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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A novel approach to characterize the variability in mass-Dimension relationships: results from MC3E" *by* Joseph A. Finlon et al.

Anonymous Referee #2

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General comment

The manuscript presents a new method developed to characterize the variability of m-D power law relationship coefficients a and b. The technique minimizes the chi-square difference between TWC and Z derived from a combination of OAPs and corresponding TWC and Z directly measured by a Nevzorov probe and an S band radar. All a and b within a specified tolerance were regarded as equally plausible solutions (EPS). A chosen chi-square criteria (though questionable) is used to produce most likely solutions (minimization of chi-square in a-b contour plot) for a and b out of all EPS.

Overall, the presented method is smart with potential to retrieve adequate and most

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Discussion paper

likely a and b coefficients for whatever specific data set, subset, temperature range, stratiform / convective cloud region, cloud type, etc...

However, instrumental shortcomings (S band radar, 2D-C with 32 photo-diodesof 25μm resolution?) and uncertainties need to be considered. Also authors are facing poor data statistics (used data < 1.5 hours of sampling with flight legs between -5°C and -35°C, only three cloud systems, with 20 May cloud not comparable in temperature to two other clouds) due to necessary co-location with ground radar. The lack of data statistics does not allow to assess reliable results for underlying data set. The results of most likely a and b coefficients for this limited data set may not be statistically well grounded to be used for model and remote sensing applications.

The question arises, if it wouldn't be better to more precisely present the method, even refine the method, where mathematical basis is missing, then applying the method to a limited dataset, just to demonstrate how the method works, without having to deal with uncoherent results due to lack of statistically valuable data. An idea could be to publish solely the method with application illustrations (AMTD ?).

Main major points

Method : - Manuscript needs to document equation illustrating how Z is calculated from PSD. - The authors have to quantify and discuss S band radar reflectivity factor sensitivity as a function of crystal size. - Don't use B&F for MMD_max calculation (Fig 11, etc... and respective arguments in text). B&F has been retrieved for mean chord length size definition and therefore would necessarily overestimate TWC, real MMD_max, etc...when using PSD in D_max definition. - Either the manuscript has to derive mathematically how TWC_diff (equ 1) and Z_diff (equ. 2) can be merged into a sum or likewise into chi-square (equ.3) both to be minimized subsequently (Fig 2). As it stands, we feel that differences (measured, calculated from PSD) of the square root of Z may be a bit comparable to respective differences in TWC, in order to merge those two terms. Of course this is scientifically insufficient... The authors may think about sensitivity studies for table 1 data to know which term controls chi-square (equ 3), as a function of different flight legs and how this evolves during individual flight legs. - Poisson statistics doesn't take into account systematic uncertainties in measuring TWC from Nevzorov and not systematic errors in Z measurements. Within a homogeneous cloud segment the statistical counting uncertainty is small, but systematic measurement uncertainty can be of the order of 50% or 100 % .... This has to be taken into account instead of Poisson statistics that seems serving to get rid of the uncertainty discussion... ? To resume, the tolerance of EPS has to take into account real uncertainties and not just Poisson statistics / natural variability. - Recommendation to limit b to 1-3. As a consequence delete everything from pg 7 line 20 to pg 8 line 21, since this discussion finally does not contribute to the study. I don't see the physical meaning to go beyond b=3 of a sphere, mathematically of course one may not care about an interpretation. - - Data / instrumentation / uncertainty consideration - Use PSD number uncertainty of 50% for larger particles and calculate associated TWC_SD uncertainty. - Likewise Z_SD uncertainty from Z equation that you have to present. See comment above. - How supercooled water has been quantified? Excluded from analysis? Be aware of the fact that Nevzorov will not correctly quantify LWC when IWC is more or less dominating TWC. - Please document average number and mass PSD (additional figure!) for table 1 flight sequences. - Nevzorov TWC uncertainty: In literature several times has been documented that 2DC+2DP is matching the shallow Nevzorov TWC. Likewise for the deep cone in other publications. This illustrates that a and b coefficients can be adapted to match TWC from instruments, but it does not prove that shallow or deep cone Nevzorov collect ice at 100%. May be something between 50-100% for the deep cone. Check literature. Thus, the manuscript has to take into account possible systematic uncertainties of PSD number, Nevzorov TWC, and also measured Z impacting the tolerance of EPS and finally guiding recommendations of most likely a and b coefficients.... - Figure 9: if the underlying dataset would be statistically a little more representative (which is certainly not the case) wouldn't we expect more organized colours (contour plot gradient) perpendicular to the diagonal line of the grey matrix elements? - 20 May flight: these are solely 'low altitude' data with T>-23, whereas for the other two flights used in this study, temperature T<-22. What is the argument to choose the 20 May flight since limited comparison with colder temperature data of two other flights? -

Further Comments

1. Pg 1, line 18: It does not make sense to establish m-D relation in mixed phase clouds. We know m-D for water droplets. You should exclude all mixed phase sequences from data! 2. Figure 1 is not thoroughly documented with literature references and thus not reproducible. 3. Pg 4, line 14: redo analysis with CDP droplet probe not exceeding 10 cm-3 on a 1 s basis may be a more suitable approach. 4. Pg 4, line 18 ff: Assuming that S band radar reflectivity is not significant for sub-millimetric particles, what is the TWC percentile at 1mm of the cumulative mass PSD ? Please don't use B&F but for example Heymsfield 2010, etc.. retrieved for D_max definition. 5. Pg 8, line 14: 85 cm? 6. Pg 9, line 26: Use m-D relation, for mass content estimation, other than BF95 for D_max size definition. 7. Pg 11, line 5; Fig 11: Idem 8. Pg 15, line 15 idem 9. Pg 11, line 12; You mean just Fig 11 in this sentence? 10. Pg 11, equation 6: How sphericity results deviate when comparing two instruments of different pixel resolution having sampled the same paricle? And how the averaging of sphericity has been performed over all crystals of a crystal population? For OAP 2D images the sphericity is size dependent, partly due to bias of 2D projections sorted into size classes, with larger particles having smaller 2D image sphericity than smaller particles. 11. Pg 12 line 16: Why applying factor 0.6 in reference volume for retrieving effective density, impacting TWC???

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