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Interactive comment on "Comparing Airborne and Satellite Retrievals of Optical and Microphysical Properties of Cirrus and Deep Convective Clouds using a Radiance Ratio Technique" *by* Trismono C. Krisna et al.

Anonymous Referee #1

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This is a review of the paper "Comparing Airborne and Satellite Retrievals of Optical and Microphysical Properties of Cirrus and Deep Convective Clouds using a Radiance Ratio Technique" submitted to ACPD by Krisna et al.

The paper describes a study on remote sensing of ice cloud optical thickness and ice particle size. It aims to compare airborne and satellite remote sensing measurements with each other and with in situ measurements. Much attention is given to the sensitivity of the particle size retrievals to the vertical variation of ice sizes.

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Discussion paper

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While the paper contains some interesting parts, I am struggling to see the general motivation of the study. The introduction mentions the validation of satellite remote sensing measurements and retrievals. These are indeed very important, but the main case study selected in this paper seems to be one of the worst situations for this, namely a thin cirrus over a liquid cloud. Operational retrievals using MODIS or other instruments (including SMART) will indeed not be able to account for the liquid clouds and will be biased. Accounting for a liquid cloud using additional information as is attempted in the paper is expected to add considerable uncertainty to the cirrus retrievals, making the comparison between in situ and remote sensing measurements not very informative. Any reader would wonder why this particular case is selected.

In addition, the use of MODIS measurements in this study is questionable. Measurements in the 2130nm band are used to ‘reconstruct’ the 1640nm band measurements using a scaling method that was certainly not design for cloud properties retrievals. The other MODIS band used is the 1240 nm band, but this is scaled in a somewhat ad hoc manner by a factor 0.86, which is rather large, because the data does not agree with the SMART measurements. Regardless where this factor originates from, I find it rather bold to assume without discussion that the MODIS values need to be corrected instead of the SMART measurements. Also, the influence of this scaling on the retrieved effective radius should also have been discussed. Finally, the operational MODIS retrievals of effective radius at 2130 nm are included, but these are known to be affected by the lower liquid cloud, so I do not see the relevance of including these.

Parts of the study on the vertical weighting function are interesting. Also, the comparisons between remotely sensed ice effective radius and the in situ measurements are remarkably good despite the lower liquid clouds and all the other caveats discussed above. This means that either the lower lying liquid cloud properties happen to be chosen well in this case or the properties of the liquid cloud (in particular droplet size) do not affect the effective radius retrievals of the upper layer that much. The latter explanation may be interesting and should then be further investigated in the paper.

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In its current form, the paper is not suited for publication, mainly because of the reasons listed above. I aimed to suggest changes to the paper to make it suitable for publication, but ended up with a long list. If all of these issues are addressed the paper might be suitable for publication by ACP.

Below my major comments on this paper are listed followed by some detailed minor comments.

1) The introduction rightfully states that validation of remote sensing retrievals of cloud properties is important and that accounting for the vertical variation of ice sizes is also important. However, the introduction fails to motivate the present study using the selected cases. The authors should argue convincingly why the two discussed cases are selected. The presence of the liquid cloud under the cirrus should be mentioned in the introduction and it should be argued why this and the DCC case are interesting cases for the evaluation of satellite remote sensing results.

2) Many of the references discussed in the introduction (page 3, lines 7-31) are about liquid clouds, while this study focusses on ice clouds. The influence of vertical variation on remote sensing of drop and ice sizes are very different. Please focus the discussion on ice clouds and remove references that focus on liquid clouds.

3) MODIS data is introduced in section 3.2. I assume the latest collection 6 data (level 1 and 2) is used? If so, please state that in the paper. If not, then please use collection 6 for the study.

4) Although the wavelength range of SMART is said to extend to 2200, the 2130 MODIS bands is not considered to be in its range. (This is stated rather late in the paper and should be brought forward.) The 1.64 MODIS band is selected instead, but this band has many unreliable detectors. Therefor a scaling function is used to scale 2.13 micron measurements to mimic 1.64 micron measurements. This scaling function was developed to apply a snow detection algorithm, and was never intended to be applied to cloud measurements and microphysical retrievals. One could argue that the method

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may work for ice clouds, because of the similarity of snow and ice surfaces, but this is not shown anywhere. I suggest to use the remaining detectors of the 1.64 band to verify the applicability of this method. Alternatively, would the remaining 1.64 micron detectors not be enough for you study?

5) The data filter described in section 4.2 is based on the cirrus case, while it is stated that the DCC case is more variable in time. Would a separate data filter for the DCC case be not more appropriate? Please include the DCC points in figure 2, or add two additional panels to this figure for the DCC case. Is a better agreement for the DCC case obtained if a stricter time difference is used? Please revise the paper to address these points.

6) The SMART and MODIS radiances are directly compared in section 4.3. The measurements at 1.24 micron are different by a factor 0.86, which is rather large. As stated earlier, this scaling should be discussed more and not directly be assumed to be owing to MODIS calibration errors without a proper reference. I do not know of any record about the 1240 band being biased by such an amount, although the 1240 nm band is used for several products. SMART is on an aircraft with atmosphere above it, causing possible biases in the derived reflectances. This is actually the reason why the radiance ratio method is used. So, I would think SMART is more uncertain than MODIS. Also, these biases may be very different between the two cases. In addition, please discuss (and investigate) the influence of this scaling on the resulting effective radius retrievals.

7) The general habit mixture of Baum et al. is used for the retrievals. Please add the level of surface roughness that is applied (is it severely rough?). Also, discuss the sensitivity of the ice size and optical thickness retrievals to the choice of optical model. Refer to, e.g., Holz et al. (2016, https://doi.org/10.5194/acp-16-5075-2016) and/or Van Diedenhoven et al. (2014; J. Geophys. Res. Atmos., 119, 11,809–11,825, doi:10.1002/2014JD022385.)

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8) To account for the liquid layer, in section 5.1 it is stated that "the properties of liquid water cloud are estimated by comparing simulated and measured spectral radiance averaged over the selected time series, where the reff of liquid water cloud agrees with values of in situ climatological data reported in e.g., Miles et al. (2000)." Firstly, please give some more information on the technique to obtain the optical thickness using the measured spectral radiances. Should you not have knowledge on the ice cloud optical thickness for that? Also, either here or in section 5.5, please discuss the influence of the estimated optical depth and effective radius of the ice cloud layer on the ice cloud retrievals. I am sure the cloud properties would be variable over the investigated flight leg. How are the ice cloud size retrievals affected when instead the liquid cloud is assumed to consist of, e.g., 5 or 15 micron drops? What is the uncertainty on the optical thickness estimate and how does that affect the ice cloud retrievals? The influence of these assumptions on the weighting functions are discussed in section 5.5, but please also show the influence on the retrieved ice cloud properties.

9) In section 5.3, a rather interesting investigation on the weighting functions is shown. At the end, it is stated that the assumption of a homogeneous layer in the retrievals leads to a systematic deviation. This is reiterated in the conclusions and section 6. However, this deviation is found to be smaller that 1 micron for the investigated cases. That can be considered quite small. Please stress this in section 5.3 and in the conclusions, as it strikes me as a good validation for the use of homogeneous layers.

10) The comparison with in situ measurements is interesting and an important part of the paper. However, it is unclear how effective radius is derived from the in situ measurements. Effective radius is proportional to the volume (or mass) over the projected area of the ice crystals. The CCP probes do not measure mass/volume per particle (there exists no probe that does that). I believe crystal area could be derived from the probe. Is there a separate IWC measurement? Is there an area-mass relationship used? Please explain how effective radius is derived and what the uncertainty might be.

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11) In addition to the previous point, it is not clear how the weighting function is applied to the in situ measurements. The weighting function is in terms of optical depth from cloud top, while the in situ measurements are derived at various physical depths within the cloud. How is physical depth converted to optical depth? Is there an extinction measurement made? Please explain in the paper.

12) I find it rather pointless and confusing to include the operational MODIS 2130 nm results in the analysis of section 6. It is clear that the lower liquid cloud is causing a bias in the ice effective radius retrievals. It is interesting though that the 3.7 retrievals are not much affected by the liquid layer. Please remove the 2130 nm results here.

13) In section 6, it is stated that "there is only a small correlation between the variation of in situ and retrieved effective radius which is in agreement with analyses reported by King et al. (2013).". I do not agree really. When the 2130 point is removed (which should be done), the correlation seems pretty good, especially considering the difference between 3.7 and the rest of the point, as well as all the uncertainties discussed above. What is the correlation coefficient? Also, the ranges shown on the in situ measurements are rather large, and all retrievals fall within them, which could be considered a good comparison. Please discuss this in more detail. Also, the King et al. reference is about liquid clouds, which have much greater extinction, minimizing the information on vertical structure in the various bands. This reference is not relevant for ice clouds. Please remove this reference here.

14) Section 6 ends with the statement that "a vertically homogeneous assumption in the retrieval forward simulation is not appropriate", which is also not backed up by the simulations shown, which show a <1 micron biases caused by the homogeneous layer. Please change or remove this sentence and refer to the simulations instead.

15) The conclusions section is pretty long and detailed. I suggest to summarize the general conclusions without going into too many details. Also, rewrite the conclusions according to all the changes made related to the above points.

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Minor comments:

Somewhere in the paper, give a definition of effective radius of cloud ice.

Page 3, line 32: Please define the SMART acronym on first use in the text.

Section 5: how high was HALO flying and how high were the clouds. Was is clear above the HALO aircraft?

Page 5, line 13: Irradiance is misspelled.

Page 9, line 22: I believe you mean 1640 instead of 2130 here.

Page 13, line 12: Please give a definition of Ip for completeness.

Page 18, line 3: I believe you mean "offers" instead of "affords".

Page 18, Line 13: Do not start a new sentence at "while". (Same on page 29)

Page 19, line 8: The Platnick et al. (2017) paper is also a good reference for the influence of surface albedo.

Page 21, figure 14: Can the oscillations for the 1240 ice + liquid case be explained?

Page 22, line 22: Refer to the Zhang et al (2010) paper when talking about the differences in ice absorption at 1.6 and 2.13 micron.

Page 25, line 4: What does the Delta symbol represent?

Page 25, line 8 and further. Note the good agreement between SMART and MODIS for the DCC case and give the mean differences, etc. in the same way as the cirrus case was discussed.

Page 28, line 10: In the list of possible uncertainties also note the uncertainties of deriving effective radius from the in situ measurements and the uncertainties caused by unconstrained choice of ice optical model for the retrievals.

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