Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2018-220-RC1, 2018

Atmospheric Measurement Techniques Discussions

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Interactive comment on "Cloud geometry from oxygen-A band observations through an aircraft side window" *by* Tobias Zinner et al.

Anonymous Referee #3

Received and published: 2 October 2018

The paper presents an algorithm for determination of cloud shape/geometry using cross-track scanning spectrometer making measurements in O2 A-band. The authors made an extensive retrieval error estimation effort and honestly discuss challenges facing the proposed approach. These challenges appear to be so serious that the whole retrieval algorithm needs substantial modification (see my comments below for details). I hope that the authors will be able to improve their technique during the discussion and reply to reviews period, so the paper could be then accepted after major changes.

General comments ————-

The main issue affecting the proposed retrieval technique is multiple scattering of light in essentially 3D cloud geometry. This creates a kind of a circle: cloud shape is determined point-by-point using LUT based on 3D radiation transfer model, which itself has

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Discussion paper

to imply a specific shape of cloud as a whole. Simplified assumption of cloud shape ("cloud wall") leads to substantial bias (3.8 km) in the derived cloud position compared to an independent stereo dataset (which is assumed to be more robust).

The authors should also note that the same stereo dataset cannot be used for both correction of A-band retrievals and for validation of them. The 3.8-km offset applied to A-band data (making it to agree better with stereo results) is discussed in the paper, however, it is not even mentioned in the abstract when the accuracy of the proposed approach is described. This misleads the reader.

The paper in its present form describes a technique that currently does not work, and its results are artificially forced to agree with stereo dataset for "validation". This is certainly is not worth publishing. In my opinion the only way to save the paper is to follow the authors' own suggestion (last paragraph of Conclusions Sec.) and incorporate stereo measurements into A-band algorithm as an occasional "calibration" source. (Note that stereo measurements may not be used for validation then.) Only after this is successfully done (which is a "major change") paper can be accepted for publication.

English in the paper is generally acceptable except for few sentences that need clarification. However, the paper (Conclusions in particular) would certainly benefit from proof-reading by a native English speaker.

Line comments ————-

p.1, l.1: Is ACRIDICON-CHUVA an acronym? If yes, spell it out (may be in the paper text rather than in the abstract).

p.1, ll.12-14: The last sentence of abstract is misleading (see general comment above), since it does not mention the 3.8-km offset subtracted from A-band retrievals. This operation makes A-band retrievals dependent on stereo dataset, which then cannot be used as direct validation source.

p.1, ll.13-14: Distance accuracy should be compared with typical intrinsic property of cloud field, such as horizontal scale (what was typical cloud size?), not with typical distance between cloud and aircraft.

p.3, l.14: Making measurements through aircraft window (rather than through instrument's own window) may present a problem. Any window can be dirty or scratched, and this should be factored into instrument's calibration. Unlike instrument's own window, aircraft's window cannot be taken into calibration lab. How specMACS was calibrated? Can additional absorption by the window potentially bias A-band measurements?

p.3. l.18: Add publication year to "Ewald et al."

p.6., ll.16-19: This paragraph needs clarification: has the spherical cloud been moved up and down relative to the sensor height in order to achieve different cloud surface angles?

p.7. Fig.7m: "sphere angle" is not defined or used in text.

p.7, caption; p.8, l.1; p.9, ll.11-12; p.10, l.6; etc.: Change "zenith" to "zenith angle".

p.7, caption: Use subscript 's' for sensor altitude 'z' (as it is used later in text).

p.7, caption: Caption is longer than the page, only part of it can be seen.

p.8, ll.7-19: Parts of this list are repeated in Table 1 and can be omitted.

p.8, l.8: The whole line is unclear. As viewing direction is not necessarily horizontal, what is "horizontal extent in viewing direction". What does "infinite, periodic perpendicular to it" mean? Please, clarify.

p.8, l.19: I suggest to include 'z_s' on this line, since it is used below it without definition.

p.8, ll.23-26: "For a given sensor height the cloud wall is visible under certain observation zenith angles only up to a certain distance." This implies certain finite cloud top and bottom heights, which are not defined or discussed. What are they in the model? This may also contradict l.8 (see above) stating that cloud is somehow "infinite". Please, clarify.

p.9, l.14: The phrase "filtering cloud edge areas for the remaining cloud situations" is completely unclear. And what is "cloud edge" (also used in other places in the paper) compared to "cloud surface"?

p.10, l.4: I suggest to define "R = R_O2A" here as it is used without subscript or with different subscripts below this line.

p.10, eqs.2&3: I am confused. Dependences of R = R_O2A on all parameters presented in Fig. 3 seem to be monotonic. Then, R_min < R_LUT < R_max or R_max < R_LUT < R_min (depending on definition of R_min and R_max) for any parameter (pressure, etc.). In both cases R_LUT cancels from the numerator of Eq. (2), thus, Delta R = 2 sigma_R. Why this relation is not satisfied in Table 1? Why both Delta R and sigma_R are needed?

p.10, l.11: sigma_R defined by Eq. (3) should be called "relative dispersion" (ratio of the standard deviation to the mean) rather than "standard deviation".

p.11, Fig.5: Here and in the rest of the paper it is not clear what the authors mean by "distance": horizontal distance or distance along the viewing direction. Please, clarify.

p.11, Fig.5 and ll.3-17: Important: the absolute accuracies (in meters, not %) of distance measurement must be presented instead of relative ones. Measured distance is only an intermediate step towards derivation of cloud shape and geographic location, which are independent of the sensor position. Thus, only the absolute values of uncertainties in derived distance have physical meaning. The same, say, 600 m accuracy of cloud location determination (the only thing that matters) can be 1% if the distance between cloud and sensor is 60 km or 10% if this distance is 6 km, thus, speculation that 1% is better than 10% does not make any sense.

p.11, ll.3-5: The sentence sounds a bit awkward. I suggest: "At low altitudes most cloud sides can be observed looking upward, while for high altitudes they are observed looking downward."

p.12, ll.15-16: "uncertainty of oxygen-A retrievals is likely to be larger than that of the stereo derivation": How much larger? Comparison is hard since the stereo accuracy is given in meters (200-300 m), while that of A-band measurements is given in Section 2.2.3 in % of the distance between cloud and sensor (see comment to Fig. 5 above).

p.12, l.23: How relative azimuth is defined in the paper? If here ∼180 deg azimuth means that the sun is behind the sensor, does it mean that the azimuth of 0 deg in Table 1 corresponds to the sun in front of the sensor (which would not make much sense)?

p.12, l.34: I suggest to replace "shown as well" by "also shown in Fig. 6".

p.13, Figs.6(c)&7(a): Are these distances horizontal or along sensor's line of view to the point?

p.13, Fig.6(c,d): It is difficult to see comparison between A-band and stereo distances/heights since the former are shown by color, while the latter - by numbers. I suggest to include (in addition to this figure) several single-scan plots showing 2D cross-section of cloud surface derived from A-band measurements with stereo points plotted over. This could also show how/if stereo points can be used for "calibration" of A-band retrievals on single scan or single cloud scene bases.

p.14, Fig.7: Make both plots square (since x and y have the same physical meaning and scale) and set them side by side. Provide means and standard deviations of (y-x).

p.14, Fig.7: The meaning of points' colors is not explained neither in text nor in caption. Do they correspond to the mentioned 27 cloud scenes?

p.14, ll.3-4: Replace "200 x 200 mˆ2" by "200 m x 200 m".

p.15, ll.9-10; p.16, ll.12-13: My impression from looking at Fig. 7(a) is that if different cloud scenes (assuming that they are identified by point colors) are considered separately the fitting offsets in them would significantly vary (and may even have different signs). I do not see convincing justification that all these scenes belong to the same "type" so the same 3.8-km offset is characteristic for all of them.

p.15, ll.18-19; p.16, ll.17-18: "In general, such a "calibration" of the method could also be reached using a limited number of synthetic cloud model based simulations." Here the authors try to downplay the complexity of 3D RT simulations contradicting their own words in the first paragraph of Sec. 2.2.2. I do not believe in "universal" 3D-RT LUT based on a few of cloud scenes.

p.16, l.9: Delete "towards the horizontal", change "shortcuts" to "decreases".

p.16, l.10: Replace "towards" by "for".

p.16. l.12: What is "simulated geometry path"?

p.16, ll. "Eventually a combination of stereo and absorption methods might be a beneficial approach, as advantages of both methods could be combined". This is what needs to be done in the next revision of this paper (not eventually), potentially creating a working retrieval method. Applying a campaign-wide offset to all A-band retrievals can be considered as a first approximation to a working synergistic approach in which stereo-based corrections should be made at each individual cloud scene.

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