Replies to the Reviewer 2 comments.
Manuscript: "Assessment of performance of the inter-particle spacing algorithm to identify ice shattering artifacts in cloud particle probes measurements" by A. Korolev and P. Field

The authors appreciate the detailed and thoughtful comments by the reviewer and time spent to perform this work.

Comment: Line 18, page 10251: It may help to define what the interarrival time of the probe is for a reader less familiar with OAP-2DCs.
Reply: The paragraph with the first mentioning of the interarrival time was modified to address the reviewer's comment: "Because of close spacing, the time difference between two successive shattered fragments passing through the sample volume will be much shorter than that for naturally occurring particles. This time difference is usually referred to as "interarrival time"."

Comment: Line 10, page 10252: "several" – 600? 700? I think something more specific than "several" is warranted.
Reply: Changed text to: "It was also found that the corrected PSDs could show a reduction in particles concentration over a wide range of sizes from 200 microns for narrow distributions up to 1000microns for the broadest distributions that are subject to the most shattering."

Comment: Section 2.2. Bullet 2. I think it should be emphasized that this particular version of the ITA uses the minimum of $\varphi(\Delta x)$. Some other versions of the ITA that do not search for a minimum in $\varphi(\Delta x)$ are based off of a certain percentage of the mean interarrival time (i.e. Lawson 2011), or base the cutoff threshold as a multiple of the peak interarrival time in the shattered mode (Field et al. 2003) after fitting 2 Poisson modes to $\varphi(\Delta x)$. It may be interesting to discuss how your results in your paper apply to those particular versions of the ITA as well somewhere in the paper.
Reply: A comparison of different algorithms for identifying the cut-off-time is an important topic, but we outside the scope of this study. The following text was added to address the reviewer's comment: "It is relevant to mention here that alternative techniques for determining τ^* were used by Field et al. (2003, 2006), Lawson (2011), Jackson et al. (2014). These techniques were based on fitting the function $\varphi_i(\Delta t)$ by the Poisson distribution."

Comment: Section 3.4. Since entire-in processing helps mitigate the issue of classifying partially view ice particles as shattered artifacts, what implications would have for people processing optical array probe data using image reconstruction or center-in techniques to improve the sample area of the probe?
Reply: This is a tradeoff point. People processing entire-in images may have difficulties processing measurement collected in clouds with large particles. One such case is shown in Fig.9b. Nearly 100% of the images are partial and they will be rejected by the entire-in processing. The software processing of the partial images extends the population of accepted images. However, the results of the processing will be affected by the ambiguity related to confusing branches of the same ice particles with shattering artifacts. There are few potential ways to reduce this ambiguity. At present this is work in progress.

Comment: Section 3.5. Do you think there is any possibility that pattern recognition software might help to help recognize what a diffraction fringe looks like? A diffraction fringe would present itself as several smaller particles surrounding a large particle, all with low interarrival times, with the large particle somewhere in the middle of the train of particles with low interarrival times. Therefore, I would expect there to be some sort of distinction between the two phenomena that could show up. Obviously, developing the algorithm is beyond the scope of the study, but it may be worth seeing if future studies could compare the size/interarrival time characteristics of diffraction fringes versus shattered artifacts and see if there is a clear distinction in the characteristics of the particles between the two. I think this should be a recommendation for future work to be listed by the authors.
Reply: Taking into account infinite variety of ice particle habits and their orientations developing an image recognition module in the 2D processing software to identify diffraction would be a challenging task. This makes sense to do for processing of high pixel resolution binary imaging probes, e.g. 2DS. In many cases filtering out diffraction fringes can be done with a set of relatively simple rules. This is not discussed in this paper.

Comment: Section 3.6: I think it would be beneficial to show an out of focus fragmented image as a figure in the paper.
Reply: New figure (Fig.4) demonstrating out-of-focus fragmented images, which were identified as shattering artifacts, is added in the text.

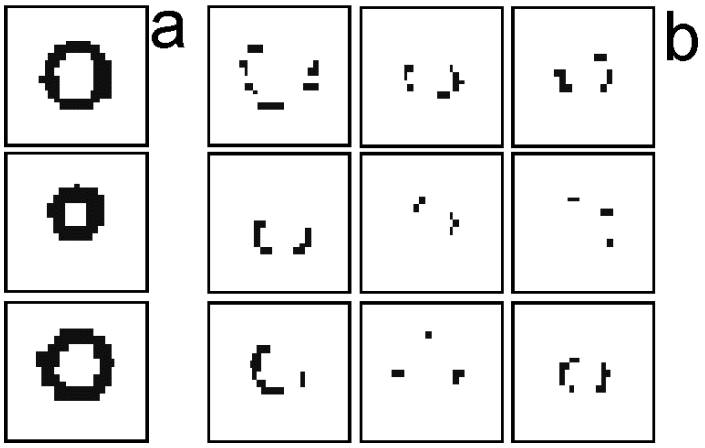

Figure 4. Examples of out-of-focus images measured by 2D-S at 10μm pixel resolution. (a) complete circle out-of-focus images; (b) fragmented out-of-focus images, which were registered in two or three image frames and identified as shattering artifacts by the interarrival time algorithm. The fragmented out-of-focus images is related to the particles passing through the sample volume near the edge of the depth-of-field

Comment: Section 4.2.1. Line 20, page 10261. I think the wide (2+ orders of magnitude variability) in the cutoff should be explained as well. It is caused by the fact that the ITA has a tough time identifying the minimum between the modes?
Reply: In general case, a nearly four orders of magnitude spread of the cut-off-distance as shown in Fig.15 is explained by difference in particle concentrations. For example changes of particle concentration from 1000L-1 to 0.01L-1 will shift the long distance interarrival mode towards large values. This will affect the cut-off-distance as well.

Comment: Lines 6-10, page 10263: A more quantitative statement is needed here. What does it mean to agree "reasonably well?" I think it would be of a benefit to mention the values of the concentrations of particles in the given size ranges.
Reply: The text is modified to address the reviewer's comment; "It is interesting to note that for the modified probe the ITA corrected and uncorrected distributions agree to better than 10% for particles larger 600μm (in Fig 6d,e,f). However, for the standard probe the separations between ITA corrected and uncorrected distributions remain approximately constant for *D*>600μm (Fig.6abc) and it varies from 20% to 30%. "

Comment: Line 25, page 10263: I would mention what the mean extinction and IWC are from the 2DC probes before and after corrections for this case to give a more quantitative estimate of the impact of the algorithm.
Reply: The mean IWC value is indicated as per reviewer's comment.

Comment: Line 6, page 10264: How much greater are the counts?
Reply: The following sentence was added in the manuscript to address the reviewer's comment: "The total number of particle counts per second in the modified probe varied from 10 to 70 and the counts for the standard probe varied in the range 100 to 300."

Comment: Line 8, page 10264: "most," Can you give a percentage here?
Reply: The percentage is indicated as per reviewer's comment.

Comment: Line 28-29, page 10265: "small," How small? 10 particles?
Reply: The fraction of rejected particles (0.8%) was indicated in the text to address the reviewer's comment

Comment: Line 2, page 10267: I think you need to show these fits in Figure 13 as a curved line.
Reply: One of the versions of this diagram at the stage of preparation of the manuscript had the linear fits on it. This diagram appeared quite busy with the linear fits obscuring the distribution curves (especially Figs.13b1,b2,c1,d2). So the authors decided to keep the distribution curves only without the linear fits.

Comment: Line 30, page 10267: Few. 10? 20? How many are in the figure?

Reply: The number of counts indicated following the reviewer's comment.

Comment: Line 23, page 10271: The relationship between the number of fragments viewed by the probe and the distance between the shattering volume and the sample volume makes sense physically, but there is little work done to determine how strong this relationship actually is. I don't think the authors have the proper data to determine the strength of said relationship, so whether the "anticipated" statement on lines 29, 30 and line 1 of the next page is still up for grabs. I think it would be useful to mention that there needs to be future work done to see how strong this relationship is. Numerical flow modeling and lab tests of probes with differing Ssn and distance from sample volume to shattering surfaces have the potential to do this. These studies could potentially be useful not only for the correction of historical datasets, but also give the community recommendations of how to design probes in the future to best accommodate the ITA.
Reply: The relations between particle counts in the short and long distance modes and S_0, n_0, S_{sh}, $\overline{N}_{sh}$ are the outputs (among many others) of the Monte-Carlo simulation model used in this study. This is a byproduct of this study. These results were not included in the manuscript in order to keep the reader's attention focused on the limitations of the ITA. Including the modeling results in the present manuscript would expand it to unreasonable size. The results of the modeling directly showed the dependence of the number of identified shattered particles versus S_{sh}. Based on these results the authors are sending a message to the community that probes with small sample areas (e.g. FSSP, CDP) have a limited capability to identify shattered artifacts with the help of ITA. A large fraction of the shattering artifacts may appear as singletons and therefore not be identified by ITA. To address the reviewer's comment a statement about how the statistical simulation may facilitate future design of the particle probes and examining of the shattering effect is included in the conclusions.

Comment: Line 7, page 10272: I think it's safe to say that the standard probe has a greater Ssh than the modified probe, but I would not argue that Ssh ! 0. The modified probe tips still shatter ice particles and, as demonstrated by your results, these artifacts still enter the sample volume, so the modified probe must have a nonzero Ssh.
Reply: The sentence was modified following the reviewer's comment: "The reduced number of the shattered fragments N_{sh} for the modified probe is explained by the fact that the anti-shattering tips have a significantly reduced shattering area compared to the standard tips."

Comment: Conclusions. I think that a sixth bullet can be added here that highlights the main conclusions of the Monte Carlo simulations with the last sentence removed from Conclusion 5. This would better highlight the main points of the last part of the paper.
Reply: The conclusions were modified following the reviewer's comment.

Technical corrections:
Comment: Line 18, 10251: "thought" should be "through."
Reply: corrected

Comment: Line 25, page 10253: "cut-off"

Reply: corrected

Comment: Line 7, page 10272: “anti-shattering”
Reply: corrected

Comment: Figure 13: I think you need to show the exponential fits as a curved line in this figure when the distributions are a good fit to the data. The fit coefficients to the relationships in Figure 13 should be shown as a table or an appendix.
Reply: These are some kind of random examples of statistical distributions of L_s, N_s, L_i, N_i. They are not anchored to statistics and the do not represent extreme cases limiting variety of possible slopes. We did not feel that is worth an additional table to list linear fit – it would draw the reader’s attention from our main message.