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Atmospheric Measurement Techniques Discussions

Interactive comment on "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

D Baumgardner (Referee)

darrel.baumgardner@gmail.com

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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

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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?

The remainder of my comments are primarily to clarify certain points in the paper.

Page 1, Line 24: “This debate is centered about measurements of 25 the effective 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.

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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.

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

Page 3, Line 5: "...at unambiguously accurate retrievals of re.". This seems contradicted by the next sentence, i.e. accurate but non-specific?

Page 5, Line 7: "The 2D-S probe uses overlapping laser beams to create two-dimensional silhouettes of particles with maximum dimensions exceeding 10 μm, 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.

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.

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?

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.

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

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shattered and contaminated the FSSP?

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