Ground-based retrieval of continental and marine warm cloud microphysics, Martucci and O'Dowd, AMTD, 2011

Reviewed by Jeff Snider, University of Wyoming, Laramie, USA

Overview -

The authors describe mult-sensor retrieval of low cloud properties. Clearly, much work has gone into the data collection and analysis; I applaud the authors for that effort. However, I see far too many errors to allow me to recommend this manuscript be advanced. Rather, I feel that the manuscript needs to be substantially reworked.

Major Criticism –

1) Section 3.4 - Your explanation of the retrieval of N (Equation 6) is confusing. While it does seem that the least-squares minimization described by Boers et al. (2000) is employed, your mention of a "curve-fit" suggests something different. I recommend that this explanation be clarified. In addition, the reader needs to be told, in this section, that alpha is not known. Also, since your Equation 6 is a repetition of an equation presented in Boers and Mitchell (1994) and in Boers et al. (2000) this needs to be explicitly acknowledged. Finally, it seems that Equation 6 translates all of the subadiabaticity into a decrease in droplet concentration, while leaving droplet size unaffected mixing (e.g., Equation 24 in Boers and Mitchell, 1994). Since entrainment and the resulting subadiabaticity can affect both number and size, your assumption needs to be acknowledged and tested in the error analysis.

2) P4831-L22 - The lidar is 1 um, so you need to be at a diameter of ~10 um for the Q=2 assumption to be reasonable. I see that you examine sensitivity to that assumption in the error analysis. Related to this, Figure 3 in the O'Conner et al. reference indicates that for D<10 um the assumption of constant extinction/backscatter introduces bias. In your work, how substantial is bias coming from the extinction/backscatter=constant assumption?

3) P4833-L12-14 – In most (all?) shallow cloud applications, the Asat is taken to be a constant set by estimate of temperature and pressure at the cloud boundary. I am not aware of a need to account for the temperature/pressure dependence of Asat through the cloud. This needs to be justified, especially in light of the fact that the cloud depths are relatively small (100 to 500 m).

4) Equations 2, 3, 4 and 5 - Because Equation (2) is developed in an atmospheric thermodynamics textbook and in the peer-reviewed literature - e.g., Albrecht et al. (JGR, 17, 89-92, 1990) - I see no reason to present these equations. There are several other factual errors in this section, and I would be glad to elaborate, but I feel that this part of the paper needs to be condensed by referencing to the original work, specifically Boers and Mitchell (1994), Albrecht et al. (1990) and Boers et al. (2000).

Other Comments and Criticism -

Abstract - “The large reff of the marine case was determined by the contribution of drizzle drops (large radii and few occurrances) and in fact the modal radius was reff = 12 um (smaller radius and large occurrances).”

Comment - It seems that the _average_ reff is large because the frequency distribution of reff is positively skewed (average > mode), but I am uncertain. I suggest that you not go into so much detail in the abstract. Looking ahead at Figure 9, I have a question. Are not the middle two panels the normalize frequency distribution of reff? If yes, the Y-axis should be labeled accordingly. If no, the explanation in Section 4.3.2 needs to be improved.

Abstract - Suggest that you round the N uncertainty to +-1 per cubic centimeter.

Abstract - Does it make sense that the 10-90% LWC range is much larger for marine but the LWC standard deviation is the same for continental and marine?

Super-saturation or supersaturation?

P4827-L8 – Don’t you mean to say “…15% of thermal radiation emitted back to the Earth’s surface.” Also, I would not define “cloud forcing” in this sentence. Rather, I would make that definition in a subsequent sentence. Also, please check the 15%.

P4827-L17 – I don’t concur with your assertion that because the “cloud” GH effect is large relative to CO2, microphysics has received emphasis. As far as I know, thermal emission is primarily sensitive to water path, not to how the condensate is distributed as a function of hydrometeor size. Because of this, I assert that the sensitivity of cloud albedo to aerosol was a significant motivation. Of course, the possibility of precipitation modification was another one.

P4827-L26/27 – Sentence should be reworded for clarity.

P4828-L8 – This set is not independent…LWC(z) and reff(z) imply knowledge of the second moment as function of z, the extinction coefficient as function of z, the optical depth and hence the albedo.

P4828-L9 – Albedo controls the amount absorbed? Perhaps, but I can imagine a situation (broken marine cloud) where the fraction not reflected by cloud is not absorbed.

P4828-L17/20 – CCN increase with what? The activation is generally thought to be independent of the cloud subadiabaticity, although Morales and Nenes (JGR, 2011) have a different take on that. Also, CCN activity spectrum (aerosol size distribution and aerosol composition) and cloud updraft are important determinants, but you don’t mention either.

Acronyms should be defined on first use?: RADAR, LIDAR, GAW, etc.

Isn't there a reference describing the Mace Head site?

E-band radar?

Regarding the Hudson et al. (2010) reference. This is standard cloud physics, there are extensive discussions in Pruppacher/Klett and in the Rogers/Yau textbooks.

"atmospheric-attenuated backscatter" – Can't this be shortened to "attenuated backscatter", provided we accept that clouds are a component of the atmosphere? It seems the lidar literature typically refers to this as attenuated backscatter. Related to this, there is a range correction, an overlap correction, and the removal of the molecular scattering that needs to be applied to the lidar detection to derive the attenuated backscatter. Are all three of these corrections discussed in THT?

P4831-L12 – You seem to be implying that the MWR reports temperature at cloud base. This needs to be explained in the methods section.

Section 3.2 – Lidar Calibration – The basis for the calibration is return from molecular backscatter. Why discuss the calibration with a sun photometer?

P4832-L13 – The lidar and ceilometer are the same thing, right? Why use "lidar/ceilometer" here, and why repeat CHM15K?

P4833-L1 – LWC is proportional to the _concentration_ of cloud droplets.

P4839 – Where do you present the RH or absolute humidity? I don't see this data presented, yet it gets a page of discussion. I recommend that this part be omitted.

Section 4.2.1 – Here you need to acknowledge the fact that the derived ss is influenced by droplet removal due to entrainment and coalescence scavenging (drizzle formation). As a consequence, the ss you derive will be an underestimate of the value of the maximum(ss) reached during activation. Jim Hudson has written extensively on this and I recommend his work.

P4842-L5 – Since you are dealing with active sensing of reflection, both in the near-IR and in the microwave, you should specify which reflectivity.

P4842-L16 – Be careful here….it is condensational growth, possibly some coalescence, which leads to large droplet radius. Not hygroscopic growth.