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Atmospheric Measurement Techniques Discussions

Interactive comment on "Evaluating the capabilities and uncertainties of droplet measurements for the fog droplet spectrometer (FM-100)" *by* J. K. Spiegel et al.

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Final response to the comments from referee 1

Our replies are given in blue color between the reviewer statements.

A very thorough analysis is presented about the performance of the FM-100 fog droplet spectrometer for ground-based field experiments. Errors resulting from the non-monotonic Mie-scatter curve and from losses at the inlet (non-isoaxial and non-

isokinetic effects) are analyzed, recommendations for the future operation of the instrument are provided. Overall, the paper is well written, informative, and in a very good shape.

Reply: We thank the first reviewer for his or her comments, which helped us to improve the quality of the manuscript.

A few questions and suggestions should be addressed here:
In general, it would have been nice to see that the manufacturer of the instrument to contribute to the manuscript more substantially than just through personal communication. Probably, a few questions could have been addressed in more detail (some of them addressed below). There may be a good reason for this procedure, which is however not indicated.
Reply: There are different views among scientist, some find it important that scientists and manufacturers are independent from each other (even if they collaborate), others see it differently. We preferred an independent approach, but consulted them several times. We also suggested to them to place a short contribution to the discussion.

How is the instrument calibrated eventually? Is a theoretical computation, or is it a lab calibration?
Reply: The instrument has been calibrated in the lab of the manufacturer (DMT) using glass beads. Based on the glass beads calibrations the manufacturer performed Mie calculations to find out the correct opening angles of each individual instrument.
We have modified the related paragraph (p.3338 l.13-16):
The exact value of the scattering angles needed for the Mie calculations differs among instruments and additionally depends on where exactly the particle passes the laser beam (Lance, 2010). They are therefore one of the sources of uncertainty of the FM-100 that will be addressed in this paper.
to

The exact values of the scattering angles needed for the Mie calculations differs among instruments. Additionally, they depend on where exactly the particle passes the laser beam (Lance, 2010). They need to be derived from glass bead calibrations followed by Mie calculations to find the solid angle that fits best to the calibration results (personal communication, D. Baumgardner, 2010). They are therefore one of the sources of uncertainty of the FM-100 that will be addressed in this paper.

If the latter is the case, do glass beads scatter the light in exactly the same way as water droplets do?
Reply: Glass beads only differ from round pure water droplets in their refractive index; this is taken into account when deriving the opening angles from the scattering cross section.

How is the Mie scattering of droplets that are non-spherical during their travel through the instrument?
Reply: Unfortunately, Mie Theory is strictly spoken only valid for spherical particles, although numerical approximations exist. However, the scattering characteristics of irregularly shaped particles in the forward direction - as in our case - are rather similar to those of spheres with the corresponding diameter (Shettle and Fenn, 1979; Jaenicke and Hanusch, 1993; Liou, 2002). Moreover, the surface tension of a cloud droplet is high enough that the deformation from a sphere during the passage in the FM-100 is generally considered to be negligible. We therefore consider the used Mie bands as a reasonable approximation of the scattering behaviour of the droplets in the FM-100.

What is their travel velocity?
Reply: The travel velocity of the droplets corresponds to the sampling flow rate (TAS) and is around 15 $\mathrm{m\,s^{-1}}$. This will be added in footnote b of Table 1.

It would be nice to see the basis of Mie scattering calculation.
Reply: Mie theory has first been presented by Gustav Mie in 1908 and since then has been used to calculate the far field solution of the scattering and absorption behaviour of spherical particles. The scattering cross section is a function of the scattering angle θ, the complex refractive index m (which is 1.33 for water and 1.51 for glass beads) and the size parameter x (which is the particle diameter times π divided by the wavelength of the incident electromagnetic wave). We decided not to show the detailed calculations as we were trying to keep the paper as compact as possible. The well-known basics can be found in the corresponding literature (e.g. , Mie,1908; Liou, 2002; Bohren and Huffman, 2004; Van de Hulst, 1981). However, we will add the sentence on p. 3345 l.8:
The derivation of the scattering cross section as well as detailed calculations can be found in the corresponding literature (e.g., Mie,1908; Liou, 2002; Bohren and Huffman, 2004; Van de Hulst, 1981).

The curve (Fig. 3) applies for the 658 nm laser light. How large would the difference be for other wavelengths? See also p 3357 l 20/21.
Reply: The wavelength is only one of three variables. If the wavelength changes e.g. from 658 nm to 685 nm (which is the wavelength that is mentioned in older versions of the Fog Monitor manual) and the opening angles are kept constant, the Mie band slightly moves towards larger droplet sizes (see Fig. 1). However, depending on the ambient droplet size distribution the effect for the rebinning could be within measurement uncertainty (due to the uncertainty of the opening angles).

p. 3340 l 17-18: “fairly undisturbed”: It seems that there are limitations caused by the massive building on top of the mountain. Aren’t there any concerns about potential disequilibrium effects through flow distortion? Where was the PVM positioned? Is it

feasible to assume that two instruments, located at some distance from each other under these conditions, yield the identical LWC?
Reply: The PVM-100 was positioned on the Eastern side of the Sphinx station (see Fig. 2 in the paper). A detailed analysis of the wind field (Ketterer, 2011) revealed that the prevailing wind direction during CLACE 2010 was North to Northwest (see Fig. 2). Under this conditions neither the FM-100 nor the PVM-100 were in the wind shadow of the building and we therefore assume that the cloud conditions were comparable during most of the sampling time. Moreover, a comparison of two PVM-100 during an earlier campaign (one at the actual PVM-100 position and the other one on the Western terrace of the Sphinx which is closed to the actual FM-100 position) revealed similar LWC. Consequently, we assume that the cloud conditions at both locations were comparable and that the influence of the Sphinx building on the cloud properties is within the measurement error. We however suggest to reduce the separation between such instruments in future studies to reduce the uncertainty in such comparisons.

section 3.1.2: For which time integration period was this procedure applied?
Reply: We applied the stochastic method to one minute mean values of the collected cloud spectra at Jungfraujoch as well as to two typical cloud droplet size distributions, a continental and a maritime size distribution.

p. 3359 l 3/4: the sentence is incomplete. Add “be” after “longer”.
Reply: Thank you very much for pointing out this error, we will rectify this in our revised version.

Final response to the comments from referee 2

Our replies are given in blue color between the reviewer statements.

General comments:

In the paper a very thorough analysis of the capabilities and uncertainties of the DMT fog monitor FM-100 is presented. Both the errors in droplet size that can arise due to the ambiguities in the Mie scattering and those in drop concentration originating from sampling biases (aspiration/transpiration/transport) are extensively studied. The way the results are resumed and represented is clear and concise. The paper is fluently to read but need some restructuring (see ‘Specific comments’). Altogether, I recommend the paper to be published after minor-major revisions (in the end I choose ‘major revisions’ since I like to see the paper again before publishing).
Reply: Thanks for this positive assessment of our manuscript.

Here, I’d like to give some suggestions that I think can help to further improve the paper (other comments are listed in the ‘Specific comments’ below).

(A) My first impression was that the paper does not present something new, since the misinterpretation of drop size distributions measured by light scattering are well known and widely discussed in the literature (see for example Jaenicke and Hanusch, 1993) and resorting the instruments particle size bins to avoid that is a common procedure for the respective instruments.
Reply: Thank you for this reference, we will include it in the paper to emphasize what is new and what has been done in our own work both in the abstract as well as in the introduction and the conclusions. Although resorting bins might be the standard for

most of the optical particle counters like the FSSP, it has only been applied once to the FM-100. We therefore think that it is important to introduce this to the community using the FM-100, as it could help to improve the respective measurements. Additionally, to the extent of our knowledge, in our work we go one step further than it has been proposed so far: first, we suggest bin widening, resulting in overlapping bins, where a minimum and maximum error estimate of the LWC can be deduced. Second, the stochastic approach allows a better reproduction of the ambient droplet size distribution. Both approaches can be applied after the measurements, and such allow to reprocess older data, while resorting the bins as e.g. done by Gonser et al. 2011 can only be done before the measurements when the FM-100 is configured.

Also sampling biases and their handling is well known and described, as is referenced in the paper.

Reply: We agree that sampling biases are well known and that they are well described. Nevertheless, for the FM-100 different groups apply different quality standards to their data concerning the positioning of the instrument towards the wind where a clear justification is rarely given. We therefore summarized the available formulas in order to show how large the losses are under certain conditions, and that e.g. rejecting data collected under large collection angles does not fully solve the problem as losses are considerable inside the FM-100.

I think this should be mentioned in the paper and it should become clear that the focus of the paper is to do an the error analysis for a commercially available instrument that is deployed often without taking care of these problems!

Reply: We will rephrase some parts of the introduction, the abstract and the conclusion to clearly state that the error analysis is the focus of the paper. We also will emphasize the given recommendations at the end of the Results and Discussion section.

(B) Following the previous point, I like to highly encourage the authors to emphasize more the new stochastic method they have developed to retrieve the drop size distribution from the original particle bins (Section 3.1.2 and Figs. 4 and 6). This is

a very useful contribution that might be applied by other groups (maybe you could provide the code in supplementary material?) and thus I think it should be mentioned in the abstract and conclusions of the paper.

Reply: We will rephrase the abstract (pp.3334 ll.4-15) to follow the advices:

In this work we focus on the error analysis of two key measurement uncertainties arising during cloud droplet size measurements with a conventional droplet size spectrometer (FM-100): first, we addressed the precision with which droplets can be sized with the FM-100 on the basis of Mie theory. We deduced error assumptions and proposed a new method on how to correct measured size distributions for these errors by redistributing the measured droplet size distribution using a stochastic approach. Second, based on a literature study, we summarized corrections for particle losses during sampling with the FM-100. We applied both corrections to cloud droplet size spectra measured at the high alpine site Jungfraujoch for a temperature range from 0 °C to 11 °C. We show that Mie scattering led to spikes in the droplet size distributions using the default sizing procedure, while the new stochastic approach reproduced the ambient size distribution adequately.

The following will be rephrased in the conclusion (p.3367 ll.17-19): *The best measurements of ambient droplet size distributions can be achieved by redistributing the measured scattering signal using the Probability Density Function approach.* to:

Moreover, we showed that a redistribution of the measured scattering signal using a stochastic approach (based on Probability Density Functions) leads to a more appropriate reproduction of the ambient droplet size distributions than conventional methods.

in order to highlight the stochastic approach.

The stochastic approach depends on the Scattering cross section and the upper (b_{up}) and lower (b_{low}) boundaries that were chosen by the user and is therefore instrument specific. The Matlab scripts specific to our instrument can be obtained from the authors.

(C) I think though it is useful to show all the equations for particle losses they are not new and can be found in the literature. Thus, I suggest to summarize them in an appendix. This would make the paper more compact.
Reply: We fully agree with referee #2 that the equations for particle losses are not new and can be found in the literature. However, we believe that they are needed in the paper as different formulas are in use for similar wind and angle regimes. So, we used our data in order to address the question which of the formulas were most reasonable to apply for the FM-100 (section 4.2 and 4.3.2). We therefore think that certain aspects of the particle losses need to be mentioned in the MS itself. In order to make the paper more compact we will shorten sections 3.2.1 and 3.2.2 and move the formulas to the appendix.

Specific comments:

1. I suggest some restructuring of the paper: (i) Insert directly at the beginning of 2 Instrumentation and site the first paragraph from page 3340, start with: The study to validate and compare the FM-100 with other instruments was performed in the frame of ... and continue with the text from page 3340: .... CLACE 2010 which took place at the Jungfraujoch (JFJ, 46 32 N, 7 59 E) situated in the Bernese Alps at 3580 m a.s.l., Switzerland (Fig. 2). Several intensive cloud characterization experiments have been conducted there for many years at different times of the year (e.g. Mertes et al., 2007; Verheggen et al., 2007; Cozic et al., 2008; Targino et al., 2009; Kamphus et al., 2010; Zieger et al., 2012). The aerosol measurements performed at the JFJ are part of the Global Atmosphere Watch (GAW) program of the World Meteorological Organization since 1995 (Collaud Coen et al., 2007). Long term studies have been conducted at the site, which indicated that the station is in clouds approximately 40% of the time throughout the year (Baltensperger et al., 1998). CLACE 2010 took place in June August 2010 (temperature range: -11 to 11 C) and its main aims were to

obtain an indepth chemical, optical and physical characterization of the aerosols at the JFJ as well as to investigate the interaction of aerosol particles with cloud droplets for improving the understanding of the aerosol direct and indirect effects. ... (ii) Rename 2.2 CLACE 2010 field experiment to 2.2 Instrumentation used for validation of FM-100 and insert directly after that 2.2.1 Particle inlets (iii) Remove the title 2.3 Instrumentation used for validation and rename 2.3.1 to 2.2.2, 2.3.2 to 2.2.3, 2.3.3 to 2.2.4, 2.3.4 to 2.2.5 (iv) Remove the title 2.4 Determination of the cloud periods and move the paragraph to directly at the beginning of 4.3 Implementation of the Mie corrections and the particle losses for the CLACE 2010 campaign (v) Rename 3 Methods: corrections for the FM-100 to 3 Methods: Sizing and counting corrections for the FM-100 (vi) Rename 3.2.3 Implementation of the corrections for particle losses for the CLACE 2010 campaign to 3.2.3 Application of the corrections for particle losses to the FM-100 (vii) Rename 4.1 The effect of the presented Mie correction to the channel widths of the FM-100 to 4.1 The effect of the Mie correction to the channel widths of the FM-100

Reply: We agree and have followed the suggestions given by the reviewer. This should now give a more adequate and clear structure.

2. Page 3358, lines 14-15: ‘The droplet size distribution for the default channels (n_{dft}) was shifted towards larger droplets for the continental size distribution (Fig.6a) ...’. Can you please explain why?

Reply: The continental size distribution has its maximum at a droplet diameter of 6.6 μm (Fig. 6a and p.3358 l.5). The Mie band at 6.6 μm fully covers channel five (81% of the band height) and extends to channel six (4% of the band height) as well as channel four (15% of band height). Following the modeling approach (p.3358 ll.9 to 11) this would mean that droplets with a diameter of 6.6 μm are recorded in channel five with a probability of 81%, and only with a low probability of 15% and 4% in channels four and six. Channel four has a default geometric mean diameter of 5.00 μm, five of 7.03 μm, and six of 8.68 μm. So most droplets are recorded as larger droplets than they were in reality. Due to the Mie curves this sizing bias is even more pronounced for droplets with diameters between 7 and 10 μm (e.g. a particle with a size of 7.7 μm leads to a Mie band covering channel five to eight and thus will be counted as a particle varying in droplet size from 7.03 to 11 μm) leading to the presented shift of the droplet size distribution.

3. Page 3358, lines 20-21: 'The distribution based on the Mie channels (n_{geo}) is plotted with horizontal error bars indicating the width of the new channels (Fig. 6a and c).' How would the size distribution looks like with the 'classical approach' to account for Mie-uncertainties? It would be good to see a comparison to the new approach!

Reply: we refer to the classical approach by using the term "default channels" which is depicted in pink in Fig. 6. The Mie uncertainties lead on the one hand to a shift towards larger droplets for the continental size distribution and to additional spikes for the maritime size distribution.

4. Tables and Figures:

(i) In addition to Figure 3, please add a Table listing the default and the Mie left, middle, right channel sizes.

Reply: We will include this table in a revised version of the paper.

(ii) Would be convenient for the reader if you explain again R_{V} in the caption of Figure 7.

Reply: We will change

For each velocity range of η_{asp}, one representative panel (values in brackets) is shown: ***(a)*** *moving air (U_0 = 5.24 m s^{-1} which corresponds to a velocity ratio R_{V} of 1.2)*

to

For each velocity range of η_{asp}, one representative panel (values in brackets) is shown: ***(a)*** *moving air (U_0 = 5.24 m s^{-1} which corresponds to a velocity ratio R_{V} =U_0/U=1.2)*

(iii) Figure 12: shouldn't the cloud residuals N_{cr} also be corrected for particle losses before they are compared to $N_{\mathrm{FM}}^{\mathrm{eff}}$?

Reply: In general, particle losses in the accumulation mode are considered to be rather small. Consequently, we only corrected the interstitial spectrum towards the total spectrum by a size dependent correction factor (p.3342 ll.21-24). However, we did not mention this in the graph and will therefor add it in the caption of the revised version of the paper.

Additional references that will be included:

References

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