f;f_{ref};q_{max}-q_{norm},P,T)$

Where A was changed to $A_{SR}$ to indicate spectrum ratio and distinguish it from the A symbols used on the left hand side of equation (9) and $f_{ref}$ was added to indicate that the result depends on the value of the reference frequency.
Page 4673, line 21 under equation (9). Change the text to be
"where $A_{SR}$ is the ratio of two spectra observed at different times and $t_1$ represents the time when the maximum specific humidity was observed. We made the approximations of replacing $P(t_1)$, $P(t_0)$, $T(t_1)$, and $T(t_0)$ with P and T the path averaged pressure and temperature.
Page 4674, line 13, column 12, add $A_{SR}$ after the word "spectra" to be
"… spectra, $A_{SR}$, obtained …"

5) Pag. 4675, line 10-14 "For various: : :.. : : :.. 182-205 Ghz." Are you sure about the position of this sentence? Wireless and cell phone make really disturb in 183-200 GHz Range??

The interference was associated with IF frequencies between 1 and 5 GHz. To clarify this, add the following text:

Page 4675, line 10, change first sentence to be

"During these measurements we encountered some interference when the receiver intermediate frequencies were below 5 GHz that did not allow us to obtain good measurements over the entire ATOMMS frequency range for this test."

6) Pag. 4675, line 14-18 "We also note: : :. ..intervals". I suggest to move this part at the beginning of section 2.2 at line 11 of pag 4673.

We agree with this rearrangement of the text.

Move text Page 4675, lines 14-18, "We also note that … shorter intervals" to page 4673, and append it to the paragraph that ends on line 10.

7) There was no specific comment numbered as 7

8) pag 4675, line 25 For me it's not clear how the red curve of Fig. 5 is computed. However the RMS error in true amplitude ratio measurements should be a combination

among instrumental errors (i.e. noise at the receiver) and those due to parameter sensitivity in estimation methods like those of Fig 5. If you can try to better explain these questions.

To clarify the reviewer's comments and questions, we propose the following changes to the text.

Page 4675, line 19. Eliminate the current paragraph and Change the text of the paragraph to be …

"Figure 5 was constructed to show the sensitivity of the amplitude spectral ratio, $A_{SR}$, to changes in the path averaged pressure along the propagation path. The blue curve shows how the RMS of the differences at the 27 measured ATOMMS frequencies across the range from 183.60 to 187.50 GHz grows as the pressure is moved from its nominal value based on AM model calculations. The red curve shows the pressure sensitivity of the ATOMMS measurements via comparison with the AM model lineshape. The minimum discrepancy occurs about 0.1 mb above the measured pressure. Errors of about 2 mb are distinguishable for this particular set of observations, which corresponds to a fractional pressure uncertainty of about 0.2%. If we assume that the blue curve represents the error due to pressure and that the differences between the blue and red curves are mainly due to ATOMMS measurement errors and turbulence, Fig. 5 also implies that the maximum RMS errors in the measured ATOMMS amplitude spectral ratios defined in Eq. (9) are about 0.3%. This value is obtained by noting that at any given pressure difference in Fig. 5, the value of the red curve can be obtained by adding roughly a 0.3% error to the blue curve in a root sum squares sense. This ignores possible errors in the spectroscopic model."

Page 4701, caption for figure 5, In the last line "0.5 mb below" needs to be changed to "0.1 mb above" to make it consistent with the earlier text. This error was discovered when answering this reviewer comment.

9) pag 4678, line 14
You assume T and P constant along the 5.4 km path. Is this approximation too much strong? Fig. 12 shows 10_ temperature variation. Is this compatible with your assumption?

Figure 12 corresponds with the data described in section 4, while the question refers to the approximations made when processing the data described in section 3. There is no figure of the measured temperature and pressure in the vicinity of the experiment described in section 3. However, to clarify the observed variations as described in the text, we suggest placing ± symbol in front of the described variations in pressure and temperature. In addition add the qualifier "relative to other errors" to the end of the last sentence of section 2.2.

Page 4678, line 22, should read "… observed variations of ± 1.2 mb and ± 2°C respectively …"
Page 4678, line 24, add "relative to other errors" to the end of the sentence.

10) pag. 4679 line 7 I don't understand the reasons to assess that AM is better than MPM. You say that this happens because looking FIG. 10 the differences in water vapour estimations between AM 23.5 and AM 22.6 are lower than those MPM. But looking table 1 the greater differences are between AM coefficients (0.0346 and 0.0285) instead of MPM (0.0312 0.0287). Moreover reading the following lines (11-19 lines, pag 4679, about calibrations issues the question about which model is better remains unclear. Please clarify this aspect.

The conclusions about which propagation model is better based on the data presented in section 3 have been toned down (see response to general comment 2 above). We do not understand the reviewer's points about the differences in coefficients between AM and MPM, which indicate differences in the lineshape representation in the models. For the conditions observed during this event, the AM coefficients predict a more narrow lineshape. In the ideal case, we would obtain the same estimate for the change in specific humidity with time for both the 22.6 and 23.5 GHz measurements. The point is that when using the AM coefficients, the estimates for the change in specific humidity for the 22.6 and 23.5 GHz observations are more consistent with each other compared with using the MPM coefficients. However, given the level of uncertainty of these early measurements, we agree that a firm conclusion about which propagation model is better is not warranted here.

11) pag 4681, line 7. The azimuth resolution in meter is non correct. Or you give the azimuth radar angular resolution (i.e. the antenna beam width) or you give the radar range distance where you have 500 m of tangent distance.

The RADAR azimuthal resolution is 0.5°. The distance from the RADAR to the ATOMMS instruments was about 60 km. At that range, the azimuthal resolution is about 500 meters. To be clear, the following changes are proposed:

Page 4681, line 7. Remove "500 meter azimuthal resolution" and replace with
"0.5° azimuthal resolution at a distance of about 60 km from the ATOMMS instruments."

12) pag. 4681, line 15-22 and fig 14. If I understood well, the radar data you show is the reflectivity of the radar cells that intersect the vertical plane that contains the 5.4 km path at 0.88 (left) and 1.28 (right) elevation degrees. Why 65 data point? How do they correlated to the 500 m azimuth resolution (see previous comment)? Which is the radar distance?

In subsequent calculations it was important to account for the variation in RADAR reflectivity along the path between the two instruments rather than try to work with some path-averaged value. The original RADAR data (native pixel format) had to be first interpolated to a new (user friendly) grid to allow us to sample the variation along the path. While the choice of 65 points between the instruments was somewhat arbitrary, it is near the spatial resolution of the original data pixels, and we found our subsequent calculations were not very sensitive to number of points so long as the sampling does not get too coarse.

*13) pag 4686, line 1 I'm not agree that ": : :.provides a strong validation: : :." . Without any discussion about the measurement uncertainty and the conversion errors, I suggest to use something like "there is a qualitative similarity among the different estimation procedures". (see general comment #7)*

When we originally looked at the 197 GHz measurements and implied changes in opacity, we were unsure of their veracity. As we noted in the text, when we found the simultaneous rapid increase in the opacity and the similar magnitude of the increase measured at visible and 197 GHz wavelengths combined with the simultaneous measurement of rain by the WSR radar, we realized that the simultaneous measurements provide a strong validation that the ATOMMS measurements are correct in timing and in magnitude. The combined results indicate that the increase in opacity at the storm's onset was due to rain, not clouds.

We don't think the replacement statement offered by the Reviewer adequately reflects the strength of these complimentary observations.

We can change the original statement to read "The agreement in the timing and magnitude also provides a good validation of the ATOMMS opacity measurements".

*14) Fig. 7. can you add the some error bars corresponding to the black markers? This request is referred to the general comment #2 and #3.*

The 0.3% amplitude discrepancy between the ATOMMS measurements and best fit solution of the AM model in Figure 5 converts to a specific humidity uncertainty of 0.01 g/kg. The resulting error bars on the figure would be smaller than the size of the asterisks.

We also note that the standard deviation of discrepancies between the ATOMMS measurements and the PAS hygrometer is 0.06 g/kg. This is about a 1.6% discrepancy relative to the mean specific humidity over the measurement period. We note that this is consistent with the 1-2% level of discrepancy expected between measurements of water vapor made at a point and along a 1 km average (e.g., Otarola et al., 2011). Therefore the discrepancies between the two measurements may be dominated by spatial variations in the water vapor field.

The bias between them is -0.013 g/kg.

Reference:

Otarola, A. C. , R. Querel, and F. Kerber, Precipitable Water Vapor: Considerations on the water vapor scale height, dry bias of the radiosonde humidity sensors, and spatial and temporal variability of the humidity field, arXiv:1103.3025v1 [astro-ph.IM], (Submitted on 15 Mar 2011)

*15 ) Fig. 15 – Which Z map do you use? Which Z-R relationship?*

See answers to general comments 7 and 8 above.

**Reviewer 1, Technical corrections**

1) pag 4676, line 2, 3, 11 16
Convert ft in m

Changes required:

Page 4676, line 2, change "(8250 ft) to "(2515 m)"
Line 3, change "(9030 ft)" to (2752 m)"
Line 11, change "7878 ft" to "2401 m"
Line 14, change "8573 ft" to "2613 m"
Line 16, change "8250 ft" to "2515 m"

2) Fig. 10. caption: shows instead of show

Agree. Page 4706, caption for figure 10, line 1, change "show" to "shows"

3) pag 4681, line 18-22 The sentence "While: : : surface" is not syntactically correct.

We are unsure of the syntax problem, unless the reviewer is questioning the use of commas, which could have been better utilized. We suggest the following change.

Page 4681, line 18, the sentence beginning with "While the … " be changed to

"While the 0.88 degree elevation scan is just above Mt. Lemmon and therefore closer to the actual volume sampled by ATOMMS, it clearly shows more ground clutter than the 1.28 degree elevation scan, which is a slightly higher above the surface."

4) Fig. 14 and 15. Add axis meaning and units

Many of the figures have been revised based on this and other comments. The updated figures will be attached at the end of this document.

5) pag 4685, Please define Q197 and Qvis in the text.

This has been addressed. The revised text that replaces the original text on page 4685 is included as a response to one questions posed by reviewer 2 below.

## ***Responses to Reviewer 2***

***Reviewer 2, General comments***

*1) It would be helpful to include some technical information (transmitter power, gains, etc.) on the instruments used for the experiment, since this information is interesting in view of the results. Also a graph showing the SNR would be interesting. What would be the minimum power needed to achieve reliable results?*

A separate paper on the ATOMMS instrument is in preparation

*2) I was wondering why slightly different procedures have been used to evaluate the high and low band measurements for water vapor resulting in water vapor pressure changes regarding the high band, and in specific humidity changes regarding the low band. Could you please comment on that? Comparison of the results would be easier if the results were harmonized.*

In response to this question as well as general comment 6 from reviewer 1, we have changed section 2 covering the 180 – 200 GHz experiment from using partial pressure (e) to specific humidity (q) to be consistent throughout. Additional comments provided below.

The High Band and Low Band systems are different. The High Band system presently has 2 tunable channels. The Low Band system consists of 8 fixed frequency channels. As a result of their differences, the data are processed differently.

The results from the Low Band system are not as good as those from the High Band system at the moment. Partially this is due to the instrument. Partially this is because the optical depth at 22 GHz are much smaller than those at 183 GHz, making them far more difficult to measure accurately along these relatively short path lengths.

In the August 20, 2010 results only the two center channels, 22.6 and 23.5 GHz performed well. Two channels were off entirely. The other four were on but their amplitudes were not very stable relative to the magnitude of the absorption signatures. Because the pressure and temperature conditions varied little over the observational period, we chose to simply calculate one extinction coefficient scale factor for the period. With only two channels, we cannot do the spectrum fits that we do near the 183 GHz line.

We are in the process of making (at least) one of the 8 Low Band channels tunable like the High Band system so that we can do spectral fits and examine the performance of the spectroscopic models.

*3) Some symbols used in the equations are not explained in the text. It would be useful to explain every symbol once in the text (also when their meaning seems to be obvious; detailed notes in the TC section).*

We agree and have added explanations in the updated version.

*4) I recommend use of the NIST guidelines when specifying ranges of values; e.g. use 22 GHz to 183 GHz (instead of 22 to 183 GHz, or 22-183 GHz) (detailed notes in the TC section).*

We have made these changes

*5) I recommend checking all equations and symbols as I assume a few errors have been introduced during typesetting by Copernicus (obvious errors are noted in the TC section).*

We agree and thank the Reviewer for finding a number of type setting errors that we missed.

***Reviewer 2, Specific comments (SC)***

*1) p. 4668, lines 15 to 17: It is not exactly clear what you mean with the last sentence ("Ground truth is much harder to establish"). Please clarify this.*

We added some text that further explains what we mean. The text now reads "In the rooftop measurements, we achieved the very good agreement shown in Figure 7 between ATOMMS and point measurements by a nearby hygrometer. The agreement implies the ATOMMS measurements were quite accurate and that the point measurements were very similar to the average water vapor along the 800 m path. Thus the point measurement provided excellent "ground truth" against which we could compare and assess ATOMMS. In contrast, the disagreement between the three point measurements of water vapor in the mountains in Figure 9 clearly indicate that the "ground truth" knowledge of water vapor along the 5.4 km path between the two ATOMMS instruments above the valley at the top of the Catalina mountains is much harder to establish than it was for the 800 m path length on campus."

*2) p. 4669, lines 1, 5, 13: I recommend to include one ore more references containing further information on ATOMMS, RO and MLS, respectively*

We added references. The respective sentences now read

"The Active Temperature, Ozone and Moisture Microwave Spectrometer (ATOMMS) is a cm and mm wavelength remote sensing system we are developing to achieve unprecedented performance and attain key unfulfilled observational goals for measuring climate and reducing uncertainty about future climate (Kursinski et al., 2002, 2009)."

"Like GPS Radio Occultation (RO) (Kursinski et al., 1997; Anthes et al., 2011), ATOMMS is a satellite-to-satellite RO system (see Fig. 1)."

"As a result, ATOMMS has the ability to retrieve the trace gas constituents profiled by NASA's Microwave Limb Sounder (MLS) (Waters et al., 2006) but with the much higher vertical resolution, precision, accuracy and all-weather, global sampling of GPS RO."

Added References:

Anthes, R. A. (2011), Exploring Earth's atmosphere with radio occultation: contributions to weather, climate and space weather, Atmos. Meas. Tech., 4, 1077-1103, 2011 www.atmos-meas-tech.net/4/1077/2011/ doi:10.5194/amt-4-1077-2011

Kursinski, E. R., G. A. Hajj, J. T. Schofield, R. P. Linfield and K. R. Hardy, Observing Earth's atmosphere with radio occultation measurements using the Global Positioning System, *J. Geophys. Res.*, **102**, 23429-23465, 1997.

Kursinski, E. R. et al., An Active Microwave Limb Sounder for Profiling Water Vapor, Ozone, Temperature, Geopotential, Clouds, Isotopes and Stratospheric Winds, in *Occultations for Probing Atmosphere and Climate* (OPAC-1), Springer-Verlag, Berlin, p. 173-188, 2004.

Waters, J. W., L. Froidevaux, R. S. Harwood, R. F. Jarnot, H. M. Pickett, W. G. Read, P. H. Siegel, R. E. Cofield, M. J. Filipiak, D. A. Flower, J. R. Holden, G. K. Lau, N. J. Livesey, G. L. Manney, H. C. Pumphrey, M. L. Santee, D. L. Wu, D. T. Cuddy, R. R. Lay, M. S. Loo, V. S. Perun, M. J. Schwartz, P. C. Stek, R. P. Thurstans, M. A. Boyles, K. M. Chandra, M. C. Chavez, G.-S. Chen, B. V. Chudasama, R. Dodge, R. A. Fuller, M. A. Girard, J. H. Jiang, Y. Jiang, B. W. Knosp, R. C. LaBelle, J. C. Lam, K. A. Lee, D. Miller, J. E. Oswald, N. C. Patel, D. M. Pukala, O. Quintero, D. M. Scaff, W. Van Snyder, M. C. Tope, P. A. Wagner, and M. J. Walch (2006), The Earth Observing System Microwave Limb Sounder (EOS MLS) on the Aura Satellite, *IEEE Transactions on Geoscience and Remote Sensing*, **44**, No. 5, May 2006, 1075-1092.

*3) p. 4669, line 14: Are there any limitations regarding the all-weather capability?*

Yes. As noted in Kursinski et al., 2009, the errors increase at lower altitudes particularly in warmer conditions like the tropics. The technique will likely be very accurate down to about 3 km in the tropics and down to the surface under high latitude winter conditions.

GPS signals can probe through any conditions with one problem of ambiguity created by super-refraction frequently encountered at the top of the boundary layer in tropical conditions typically 1 to 2 km above the surface (e.g., Xie et al., 2010). ATOMMS will have the same super-refraction caused ambiguity.

Because of the strong absorption by liquid water at the High Band frequencies, the ATOMMS High Band signals will probe down to where liquid water clouds are encountered which is roughly the freezing level. The ATOMMS Low Band signals will propagate through most weather conditions. To quantify this, we can scale off the Mie scattering Figure 16 which indicates the 22 GHz optical depth due to clouds will be approximately 30 times smaller than that at 197 GHz. Given that a typical continental cloud LWC extended along a 5.4 km path caused an optical depth of 4 at 197 GHz, then the thickness of a liquid water cloud that would produce an optical depth of 10 at 22 GHz would be about 400 km thick. This is quite an extended cloud.

In terms of the optical depth due to water vapor, as the signals probes deeper in the atmosphere keeping the optical depth below 10 is accomplished by using frequencies farther away from line center. Kursinski et al. (2002) showed that an 18 GHz signal frequency can probe to it the surface even in the tropics. Probing both the high and low sides of the 22 GHz line as needed to separate liquid water from vapor (Kursinski et al., 2009) can be accomplished down to 2-3 km in the tropics and the surface at higher latitudes.

Our guess is accurate ATOMMS profiles will extend down to 2 to 3 km altitude in the tropics and to the surface at high latitudes.

References:

Kursinski, E. R., D. Ward, A. Otarola, R. Frehlich, C. Groppi, S. Albanna, M. Schein, W. Bertiger and M. Ross (2009), The Active Temperature Ozone and Moisture Microwave Spectrometer (ATOMMS), in *New Horizons in Occultation Research, Studies in Atmosphere and Climate,* Steiner, A., Pirscher, B., Foelsche, U. and Kirchengast, G. (Eds.), 336 p., Hardcover, ISBN: 978-3-642-00320-2.

Xie, F., D. L. Wu, C. O. Ao, E. R. Kursinski, A. J. Mannucci and S. Syndergaard (2010), Super-refraction effects on GPS radio occultation refractivity in marine boundary layers, *Geophys. Research Lett*., 2010GL043299R. (*selected by GRL editors as an "AGU Journal Highlight."*).

*4) p. 4669, lines 20 to 23 and/or p. 4671 lines 2 to 4: I recommend inclusion of one ore more references about the differential absorption (DA) technique used by ATOMMS; if possible, these references should also contain quantitative information on how well noise sources can be reduced by using DA.*

We added three references. "ATOMMS achieves its unique performance via differential absorption by measuring signal levels at two or more frequencies simultaneously in order to reduce or eliminate many types of common mode noise (Kursinski et al., 2002, 2004, 2009)."

*5) p. 4671, line 10: what means "… above the atmosphere…" in practice? Please include a note on that.*

The sentence now reads "In the satellite to satellite geometry, $A_0(f_1)$ and $A_0(f_2)$ will be determined by measurements immediately before or after each occultation when the lowest point along the signal path between the spacecraft is several hundred km above the ~100 km altitude at which the atmosphere is detectable by ATOMMS."

*6) p. 4671 to 4672, lines 26 to 1; p. 4673, line 10: It is not clear here, if you use one ore more reference/normalization times (even if it becomes clear when reading the following paragraphs). Maybe you can clarify this here already.*

We use measurements made at one reference time to normalize against.

*7) p. 4672, line 1 to 3: Could you please explain why the multipath effect remains constant? Will it really divide out or will there be a residual error? Please clarify this.*

There will be a VERY small residual error. See the answer to Reviewer 1 Specific Question 3. Because both reviewers asked this, we are asking the Reviewers whether we should add this explanation as an Appendix to the paper.

*8) p. 4672, line 14 to 16: Could you please quantify to what degree the continuum cancels out? What is the maximum acceptable separation of f and f_ref so that this assumption is valid?*

We compute the difference in the measured optical depth at pairs of frequencies. Considering the worst case: maximum frequency separation 205 GHz to 175 GHz (full range of instrument) and minimum water vapor observed 2.80 g $kg^{-1}$ (when we will be most sensitive to ignoring the dry continuum), we find that a 0.07% error in the difference optical depth if we ignore the dry continuum, which is small enough to be negligible.

*9) p. 4672, lines 22 to 23: How strong can pressure and temperature vary so that the relationship remains valid? It would be helpful to quantify this or at least to include a reference to section 2.2 where it is shown, that the assumption is valid for the experiment conditions.*

We added a reference to Section 2.2.

*10) p. 4673, line 18: in line with SC 9: please include a note that it is explained below, that Eq. 8 is valid for the experiment conditions.*

???? As noted below, Eq (8) is valid for the range of pressures and temperatures of the experiment.

*11) p. 4674, line 9: "A series of these amplitude ratio spectra can be calculated…" Does this mean, that one e_max – e_norm spectrum is calculated for every of the 27 tones? Or am I misinterpreting that. Please clarify it.*

We clarified it. A single spectral fit is done to a set of 27 tones.

*12) p. 4675, lines 6 to 7: "In contrast, ATOMMS…". This sentence can be skipped if you want, since this is clear.*

We combined the sentences

*13) p. 4675, lines 12 to 14: What exactly have been the reasons why the high band range could not be used entirely?*

As noted, there was at least interference from cell phones and wireless internet. We modified this to just say interference did not allow good measurements when the receiver Intermediate frequencies were below 5 GHz.

*14) p. 4675, lines 17 to 18: What were the reasons and advantages of the test configuration run on this day?*

The particular version of the instrument control software and hardware we were using at that time could tune up to 1 tune per second. The version we have now tunes 10 times per second. In the future it may go faster.

*15) p. 4675, lines 19 to 26: I don't understand this paragraph and in turn the red curve on Fig. 5. Could you please try to clarify it?*

This was addressed in the response to reviewer 1, specific comment 8.

*16) p. 4676, chapter 3.1: Could you please include some notes on the measurement cycle (frequencies, repetition time, etc.) used in the mountaintop observations (as you did it for the 183 GHz measurements?*

The 22 GHz system is designed as an eight tone, fixed frequency system without any tuning. It has 8 frequencies, 18.5, 19.5, 20.2, 21.5, 22.6, 23.5, 24.4, 25.5 GHz. However, in this initial outdoor test, only 2 of them, the 22.6 and 23.5 GHz channels worked well. Two were off entirely, 4 were on but unstable and 2 were stable.

*17) p. 4677, lines 13 to 17: The last 2 sentences of this paragraph can be shortened as the issue with the calibration is the same as it was the case for the high band.*

We shortened the sentences
"The amplitudes of the signals measured by ATOMMS 22 GHz receiver have been reduced by absorption due to the atmospheric water vapor along the path between the two instruments according to (1). To isolate the optical depth, $\tau$, via (2), we must know both $A$ and $A_0$. As in the campus tests, because we cannot determine the $A_0$ term, we determine changes in water vapor using the differential approach in time."

*18) p. 4677, Eq. 11: Should this better be an "approximately equal" sign? What is typically the stability of the transmitter?*

We changed it to an approximately equal sign.

*19) p. 4678, line 21 to 22: It would be interesting to see also graphs of the P and T conditions during the 22 GHz experiment.*

We have estimated that the variations in temperature and pressure during the 22 GHz experiment will not significantly contribute to the overall error or uncertainty in our analysis. We do not think another figure is required. The text discussing the temperature and pressure variations and the corresponding effects in our results has been clarified. Please see the response to reviewer 1, specific comment 9.

*20) p. 4681, line 14: Consider using "number of scattering elements per unit of volume and size" instead of "particle size distribution".*

We changed "particle size distribution" to "droplet size distribution" which is more standard. We also changed the units to be the correct combination of m and mm.

*21) p. 4682, Eq. 15: Even if this is a basic relation, it would maybe be useful to include a reference for it.*

We added a reference to Bohren and Huffman, 1983.

*22) p. 4685, lines 19 to 20: "Since the signals (note: 0.5 micron and 1.5mm) at both wavelength bands propagate through the same atmosphere and particles". I don't agree with that as the MW refractivity (Smith-Weintraub) and the VIS refractivity (Edlen) differ especially in moist* *conditions. Also the Fresnel zones are different. However, assuming the same propagation path is probably a good approximation for this rather short path.*

We agree with the reviewer's points and have reworded the sentence to be
"Since the signal paths of the two wavelength bands through the atmosphere and droplets are almost the same, the major difference in the measured opacities is due to the difference in the Mie scattering efficiencies, the $Q_{197}$ and $Q_{vis}$ terms."

*23) p. 4687, lines 19 to 20: The last sentence of this paragraph can be skipped/shortened, as this was already mentioned.*

We removed it

*24) p. 4690, lines 13 to 16: It is right, that long wavelengths have many advantages regarding penetration of water vapor and clouds. But also other wavelengths can provide useful measurements regarding climate and weather forecasting. You should consider to rephrase the sentence (to weak the absoluteness of this statement).*

We have rewritten this to make our point while also trying to address the reviewer's point.

"The whiting out of the photographs caused by scattering by rain and cloud droplets reminds us of the limitations of remote sensing at visible wavelengths on a planet with 60% to 70% global cloud cover (Rossow and Shiffer, 1999). Remote sensing at IR wavelengths is quite powerful for both weather and climate monitoring and forecasting and in fact critical for monitoring outgoing longwave radiation (OLR) to space. However, its limited ability to penetrate clouds leads to preferential sampling of drier, cloud-free air that creates dry biases for instance in upper tropospheric humidity (UTH) (Lazante and Gahrs, 2000). John et al. (2011) note that "The clear‐sky HIRS (High Resolution Infrared Radiation Sounder) measurements are sampling meteorologically unusual situations of cloud free conditions, so they only represent a limited aspect of the climate system". Interpreting trends in such biased observations is inherently difficult and ambiguous because changes in the sampling of clouds, whose properties are likely to change in a changing climate, will cause shifts in the sampling biases over time (e.g. John et al., 2011). Unambiguously monitoring and understanding trends in the climate state can only be accomplished by sampling the entire range of behavior of the Earth's atmosphere which can only be achieved at wavelengths long enough to routinely penetrate through clouds."

References:

John, V. O., G. Holl, R. P. Allan, S. A. Buehler, D. E. Parker, and B. J. Soden (2011), Clear‐sky biases in satellite infrared estimates of upper tropospheric humidity and its trends, *J. Geophys. Res*., **116**, D14108, doi:10.1029/2010JD015355.

Lanzante, J. R., and G. E. Gahrs (2000), The "clear‐sky bias" of TOVS upper‐tropospheric humidity, *J. Clim*., **13**, 4034–4041.

***Reviewer 2, Technical corrections (TC)***

1) *p. 4668, line 7: 22 GHz and 183 GHz (in line with GC 4)*

   GHz added after 22

2) *p. 4668, line 15: unit is missing (5.4 km path)*

   km added after 5.4

3) *p. 4670, line 8: 182 GHz to 205 GHz (GC 4)*

   GHz added after 182

4) *p. 4670, line 10: include also the acronym for the AME building*

   added

5) *p. 4670, line 16: I mean it should be "… measures the attenuation of the signal due to WATER VAPOR along ITS propagation path."*

   "it" has been changed to "its"

6) *p. 4672, line 20, Eqs. 7 and 8: The usage of the symbols is not uniform: Deltatau(…,e_norm…) or Delta-tau_norm(…e(t_0)…); e(t_0) or e_norm ?*

   Eqs 7 and 8 have been modified to use norm in the denominators

7) *p. 4672, Eq. 5: tau_dry is not explained in the text (in line with GC 3)*

   It is now noted in the text

8) *p. 4672, Eq. 6: e_norm is not explained in the text (GC 3)*

   it is now defined in the text

9) *p. 4673, line 4: the year in the date is missing (13 March 2010)*

   the year has been added

10) *p. 4673, line 6: you could mention in brackets, that the range described comprises 27 tones*

   We added a statement about 27 frequencies

11) *p. 4673, line 12: maybe more correct "… measured DURING this test…"*

   changed

12) *p. 4673, Eq. 9: Can the time index be specified for P and T on the right hand side of Eq. 9? And should the time index for e_max and e_norm be added, too?*

   The problem is the equation becomes so long that it does not fit on a line.

13) *p. 4675, line 5: probably better "The PAS HYGROMETER changes in capacitance…" (instead of "The first…")*

changed

*14) p. 4675, line 14: 128 GHz to 205 GHz (GC 4)*

changed

*15) p. 4676, lines 7,8,11,14,16: please use m or km instead of ft.*

changed

*16) p. 4676, lines 9, 17: There are no "a" "b" labels in Fig. 8*

added

*17) p. 4676, line 9: 20 to 21 August (GC 4)*

h was supposed to be hour. We changed from PM and AM to 21:45 to 09:00 the following day

*18) p. 4677, line 2: isolateS (?)*

s added

*19) p. 4677, line 5: no comma in between "…hygrometeors track…"*

comma removed

*20) p. 4677, line 7: no "a" "b" labels in Fig. 9*

added

*21) p. 4677, line 19: maybe better "…relative to the specific humidity AT THE BEGINNING of the test…"*

changed

*22) p. 4678, line 3 to 4: I recommend writing simply "Using the AM 6.2 model or the MPM 93 model…" as both models have already been mentioned before.*

Modified as suggested

*23) p. 4678, line 7: comma missing after "K"*

added

*24) p. 4678, line 19: 22.6 GHz and 23.5 GHz (GC 4)*

added GHz

25) *p. 4679, line 2: 22.6 GHz (red) and 23.5 GHz (blue) (GC 4)*

added GHz

26) *p. 4679, line 7: no "a" "b" labels in Fig. 10*

added

27) *p. 4679, line 9: 22 GHz (not GHZ)*

Z => z

28) *p. 4679, line 11: 22.6 GHz (GC 4)*

GHz added

29) *p. 4679, line 20: "... estimates of the water vapor CHANGE…"*

"change" added

30) *p. 4679, line 21: 22.6 GHz and 23.5 GHz (not 23.5 Hz)*

Problem with the typesetters. Our version is correct

31) *p. 4679, line 24: 5.3 g kg-1 to 6.3 g kg-1 (CG 4)*

g/kg added

32) *p. 4679, line 28: "… ATOMMS instrumentS…"*

added 's'

33) *p. 4680, line 13: "Figure 12 (LEFT HAND SIDE) shows the temperature and…"*

added '(left hand side)'

34) *p. 4681, Eq. 14: D is not explained in the text.*

D is now explained in the text

35) *p. 4681, line 24: maybe "… measured by ATOMMS at 197 GHz SHOWN in Fig. 14*

added 'shown'

36) *p. 4682, line 5: consider including a reference to Fig. 15 "… radar derived estimates (Fig. 15)… "*

added '(Figure 15)'

37) *p. 4862, Eq. 15: r and lambda are not described (GC 3)*

lambda is now described. The ‘r’ was replaced by ‘D’, the droplet radius to be consistent with D used and defined in equation 14.

38) *p. 4682, line 17: n_0 (m-3) and alpha(m-1) are not described (GC 3)*

We defined $n_0$. We replaced alpha with $1/D_0$ and defined $D_0$ as the mean droplet diameter.

39) *p. 4862, Eq. 16: the r in the exponent must not be superscript*

typesetting mistake we did not catch. Now fixed in our version

40) *p. 4683, line 3: include the symbol R: “and rain R is in mm h-1” (GC 3)*

added ‘and rain R is in mm h-1’

41) *p. 4683, Eq. 18: Typo? To the power of 0.71 > why not 0.7?*

We have substantially modified our description of the conversion of radar reflectivity to optical depth at 197 GHz in this section so this equation is no longer present

42) *p. 4683, lines 7, 8: symbols r and R are explained here; explanation can be removed if they were already explained before (see TC 37 and 40)*

Again, this section has been substantially rewritten

43) *p. 4683, Eq. 20: rho is not explained (GC 3)*

rho is now defined. Again the section has been substantially altered.

44) *p. 4684, line 2: Fig 16 (not Fig. 15)*

15 => 16

45) *p. 4684, Eq. 23: n_rain is not explained (GC 3)*

It is now explained

46) *p. 4684, Eq. 25: t_197 and z are not explained (GC 3)*

These are now explained

47) *p. 4684, lines 22 to 23: “The distances are… 0.010 km, 0.30 km and 0.78 km” (GC4)*

changed

48) *p. 4685, lines 5, 13: Eq. 26 (not Eq. 25)*

corrected

49) *p. 4685, line 19: Eq. 15 (not Eq. 16)*

fixed

50) *p. 4686, line 5: symbol x was not explained*

added definition of x

51) *P. 4686, Eq. 27: I assume the symbol p in the equation should be a rho. Please check the equation! Explain symbol k_cloud in the text (GC 3)*

Yes, this is a type setting error we missed. $k_{cloud}$ is now defined.

52) *p. 4686, Eq. 28: overbar(k_cloud), L are not explained in the text (GC 3)*

defined *L* and the meaning of the overbar

53) *p. 4686, Eqs. 28 and 29: again the p instead of the rho*

Yes, typesetting errors again

54) *p. 4687, line 6: maybe better "… that make the radar data noisy as VISIBLE in…"*

added as 'is visible' in

55) *p. 4687, line 11: 15.6 hPa to 17.5 hPa (C4)*

added hPa

56) *p. 4687, line 16: Fig 14 (not Fig. 17)*

corrected

57) *p. 4690, line 13: 60 % to 70 % (GC 4)*

added %

58) *p. 4692, Eq. 20: one bracket in the lowest row of the first term is wrong*

Typesetting problem we missed

59) *p. 4692, line 22: "calculate the tau" (instead of the "r")? "relative" (instead of "celative")?*

Typesetting problem: calculate the relative change in visual optical depth

**Reviewer 2, Comments on the tables and figures:**
*General comments:*

*- Please label all axes and/or add their dimensions (also dimensionless; time axes e.g. like in Fig. 11). Especially Figs. 4, 5, 6, 7, 13 to 17, 19.*

*- Depending on the final figure size consider to increase the font size of the individual figures.*

*Specific comments:*

*Table 1: - Either describe the abbreviations "Avg-", "Avg" and "Avg+" in a footnote or in the caption of the table, or use the full terms (average optical depth) directly in the table.*

Renamed Avg to q-overbar

*- Also K is a mean value.*

K has been changed to K-overbar

*Fig. 1: Could be optimized using a vector graphics program (e.g. Inkscape).*

We have updated it a bit to trim off rough edges

*Fig. 2: - As the vertical axis shown in the figure is altitude, you could consider including approximate altitude levels additionally in the caption (esp. supplementing the "230 K altitude", "240 K" and the "~ 10 mb level").*

This is complicated because the altitude level depends on both latitude and season.

*- I think the arrow indicating the GPS H2O profiling should not extend up to the Earth's surface.*

Some of the GPS H2O profiling does extend to the surface particularly at higher latitudes

*Fig. 3: Left hand: You could consider including symbols indicating the transmitter and receiver on the figure or simply use an arrow to display the measurement direction.*

Added an arrow head

*Fig. 5: In line with SC 15: the last sentence of the caption is not clear to me.*

Tried to clarify it

*Fig. 8: The labels in the left hand figure should have a readable font size (also the arrowend).*

This is a map from Google.  Do we write over the existing labels?

*Fig. 10: Caption: consider including something like: "Left hand panel showS results of conversion OF ATOMMS MEASURED AMPLITUDES to specific humidity…"*

The caption has been modified as suggested

*Fig. 11: Caption: Consider including something like: "The ATOMMS 22.6 GHz and 23.5 GHZ channel AMPLITUDES have been converted…"*

The caption has been modified as suggested

*Fig. 12: Correct typos in dimension of specific humidity: g/kg (not gr/Kg)*

Done

*Fig. 14: Caption: No figure labels "a" "b" ;*

Added

*0.88 degree and 1.28 degree (GC 4)*

Changed to $0.88^o$ and $1.28^o$

*Fig. 15: Caption: You could consider including the name of the radar*

Added Tucson WSR-88 radar

## Figures and Tables that have been changed

**All of the tables, figures and figure captions that have changed are shown below.**

Table 1. Conversion Between Changes in Optical Depth and the Average Specific Humidity, $\bar{q}$, Along a 5.4 km Path

| **AM6.2 Model** | $\bar{q}-1$ **(g/kg)** | $\bar{q}$ **(g/kg)** | $\bar{q}+1$ **(g/kg)** | $\bar{K}$**=dτ/d**$\bar{q}$ **(g/kg)$^{-1}$** | ***dln*$\bar{K}$*/dP (%/mb)*** | ***dln*$\bar{K}$*/dT (%/$^o$C)*** |
|---|---|---|---|---|---|---|
| Vapor pressure (mb) | 11.25 | 12.43 | 13.61 | | | |
| 22.6 GHz Tau [nepers] | 0.359 | 0.394 | 0.428 | 0.0346 | -0.105 | -0.25 |
| 23.5 GHz Tau [nepers] | 0.302 | 0.331 | 0.359 | 0.0285 | -0.21 | -0.56 |

| **MPM93 Model** | $\bar{q}-1$ **(g/kg)** | $\bar{q}$ **(g/kg)** | $\bar{q}+1$ **(g/kg)** | $\bar{K}$**=dτ/d**$\bar{q}$ **[g/kg]$^{-1}$** | ***dln*$\bar{K}$*/dP (%/mb)*** | ***dln*$\bar{K}$*/dT (%/$^o$C)*** |
|---|---|---|---|---|---|---|
| Vapor pressure (mb) | 11.25 | 12.43 | 13.61 | | | |
| 22.6 GHz Tau [nepers] | 0.322 | 0.354 | 0.385 | 0.0312 | -0.086 | -0.43 |
| 23.5 GHz Tau [nepers] | 0.285 | 0.314 | 0.342 | 0.0287 | -0.049 | -0.54 |

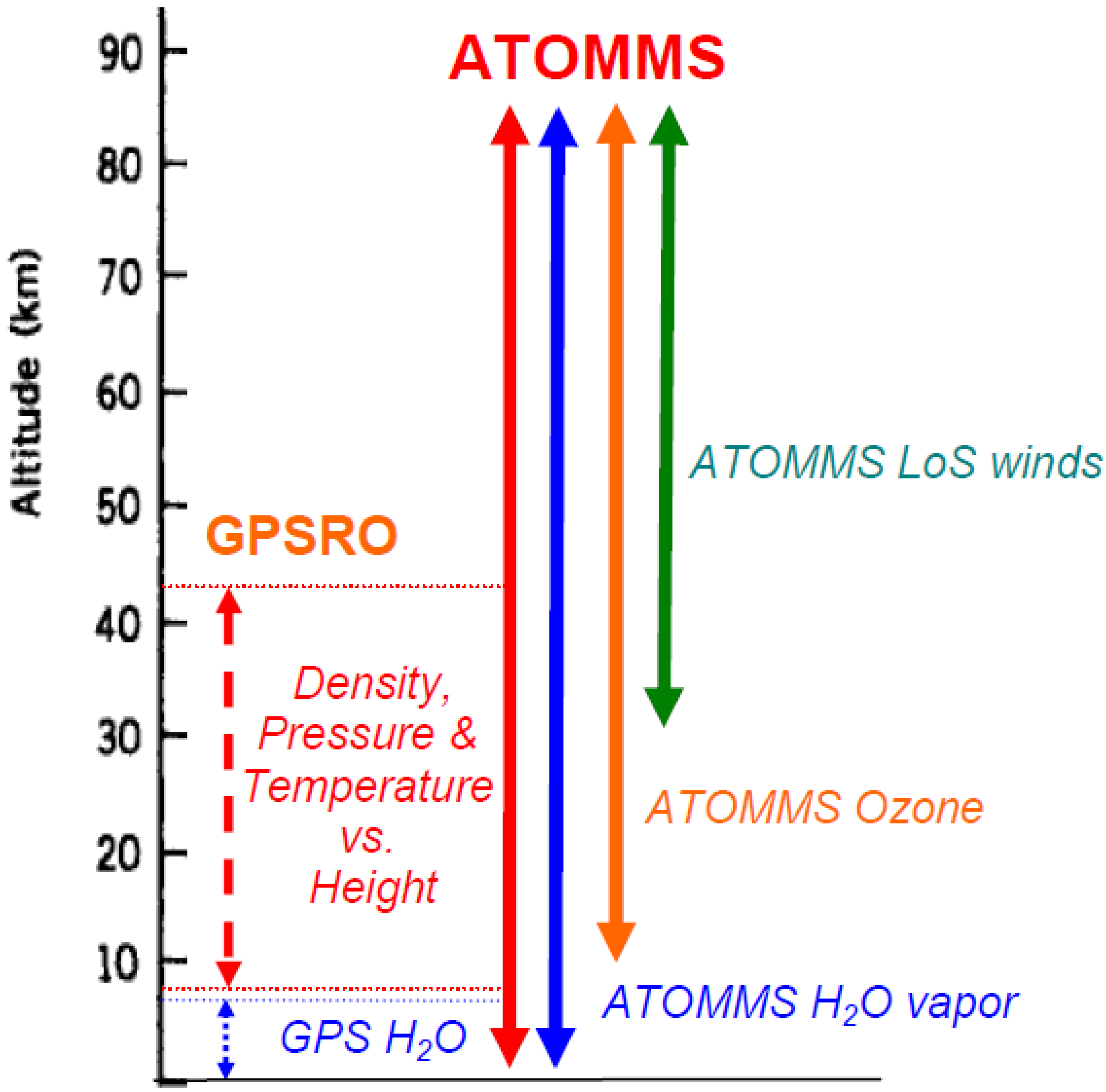



Figure 2. Altitude comparison of ATOMMS vs. GPSRO. GPS profiles atmospheric density, pressure and temperature between the 230 K altitude in the troposphere (where water vapor begins contributing significantly to refractivity) to approximately 45 km, depending on electron densities in the ionosphere. At tropospheric temperatures warmer than 240K, GPS refractivity primarily constrains water vapor. In contrast, ATOMMS simultaneously profiles density, pressure, temperature and water vapor from the lower troposphere to the mesopause. ATOMMS profiles ozone from the upper troposphere to the mesopause. Above the ~10 mb level, ATOMMS will also determine line of sight (LoS) winds via the Doppler shift of the center of absorption lines.

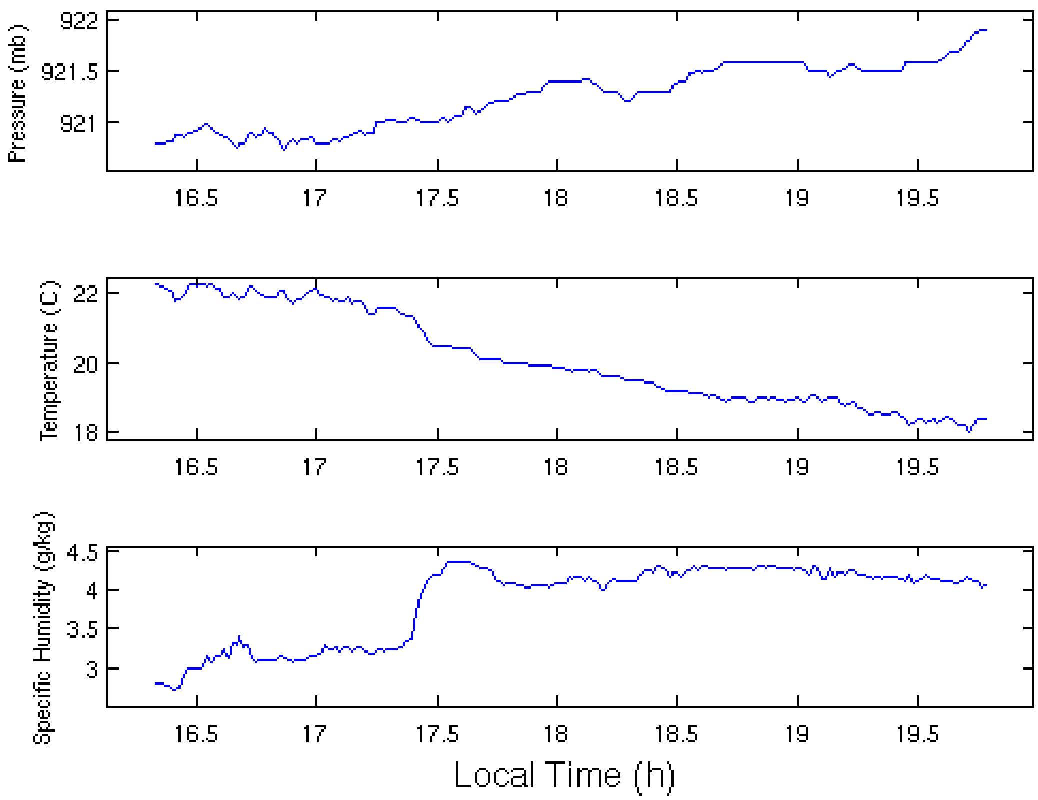



Figure 4. The atmospheric pressure, temperature, and specific humidity measured at the PAS building during the ATOMMS rooftop tests on March 13, 2010.

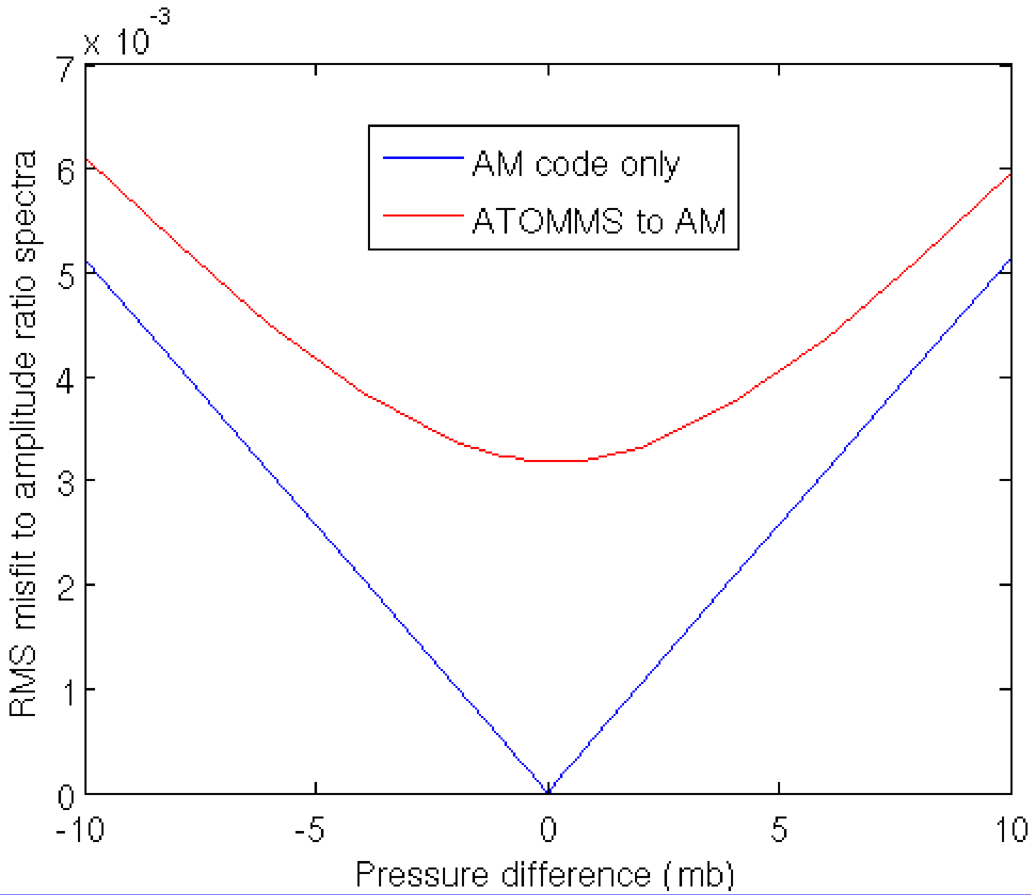



Figure 5. Sensitivity of ATOMMS 183 GHz spectra defined in Equation (9) to variations in pressure. The blue line is the RMS difference between the AM 6.2 model and itself as the pressure difference is varied. The red line is the difference between the ATOMMS spectra and the AM 6.2 model. The minimum error occur for a pressure about 0.1 mb above the measured surface pressure.

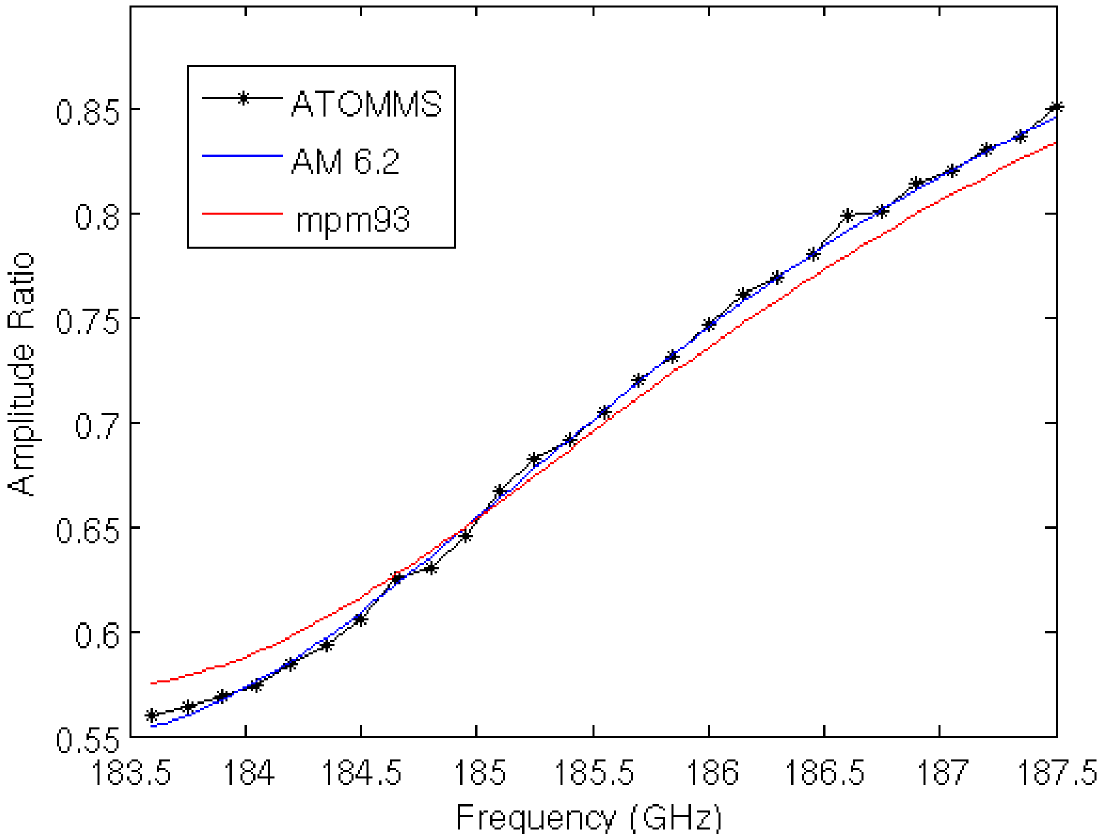



Figure 6. Amplitude ratio as defined in Equation (9) based on ATOMMS measurements of amplitude compared with the best fit value for $q_{max}$ - $q_{norm}$ to the ATOMMS amplitude ratio as computed by two microwave propagation models, AM 6.2 and MPM93.

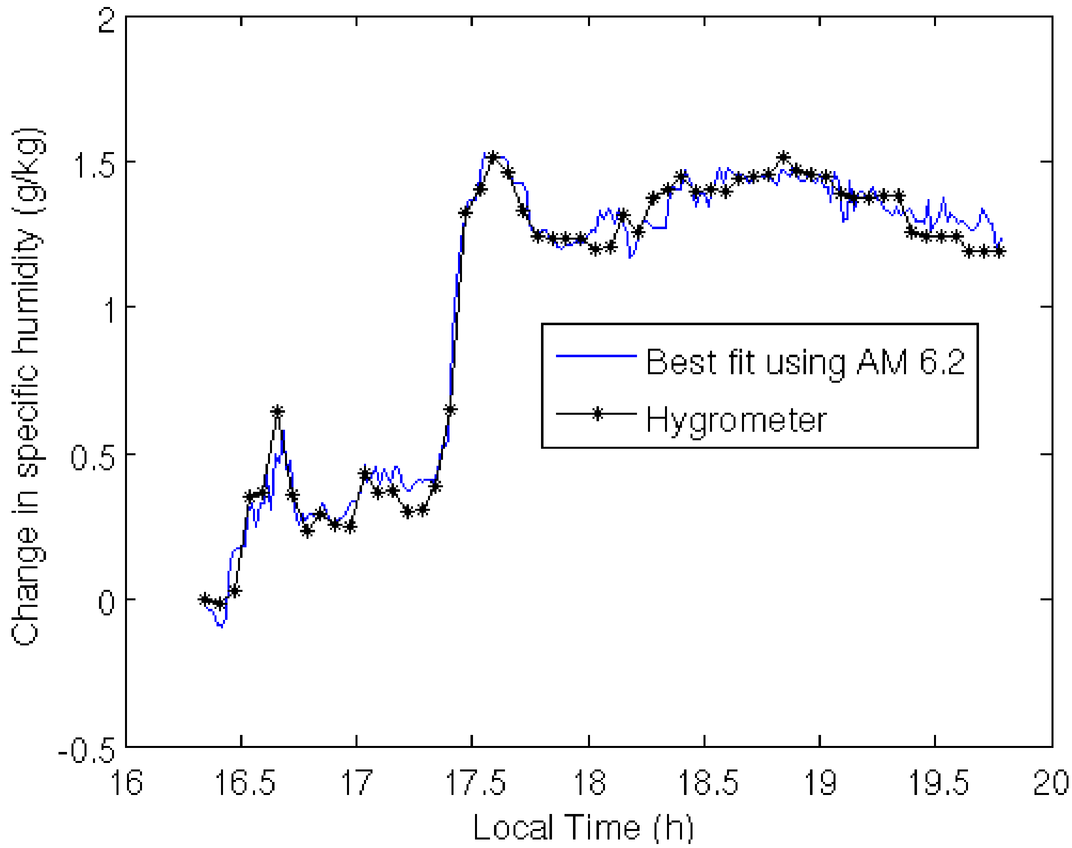



Figure 7. Change in specific humidity relative to first observation. The black asterisks represent the best fit changes in specific humidity along the signal path to the ATOMMS amplitude ratio spectra each time an ATOMMS amplitude spectra was measured. The blue curve shows the changes in specific humidity as measured by the nearby hygrometer on the PAS building.

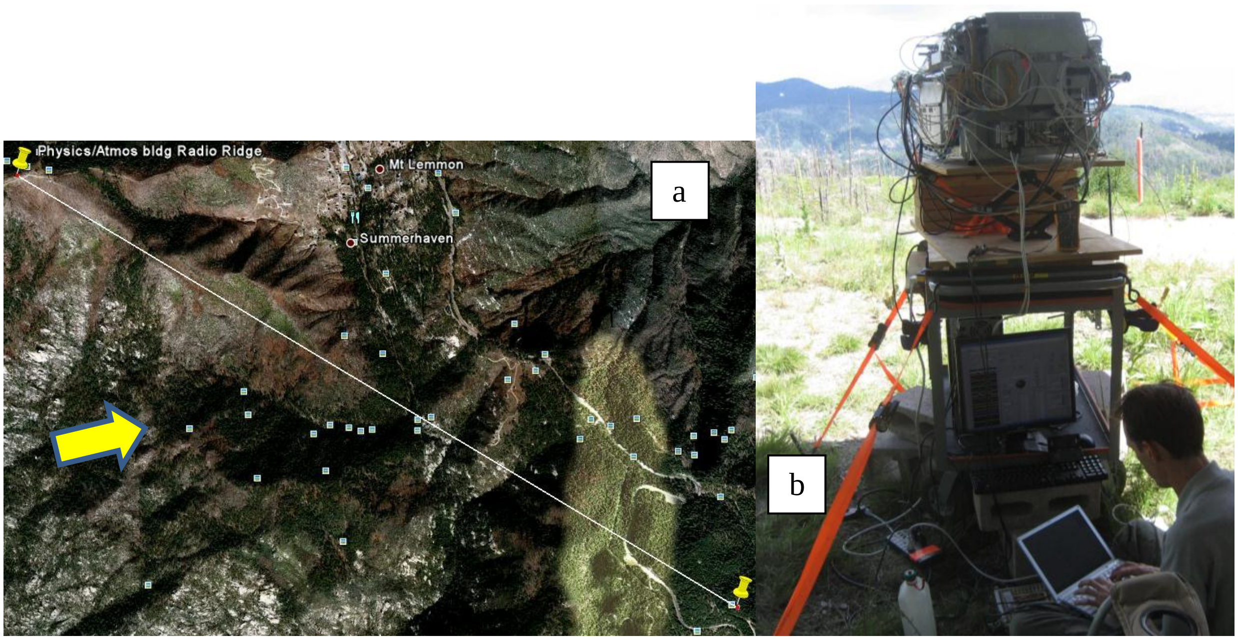



Figure 8. Left: The Mt. Lemmon (left) to Mt. Bigelow (right) geometry.  Right: Abe Young at the ATOMMS-A instrument on Mt. Lemmon.  The white dome in the distance in the upper left corner of the picture is the Mt. Bigelow observatory where the ATOMMS-B instrument is located. Yellow arrow indicates the approximate direction of the propagation of the storm on August 28, 2010.

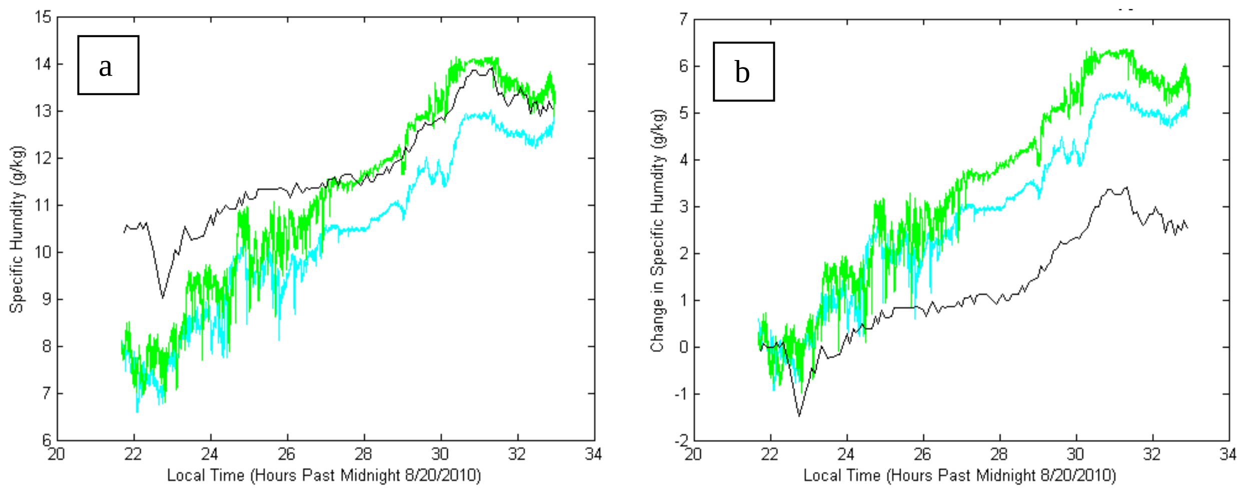



Figure 9. Specific Humidity measurements during the test from hygrometers at Summerhaven (black), the SAHRA flux tower on Mt. Bigelow (green) and our chilled mirror hygrometer (cyan) at ATOMMS-B on Mt. Bigelow. Left: Specific humidity. Right: Change in specific humidity relative to beginning of test at 21:45 local time.

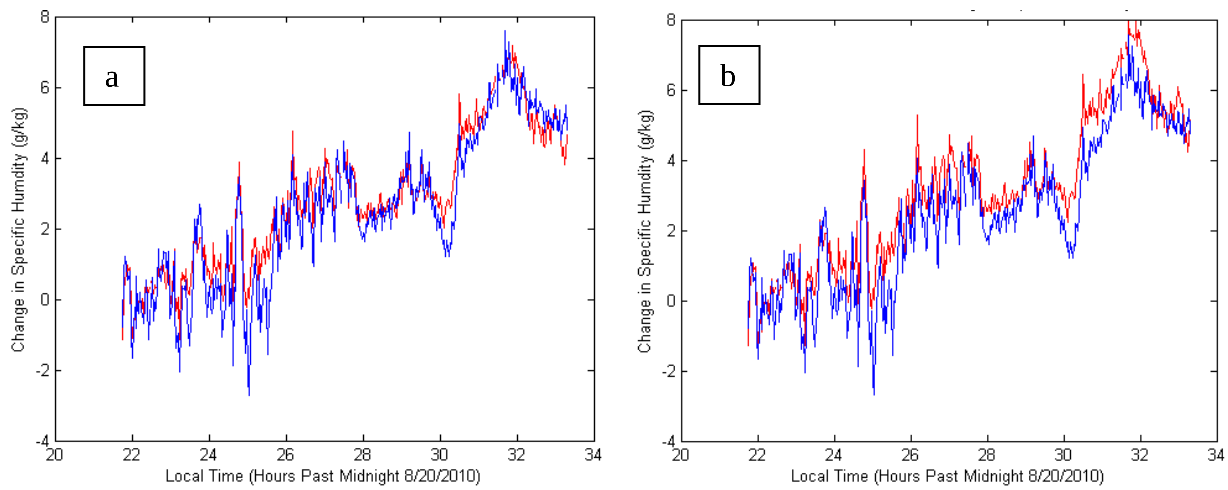



Figure 10. Left hand panel shows results of conversion of ATOMMS measured amplitudes to specific humidity using the AM model. Right hand figure shows conversion to specific humidity using the MPM 93 model. The 22.6 GHz and 23.5 GHz channels are red and blue respectively.

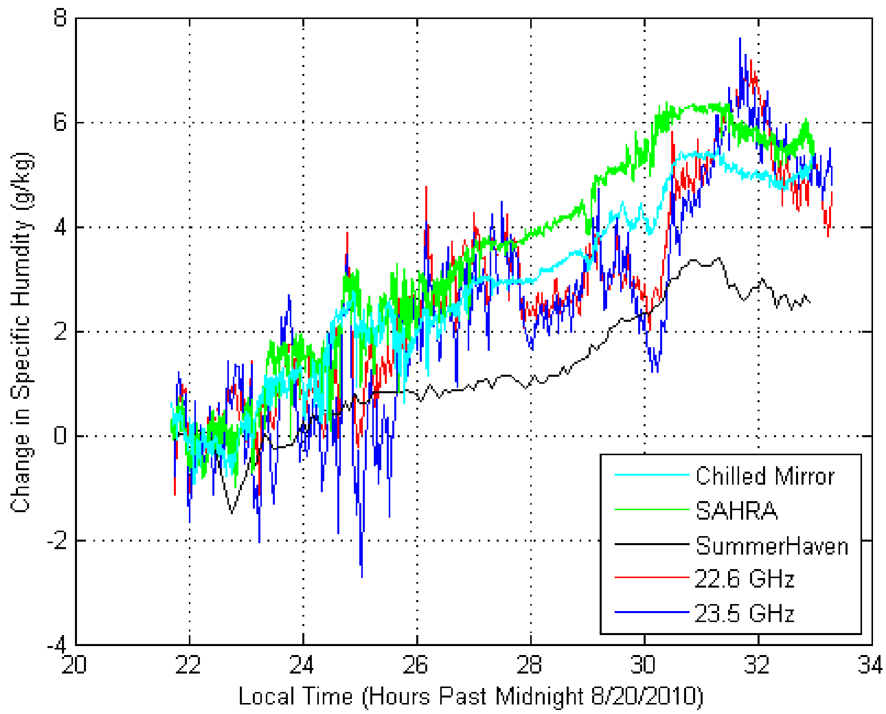



Figure 11. Five curves each representing a particular measured change in specific humidity. The ATOMMS 22.6 GHz and 23.5 GHz channel amplitudes have been converted to specific humidity using the AM 6.2 spectroscopic model.

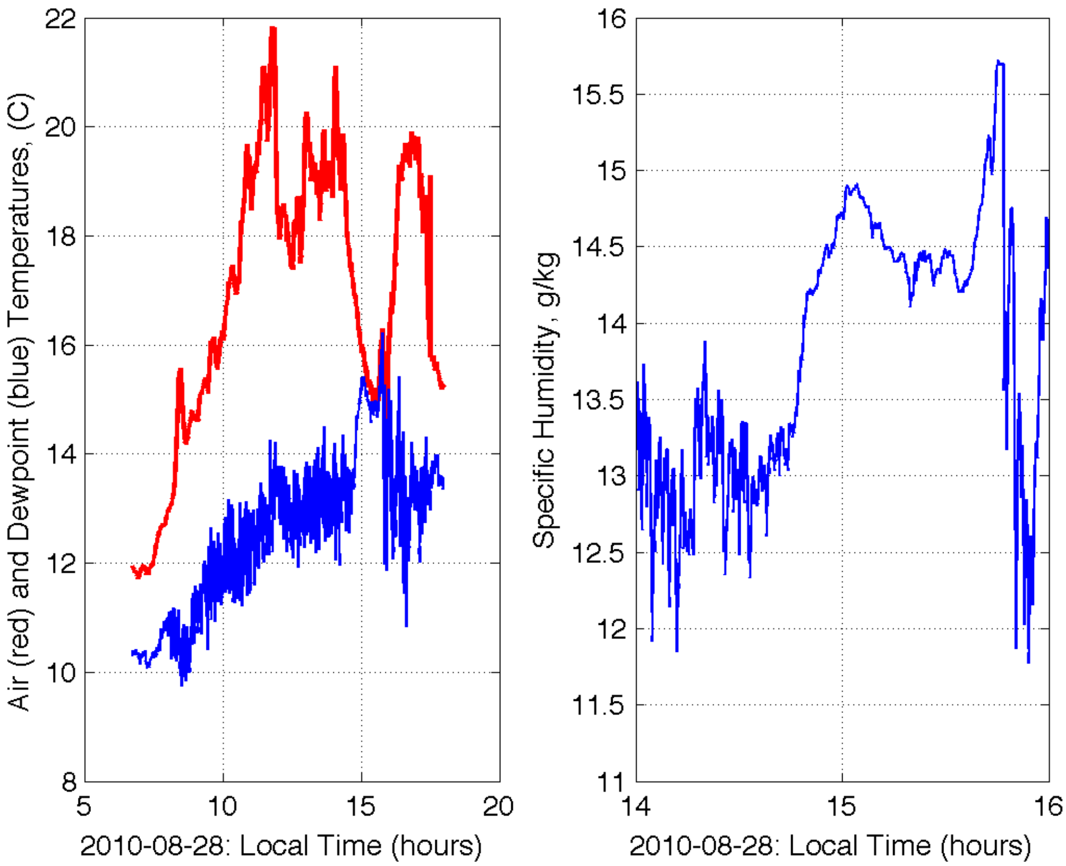



Figure 12. Left: Measurements of the temperature and dew point from the chilled mirror hygrometer on Mt. Bigelow. Right: Specific humidity from 14:00 to 16:00 local time.

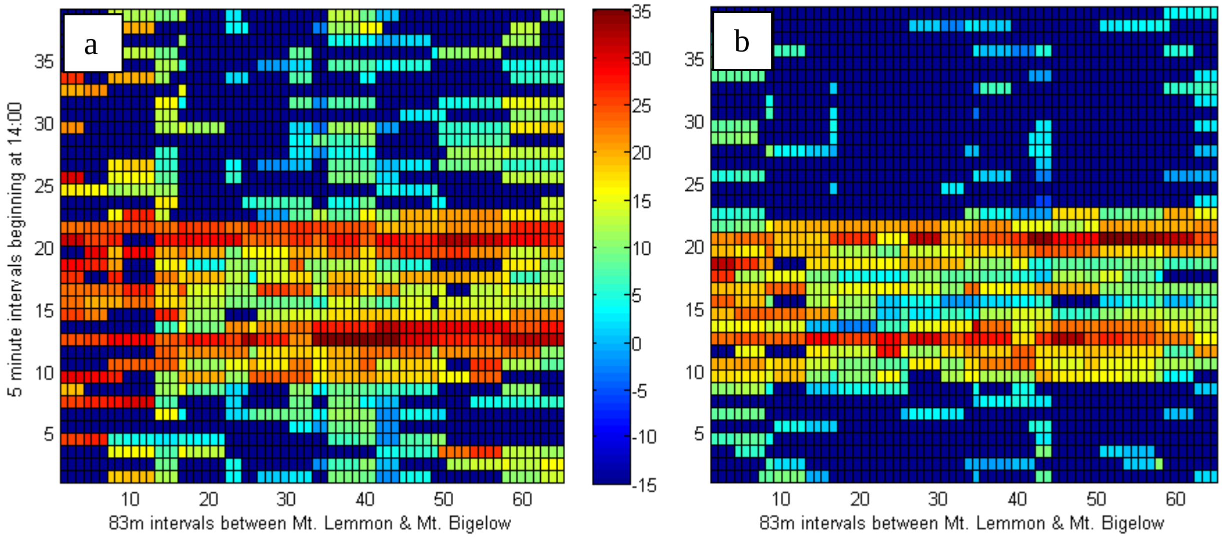



Figure 14. Radar measured reflection in dBz, at radar elevation of (a) 0.88$^o$ and (b) 1.28$^o$, time is vertical scale in units of 5 minute intervals beginning at 14:00. The left edge is Mt Lemmon, and right hand edge is Mt Bigelow. The 5.4 km path between them is divided into 65 equal intervals. The units of the color scale in shown in the center of the figure are dBz.

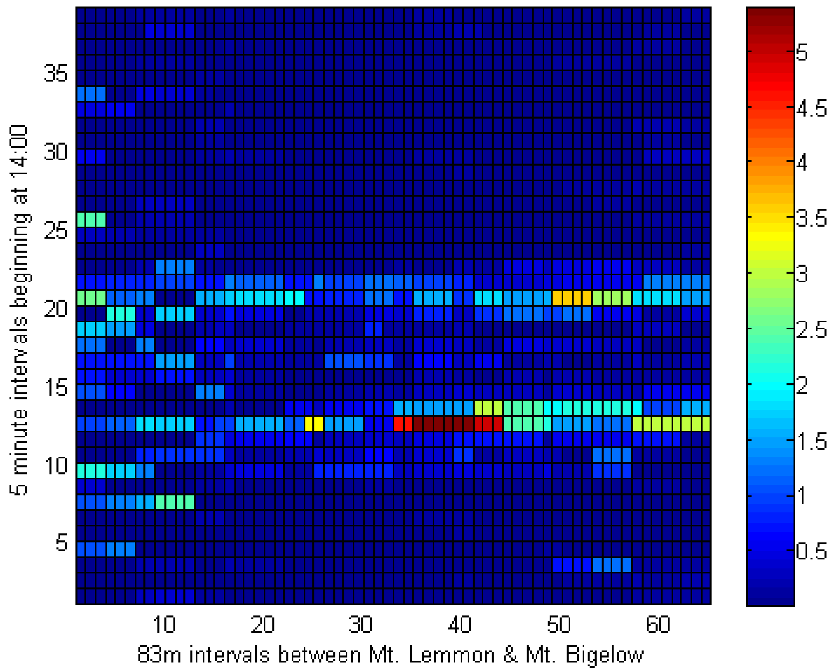



Figure 15. Rainrate in mm/hr derived from the Tucson WSR-88 radar data via the standard Z-R relation. The x and y axes are the same as in Figure 14. The units of the color scale to the right are in mm/hr.

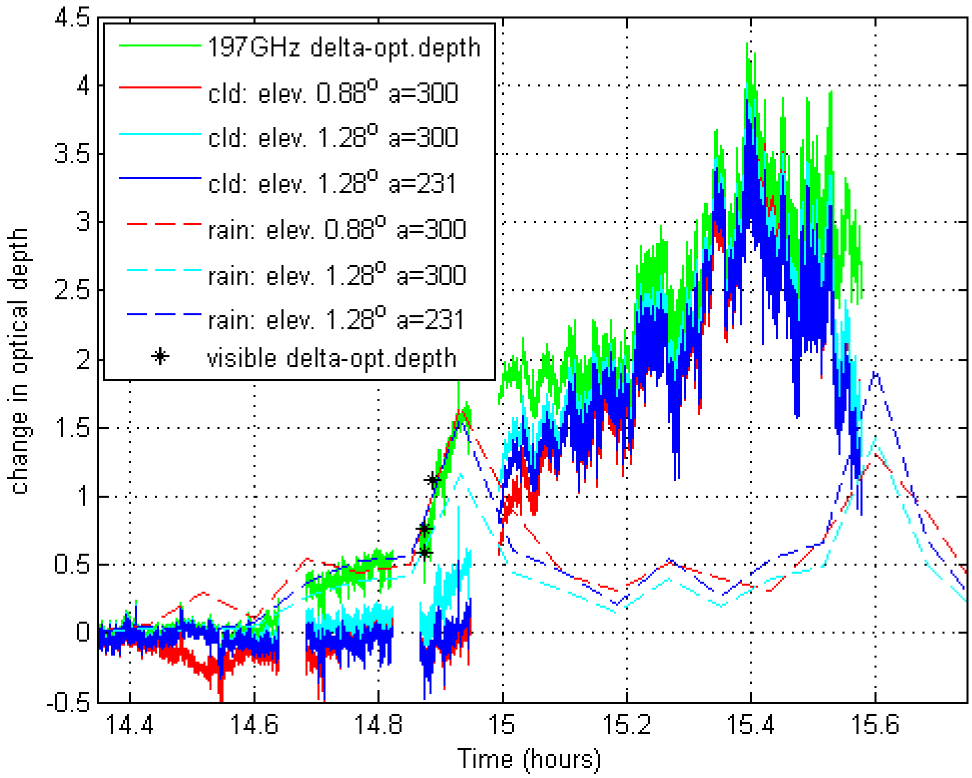



Figure 17. Plots of the change in optical depth during the storm. Green is the optical depth measured at 197 GHz. The optical depths due to rain estimated from the WSR-88 radar are dashed lines at 0.88$^o$ and 1.28$^o$ elevations, The estimated optical depths due to clouds are shown as solid red, cyan and blue lines which are calculated as the difference between the 197 GHz measurements and that due to rain. The asterisks are changes in optical depth at visible wavelengths derived from photographic images taken during the storm. See text for details.

**Note that we have switched the order of figures 18 and 19 from the original submission.**

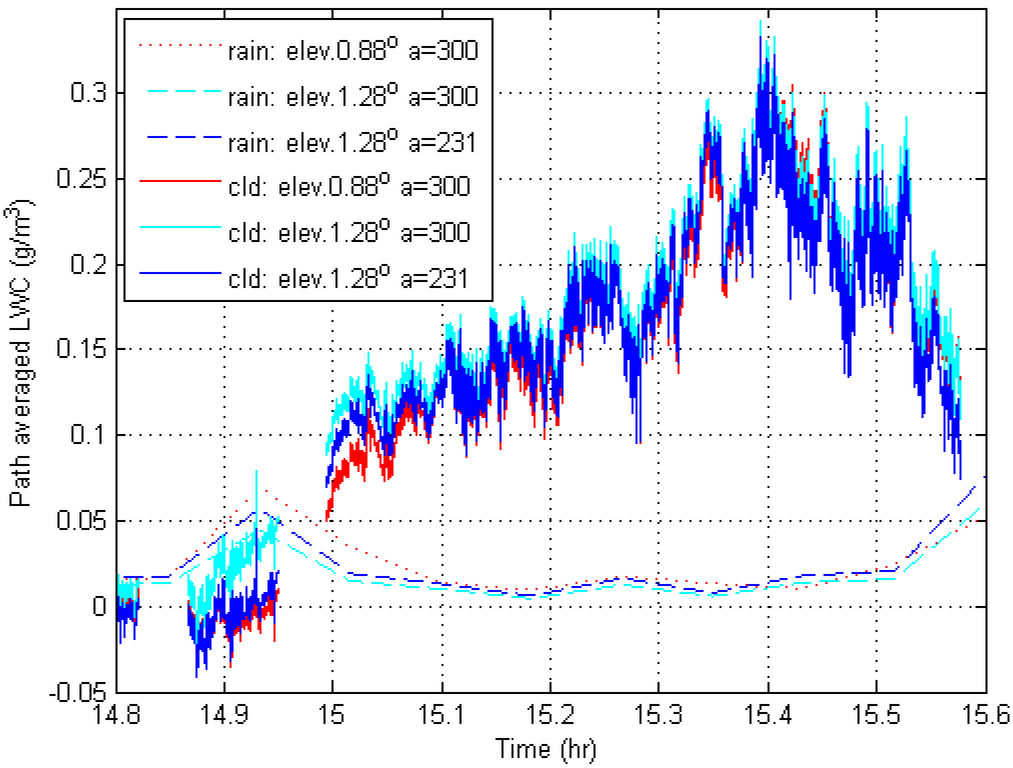



Figure 18. Estimated average liquid water content in the rain and clouds along the 5.4 km path between the mountains.

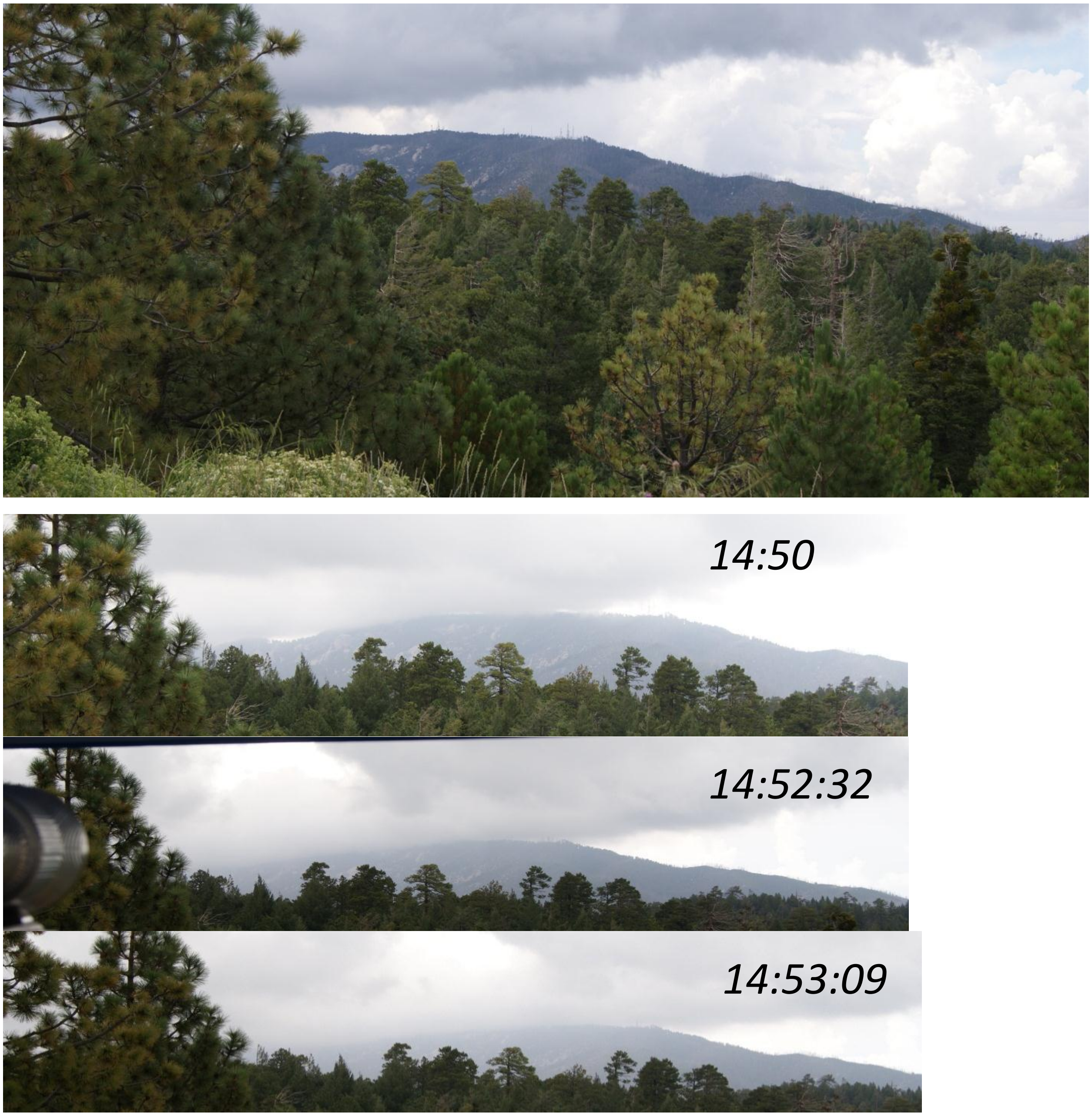



Figure 19. View of Mt. Lemmon from Mt. Bigelow at visible wavelengths showing the evolving opacity at the onset of the storm.