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Atmospheric Measurement Techniques
Discussions
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Interactive comment on "The feasibility of water vapor sounding of the cloudy boundary layer using a differential absorption radar technique" *by* M. D. Lebsock et al.

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Comment:

There are two studies that I'm familiar with on the use of radar for estimating water vapor that are not cited but that are relevant to the present study. . ..

Response:

Thanks for informing us of these papers. Both the Ellis and Meneghini studies have been cited. The meneghini paper in particular is using the same approach on a different water vapor line.

Comment:

One of the things missing from the analysis in the paper is the recognition that, in general, the differential Z is directly related to the characteristic size parameter of the raindrop/cloud-drop distribution. Of course, if all the hydrometeors are Rayleigh scatterers then Z is just the 6th moment of the size distribution and the difference is zero. However, for precipitation sized particles, and for the frequencies that are being considered, I would guess that the differential Z is significantly different from zero. Many of these issues could be addressed by computing delZ = dBZ(upper freq)-dBZ(lower freq) versus D0 (median mass diameter) or Dm (mass-weighted diameter) for the various pairs of frequencies that are being considered. If a gamma distribution size distribution is chosen, the shape parameter must be specified; the number concentration parameter, however, will be independent of the DFR. (An alternative would be to sort the data in the radar model according to D0/Dm and then plot the delZ versus D0/Dm results.). The simulated results do seem to show evidence of this type of non-Rayleigh scattering error (Fig. 7). I think it also explains why the performance improves as the frequency separation decreases – since the differential non-Rayleigh scattering is being reduced. Although the authors consider this as noise and an error in the context of water vapor retrieval, it is an important parameter from the standpoint of estimating properties of the cloud/rain.

Response:

Of course the reviewer is correct that the reflectivity does depend on the hydrometeor size in the Mie regime that we examine. The effects are included in the model and are generally well known and understood (e.g. kollias, 2007). The attached figure shows the reflectivity difference sorted by mass weighted diameter as suggested by the reviewer. There are some signs of sorting by Dm as the reviewer suggested but this is complicated by the use of bin microphysics instead of bulk analytic distributions. We don't find this useful to add to the manuscript. The reviewer is correct that error due to non-rayleigh scattering is evident if Figure 7 and that it is reduced as frequency separa-

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tion is reduced. Plotting delZ versus Dm as the reviewer suggests is more complicated than it initially seems because delZ is mostly a function of the attenuation between the radar and the target volume. This includes water vapor attenuation and attenuation from hydrometeors, which is also a function hydrometeor size distribution in every bin between the radar and the target volume. We have shown the total scatter in delZ resulting from variability in the hydrometeor size distribution in addition to all of the other sources such as spatial inhomogeneity, temperature and pressure. We find the inclusion of all effects to be the most useful way to demonstrate the errors.

Comment:

If I'm interpreting Fig. 7 correctly, then the variability of the retrievals at the right edge of the plots corresponds to estimates made from surface returns. These should consist of two types of errors, the variability of the differential path attenuations caused by hydrometeors and the variability in the surface reflectivities. I also assume that the fraction of delZ contributed from the hydrometeors is always a positive bias. Is this correct?

Response:

The interpretation is correct. The reviewer is right that the error due to transmission through hydrometerors is always a positive bias. This occurs because the spectral dependence of both the water vapor attenuation and the hydrometeor attenuation is of the same sign. Figure 8 (Panels A-C) demonstrates the effect of hydrometeors on the water vapor-deltaZ relationship at the native LES resolution. However we see that once the signal is convolved over a realistic antenna footprint NUBF and hydrometeor attenuation have biases of different signs. In the convolved DYCOMS case (Panel D) the biases are all still positive, however in the convolved RICO case (Panel B) the bias can be either positive or negative due to the competing effects of NUBF and a spectral dependence of the hydrometeor attenuation. Section 4.4.2 describes these results. We have added some wording stating 'First notice in Panels A and C, which show the results at the native LES resolution, that the effect of condensed water is to result in a positive bias in the CWV that would be inferred from ΔZ0.' In this section.

Comment:

Although eq. (4) is correct it should be noted that the eta's are equal to the integral of the backscattering cross sections of the hydrometeors integrated over the size distribution; similarly, the kappa(hydro) are equal to the extinction cross section of the hydrometeors integrated over the size distribution. Only for Rayleigh scattering are these quantities inversely proportional to the fourth power of the wavelength (eta) or directly proportional to frequency (kappa(hydro)). The paper does not mention ice clouds but there seems to be no obvious reason why the method would not work for ice clouds as well as water clouds. The authors state that the system is optimized for low level water clouds but with a -35 dB detection threshold, it seems that many ice clouds will be seen as well.

Response:

We've added wording on page 5977 explicitly stating that the eta and kappa_hydro are integrated over the hydrometero size distribution. At no point in the paper do we make any assumption of Rayleigh scattering. The last paragraph of the paper actually does mention high altitude clouds. Of course the reviewer is correct that the technique would be applicable to any clouds including cirrus. As we state in the paper a future study will examine the applicability more generally in the atmosphere. One caveat to the extension to ice clouds is that channels that one would want to use would be closer to the 183 GHz line where the sensitivity is higher because the water vapor content is so much lower in the upper atmosphere than it is in the boundary layer.

Comment:

I would take issue with the definition the authors use for the dielectric factor when they state on p. 5977 that 'K is the dielectric factor of the target'. But if ice and water

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clouds are detected, sometimes in the same profile, how is K to be chosen? I think it is better to choose K to be the dielectric factor for water at a particular temperature. If an ice cloud is encountered then eta will be proportional to the dielectric factor so a KËĘ2(ice)/KËĘ2(water) will appear on the right-hand side of eq. (1).

Response:

I understand the reviewers comment. This convention is common in the radar meteorology community. However, to forward model the reflectivity then K is in fact the dielectric factor of the target. And this is in fact how we model the reflectivities using equation 1 with a temperature dependent K. It is not constant in our application. In practice if we had observed reflectivities then the reviewer is correct that we would have to assume a value for K and the observed receiver powers would be converted to a reflectivity using this assumption.

Comment:

Some discussion on the radar characteristics would be useful. Are matched beams important? Would this be a nadir-looking radar or would it be scanning?

Response:

Matched beams are important. We've added some clarity on this point in section 3.3 'Applying the Radar Model to the Cloud Simulations'. The paper assumes a nadir pointing system. In practice the technique could work for scanning as well if one could tolerate that sensitivity loss that would be commensurate with scanning. This level of specificity on the is way beyond the scope of this paper however and would be a function of a complicated trade space involving sensitivity, sampling, and cost.

Comment:

Fig. 4: The surface reflectivity depends on incidence angle and surface type. Are ocean background and nadir incidence being assumed?

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Response:

Yes nadir is being assumed. See response above.

Comment:

Data from the JPL APR-2 radar and the GPM-DPR, which operate at Ku/Ka-bands, show that the normalized radar cross sections of the surface, in rain-free areas, are highly correlated. This provides a stable reference against which the differential attenuation, caused by precipitation along the beam, can be estimated. For the application here, it would be differential attenuation from water vapor. Do the surface reflectivity models used here have any correlation properties (with respect to frequency) associated with them?

Response:

Yes the model does have this correlation. It is essentially a result of the slow variation in the dielectric constant of water with frequency within the frequency range examined. The models are described in the referenced literature (li et al., 2005; Freilich and Vanhoff, 2003) described in section 3.2 ‘Radar Model’. The analogy made by the reviewer is exactly correct. The DPR can use differential attenuation to constrain precipitation whereas in this case we propose to use it constrain water vapor.

References: P. Kollias, E. E. Clothiaux, M. A. Miller, B. A. Albrecht, G. L. Stephens, and T. P. Ackerman, 2007: Millimeter-Wavelength Radars: New Frontier in Atmospheric Cloud and Precipitation Research. Bull. Amer. Meteor. Soc., 88, 1608–1624. doi: http://dx.doi.org/10.1175/BAMS-88-10-1608

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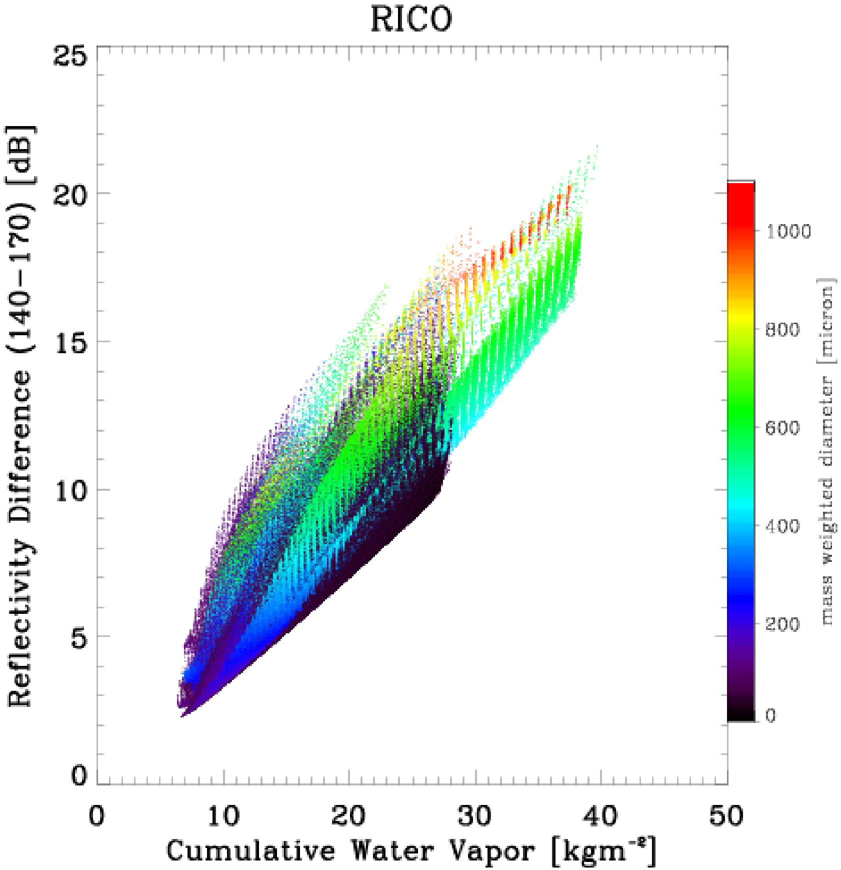

Fig. 1.

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