Response to Invited Review from Darrel Baumgardner for 'Application of infrared remote sensing to constrain in-situ estimates of ice crystal particle size during SPartICus' by S. J. Cooper and T. J. Garrett

We thank Dr. Baumgardner for his comments in support of our work, especially in regards to the potential utility of our technique as an independent check upon in-situ probe measurements.

This paper is a nice follow-up of the first article that describes the split window technique for separating thin cirrus clouds with respect to their effective radius. In the study described in the current manuscript, three cases are described whereby the technique is applied to thin cirrus whose microphysical properties had also been measured with airborne particle spectrometers. The technique appears to be robust when the given assumptions and conditions are met and I agree with the authors' recommendations that this technique should/could be used to ascertain if ice crystal shattering on in situ probes is contaminating the measurements. There are clearly conditions under which it is highly likely that measurements from the insit instruments are jeopardized, i.e. when significant concentrations of large ice crystals are present. The problem is that "large" and "significant" remain undefined at this time. There are different schools of thought at present in the cloud microphysical community regarding this issue. The use of an completely independent indicator of small versus large effective radius is compelling and should be seriously considered when analyzing data sets consisting of optically thin clouds.

Given that this article is being published in AMT, I wasn't expecting that there would be that many cases to demonstrate the technique. The three that are used have clearly established the utility of the methodology. I was only a little disappointed that there wasn't a case demonstrating more clearly when shattering did impact the measurements. Perhaps, this would be possible using the recent MACPEX data set?

We too were disappointed that we could not find a case where shattering clearly affected the in-situ measurements. But given the limited number of MODIS overpasses for the SPartICus campaign and the required environmental conditions and plane location etc., we simply could not find such an example. We, of course, would like to apply the technique to MACPEX and other campaigns but simply do not have the necessary funding to do so. Ideally, our infrared technique would be applied in concert with an instrument with high temporal and spatial resolution such as the MODIS Airborne Simulator both to maximize the number of cases and to minimize sampling issues. Furthermore, it would be useful to consider infrared brightness temperature differences in design of in-situ flight plans (to the extent it is possible) to know a priori whether the clouds are composed primarily of small and large particles.

Page 1, Line 24: "This debate is centered about measurements of 25 the effective radius re, which : : :". I don't think that the debate is really centered on

measurements of the "effective effective" radius. First of all we don't actually measure the effective radius, we derive it. More importantly the debate is fueled by the discrepancy between how modelers see the world, their expectations of how clouds should work and what is actually measured. There have been some clear examples where ice shattering has produced unreasonably high populations of small ice particles, and these were identified at first because they were not consistent with the models. To take the approach that has been suggested by some, that all in situ measurements are contaminated when any ice exists, is unreasonable until sufficient evaluation has been carried out to warrant such drastic measures. As mentioned above, the approach described in the current paper could go a long way towards resolving some of the questions associated with the measurement of small particles.

We have changed the sentence to '*This debate is centered about estimates of the effective radius, r_e, which is derived either from in situ measurements of ice crystal size or from bulk measurements of the ice water content (IWC) and extinction coefficient…*' to clarify to the readers that we do not directly measure effective radius. We do think that, regardless of motivation of the various parties in the shattering debate, there is still considerable debate as to the true size distribution of cirrus clouds. An accurate characterization of effective radius is important, as it is needed to determine cloud radiative properties for both climate studies and remote sensing algorithms. We choose to leave the paper framed in such a manner.

Page 2, Line 5: Page 2, Line 5: ": : :there is a concern that 5 in situ measurements of re are strongly biased: : :". As previously, the concern is that the concentrations of small ice crystals seem unreasonable high, not that Re is too low.

We changed 'measurements' to *'estimates'*.

Page 2, Line 27: ": : :presence of small particles less than about 20 um", Presence or predominance?

Changed to *'predominance'*

Page 3, Line 5: ": : :at unambiguously accurate retrievals of re.".

We removed 'unambiguously accurate retrievals of *r*e' and added *'the confident identification of clouds with 'small' values of ice crystal effective radius regardless of the range of expected uncertainties for these inversion assumptions'* in attempt to be more straight-forward.

Page 5, Line 7: The 2D-S probe uses overlapping laser beams to create two-dimensional silhouettes of particles with maximum dimensions exceeding 10 um, and: : :" Note, the overlapping beams are not used to create the images. They were originally designed to constrain the sample volume of small particles but this never worked so the two beams are just two-2D probes.

The term 'overlapping' was removed.

Page 5, Line 10: "Similarly, Jensen et al. (2009) argues that since the 2D-S probe has sample arms and not "inlets", it should have limited susceptibility to shattering effects.". Prefer that this sentence be removed because not only is it an unsubstantiated statement, the recent paper by Korolev et al, 2010 in BAMS clearly shows the susceptibility of OAPs to shattering.

The offending sentence was removed. We altered the following sentence to say *'The FSSP-100 and 2D-S design types, however, have previously been demonstrated to potentially cause shattering artifacts (Korolev and Isaac , 2005; Korolev et al., 2010).'*

Page 5, Line 26: "For these analyses, MODIS based BTD estimates of re are compared to in-situ estimates provided by SPEC inc., which operated the in-flight instrumentation during the SPartICus campaign. ". Shouldn't Paul be a co-author?

Dr. Lawson declined participation. We simply used data provided by Spec Inc. available on the SPartICus data server. We were not involved in creating this data in any manner.

Page 6, Line 2: "The effective radius re is subsequently calculated using Eq. (1).". There should be some uncertainty values associated with the numbers in the table. Given the large uncertainties in LWC and extinction, it is hard to argue the second case of marginal Re without an associated error.

We agree that there are uncertainties associated with the estimates of IWC and extinctions provided by Spec Inc. We suggest in our paper that such uncertainties from possibly 1) limited 2D-S collection efficiencies or 2) shattering algorithm assumptions used to produce final cloud properties may cause the slight discrepancy between our technique and the 2D-S for the intermediate case. Although it would seem possible to quantify these uncertainties, Dr. Lawson in a recent AMT paper and discussion on the 2D-S argues against such a quantitative approach due to large uncertainties in the uncertainties through the following argument. He states…

'While I agree with the reviewer that uncertainty analyses are an extremely important component of instrument analysis, I disagree that an uncertainty analysis should be attempted in this paper. An uncertainty analysis is useful only if there is a way to perform the analysis with a reasonable degree of accuracy. Generally, this requires a standard for comparison of the measurements. However, there is no standard to which one can compare cloud particle measurements. Without a standard, a propagation of error approach is only a guess. It is problematical that many papers have published "uncertainties" that are based on crude estimates of bias errors, and that these "error bars" are then used as justification by subsequent authors for comparing with remote retrievals, models, etc. In this case and uncertainty analysis would do more harm than good.

(See 'Effects of ice particles shattering on optical cloud particle probes' by R. Lawson (http://www.atmos-meas-tech-discuss.net/4/939/2011/amtd-4-939-2011-discussion.html)

Given a lack of published quantitative uncertainties associated with the Lawson 2D-S

Spec Inc. technique and hesitancy from Dr. Lawson to provide such results, we do not presume to make hard quantitative assumptions ourselves. And again the purpose of this paper is not to rigorously define instrument performance or Spec Inc. cloud property algorithms. It is simply to show how our bi-spectral technique can be applied to in-situ campaigns through use of SPartICus Spec Inc. data and to provide a first order estimate of instrument/ algorithm performance. We, of course, would be interested in pursuing a more in-depth analysis of instrument/ algorithm performance. But we simply had too few good cases during the SPartICus campaign for more definitive conclusions. As above, ideally, our infrared technique would be applied in concert with an instrument with high temporal and spatial resolution such as the MODIS Airborne Simulator both to maximize the number of cases and to minimize sampling issues. Input on flight plans and campaign instrumentation as well as access to all in-situ data would be greatly beneficial for a more definitive work.

To stress the fact that we do not aim to rigorously define instrument performance, we added the following sentence in the last paragraph of the introduction,

'However, given the limited number of good test cases for our technique during the campaign, and the fact that our infrared technique was not considered for design of the campaign, we cannot present either a broad characterization of SPartICus cloud properties or a definitive analysis of in situ instrument performance.'

Page 6, Line 17: "The combined FSSP and 2D-S approach yielded an re of 36.8 um". Could there have been shattering, i.e. were there and larger crystals that could have shattered and contaminated the FSSP?"

Surely, shattering may have occurred. We can only say that the in-situ results are consistent with our remote sensing approach for this large crystal case. A more detailed analysis using more test cases would be needed to address this question.