Responses to Anonymous Referee #1

Introduction

We thank the Referee for their comments. We have made a concerted effort to make as many changes as possible and believe these improve the manuscript.

We are compelled to note that we found this review problematic for two resons.

First, we would like to request that the Referee more carefully consider their choice of wording. Wording choices were in many cases problematic, not conducive to improvement of the manuscript, and in some cases inappropriate: "a very poor citation and should not be used" (this may be the Referee's opinion but no reasoning or alternative was given); "This is annoying"; "This paragraph is really confusing for the reader." (with no suggested wording or reasoning); "It is incomprehensible" (multiple).

Second, the Referee was very clear that they would have undertaken a different methodology for lab and field verification than we did. We acknowledge that multiple methodologies could successfully demonstrate instrument ability; we are compelled to note that the review should independently consider our methodology and results, not judge them based on their similarity to what the Referee would have done.

Comments

1. 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".
   Change made.

2. The citation of Hiranuma et al. (2016) is found 2 times in the reference list and one time in a wrong way.
   Change made.

3. There is no information what kind of citation Koolik (2017) is about at all.
   Koolik (2017) is fully cited in the references

4. The figure caption of Fig.14 is incorrect.
   Figure 14 has been removed in this draft.

5. Fig.12 shows 2 cloud periods, but in the text 3 cloud periods are mentioned.
   Updated to two cloudy periods.

6. Fig. 15 (a) contains 2 black data points that are not explained. -
   The two black data points are due to overlap from the surrounding circles. The figure was updated so that only one color appears and was moved to the supplementary section since there is no correlation.

7. Mix-up of Table 1 and 2 several times in the text. -
   Removed Table 1 entirely and updated all references accordingly.

8. Figures with several panels are labelled with (a), (b), (c). . ..but in the text they are mentioned as A, B, C
Change made.

9. 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.
Change made.

10. L. 21: change to "deployed at Strom Peak Laboratory"
Change made.

11. 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"
Change made.

12. L.46: "become activated" instead of "activate"
Change made.

13. L.61: "microphysical properties" instead of "microphysics"
Change made.

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

15. L.78: "on the CVI design" or "on the design of the CVI"
Change made.

16. 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.
The figure has been updated to show that there are two symmetrical pump flow lines in the 3D printed PCVI.

17. L.87: Friedmann et al. (2013) is a very poor citation and should not be used in such a manuscript.
Friedmann et al is a peer-reviewed publication and is therefore an appropriate reference.

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

19. 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
We have converted the values and the units of the flows to L/min to be consistent with Hiranuma et al. (2016) throughout the manuscript.

20. 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.
The text has been updated to consistently refer to both cases using the ratio and flows.

21. 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.
Updated text for clarity regarding what information is being cited.

22. 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
As we have changed all flow values in the manuscript to L/min as suggested in comment #19 we also double checked that all flow values are now correct and consistent throughout the manuscript.

23. 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.
This information has been moved to the Supplementary Materials section.

24. 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
Thank you for your suggestion about the inlet. It was named for completeness of the system in the text. Since it was not used in this study neither the lab nor the field measurements the passage in the text about the inlet was deleted.

25. 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).
We have now defined the AF=CF at the beginning of the manuscript to clarify the terminology for the reader.

26. 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.
The flow ratios and flow rates have been corrected in the manuscript. Also, a change in the sample flow rate does not affect the cut-size (D50) of the PCVI it only changes the enhancement factor as this was also shown in Hiranuma et al. 2016, section 4.4 and section 3.3 respectively.

27. 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.
As suggested also in comment #19 and #22 we double checked all flow values and applied corrections throughout the whole manuscript.

28. 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.
The figure has been updated to have consistent units in both panels. Additionally, language has been added to explain the difference in the concentrations in each channel.

29. 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.
The missing flow values have been added to the text. Please see line 161.

30. 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.
The flow values have been corrected in the table. The sample flow of 2 L/min was chosen as Hiranuma et al. 2016 also performed their test with these flow values, IF=50 L/min, AF=7 L/min, SF=2 L/min. Therefore, for this experiment we choose the same flow rates, please see corrected table 1. In the regular measurements with SPIDER the sample flow rate of the L-PCVI was set to 6.5 L/min, please see line 95.

31. 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.
The discussion has been expanded to explain that the particles entering the IF of the L-PCVI with only AF turned on are diluted but not size-restricted. Once the PF is turned on, the population of particles is size-restricted.

32. Nevertheless, it means that 40 µm droplets are already evaporated to 4.7 µm wet particles when they leave the L-PCVI, correct?
That is correct for the AS droplets we have tested.

33. 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.
The dilution is now included in the discussion.

34. 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.
The scales of Figures 6B and 6C have been updated to be consistent. Additionally, the text has been updated to remove the misleading statement and replace it with a more appropriate discussion of the difference between Figure 6B and 6C.

35. 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.
Based on your suggestion we made changes to the manuscript, please see lines 179-184.

36. 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.
The flow ratios and flow rates have been corrected in the manuscript. Also, a change in the sample flow rate does not affect the cut-size (D50) of the PCVI it only changes the enhancement factor as this was also shown in Hiranuma et al. 2016, section 4.4 and section 3.3 respectively.

37. 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
This figure has been moved to the Supplemental Materials section. Additionally, the boundaries determined experimentally in this work are more explicitly referenced in the text.

38. L.221: The authors should precisely mention the diameter range of "large droplets".
The specifics of the droplet evaporation calculations were moved to the Supplemental Materials section. The specific diameter range is added there.

39. L.235-236: What is meant by "the dry L-PCVI" sample flow? What is its RH or dewpoint? 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.
Updated text to indicate that the dry flow is through the AF not SF. While it is correct that in the lab setting, 40 µm droplets did evaporate to approximately 4.9 µm in the L-PCVI, particles larger than 40 µm could enter the L-PCVI in field studies. For completeness, a range of initial droplet sizes (up to 50 µm) is shown in the figure in the Supplemental Material section. A sentence has been added to the main text to describe that these larger droplets are possible but not expected.

40. L.243: ". . ..droplets would rapidly freeze".
Updated inline.

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

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

43. 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.
It is unclear what the suggested change is here as this section deals with the lab performance tests of the PCVI. The data requested appears to be in Figure 5 which is the Referee 3 suggested dN/dlogDp version of the original Figure.

44. 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.
We agree that the figure gave the appearance of undersized particles due to the format and data ranges. This has been updated now and we believe it will not cause the confusion it did initially.

45. 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.
We believe the Referee may have misunderstood the wording in Section 4.1. Droplet tests were conducted for the apparatus ("after residence time in the apparatus"), including the L-PCVI as well as the chamber, not only the L-PCVI.
Ice crystals are not expected to sublimate in a chamber held at or near ice saturation. The point of section 4.2.2. was to verify this experimentally, in this case showing that the particle concentration passing through the PCVI was consistent with the number of ice crystals produced.

46. 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.
Text has been added to indicate that lab air was used for this experiment. Additionally, updates have been made to Figure 10 (now part of the Supplementary Materials) to better demonstrate the concentration of larger particles within the lab air.

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

48. 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.
Figure 10 has been moved to the Supplementary Materials section. In this new figure, we have added a fourth pane which shows the Figure 10A with the scaling of Figure 10C.

49. 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.
This experiment has been moved to the Supplemental Materials section. The wording has been adjusted to make the discussion clearer.

50. 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.
This comment has been addressed as part of comment #49.

51. 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.
The D50 values obtained in the Kulkarni et al. (2011) study are not based on normalized transmission efficiency. By normalizing our curve, we obtain a slightly higher D50.

52. L.269: The data points for each size in Fig.11 needs to be complemented by error bars.
Representative error bars have been added to a point near the D50 and to the D50 itself.

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

54. L.273-283: chapter 5.1 should be substantially shortened since it contains much information which is not relevant for this study.
Text was shortened by four sentences. Extraneous information about SPL was omitted and only a brief description of the site location remains.

55. 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.
The cut size diameter for the L-PCVI was added to the text as well as the SPL inlet: "The outdoor inlet at SPL has a cut-off size at 13um for at wind speeds of 0.5 m s^{-1} (Petersen et al., 2019) and the L-PCVI of SPIDER was set to have a cut-off size of 8mm."

56. 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.
Updated text to mention that there is an OPC for the interstitial aerosol channel. The OPC had a high size detection limit, thus was only one bin of data for the 380nm-540nm range. No meaningful conclusions could be established for the interstitial aerosol channel to compare clear and cloudy days.

57. L.291-292: Fig.12 and Fig.13 should be merged to one figure.
Updated the figure. Figure 13 is now Figure 8c.

58. L.294: The authors should provide the information which kind of cloud imaging probe is used and which cloud element size range is measured.
Added a sentence that mentioned the cloud imaging probe is from Droplet Measurement Technologies. The figure 14 caption was accidentally missing from the publication, but includes the size ranges that were measured and plotted. While the CIP sampled cloud elements from 15um-940um range, only small ice particles were used to calculate LWC ranging from 30um-105um.

59. 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?
1) Added an additional sentence suggesting that the lower wind speeds may have been a reason for no association between wind speed and blowing snow. Also included "Lowenthal et al.'s (2019) study which looked at ice crystal concentrations also found that there was no association between blowing snow and ice crystal concentrations at SPL." 2) An additional sentence was added to explain why there are particles being detected by the ice crystal residual channel when there are no clouds present and at lower wind speeds. The reason for this may be background aerosol particles that have enough inertia to make it through both PCVIs and are then counted by the SP2-XR. 3) Three additional hypotheses for the presence of smaller particles were added and as was observed by Pekour and Cziczo (2011): a) kinetic energy is imparted from larger particles onto smaller particles which allow smaller particles to pass through the stagnation planes of the L-PCVI and PCVI, b) small particles become entrained in the wake of larger particles that have enough inertia to pass through stagnation planes, and c) small particles collide and coalesce with larger particles that then get detrained in the sample flow.

60. Blown snow should be also seen by the cloud imaging probe mentioned in line 294. Why is this information not used in this discussion?
Reworded the text. The third paragraph explains windblown snow was something that the authors were initially concerned with, but no association was found between wind speed and blown snow. This is supported by observations done at SPL on ice microphysics by Lowenthal et al. (2019) as well. The CIP does not measure IWC and only measures LWC, thus it would not be a good measure for windblown snow.

61. 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.
This is correct, the sentence was deleted for clarification.

62. Further information should be given, whether background particles are observed in the droplet residual channel, i.e. the PF of the PCVI.
Background particles are observed in the droplet residual channel however the results provide little insight due to the low sizing resolution and high lower size detection limit. An additional sentence was added to state that background concentrations were observed for this channel but that there was little valuable information that could be garnered from these.

63. 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?

Sentence was changed to ice crystal residual *concentrations*.

64. In addition: Fig.14 cannot be interpreted since its figure caption is missing.
The correct caption has been added.

65. 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.
As mentioned in comment 59, there was an additional line added to explain that the background concentrations that were detected could have been from larger particles with enough inertia to pass through both PCVIs.

66. 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.
Paragraph was reworded for clarity to eliminate blowing snow as a potential reason. Reasons for inadvertent transmission are explained further in comment 59. To summarize, NPF particles are small and we saw a lot of particles under 200nm show up in the ice crystal residual channel during *clear* days. Thus, ambient atmospheric aerosol conditions are being introduced to the SPL inlet and into SPIDER.

67. 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.
The interstitial aerosol channel was measured, but provided little information due to the high lower size detection limit and poor sizing resolution. Additional details about the inadvertent transmission of particles under 200nm in the ice crystal residual channel is elaborated on comments 59 & 66.

68. 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 IceCVI and ISI inlets published by Mertes et al. (2007), Kupiszewski et al. (2015) and Kupiszewski et al. (2016).
Included the following sentences "This would be consistent with other studies that have done ground-based studies that have looked at ice residual concentrations (Mertes et al., 2007; Kupiszewski et al., 2016). While Mertes et al. (2007) found that super-micrometer aerosol particles dominated the ice residual concentrations, they also observed sub-micrometer ice residual particles and Kupiszewski et al., 2016 found that their ice residual particle concentrations peaked at around 200nm-300nm. Looking across multiple field studies, DeMott et al. (2010) found that INPs were typically >300 nm diameter with a strong preference for those >500nm, which would support the findings of Mertes et al. (2007). While our findings suggest an increase in ice residual concentrations in the 200nm-500nm range, the SP2-XR was limited to a maximum size range of 540nm and thus further conclusions could not be drawn on the preference of ice crystal residual particle size."

69. 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.

Blowing snow was dismissed as a potential source of background particles. Precipitation is usually larger and is on the size range of 0.2mm or greater for raindrops and larger for snow. As mentioned above and included in the text (comment 59), the SPL inlet has a size cut-off at 13um (Petersen et al., 2019), thus precipitation artifacts should be minimal. However, we are unaware of mechanisms by which additional artifacts would be produced by precipitating clouds; if you could clarify, we could address this question better.

70. 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).
This is correct. The figure caption was updated to include that the background ice residual concentrations were subtracted from the total in 17C,D when cloudy periods occurred.

71. 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.
Unfortunately, the cloud imaging probe (CIP) only measures liquid water content (LWC). The forward scattering spectrometer probe (FSSP) measures ice crystal particle concentrations, however the instrument was malfunctioning during the study period and could not be repaired on time. Thus, LWC was the only remaining variable and it allowed for a qualitative comparison. An additional sentence was added to emphasize this qualitative comparison.

72. 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.
The optical particle sizer (OPS) which was originally the sensor used for the ice crystal residual channel arrived damaged upon arrival to SPL. Due to the government shutdown during this time period it was difficult to acquire more suitable or additional sensors. This is a valid point for the CCN channel, but ice crystal residuals were the main interest for this study. Future studies with SPIDER will aim to have more suitable sensors for each channel.

73. A higher size resolution is not the main point. It is much more the lower detection limit which is required.
Lower detection limit was added to the higher size resolution statement for further clarification.

74. L.325-328: In the present form this manuscript does at most verify the qualitative functionality. 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.
We strongly disagree with the reviewer. Figures 3-7 in the revised manuscript are all quantitative descriptions of the system functionality in the lab. Figures 7 and 8 demonstrate quantitative measurements in the field with a comparison to cloud probe measurements in Figure 7.
We agree that, due to instrument constraints, the most capable sizing instrument was dedicated to what we consider the most important measurement channel, that for ice

residuals. That does not mean that the interstitial aerosol and droplet residuals were not quantitatively tested, they were tested with a lower capability instrument.

75. 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.
The reviewer is correct; this is a historic limitation of CVI instruments. To clarify we now state "Because droplets and ice crystals are typically 10 µm or larger (Kleinman et al., 2012; Pruppacher and Klett, 1997; Rogers and Yau, 1989), only these activated droplets and ice crystals are large enough to exit the SF. It should be noted that any droplets and/or ice crystals below the cut size will be inadvertently stopped and transmitted into the PF." In the revised manuscript at L 93-96.

76. 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.
77. 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.
As stated, the low resolution of the OPCs did not allow us to make measurements that were comparable to other instruments as was done in Figure 7 for the ice residual channel. The laboratory data for these measurements is shown in the earlier sections. This is stated explicitly at L 290 "However, due to the high size detection limit and lower sizing and timing resolution, we were unable to come to meaningful conclusions. Future studies at SPL with SPIDER should focus on higher particle sizing resolution and a lower size detection limit for the cloud droplet residual channel and extended size ranges beyond 500 nm for the ice crystal residual channel." and with the suggestion that in the future more capable instruments be used L306-309 "Future experiments with SPIDER will include a lower detection limit and higher time and size resolution OPC on both the interstitial aerosol particle channel and cloud droplet residual channel to further evaluate its efficacy for simultaneous sampling of interstitial aerosol particles, cloud droplets and ice residuals." We hope this is comprehensible.

78. 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.
We appreciate the Referee's comments and we continue to test and improve the SPIDER system. However, the phrase "the next step" used here means a fundamental advancement of the measurement, in this case the use of single particle mass spectrometry instead of optical particle counting / sizing.