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Atmospheric Measurement Techniques Discussions

Interactive comment on "Radar-radiometer retrievals of cloud number concentration and dispersion parameter in marine stratocumulus" *by* J. Rémillard et al.

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Reply to Reviewer 4

Major Comment:

A fundamental assumption in the new method is that cloud droplet number concentrations vary with height and the dispersion – as measured here by log(sigma_g), the logarithm of the geometric mean radius of a log-normal distribution – is independent of height in the cloud. They do cite one reference (Miles et al. 2000) to justify this approach, but the assumptions are highly questionable. The authors only consider *non-drizzling cases and stay away from cloud edges, so the only microphysical processes at play should be condensational growth and evaporation. But it is a textbook principle in cloud physics that as a distribution of cloud droplets grows through condensation, the distribution narrows, and conversely broadens during evaporation. At best – in terms of the assumptions here – the standard deviation of the size distribution is constant when surface tension is taken into account (Srivastava 1991), but even in that case the relative dispersion decreases with height in a cloud layer. (Relative dispersion is not identical to log(sigma_g), but the same sort of vertical trend holds.) It is not difficult to find in-situ studies that show relative dispersion and its variants increasing with height in stratocumulus clouds, for instance Wood 2000 and Lu et al. 2007, but there are many others. Wood 2000 also shows that the k parameter, which can easily be cast in terms of log(sigma_g), increases with droplet mean volume radius, which increases with height in a non-drizzling cloud. Similarly, Geoffroy et al. 2010 show that sigma_g increases with cloud water content, which also increases with height in stratocumulus clouds. It is easy to show that these two studies agree that log(sigma_g) should decrease by about 30-40% over the depth of a typical stratocumulus cloud. It is also widely found (perhaps starting with Nicholls and Leighton 1986) that droplet number concentrations are roughly constant with height in stratocumulus clouds, at least away from the edges, which is the only region analyzed here. It seems rather problematic that the new retrieval method makes assumptions that contradict first principles and many measurements that support them. Such a surprising assumption merits an extensive justification, in my view. Are the retrievals of droplet concentration and cloud optical thickness different if the more conventional assumptions of vertically uniform droplet concentration and dispersion increasing with height?*

Authors Answer:

The reviewer raises important points here. First, it is true that N_{cld} is usually observed to be roughly constant with height in marine stratocumulus clouds. The technique developed here is still strongly based on that: although vertical variations are allowed, they are requested to be the smallest possible through the minimization process. Such small variations are inherent to the observations, and could have been caused by mixing inside the cloud. Second, the vertical variability of the width parameter is arguable. Different observations report different behaviors (decrease, increase, constance). Brenguier et al. 2011 have a nice discussion of the observed variability for the coefficient k. For instance, they show a stratocumulus case where k appears constant with height, despite some intra-cloud variability (especially near the base). Overall, due to its wide range of observed behavior, it was decided to keep the width parameter constant with height. However, it is our belief that the vertical variability of the width parameter has a limited impact on the retrieved values. Testing is underway to verify the validity of this assumption, and to quantify the extent of the impact this assumption has on the retrievals.

Minor Comments:

1. *I would insert "non-drizzling" in the title before "marine" since it is such a fundamental limitation on this method.*

Authors Answer: That will be added, as it seems to be a concern for everyone. It should be noted though that the limitation is rather about the knowledge of the cloud contribution to the measurements (which is straightforward in nondrizzling clouds).

2. *As I understand it, the F95 method works on drizzling and non-drizzling clouds, so "drizzling and" should be inserted before "nondrizzling" [sic] in line 4 on p. 7508.*

Authors Answer: The F95 method referred to here only works for nondrizzling, all-liquid clouds (see for instance a discussion in Shupe et al. 2005). It is another, similar, technique that Frisch et al. developed in the same paper for the drizzle-dominated clouds.

3. *I would mention evaporation in downdrafts on line 15 of p. 7509.*

Authors Answer: The revised version will include that.

4. *The sentence starting on line 21 of p. 7509 should be removed, since drizzle occur-*

rence does not limit the applicability of the F95 technique, which retrieves parameters for cloud droplets and drizzle drops in the same column.

Authors Answer: The paper written by Frisch et al. in 1995 described two techniques: one to retrieve drizzle parameters when a cloud is drizzling, and one to retrieve cloud parameters when drizzle is much less significant. For instance, when they start their discussion of the results for cloud parameters, they mention that for parts of the analyzed period, “there was drizzle, which invalidates our calculation assumptions for cloud properties”. Subsequent work by Frisch and coauthors referring to this paper makes this limitation even clearer.

5. *In section 2 there should be references provided for the instruments used.*

Authors Answer: The revised paper will include references.

6. *The Wood and Hartmann 2006 expression for effective radius is not the column average, as described here. Instead it is the cloud-top value, which Wood and Hartmann needed because they were using remote-sensing measurements from above. This misinterpretation likely has a bearing on the retrieval comparisons that appear later in the manuscript.*

Authors Answer: The reviewer is right. This was an oversight, and it will be corrected accordingly in the revised paper.

7. *Confusing that on p. 7511 that “the liquid is distributed in the cloud layer using the Frisch et al. (1998) method” when so much in this new method seems to involve the profile of cloud water. Why is the new method not used to distribute cloud water vertically?*

Authors Answer: This will be clarified in the revised version. It was done this way to enable a first-order correction of the radar reflectivity factors for liquid attenuation, which can be substantial for marine clouds. Further fine-tuning of the LWC profile will have a much smaller impact on the reflectivity values, and is therefore included in the errors.

8. *The last sentence of section 2, regarding two-way attenuation, needs some further explanation or at least should cite a reference.*

Authors Answer: The revised version will precise that these attenuation values were obtained by applying the techniques just described (or in the cited papers) to the two cases considered in the Azores.

9. *Definite integrals require upper and lower limits, throughout.*

Authors Answer: Such limits will be added for the revised version, where appropriate.

10. *Unclear what is meant by "vertical resolution of in situ measurements is usually coarse". I can only guess that statement applies to coarsely averaged measurements. There is nothing intrinsic to in-situ measurements that makes them vertically coarse.*

Authors answer: This sentence will then be rephrased. What was meant is that the vertical dimension usually comes from different passes in the cloud (and typically 3, with one near the base and one near the top). Therefore, the height difference between measurements (and time resolution) appears coarse to conclude an invariance in height.

11. *I can't make sense out of the sentence immediately following equation 13. Perhaps "appears" should be "disappears"?*

Authors Answer: That sentence will be removed. It was a side note that simply emphasizes the appearance of Nr_0^3 in the denominator (which relates to the LWC). It does not relate in any way to the derivation of the equations.

12. *It is stated the the profile of N_cld must remain close to its column-averaged value. It should be stated how close – one percent, ten percent, what?*

Authors Answer: This will be better explained in the revised version. This criterion is based on the observations showing that, in general, the variability of N_{cld} in a cloud column is relatively small. The minimization step is used to find the average value for N_{cld} that provides the best closeness, or in other words, that assures the less variability in the column.

13. *It should be stated how rogue radar volumes are defined and how close to the edge is too close, and how cloud edges are defined.*

Authors Answer: The reviewer is bringing a good point. The cloud base is defined by the ceilometer, and the radar gate that corresponds to that height is not considered in the retrieval. The cloud top is defined by the cloud radar. Since the derivative of the reflectivity is used, the first and last gates where the PSD is retrieved (CB+1 and CT) does not have such a measurement, and are thus "rogue volumes" by default. Other "rogue volumes" are those that have a negative Z derivative (actually < 0.02 dB/m). This will be included in the revised version.

14. *The description of the treatment of mixing above the reflectivity profile on p. 7516 is too brief to be understandable. Further description and justification are needed.*

Authors Answer: The reviewer has an excellent point. This will be further described and justified in the revised version.

15. *The wavelength of the radar should be provided as should the vertical and temporal resolution of the retrievals.*

Authors Answer: The WACR from the AMF deployment at Azores is used: 94 GHz (3.2 mm), with 2-s time resolution and ~43-m vertical resolution. These resolutions are kept for the retrievals. This information will be included in the revised version, although it does not impact the retrieval technique.

16. *It seems odd to refer to LWP that varies by a factor of two over a couple hours as being stable. I would describe that instead as varying substantially.*

Authors Answer: The reviewer has a good point. Although the LWP varies substantially, it did so in a smooth manner, increasing somewhat gradually from 50 to 100 $\mathrm{g\,m^{-2}}$

over about 20 min, and decreasing back to 50 $\mathrm{g\,m^{-2}}$ in about another 20 min. "stable" was referring to this smooth character of the variations.

17. *In the text or caption of fig. 1 there should be an explanation of why the radiometer retrievals twice disappear for a few minutes.*

Authors Answer: This is for calibration purpose: these are periods when the MWR is doing "tip-cal" measurements to calibrate its temperature retrievals. This will be mentioned in the revised paper.

18. *Given that the two optical thickness retrievals are independent, I don't understand this statement: "since the LWP variability drives to a large extent the [cloud optical thickness] variability, it is not surprising that the radar-radiometer derived and the shortwave derived optical depths agree in the observed scales of variability". If the idea is that it is not surprising that both retrievals appear to agree so well, why is that not surprising? Because it was obvious from the start that both retrievals would be correct and thus agree? To many readers this may be quite surprising.*

Authors Answer: The revised version will clarify this statement. Instruments used in both techniques are equally sensitive to LWP variability. If for instance, the sizes had been the driving factor instead, the radar-based technique would have been biased toward the larger particles impact, while the shortwave-based technique would have been more sensitive to the smaller particles contribution.

19. *I would insert "and dispersion" after "radius" on line 25 of p. 7518.*

Authors Answer: Although it is true that the optical depth relationship with LWP is more complex than simply depending on the radius, the objective of the scatterplots and regression lines was to compare the spread of the relation widely used in satellite studies: $\tau = Cst \times \frac{LWP}{r_e}$. However, it will be specified that the dependence is on a characteristic effective radius.

20. *Why are the effective radius retrievals so different between the NFOV and the new*

AMTD
5, C4078–C4085, 2013

Interactive Comment

method, particularly when the optical thickness retrievals agree?

Authors Answer: This has been discussed briefly in the results section when applicable. A supplemental point is that the NFOV–MWR-retrieved radius is likely closer to a cloud-top effective radius, making it bigger than the column-averaged one.

21. *How is effective radius computed for the NFOV retrievals, given that the paper describing the NFOV retrievals does not mention effective radius retrievals?*

Authors Answer: The effective radius reported here for the NFOV is retrieved using the LWP from the MWR and a typical relation for satellite studies (providing a cloud-top r_e). This is mentioned in the observation section (p. 7511 / l. 8), albeit too briefly to be remembered in the results section. It will be made clearer in the revised version.

22. *Some explanation or justification or both are needed for this statement: "the statistical LWP retrieval was used instead of the physical one". Is this only for the 29 June case or for both cases? Why not use just one LWP retrieval method throughout?*

Authors Answer: The revised paper will clarify this statement. The physical retrieval gives more robust results, as it uses input from other sources to constrain the retrievals. It also has a slightly better time resolution. However, the results for the 29 June case were not usable, as too often the LWP retrieval failed. For the 13 June case, LWP from both methods were available, showing no strong discrepancies between them. Although the paper would be simpler to follow if only one MWR retrieval was used, the presence of the physical one allows the use of a smaller uncertainty on the LWP for the error propagation part.

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