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Interactive Comment

Interactive comment on "Design and characterization of specMACS, a multipurpose hyperspectral cloud and sky imager" *by* F. Ewald et al.

F. Ewald et al.

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Introduction

We thank referee #2 for his/her careful reading, comments and suggestions which we address in the following. The authors' answers are printed in italics:

Remark: The figure numbers in the referee comments and the page numbers in the authors' answers are corresponding to the original manuscript. If not stated otherwise, figure and equation numbers in the authors' answers are referring to *the revised, marked-up manuscript version (showing the changes made) which can be found attached to this answer.*

Major comments

- This paper should focus more on results of the specMACS rather than general background of FPAs, radiometry, and characterizations. For example, §4.1 and §4.2 describe photon and noise sources, polarization effects, non-linearities, and bad pixels. Nearly all of §4.1 is a tutorial on characterizing FPAs. That is already done in other publications and needn't be reproduced here. What is presented in §4 is generic and presents no new or specific information regarding the specMACS itself. For this reason alone, this section should be eliminated, referencing other papers for general FPA and characterization background. The only remaining items should be those specific to how the specMACS was actually characterized, and they should be moved into §5. As currently written, this generic background section (§4) detracts from the actual specMACS characterization results. §5 and parts of §4.2 are where the paper gets to be non-generic.

  → *Thank you very much for your time and effort in compiling this thorough and detailed review! Attached to this answer you will find a diff for the revised manuscript. As one mentioned issue affected the overall structure of the manuscript, we first restructured the text before doing the diff. By doing so, all suggested reductions/changes can be tracked more efficiently. Please also note our answers to referee #1.*

  – My suggestion would be to eliminate §4 altogether and add only the necessary specMACS specific items to §5, where the specMACS results are presented.

AMTD

8, C4642–C4661, 2016

Interactive Comment

→ *This was also suggested by the other referee. We therefore restructured the manuscript: each calibration theory section is now directly followed by the results and the discussion of the results. All passages, which were not essential for the performance of the instrument, have been removed and we focused more on the performance and the measurement uncertainties.*

- §1.1 fails to clearly show requirements flow and derive radiometric accuracy requirements (vs. goals).

  - Absolute accuracy requirement is not well derived. "We aim for an absolute radiometric calibration uncertainty of 5% or below" seems a bit arbitrary from the preceding text, especially when followed by a 10% radiance error.

    → *This issue is dealt with in the following answers.*

  - "...a spectral bandwidth and accuracy of 10–50 nm is supposed to be sufficient" is not very definitive. What is this based on? What are specMACS' driving spectral requirements?

    → *In the revised manuscript, we remove this specific accuracy requirement, since it was unfounded as also noted by reviewer #1. However, we reformulated the spectral accuracy requirement as follows:*
    P9857, 27ff:

    > *Spectral accuracy requirements are not too strict for current microphysical cloud retrievals, as no sharp absorption line is evaluated. However, the solar spectrum itself exhibits many narrow absorption lines. For this reason, the spectral accuracy should be comparable or better than the spectral bandwidth of the instrument. The radiometric accuracy can be compromised if resolved absorption lines are spectrally misaligned.*

- It is ambiguous whether the accuracy requirements are 5, 10, or 20%. §Does 2 μm in aerosol radius correspond to a 20% radius error (for what type of aerosol?), and does this in turn correspond to 20% in radiance uncertainty? Confusingly presented.§Is 20% at 2100 nm truly an absolute radiometric accuracy and not just a relative accuracy across the spectrum? This seems more a relative rather than absolute radiometric accuracy requirement.

  → *As this was also mentioned by reviewer #1, we reassessed this accuracy requirement. The retrieval of Nakajima and King (1990) is not based on a relative, channel ratio technique but is rather based on absolute radiometric measurements at* $870\,\mathrm{nm}$ *and at* $2100\,\mathrm{nm}$*. Thus, we aim for an absolute radiometric calibration uncertainty way below the mentioned radiance uncertainty of 20%. For a constant droplet size, this value corresponds to the spread of radiance values caused by 3-D effects as found by Martins et al. (2011) and Zinner et al. (2008).*
  P9857, 17ff now reads:

  > *For optically thick liquid water clouds an uncertainty in effective radius of* $2\,\mu\mathrm{m}$ *relates roughly to a radiance uncertainty of 20 % at the near-infrared wavelength* $2100\,\mathrm{nm}$ *used in the retrieval of (Nakajima and King, 1990). To limit the uncertainty in microphysical retrievals due to sensor characteristics, we aim for an absolute radiometric calibration uncertainty well below the retrieval uncertainty.*

- Uncertainties are not addressed. No attempt is made to demonstrate that they sum to less than the required accuracy (which is not clear itself). Thus, the paper's claims that the requirements are sufficient for the desired atmospheric measurements are not justified.

AMTD

8, C4642–C4661, 2016

Interactive Comment

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Interactive Discussion

Discussion Paper

→ *Thank you for this suggestion. I think we really missed out on this point, since the combination of all uncertainties into an error budget and a subsequent application to real-world measurements is of highest interest to the reader.*

For this reason, we added a new section "Overall radiometric uncertainty budget", where we show how the different uncertainties can be summed up to an overall uncertainty - this can be found in the diff below this text. Furthermore, we now give detailed equations/descriptions, how the dark signal uncertainty, nonlinearity uncertainty, uncertainty due to polarization, instrument noise and calibration uncertainty can be evaluated for real-world applications. The different uncertainties are then combined into the overall radiometric uncertainty budget.

- §4.1 and §4.2 describe idealized effects only. Non-ideal effects are largely neglected. Examples include:
  - thermal background from the instrument itself;

    → *Thermal background other than noise should vary only on timescales of the temperature variation of the instrument. We do consider thermal background implicitly through repeated measurements of the dark signal (typically at an interval $\leq 2\,\mathrm{min}$) and the consideration of the unknown drift of the dark signal between those measurements into the error calculations. Noise is generally monitored through repeated dark signal measurements as well and considered separately in error calculations. We think that the discussion of dark signal dependence on sensor ambient temperature as described in the dark signal section does emphasize this relationship and its mitigation sufficiently.*
  - polarization leakage through the wire grid calibration polarizer (which varies with wavelength);

- → *The polarization leakage (99.9 % degree of polarization between 400–2500 nm) was already stated in the discussion manuscript. We kept that statement.*

- the use of air conditioning to hold ambient air temperatures constant (HVAC systems are bang-bang closed loop feedback systems with thermal variations that must be accounted for);

  → *Those systems can indeed show thermal variations. Thus, we ensured that the time between connected measurements was minimized. Most importantly, dark signal measurements were performed as fast as possible for every single characterization measurement (typically* $\leq 30\,\mathrm{sec}$ *time difference). Furthermore, we included an estimate of the dark signal drift in our error calculations throughout the characterization process and all operational measurements. We assume that this is enough to consider uncontrollable thermal variations like those induced by HVAC systems.*

- the difference between nominal and actual FPA integration times (which was nice to see identified in §4.1.3 – although how this difference varies with integration time was not described).

  → *We have no good method to directly measure the difference between nominal and actual integration time. However, the nonlinearity model as described in the manuscript shows a very good agreement of all our measurements with different integration times. As we used 9 different integration times and only needed to include a single offset, we are quite confident that the difference does not change significantly with integration time. We added a note about this to the manuscript.*

• Several sections of the instrument itself provide far more details than needed. For example, each of §3.2.x is overly detailed and should be shortened to a sen-

AMTD

8, C4642–C4661, 2016

tence or two and consolidated into a single §3.2. These details are not particularly relevant to the overall instrument capability for achieving and demonstrating radiometric accuracy. The corresponding figures (5, 6, and 7) should similarly be eliminated as they provide no new information to the paper.

→ *In our restructured version of the manuscript, following sections have been drastically reduced or moved into the appendix:*

  - *Removed: "Detailed instrument concept" and "Software description"*
  - *Moved into the appendix: "Auto Exposure" and "Dark current measurements"*

  We left a reduced version of "Instrument automation", where the reader is referred to the detailed descriptions in the appendix. See the diff attached to this answer to see all reductions in detail.

  In the revised manuscript, Fig. 5 was removed and Fig. 6 moved into the appendix. We think that Fig. 7 gives a good overview which aids the reader in following the described characterization and calibration process. Likewise, we left Fig. 1 and Fig. 17 in the manuscript (they are now both in the application section) since we want to give the reader an idea about the different instrument setups.

- Conclusions: The Conclusion fails to summarize the intended and achieved requirements or present justifying uncertainties to show those requirements were met. Result 3.) (R being stable over time) is not demonstrated via any long-term data. This result is also unclear on the radiometric accuracy level obtained and needed (10%, 5%?).

  → *We approached this issue twofold. First, the conclusion now summarizes the present radiometric uncertainties in the following new paragraph:*

The available error budget calculation now allows to estimate the significance of different radiometric uncertainties. For the VNIR, major contributions to the overall radiometric uncertainty of around 5 % are caused by the calibration uncertainty of R (error of $\approx 3\,\%$) and the polarization sensitivity for highly polarized light (error $\leq\ 5\%$ for fully polarized light). Without the nonlinearity correction, the radiometric signal would furthermore be strongly biased (-9 % at high signal levels). For the SWIR, major error contributions to the overall radiometric uncertainty of around 10 % are caused by the uncertainty of the absolute radiometric standard itself (error of 5 to 10 %, $\lambda > 1700\,\text{nm}$) and the dark signal drift for low exposed regions (error of $20\,\%$ and more, depending on the frequency of dark frame measurements).

In a second paragraph, the radiometric and spectral uncertainties are summarized and compared with the intended requirements as follows:

The final evaluation shows that the instrument performance complies with the accuracy requirements stated in the introduction. Absolute radiometric accuracy well below the mentioned 3-D radiative effects can be achieved when the described signal calibration procedure is applied. The radiometric error budget proves that the radiometric uncertainty for well lit cloud scenes can be held well below 20 % over the full wavelength range of the instrument. This is also confirmed by the good agreement between both spectrometers in the overlap region around $1000\,\text{nm}$. As initially demanded, the spectral bandwidth is the limiting factor for the spectral accuracy of the instrument. Thereby, the spectral bandwidth of the VNIR with $3.1\,\text{nm}$ is well above the found spectral smile of $0.3\,\text{nm}$ and one order of magnitude larger than the spectral calibration accuracy of $\pm$0.1 nm. As well, the SWIR spectral bandwidth of $10.3\,\text{nm}$ is *larger by one magnitude than its spectral smile of* $1.1\,\mathrm{nm}$ *and larger by two magnitudes compared to the spectral calibration accuracy of* $\pm 0.2\,\mathrm{nm}$*. Spectral calibration accuracy fully meets the requirements of current microphysical cloud retrievals and enables reliable identification of gaseous absorption lines. The spectral bandwidth below* $1000\,\mathrm{nm}$ *should be sufficient for the analysis of absorption line depths of features like the oxygen A-band.*

Minor comments

- §5 is where this paper gets to be relevant for specMACS. Figs. 8-16 nicely show results.
- The beginning of several sections includes repeated background that was presented in prior sections, making the paper disjoint. These redundancies in descriptions and even acronym definitions should be removed by the authors to better focus on the paper's intended results. Examples include:
  - The first paragraph of §4.2.4. (The remaining four paragraphs in that section, however, are a good example of the specMACS-specific characterization details that should remain in the paper, albeit moved to §5.)

    → *After the suggested restructuring of the manuscript, this section is now moved to §5.*
  - "Either k [DN] varies with signal level S0, which would cause a photon response non-linearity (PRNL) or a charge sharing is occurring between pixels which would violate the Poisson assumption" is redundant with §3. Suggest removing from §3 when shortening/eliminating that section and including such statements only here in §5.

AMTD
8, C4642–C4661, 2016

Interactive Comment

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Interactive Discussion

Discussion Paper

→ *This is correct - in the revised manuscript this information can now be found in §5.*

- Some of the calibrations are lacking in thoroughness. These are not sufficient to reject the paper, which describes what the authors actually did and which is still relevant for the instrument's characterization, but overlooked calibration items such as those listed below should be acknowledged as lacking in the paper.

→ *We have added a subsection "Open points on characterization" which lists open points.*

  - §4.1.2: Both FPAs are sensitive to thermal background, so will never have S0=0 even with incident light blocked. That thermal background similarly scales with integration time, providing an additional component affecting Eqn. 5.

    → *It is true that FPAs are sensitive to thermal background. However, in our calibration method, $S_0 = 0$ by definition and the thermal background gets absorbed into the repeatedly measured dark signal. We improved the wording to point this out more clearly.*

  - Thermal characterizations seem to have been done inadvertently (via flight results) rather than controlled intentionally as part of the planned calibrations (§4.1.2).

    → *This is somehow true. However, the thermal characterization is not needed during our data calibration procedure, as we do not explicitly use a thermal model. We already anticipated variations due to changing environmental conditions and therefore scheduled repeated dark signal measurements for all measurements preceding this characterization. The presented characterization of the thermal influcence on the signal is therefore primarily to obtain an estimate for the dark signal drift*

AMTD
8, C4642–C4661, 2016

during normal operating conditions as this is needed for the radiometric uncertainty estimation. An additional note has been added.

- Diffraction contributions to angular resolution are never mentioned (§5.2.1).

  → *This is true. We did only characterize the total angular response of the full system including foreoptics, spectrograph and FPA. We see no need for a discussion of separate contributions as we later only use the FWHM of the full-system line spread functions.*

- §4.1.1: FPN can change with temperature and intensity. These variations do not seem to be taken into account in characterizations.

  → *This is true. A note has been added.*

- §4.1.3: Non-linearities are only characterized by changing integration time with constant signal level. This folds in variations in integration time and does not allow for true FPA non-linearities due to varying incident intensities. Varying intensity levels can give different non-linearity results than obtained by only varying integration time.

  → *This is true, however very difficult to characterize reliably. We currently do not have a setup which reliably allows this kind of characterization. However, we have some indications that non-linearities due to varying incident intensities are only playing a minor role. We added a description of these indications and added a further note that a characterization with varying light levels would be needed for final confirmation. See also our answer to the issue "Approach to vary sensor signal for nonlinearity calibration" raised by referee #1.*

- Calibrations would be more convincing than mere stray light modeling (especially since that modeling was not done prior to initial detection of stray light problem, which, although seemingly obvious, apparently came as a surprise to the authors).

AMTD

8, C4642–C4661, 2016

→ *This is true. We did not do this and added a note. We also added a plot which illustrates the effectiveness of a prototype of the stray light protection. See Fig. 3 in the appended manuscript diff attached to this answer.*

- §4.1.1: Poisson distribution presumes the dark noise during each readout is large. Is it?

  → *For the SWIR spectrometer, the largest part of the dark signal is the dark current signal level (comparable with the photoelectric signal for long integration times). We therefore assume a Poisson distribution for the noise of this level. With the VNIR, the read-out noise level seems to be larger than the dark current signal (there is almost no dependence of the dark signal level on integration time). Therefore, we can not be sure if this assumption holds true for the VNIR. We therefore removed this statement for the dark current noise, because we do not use this assumption in the following analysis (it is only used for the photoelectric signal).*

- Read noise in §5.1.1 is conflicting. The text gives 5.0 and 4.5 DN for the VNIR and SWIR, whereas the y-intercept from Eqn. 15 and 16 would instead indicate 5.07 and 4.77 DN respectively. (These latter, from Eqn. 3, include dark current noise, but the text claims the read noise is from this y-intercept alone. And no explanation is given for how the read noise and the dark current noise are differentiated at the low signal levels here.)

  → *That is correct. The y-intercept from (former) Eqn. 15 and 16 is from the sqrt fit and thus should include dark current noise as well as read-out noise. The red lines, indicating the read-out noise were obtained by a mentioned second fit with a constant function for data points between 0 DN and 30 DN to differentiate between dark signal noise and read-out noise (since these*

AMTD
8, C4642–C4661, 2016

data points originate from very short integration times). There is surely some residual dark noise in there. Therefore, we renamed the label from 'read-out noise' to 'read-out channel'. The method to differentiate at the low signal levels here was already described in the original manuscript:

P9879, L21ff

> *(...) At low values of $\langle S_0 \rangle$ the signal-independent read-out noise becomes apparent. The read-out noise for $t_{\text{int}} = 0\,\text{s}$ is derived as the y-intercept of a constant fit on $\sigma_\mathcal{N}$ for $\langle S_0 \rangle < 30\,\text{DN}$. By doing this, the noise associated with the readout channel was found to be $5.0\,\text{DN}$ for the VNIR and $4.5\,\text{DN}$ for the SWIR spectrometer. For larger values of $\langle S_0 \rangle$ the noise begins to increase.*
> *When the standard deviation $\sigma_\mathcal{N}$ is fitted with the square root model following Eq. (12), the noise characteristics can be further investigated (...)*

- §5.1.4: This is a response of the entire system, not just the cameras (FPAs) since it includes the transmissivity of the optical system. Text suggests it is only the response of the FPAs.

  → *We sometimes use the term "camera" to denote the whole assembly consisting of the spectrograph, the FPA, other optics, electronics and the case. This might be confusing in this place so we changed the word "camera" to "sensor". In the next sentence, we also refer to the optical system as responsible for the shape of the response, which should sufficiently clarify that the response is meant for the entire system.*

- §5.1.5: The maximum error would be P, not P/2, assuming the (unknown) input light is completely polarized in a direction of minimum or maximum instrument sensitivity.

AMTD

8, C4642–C4661, 2016

→ *This is a very good point. However, we think that neither P, nor $P/2$ is actually correct. We have reformulated the fitted formula slightly for clarification. Now P is defined more intuitively: it is 0% for an instrument totally insensitive to polarization while it is 100% for an instrument with maximal polarization sensitivity (e.g. a polarization filter). We added a note about the derivation of the resulting radiometric error which now also allows to include an estimate p_{max} of the maximum degree of polarization of the incoming light. In the revised version, the definition of P changed such that it is now approximately $1/2$ of the previous P. The maximum error introduced by an unknown polarization of the incoming light is now described as: $\Delta s_{\text{n}} \overline{s_{\text{n}}^{\text{P}} \leq \frac{p_{\text{max}} P}{1 - p_{\text{max}} P}}$ which is approximately $p_{\text{max}}P$ for small values of $p_{\text{max}}P$ but grows faster as $p_{\text{max}}P$ rises. For small P and completely unknown polarization of the incoming light, the maximum error is thus approximately P. This is now inline with your statement.*

– Conclusions section: Result 9.) (polarization sensitivity) should be 10% and not 5%.

→ *Following the change above, the radiometric error introduced by an unknown polarization amounts to about 5.3%. This has been corrected in the conclusions section.*

- No discussion is given of long-term stability of instrument sensitivity or cleanliness environment in which the instrument is stored to reduce optical surface contaminants and maintain calibration accuracy.

→ *That is correct. Since the instrument is quite new, this manuscript marks the beginning to monitor the long-term stability of the instrument sensitivity. This is also mentioned in the new recommendation section for future studies:*

AMTD

8, C4642–C4661, 2016

> *Although R seems to be quite stable, the calibration should be repeated over time (...)*

Only after evaluating the radiometric stability in a further study, we are in a position to discuss the effectiveness of our instrument storage.

- Usage of "spectral channel" by number in text (§5) and figures (Figs. 12, 14, and 15) is meaningless to a non-specMACS reader. Convert to wavelength units.

  → *In the revised manuscript, this information will be shown in a secondary axis.*

- Should delete Figs. 1, 5, 6, 7, and 17, which do not show relevant characterizations or results.

  → *Already answered in the major issues section.*

Typographical and grammatical suggestions

- There are many typographical and grammatical mistakes throughout the document. I started listing these but eventually decided that with the major reductions/eliminations I suggest of §3 and §4 that the authors should improve the paper's grammar, spelling, and punctuation with that rewrite. Such corrections are more their responsibility than mine, and I assume the authors or the editor will find and correct these prior to final publication. I do include several such corrections below.

  → *Thank you very much for pointing this out. During the reduction/restructuring of the manuscript, we tried to corrected grammar, spelling, and punctuation. The revised manuscript was also proofread by a native speaker.*

AMTD
8, C4642–C4661, 2016

- Past, present, and even future tenses are often mixed even in single sentences. Needs consistency.

  → *We tried to correct for that as appropriate.*

- Overuse – and in other places the lack of use – of appropriate commas is persistent in paper.

  → *We rechecked the use and overuse of commas in the revised manuscript.*

- Some acronyms, such as LIS, are defined repeatedly; and some are then not even used.

  → *This was corrected in the revised manuscript.*

- The word "data" is plural but is incorrectly used throughout this paper with a singular verb.

  → *We replaced the word "data" with "measurements" where applicable.*

- "Can not" should be replaced with "cannot" in most instances.

  → *Corrected.*

- Naming of figures is inconsistent ("Fig." vs. "Figure").

  → *This was, in part, done during the typesetting of our manuscript. We will pay attention to that during the typesetting of the revised manuscript.*

- A "line camera" (Abstract and later in text) implies a single-row array rather than a 2-D spatial/spectral FPA.

AMTD

8, C4642–C4661, 2016

→ *We removed the word "line".*

- What are "core sensors" in Introduction? Suggest removing.

  → *We removed this subclause.*

- §2.1: λB is undefined.

  → *This paragraph was removed during the reduction in the manuscript.*

- §3: What is "MiB"?

  → *According to NIST (http://www.physics.nist.gov/cuu/Units/binary.html) and various international standards (e.g. IEC 60027-2, Second edition, 2000-11, Letter symbols to be used in electrical technology - Part 2: Telecommunications and electronics) the unit* MiB *was introduced to avoid confusion between prefixes corresponding to a multiple of* 1000 *and a multiple of* 1024 (2^{10}). $1\,\text{MiB}$ *is equal to* 2^{20} Bytes.

- Eqn. 3: σ_{dark} is not defined.

  → *After the restructuring of the manuscript, this is now defined before in the dark signal section.*

- §4.2.6: What does "problem dependent" mean?

  → *The line now read: "As the adequate grid depends on the particular application ..."*

- Figure 10: Are values in DN, as caption states, or DN range in percent, as plots show? (The latter seems correct, but caption conflicts with plots and with §5.1.2.)

AMTD

8, C4642–C4661, 2016

→ *Correct. The caption now reads "Dark signal (avail. DN range)".*

- Some examples of needed typographical and grammar corrections are listed below with their current (rather than corrected) incorrect wording underlined:

  - Abstract: "Equipped with a high spectral and spatial resolution, the instrument..."

    → *Now reads: "With its high spectral and spatial resolution"*
  - Abstract: "...the spatial and spectral performance was assessed."

    → *Now reads: "...the spatial and spectral resolution was assessed."*
  - Lots of "e.g."s break up flow in Introduction

    → *Removed some "e.g."s.*
  - Introduction: "...improvements of our understanding ... is pursued."

    → *Now reads: "... are pursued."*
  - Introduction: "...measurements becomes possible."

    → *Corrected.*
  - Introduction: "High spectral resolution measurements are needed" would be better than "Spectrally high resolved measurement is needed..."

    → *Changed.*
  - §2: "A scanning setup allows to capture..."

    → *Now reads: "A scanning setup facilitates..."*
  - §3: "...the system must be able to run fully autonomous..."

    → *Now reads: "...the system must be autonomous..."*
  - §5.1.4: There are lots of unnecessary "case of" pairings that should be removed.

AMTD
8, C4642–C4661, 2016

→ *Removed most of these "case of" pairings.*

- §5.1.5: What does "...the polarization dependent signal loss becomes maximal for the particular pixel" mean?

  → *Now reads: "and ϕ_0 the polarization orientation for which $s_\mathrm{n}^\mathrm{P}(\phi)$ is maximal."*

- §5.2.1: "...LSF for the VNIR sensor is show in Fig. 8a"

  → *Corrected.*

- §5.2.1: "For both spectrometer the strongest keystone distortion occur..."

  → *Corrected.*

- Conclusions: "...allowed to measure..."

  → *Now reads: "...facilitates to measure..."*

- Conclusions: "...allow to estimate..."
- Conclusions: "...straylight..."
- Conclusions: "...achived..."
- Conclusions: "...requierments..."

  → *Corrected.*

- Conclusions: "...exemplarily..."

  → *We think that "As shown exemplarily in Fig." should be correct.*

References

Fielding, M. D., Chiu, J. C., Hogan, R. J., and Feingold, G.: 3D cloud reconstructions: evaluation of scanning radar scan strategy with a view to surface shortwave radiation closure, J. Geophys. Res.-Atmos., 118, 9153–9167, doi:10.1002/jgrd.50614, 2013.

AMTD

8, C4642–C4661, 2016

Martins, J. V., Marshak, A., Remer, L. A., Rosenfeld, D., Kaufman, Y. J., Fernandez-Borda, R., Koren, I., Correia, A. L., Zubko, V., and Artaxo, P.: Remote sensing the vertical profile of cloud droplet effective radius, thermodynamic phase, and temperature, Atmos. Chem. Phys., 11, 9485–9501, doi:10.5194/acp-11-9485-2011, 2011.
Nakajima, T. Y. and King, M. D.: Determination of the optical thickness and effective particle radius of clouds from reflected solar radiation measurements. Part I: Theory, J. Atmos. Sci, 47, 1878–1893, 1990.
Zinner, T., Marshak, A., Lang, S., Martins, J. V., and Mayer, B.: Remote sensing of cloud sides of deep convection: towards a three-dimensional retrieval of cloud particle size profiles, Atmos. Chem. Phys., 8, 4741–4757, doi:10.5194/acp-8-4741-2008, 2008.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/8/C4642/2016/amtd-8-C4642-2016-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., 8, 9853, 2015.