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Atmospheric Measurement Techniques Discussions

Interactive comment on "Effects of ice particles shattering on optical cloud particle probes" *by* R. P. Lawson

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This study purports to demonstrate that the arrival time technique for removing shattering artifacts is more effective than using mechanical means to avoid the products of shattering. The evaluation is incomplete, unconvincing and the overall effort disappointing. There is no question that ice crystal shattering is a source of measurement contamination. There have been sufficient studies prior to this one that have documented how crystal fragments can increase the number of detected particles, particularly at small sizes. Hence, the introductory section of the paper is not only redundant and not very informative, but not nearly as thorough as the studies by Field et al. (2006) and others. The author had the opportunity to make a major contribution with respect to documenting the characteristics of shattering, e.g., taking an approach like Field et

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al (2006) and looking at how the arrival times are distributed depending on different cloud types, size distributions, particle habits etc., and quantifying these results in a way that would be useful to the community with respect to interpreting older data sets and avoiding the issue in future measurements. As it is, due to a serious lack of detail in the analysis (see below), the results are of limited value and contribute little to what is already known about this issue.

The evaluation and conclusions of this study are based on two major premises: 1) the 2D-S is superior to all other OAPs because of its advanced electro-optical design and 2) the arrival time algorithm that was described by Baker et al. (2009) effectively removes shattered ice crystal fragments from the measurements. I would argue that both of these premises are questionable.

Starting with the assertion that the 2D-S is superior to other imaging probes, the 2D-S is an optical array probe (OAP), no more and no less. This means that the uncertainties, none of which are discussed in this paper, are the same as in all OAPs. The DOF is uncertain for particles less than about 100 um and highly uncertain for particles less than 50 um. This is because the equation that relates the DOF to the particle diameter is only a very rough estimate. As shown by Korolev et al., particles can be quite a distance outside of this nominal DOF and still be imaged, albeit mis-sized. Korolev derived algorithms that could correct the size and estimate the DOF, but these will work mostly with water droplets not ice crystals. It is not known if the present data set was corrected for these errors and even if they were, their application to ice crystals will also be uncertain.

Hence, the measurements have to be interpreted with these uncertainties taken into account. In addition, the majority of the corrections to the size distribution seem to happen in the 10-30 um bin. Using the Knollenberg (1970) formula, the DOF of a 10 um particle is approximately 1 mm. This formulation, however, was derived for 300 um drops and was used to constrain the mis-sizing of droplets. Has this formula been validated at 10-30 um? I know of no such published study. This paper has been submitted

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to Atmospheric Measurement Techniques, a journal dedicated to the advancement of measurement technology. To submit a paper on measurement techniques with no uncertainty analysis or description of how the data were processed places all the results in question. How was the data filtered, what algorithm was used to calculate sample volume, how are zero images handled, etc.?

The only advantage that is touted in this paper with regard to the 2D-S is that its fast response allows it to measure crystals in the 10-30 um range at jet aircraft speeds with the contention that the earlier model 2D-Cs and CIP could not respond and thus were not able to show the shattering signature. Hence, this is the reason that the 2D-S sees shattered particles and the 2D-C and CIP do not. This is contrary, however, to what Field et al (2006) showed. Although the Citation is not as fast of an aircraft as the Lear, it is still fast enough to limit the response of the 2D-C and CIP if that was the major factor as to why the 2D-S sees more shattered particles than the 2D-C or CIP. From Field et al. (2006), the 2D-C is clearly seeing many shattered particle while the CIP sees significantly less. I would contend that the author has ignored the other possibilities. One is that the 2D-S has four arms on which particles shatter, rather than two. Secondly, each of the two 2D-S arrays has 128 elements as opposed to 32 for the original 2D and 64 for the CIP. This means that there is a higher probability of seeing shattered fragments. Finally, the 2D-S arms are closer together than the 2D-C or the CIP, so more of the crystal fragments are likely to end up in the sample volume. This was also concluded in Field et al (2006), i.e. the farther apart the arms, the fewer fragments making it to the sample volume. Hence, it is not clear to me that the 2D-S has really improved the situation, other than improving the ability to respond to fragments.

The second premise upon which all the conclusions are drawn is that the Baker et al. (2009) arrival time algorithm successfully removes crystal fragments while keeping good particles. If this is not the case, i.e. if a significant number of legitimate particles are removed along with the fragments (Type II error) and some of the fragments are not removed when they should have been (Type I error), then in all the figures where application of the arrival time algorithm is shown, when the author contends that the arrival time algorithm is doing a better job than the probe tip modification, in reality, the tips have already done a good job of removing the fragments and the arrival time algorithm has erroneously removed legitimate particles. Given that the author has not described the algorithm, evaluated the Type I and Type II errors, and generally not convinced me that this algorithm doesn't do more harm than good, I am unable to accept any of his conclusions about the improvement that the correction is making.

The author's opening comment on Page 941, line 16 that "The work presented by Baker et al. (2009) considers removal of splashing raindrops, which closely resembles shattered ice particles." places this whole analysis in doubt as I seriously question that water droplets and ice crystals have the same signature when breaking up. Do water droplets bounce like ice crystals? There is some evidence that they do, to some degree, but there is no evidence that splashing raindrops resemble shattered ice crystals. Field et al. (2006) did a very careful study, with the bimodal distributions of interarrival times, that the short arrival time mode can vary a lot depending on the ice crystal characteristics, air speed and location of an instrument on the aircraft. Their evaluation was convincing – the evaluation here is not, and won't be until the author takes a similar approach as Field et al. (2006) to the analysis of ice crystals using the Baker et al (2009) analysis.

Finally, with respect to the other evidence that is used to show that the modified tips don't work, a single figure (Figure 4) is used to show shattering signatures from two 2D-S probes, one with and one without modified probe tips. Several things are misleading about this figure and the conclusions drawn. First of all the statement is made that the 2D-S tips were "based on the Korolev design technique". Does this mean that these were actually designed by Korolev or that they were the author's rendition of Korolev's designs. This is an important point since the Korolev tips have undergone a number of renditions, some of them quite subtle that made significant difference for avoiding the fragments from shattering. The author concedes that the tips make a significant difference, so the general concept works; however, unless the 2D-S was using the latest design, it cannot be concluded that there are still fragments being generated by the Korolev tips, only that the author's version of these tips are generating fragments.

The second point is how many images did the author sort through to find the ones that support his contention that the modified tips still produce shattered crystals (Fig. 14)? The fair approach is to use image analysis to detect all the shattered patterns like he shows in Figure 4 and calculate the actual fraction for the two probes with and without the modified tips. The other thing, as shown in Korolev (2010), is that it is not only the obvious shattering pattern that is caused by breaking ice crystals, but there are also an inordinate number of out-of-focus particles that are just fragments passing far from the center of focus. These types of analyses would be far more convincing than the approach that was taken for this evaluation.

To summarize, for this paper to make a significant contribution to our current knowledge on ice crystal shattering and measurement contamination, it will need a major overhaul, starting with an error analysis, detailed discussion of data analysis and a convincing description of the arrival time correction algorithm. I do not recommend it for publication beyond the current discussion stage.

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