

Interactive comment on “CLABAUTAIR: a new algorithm for retrieving three-dimensional cloud structure from airborne microphysical measurements” by R. Scheirer and S. Schmidt

R. Scheirer and S. Schmidt

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First we want to thank all three referees for their careful review. Our responses in detail:

Referee J. Brenguier

General comments

- *...They claim twice that the algorithm generates 3-D cloud fields without any ad-*

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ditional assumptions...

The reviewer is right, our statements are misleading. We follow the suggestions and discuss the mentioned items.

- *Advection of the cloud field during sampling*

During the measurements described in that paper, we've had no trouble with advecting cloud fields, but even if the cloud field is shifted, as far as the cloud speed is small compared to the aircraft's speed and the cloud-structure is almost stable one may simply correct the initial field by the vector of the cloud-shift. The autocorrelation function shouldn't be affected seriously. Anyway, we address a remark on this topic.

- *Stationarity of the cloud system during the sampling*

We are sorry for the obviously incomplete description of our tests. We didn't sample 200 flight legs for the comparison. We performed 200 random flights consisting of about 10 to 20 flight-legs (depending on the random-directions as can be seen in Fig. 2) each. The distance covered by the aircraft within the cloudy layers is constant. For the ascend we got (climbing height 400m, 10° ascending angle) about 2304m and for the descend (0.5° descending angle) about 45837m. We add some more details to clarify our approach.

- *Horizontal isotropy*

Horizontal isotropy is not presumed. The autocorrelation functions are used only for the same direction as they were sampled. Explanations are added respectively.

- *Vertical stratification*

Though it is a good idea to give the height of measurements above cloud-base rather than above ground we don't see a possibility to apply a cloud-base-following vertical coordinate to aircraft measurements. These aircrafts are unable

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to perform vertical flights and so there is no chance to estimate the actual distance to the cloud-base.

- *Finally, I would remove the word “automatic” in the description*

We don't think that this is necessary. Far from it, the algorithm remains straightforward. While there is a need for initial settings (case by case) of wind speed and direction, requested resolution and so on the further calculation process requires no additional intervention.

Specific comments

- *The statement that LES simulations do not necessarily represent real or even realistic cloud fields is not supported by the literature and should be removed*

We follow that suggestion and replace the above phrase. It reads now: “Physical cloud-models (e.g. LES) can provide the full 3D information of all needed micro-physical properties in a realistic manner but it is hardly possible to make them reproduce the properties of measured cloud fields”

- *The statement that cloud droplets do not follow Poisson statistics is wrong and shall be removed*

We wouldn't call this statement wrong but we agree in the opinion that it has to be changed and that we have to be more precise. Our intention was to point out that though individual droplets are poissonian distributed there is an underlying structure determining its probability. The regarding text passage is revised.

- *replace “during which the cloud can be considered constant” by “during which the cloud system is statistically in a steady state”*

We do so.

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General comments

- *It still remains to be shown whether, and which situations, the approach can generate realistic cloud structure*
That's true. Our tests applied so far implicate static fields only. We don't regard these tests as sufficient but they are necessary. In fact we've extensive tests (including radiative transfer tests) in preparation.
- *...the algorithm is not free of assumptions*
We remove this statement (see above).

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Specific comments

- Theoretical basis and behavior of Equation 1*
Equation 1 simply describes a weighted mean. A linear weighting is as good as a quadratic one. The further procedure of this algorithm equals out any difference as long as the sort sequence is not changed. We add some more details on this topic.
- ...Equation 1 cannot represent anisotropic cloud fields properly.*
We disagree. The weightings are performed only in the originally measured direction (see above). We add a clarifying explanation.
- The vertical correlation receives only marginal attention*
The vertical correlation coefficient of 0.95 seems to be somewhat arbitrary. In fact it is our first guess of an assumed large vertical correlation coefficient in lack of measurements or calculations. We wouldn't call it a tuning coefficient. In

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the moment we are collecting profiles of LWC and effective radii to get a funded estimate of this parameter - probably resolution dependent. We add a note on this topic.

iv) *Anomaly field generated by equation 1, resulting LWCs, and figure 5*

It is right that the averaging by Eq. 1 leads to a smoothed field but it is also right that this is just an intermediate result. This field is taken as a probability field, roughened by mapping the measured PDF onto it. You are also right that there is a need for a detailed description of Fig. 5. In brief the upper dash-dot line should rather be plotted stepped and the lower line is extended towards zero by (not shown) measurements without liquid water found. An explanation is added.

v) *...is your coordinate system Eulerian or Lagrangian*

The coordinate system is fixed to the ground (Eulerian). An appropriate notice is added.

vi) *What is the physical reason for the bias in cloud fraction*

The minimum in cloud fraction is realized with a maximum overlap. Any deviation from this maximum overlap towards a random or even minimum overlap increases the cloud fraction. Therefore the vertical correlation coefficient could be responsible for this bias. This result could be a hint that the assumed value of 0.95 is too small for the given vertical resolution.

vii) *It would be pertinent to show how the radiative properties of generated cloud fields compare to those for the original LES field.*

While this algorithm is construed to generate input fields for 3D radiative transfer calculations we don't think that inter-comparisons of radiation fields are helpful in this matter. Comparisons based on calculations using the independent column approximation will be in good agreement if the comparisons of optical thickness distributions are in good agreement for there are no 3D effects possible. This is different for real 3D (e.g. Monte Carlo) calculations. But such results depend

strongly on the geometry of the individual case and are difficult if not impossible to generalize.

A comparison of different cloud generating algorithms is in preparation.

Technical comments

We revised both items mentioned by the reviewer.

Anonymous Referee 3

Specific comments

- 1) *Title: this method is more a simulation than a retrieval*

We don't agree in the opinion that this method is aiming in simulating just a realistic cloud. We rather expect a close connection between the reproduced and actual cloud.

- 2) *First sentence of introduction is written as if performing 3D radiative transfer is an end in itself*

The introduction is changed according to the reviewers remark for we don't want to give the impression that 3D radiative transfer is an end in itself.

- 3) *A striking omission from the introduction is mention of active instruments, specifically cloud radar and lidar*

This is true for combinations of instruments, radar/lidar or radar/microwave-radiometer but not for single instruments. While radar retrievals depend strongly on assumptions on droplet-spectra lidar retrievals are limited to optically thin

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clouds (and are effected by contributions from multiple scattering). Anyway, the reviewer is right that these instruments should be mentioned. We also reference the work of Evans and Wiscombe.

- 4) *The description of the self-similar nature of clouds is far to vague*
The passage you are referring to is rather on similar spacial patterns than on similar behavior in different scales. This assumption is the base of our algorithm. We agree that we have to be more precise and changed this paragraph.
- 5) *...when there are no observations in the vicinity of a region of the cloud, the simulated cloud field there will be too smooth*
It is true that the uncertainty (and therewith the smoothing) of the estimated LWC and effective radius will increase with increasing distance to measurements. We add a discussion on this topic.
- 6) *Values in the simulated cloud field will always be bracketed by the maximum and minimum value in the aircraft data*
This is true since we can only try to reproduce the measurements. It would be also dangerous to enhance the data-spread for we can not rule out the possibility that extrema are included in the measurements. The importance of these extreme values is questionable since we postulate the measurements to be representative. Nevertheless it is a possible source of error and we extend the discussion.
- 7) *What is the color scale used in Figure 2? What is the horizontal size of of the domain being shown? What, indeed, is the parameter being plotted?*
We are sorry for this fault. The horizontal domain size in the upper row (original cloud field) is $3.5 \times 3.5 \text{ km}^2$ and for the lower row (reconstructed cloud fields) $3.526 \times 3.526 \text{ km}^2$. The plotted parameter is the liquid water path. The informations are added.

8) *Is the cloud allowed to evolve in the time taken for an aircraft to sample it, or is a single snapshot used*

We used a single snapshot. We agree in that the overlap characteristic (or in other words the cloud fraction) is important for the radiative transfer. These characteristic is determined by the vertical correlation parameter (see comments above). For the cloud evolution does not effect the this parameter we do not expect our algorithm to predict the overlap more random-like. The discussion in section 5 is expanded.

9) *Getting cloud fraction or volume tells you very little about the skill of your method. At least a power spectrum comparison should be performed.*

You are right, we add a power spectrum comparison. The vertical correlation is hard to compare since there are different vertical resolutions we also don't see any possibility to take the clouds evolution into account.

10) *...show a retrieved LWC field, or perhaps a typical trace of LWC measurement...compared with an LWC trace extracted from your simulation...*

A good idea! We follow this suggestion.

Technical comments

- *...the authors might consider asking a native English speaker to proof-read the revised document*

That one gave us a hard time – we promise to do so.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 8609, 2004.

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