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Atmospheric Measurement Techniques Discussions

Interactive comment on "A Phase Separation Inlet for Droplets, Ice Residuals, and Interstitial Aerosols" *by* Libby Koolik et al.

Anonymous Referee #1

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This paper presents a new sampling system consisting of mainly three different modules to separate and measure droplet and ice particle residuals and interstitial aerosol particles. Such a device is of imminent importance to study the aerosol cloud interactions for the formation of atmospheric mixed-phase clouds. The technical working principle of each module and thus of the complete sampling system is described and an attempt is made to demonstrate its applicability for an ambient cloud measurement. Therefore, the topic of the manuscript is vitally important and within the scope of AMT.

Unfortunately, there are three main negative aspects:

1. Unconceivable carelessness in the preparation of the manuscript

2. Insufficient consistency of the laboratory characterization of the SPIDER system

(including the inlet properties)

3. Incompleteness of the field measurement verification of the functionality and applicability of the SPIDER system

All this together leads to the conclusion that the manuscript is not appropriate for publication in the present form. The key points in this decision are, that 1) not any particle sampling efficiency for the three SPIDER channels are elaborated in the laboratory characterization and that 2) only measurement results of only one of the three SPIDER channels from the filed deployment is presented. This implies that further laboratory and field measurements are required to confirm a proof of principle. It is not clear how long it takes to carry out this additional and indispensable work, which leads to the rejection of the manuscript in the current state and not only to a decision of a major revision. On the other hand, in case the missing aspects will be added, the authors should feel encouraged to resubmit their work. In the following a long list of critical comments is written down that should explain the missing issues (and more) in detail as well as might serve as a red thread for the re-writing, which will hopefully happen.

To 1.:

a) Much more diligence in the scientific terminology is required. Throughout the text (including the title) the ambigious term "aerosol" or "aerosols" is used and should be changed by the clear term "aerosol particles".

b) Another weakness is the unexpected sloppiness in the paper. - The citation of Hiranuma et al. (2016) is found 2 times in the reference list and one time in a wrong way. - There is no information what kind of citation Koolik (2017) is about at all. - The figure caption of Fig.14 is incorrect. - Fig.12 shows 2 cloud periods, but in the text 3 cloud periods are mentioned. - Fig. 15 (a) contains 2 black data points that are not explained. - Mix-up of Table 1 and 2 several times in the text. - Figures with several panels are labelled with (a), (b), (c)....but in the text they are mentioned as A, B, C - The SF and PF of the PCVI of the SPIDER system are named ice crystal and droplet channel. Since no ice crystals or droplets are measured, these channels should be more precisely denoted ice residual and droplet residual channel.

To 2. And 3.:

Abstract:

L. 21: change to “deployed at Strom Peak Laboratory”

Chapter 1:

L.33: What should that be: the number density of liquid and solid water? It should be “the number density of liquid and solid cloud particles and mass ratio of liquid to solid water”

L.46: “become activated” instead of “activate”

L.61: “microphysical properties” instead of “microphysics”

L.68-72: This information is not needed for the objective of this work and could be taken out

L.78: “on the CVI design” or “on the design of the CVI”

L.79: It is not clear if the PCVI shown in Fig.1 is indeed the design from Kulkarni et al. 2011. In this work a PCVI with two exist lines for the PF is presented whereas in this work PF seems to exit the PCVI only by one line. The authors should explain this contradiction.

L.87: Friedmann et al. (2013) is a very poor citation and should not be used in such a manuscript.

Chapter 2:

L.100-105: This paragraph is really confusing for the reader.

1. The Hiranuma et al (2016) flows are given in L/min, but their own flows are given in

SL/min. This should be done consistently.

2. For the D50 of 9 μm the IF and CF rates are given, but for the D50 of 20 μm the AF/IF ratio. It is not possible to derive the ratio for the first case or the CF for the second case. This is annoying and should be done consistently. For the reader it would be best to mention all flows and the ratio for both cases.

3. And why only droplets or ice particles larger 10 μm could reach the SF? Is there a limit for the L-PCVI concerning the lowest possible D50 diameter? This statement needs to be supported by some arguments. It is totally unclear why the respective citations in the brackets should provide any answer to the L-PCVI performance.

L.120: It is stated that the L-PCVI IF was 42 SL/min. But looking into Table 1, which was referred to in L.105, it becomes clear that IF was 39.7 SF/min (IF= PF-CF= PF-(AF-SF)=43-10+6.7=39.7). This is not a big difference but this difference shows again the inattentiveness that is noticed through the whole paper. So, the authors should clarify this inconsistency.

Chapter 3:

A review of chapter 3.1 is not possible due to insufficient knowledge in 3D printing.

L.158-163: Whereas the second part of chapter 3.2 provides required information, the information in the first 5 lines is rather uninteresting and could be taken out.

Chapter 4:

Since the omni-directional inlet is one of the four components of the SPIDER system, the reader expects a quantitative technical description of this inlet as a first sub-chapter. Some words are spent in chapter 2 (L.96-99) but statements like “particles too large to follow the streamlines are lost due to impaction” are totally insufficient. An inlet sampling efficiency as a function of particle size, wind speed and L-PCVI IF must be presented to evaluate which cloud particles (with respect to size) could make it into the SPIDER system for further processing and which not. Theoretical calculations would be sufficient.

L.171: This paper refers the L-PCVI cut-off behaviour always to the AF/IF ratio and claims to be consistent with the results published by Hiranuma et al. (2016). But in this publication the cut-off diameters are always given regarding the CF/IF ratio, whereas indeed CF is the same as AF used in this work. But since also the term CF is used here, this is very confusing for the reader. Thus, the author should clarify this difference in the terminology regarding Hiranuma et al. (2016).

Moreover, it is still not clear if the flow settings in the actual work and in Hiranuma et al. (2016) are consistent. Because the ratio of CF (this work) or the effective CF (ECE) in Hiranuma et al. (2016) to the IF is decisive for the cut-off diameter. And since the SF (this work) and the output flow (OF) in Hiranuma et al. (2016) might be different (unfortunately the authors do not provide this essential information), the CF/IF and ECE/IF and thus the cut-off diameters are different, although AF/IF and CF/IF are equal. So the authors also need to clarify this point, to confirm that they are consistent with the cited publication.

L.180: Table 1 states a PF of 43.0 SL/min-1, but here it is written that PF is 42.5 SL/min. This is close but not the same. The authors need to check the applied flow and correct the text or table.

L.184-188: In order to improve the understanding of Fig.5, it is required to use the same units in Fig.5A and 5B. Using [cm-3] in both panels it would become clear how many of the incoming particles are transferred into the PF when AF is switched on and how many particles are lost.

L. 189-190: Again, without the information of SF it remains unclear whether the all cut-off relevant flows are identical to Hiranuma et al. (2016). Thus, more information is needed in the text by the authors again.

L. 198-199: Does this statement mean that SF was only 2 SF/min? Table 1 states 6.7

SF/min! And also Table 2, which is not mentioned in the text, but is declared as “L-PCVI Flow tests” states SF = 6.5 SF/min. This needs to be clarified, especially because 2 SF/min is not the SF flow of the measurements presented later in the text.

L. 202: Fig.6B: Viewed relatively, there are more larger particles observed at the outlet of the L-PCVI with counterflow than without counterflow? How could that happen? Is the AF more humid than the IF? The authors should explain this observation.

Nevertheless, it means that 40 μm droplets are already evaporated to 4.7 μm wet particles when they leave the L-PCVI, correct?

Comparing the y-scales of Fig.6B and Fig.6A, that would mean that there is a transmission efficiency of about 1%, and even more since the L-PCVI enrichment is not considered. Is that correct? If yes this has to be mentioned in the manuscript. If not, the authors have to explain the different y-scales.

L.205: The statement “essentially no particles are transmitted” cannot be confirmed by Fig.6C, because the y-scale is not appropriate. Indeed, it looks like that there are 4.7 μm particles in the same amount compared to the normal L-PCVI operation (IF and AF are on). Thus, Fig.6C has to be presented with a y-scale of Fig.6B.

L.206-207: The statement here that “these two experiments provide sufficient information to approximate the D50 of the L-PCVI” is again totally overblown. It was simply shown that there was one L-PCVI flow setting where the cut-off diameter was larger than 10 μm and another setting where the cut-off diameter was lower than 40 μm. That’s all. This cannot be described as an approximation of the D50 of those settings.

L.208: Again, a direct comparison of the flow ratios of this work and this of Hiranuma et al. (2016) is not legitimated as long as both studies uses a different output flow or sampling flow, respectively. See the reviewer comment for L.171.

L.208-210: For the reader it is totally unclear how these grey vertical bars and their size ranges in Fig.7 are motivated. The authors need to put much effort to explain this in the manuscript together with the other points already raised up for this sub-chapter. More or less it looks like that the cut-off diameters from Hiranuma et al. (2016) will be applied, so that the whole work described in this section was unnecessary. Again, the consistency of the x-axis is questionable.

L.221: The authors should precisely mention the diameter range of “large droplets”.

L.235-236: What is meant by “the dry L-PCVI” sample flow? What is its RH or dew-point? And a small calculation should be presented how the RH in the chamber will be reduced from 85% to 75%.

In section 4.2.1 it was shown that 40 μm droplets shrink to sizes of about 4.9 μm when they leave the L-PCVI. So, it is incomprehensibly why the fate of much larger droplets (up to 50 μm) is discussed, since those sizes will not enter the droplet evaporation chamber. Here another explanation of the authors is needed.

L.243: “. . ..droplets would rapidly freeze”.

L.244: The PCVI flow settings, that establish the mentioned D50 diameters, should be explicitly given here.

L.244: Fig.9: x-axis shows two times a value of 0.3, that has to be corrected.

L.245-246: The statement that the PCVI residual particle and the initial ice particle concentration are consistent needs to be confirmed by a measurement presentation (Figure) to convince the reader about this important issue.

Moreover, this statement implies an ice particle concentration measurement, which has to be mentioned.

L. 246: When the dry AS particle size is expected to be 1.4 μm it is unclear why many particles were counted that are much smaller (cf. Fig.9). The authors need to explain the size distribution illustrated in Fig.9 in this direction.

L. 240-248: When reading the section 4.2.2, the question arises why the fate of ice

particles in the L-PCVI was not investigated. This was shown for droplets in section 4.1, resulting in a droplet shrinking from 40 μm to 4.7 μm. But this is not discussed for ice particles. So, it is not clear how much ice particles shrink in the L-PCVI. In general, it might be possible that they leave the L-PCVI and enter the droplet evaporation chamber with such a small size that they cannot overcome the counterflow of the PCVI. So, if a test measurement might be too difficult, a robust calculation demonstrating the shrinking of ice particles at the outlet of the L-PCVI for the range of possible diameters will be sufficient.

L. 251: The authors should precisely describe if they simply have used lab air aerosol particles to characterize their PCVI or if they used particles from a particle generator. The latter would be much better for the characterization, because it offers the opportunity to generate much more particles in the cut-off diameter size range. Fig.10A is really a poor size distribution to demonstrate the reader a cut-off of about 5μm, but hardly any number concentration in this size range is visible.

L.254: It looks like that the authors mixed-up Fig.10B and Fig.10C.

L.255: The comparison of Fig.10A and Fig.10C is again rather difficult, since totally y-scales are used. This strongly limits the sense and purpose of this comparison. So, the authors should think about how to present the comparison in a better way, e.g. to have a fourth time series of Fig.10A but with a scale of Fig.10C.

Moreover, there exist a significant amount of particles below the PCVI cut-off in Fig.10C. The authors have to carefully discuss this point. Processes are known that could be responsible for this observation, like wake capture, but an estimation has to be included here, if the amount of particles in the SF with sizes below the cut-off diameter is consistent with the amount of particles larger than the cut-off diameter.

L. 258-260: The statement “increase to within 50% of each other” is not clear to understand. In contrast to the information in these two lines it would be much better to present a size dependent transmission efficiency of particles into the PF. Only in this way the reader can envision the representativity of an aerosol measurement in the PF.

L. 260-263: This statement has to be quantified. How large is the part of particles larger than the cut-off diameter in the PF per size bin? According to the PCVI design substantial losses of super-micrometer particles should be expected.

L. 264: According to Table 1 in Kulkarni et al. (2011), the D50 obtained in this study seems unrealistic high. For similar flow settings a D50 between 2 and 3 μm are determined. So the authors should explain why they obtained such a high D50.

L.269: The data points for each size in Fig.11 needs to be complemented by error bars.

L.269: It should be Table 1, correct?

L.273-283: chapter 5.1 should be substantially shortened since it contains much information which is not relevant for this study.

Chapter 5:

L.287: The sampling properties of the inlet needs to be much more described in order to illustrate which cloud element sizes can be expected at the entrance of SPIDER.

L.287-290: Why was no aerosol sensor connected to the PF of the L-PCVI, which should be the interstitial aerosol channel and should be easily compared to no cloud measurements.

L.291-292: Fig.12 and Fig.13 should be merged to one figure.

L.294: The authors should provide the information which kind of cloud imaging probe is used and which cloud element size range is measured.

L.296-302: The authors should explain/discuss: 1. Why is there no dependence of observed blown snow with wind speed in their ice residual measurements in Fig.15? 2. Why do they observe blown snow already at 2 m/sec, when a wind speed of at least 4 m/sec is required for the blown snow production? 3. Are there other mechanisms that

could create the observations in Fig.15 and Fig.16 without cloud?

Blown snow should be also seen by the cloud imaging probe mentioned in line 294. Why is this information not used in this discussion?

L.302-303: This sentence is unclear, since the enhancement factor of the PCVI is the same independent whether it is operated in or outside cloud. The authors therefore should better explain what they want to say.

Further information should be given, whether background particles are observed in the droplet residual channel, i.e. the PF of the PCVI.

L.310: What is meant by “the ice crystal residual size distribution that followed similar trends to the cloud imaging probe”? How does a size distribution follow a concentration measurement?

In addition: Fig.14 cannot be interpreted since its figure caption is missing.

L.312: It is not clear that the presented concentration in Fig.17A and B represents the ice crystal concentration. According to the presented argumentation those particles are related to blown snow where each crystal could contain many aerosol particles.

L.312-315: It is hard to follow the argumentation of the authors. Does it mean that newly formed particles become deposited on the snow surface and are then transported to the ice residual channel by blown snow? The authors need to explain the whole pathway from NPF to ice residual detection.

Furthermore, NPF events are related to particles much smaller than 120 nm (Fig.17). To grow to sizes larger 100 nm or larger it takes considerable time. So, the connection NPF and measurement as ice residuals needs a further motivation. Again, it would have been of great advantage when the interstitial particle channel had been measured.

L.320: The relation to DeMott et al. (2010) is a very poor confirmation of the SPIDER sampling. It is mandatory to compare the SPIDER results at this point with the only other ground-based ambient ice crystal residual particle measurements with the Ice-CVI and ISI inlets published by Mertes et al. (2007), Kupiszewski et al. (2015) and Kupiszewski et al. (2016).

Moreover, most mixed-phase clouds are precipitating. So, if blown snow might produce some background particles, precipitating snow crystals or graupel might even evoke even more artefacts residuals, that might be seen in the increase of ice residual particles inside cloud. The authors need to discuss this point, because it is very important for the interpretation of the results.

When comparing Fig.17A, B with Fig.17C,D the question directly comes up, how the “true” ice residuals will be separated from the background measurements. Should the background subtracted to infer the “true” ice residual size distribution? If yes, this should be included in Fig.17. The authors should comment on this as well, since according to the concentration shown in Fig.15 and Fig.16, 25% of the particles are related to the background (0.005 cm-3 background vs. 0.02 cm-3 ice residual particles).

Furthermore, the authors should explain why no attempt is made to compare the ice residual particle and ice crystal concentration obtainable from the cloud imaging probe.

L.321-323: It was clear before the measurements that the OPC is unsuitable for the measurement of a droplet residue size distribution and concentration due to the high lower detection diameter of the instrument. Therefore, it is incomprehensible why not a more suitable sensor was used or at least the SP2-XR sensor was connected to the CCN channel from time to time.

A higher size resolution is not the main point. It is much more the lower detection limit which is required.

Chapter 6:

L.325-328: In the present form this manuscript does at most verify the qualitative func-

tionality. But for a characterization of ambient interstitial, cloud droplet residuals and ice particle residuals a quantitative investigation of the sampling efficiencies of these three channels is needed. Many suggestions which is missing or needs to be done are given above in this review and will be not repeated here again.

L.329-332: Even it is certainly possible to achieve a quantitative description for the droplet and ice residual channels, there is indeed a principal caveat concerning the interstitial particle channel. Although it is not precisely clear which L-PCVI cut-off is used it will be in the range of about 20 μm, i.e. most or at least many droplets and small ice particles will be transported into the PF of the L-PCVI, which cannot be avoided. These particles or their residuals will be then falsely measured as interstitial particles.

L.333-337: It is incomprehensible why the authors carry out something like a proof of principle, but measure only particles in one of the three SPIDER channels. Beside the measurement issues of the ice particle residual channel addressed in this review a proof of principle of the other two channels is completely missing.

L.337-339: The future work proposed here, has to be a necessary part of this manuscript and cannot be sold as a desired but not binding outlook. But with this additional measurements and analysis of the interstitial and droplet residual channel a resubmission will be most likely successful.

L.340-346: Indeed, the proposed single particle chemical characterization of the particles sampled at all three channels of the SPIDER system could offer significant knowledge for the understanding of the formation of mixed-phase clouds. But this is not the next but the second next step. The next step would be to complete the verification of the functionality of SPIDER by further lab tests and a further field application.

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