Review of paper: 'Investigation of the adiabatic assumption for estimating cloud micro- and macrophysical properties from satellite and ground' by D. Merk1, H. Deneke1, B. Pospichal2, and P. Seifert

I am reviewing the revised version of the above paper. I think this paper contains some interesting science but attempts to over-achieve by doing two things at the same time: (1) attempting to get at cloud adiabaticity from different combinations of ground-based measurements, and (2) comparing ground-based to space-borne measurements. As a result neither topics (1) nor (2) are fully developed.

As for topic (1) I think the combination of radar and passive microwave radiometers is interesting and the comparison between two approaches (combining radiometer and radar versus the method following Illingworth) forms a solid basis for a paper. I wish the authors had fully developed just this idea and addressed various remaining open issues including for example the variability in k, which will have a huge impact on radar reflectivity because k drives droplet spectrum dispersion. (There's a relatively new paper by Parol and Brenguier addressing variability in k in much detail). Another interesting variable is the shape of the N profile and rthe impact of homogeneous versus inhomogeneous mixing. The authors are aware of the issue but dismiss it a bit too readily (around line 280). I would be interested in seeing the impact of different mixing assumptions on Z and thus on the theoretically derived N in the Illingworth approach.

Topic (2) is the comparison satellite against ground-based and space-borne observations. This is also interesting but the paper falls short of providing any general conclusions because of the limited nature of the case studies. The paper also completely ignores recent results by Suzuki et al., Zhibo Zheng et al., Di Girolamo et al., Maddux et al., and possibly others dealing with uncertainties and systematic errors in satellite retrievals of effective radius and other cloud parameters.

In summary, this is a paper with potential but as it stands it is falling short of what it could be.

My recommendation would be the authors take a step back and fully address topic (1), which could make for an excellent self-standing paper.

Once the ground-based approach is squared away the satellite/ground-based comparisons could be addressed.

Couple of minor issues I noted reading the paper.

Page 4, line 262 pp

Although Nd may vary vertically, it is commonly suspected that it stays nearly constant throughout the vertical column of a nonprecipitating cloud (Bennartz, 2007; Brenguier et al., 2000).

The two papers cited here are not quite relevant here. Both are dealing with remote sensing applications. There are various papers out there showing that N is actually constant vertically through large parts of Sc clouds. Search for Brenguier, Rauber, Pawlowska, and other papers using aircraft experiments….

Page 4, line 270 pp, Eq (10) and Eq (14)

While Eq (10) does give N, it is overly sensitive to large droplets embedded in the cloud, b/c $Z \sim r^6$. This severely limits the use of radar data for this purpose as especially in clean clouds there are always some large droplets available. The same applies even more so to Equation (14). I see that lateron the authors mention in passing that the maximum radar reflectivity did not exceed -20 dBZ (end of page 6), so that likely there was no drizzle present.