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

Anonymous Referee #2

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REFEREE COMMENTS on "CLABAUTAIR: a new algorithm for retrieving three-dimensional cloud structure from airborne microphysical measurements" by R. Scheirer and S. Schmidt

GENERAL COMMENTS

This fairly short paper presents a new algorithm for generating 3-dimensional cloud structure based on in-situ aircraft measurements, for potential use in 3-D radiative transfer tests. This is a challenge because, as stated in the paper, aircraft measurements can only sample a small part of the cloud field. An attempt is presented to extend these measurements to the entire volume of the cloud using autocorrelation functions.

I think the algorithm suggested in this paper is original, and it also has the advantage of being (as the title says) "automatic" (e.g., it can use all aircraft measurements gathered in a cloud in a straightforward manner). The testing presented in the paper is reasonable, although limited. It still remains to be shown whether, and which situations, the approach can generate realistic cloud structure, especially considering the fact that real cloud fields are not stationary but evolve and move during the time it takes to complete aircraft measurements. However, I think this approach is worth giving a try.

Contrary to the authors' claim (line 24 on p. 8611), this algorithm is not free of assumptions, and some of its details seem somewhat arbitrary. The authors should be more explicit about the assumptions behind the algorithm and the algorithm's limitations, to help the reader estimate its usefulness. Finally, the paper would benefit from some radiative transfer testing.

SPECIFIC COMMENTS

i) What (if any) is the theoretical basis for Eq. (1)? For example, is linear weighting by correlation r better than, say, weighting by r times absolute value of r? Have you tested Eq. (1) with idealized cloud fields?

ii) The autocorrelation functions are originally determined from measurements along different "directions" (flight legs). However, Eq. (1) assumes that autocorrelation is a function of distance only, independent of direction (although it can be different for the different legs j, that is, in different parts of the cloud field). This suggests that Eq. (1) cannot represent anisotropic cloud fields properly. For example, it would likely have difficulties with cloud rolls (length scale for autocorrelation much longer along the rolls than across them). Can you think of ways of better using the directional information contained in the aircraft measurements?

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iii) For the most part, this seems like an algorithm for generating 2-dimensional (x,y) cloud structure, although at several levels if available data allow that. An essential part of the description of full 3-D structure is cloud overlap (i.e., the correlation of cloud water distributions at different levels), and this receives only marginal attention in the algorithm: it seems to be contained in a (tuning) coefficient 0.95 (p. 8613, line 10). It is stated in a footnote that this weight depends on vertical resolution, but it also likely depends on the meteorological situation, such as wind shear (e.g., Hogan and Illingworth 2003, J. Atmos. Sci., 756-767). Thus, in practice this weight factor should be chosen (or guessed) on a case-by-case basis.

This simplistic treatment may well be justified on the basis that it is very difficult to constrain the vertical correlation from aircraft measurements. However, the authors should express this limitation clearly in the paper.

iv) I think you should be precise about how the anomalies generated using Eq. (1) are used. Eq. (1) calculates anomalies as a weighted average of anomalies for several points in the cloud field, which, at face value, should lead to smoothing. The sentence about "mapping the probability density functions of measurements ..." (p. 8613, lines 10-11) suggests, however that the anomalies generated by Eq. (1) are only an intermediate product: they are put into an increasing order, and then the PDF of anomalies is forced to follow the observed PDF (is this right?). If so, it is still unclear to me how the generated PDF of LWC in Fig. 5 can be wider than the range of actual measurements. While that is not unreasonable, I have difficulties figuring out how this is realized.

v) When applying Eq. (1) to real aircraft data, such as in section 4, is your coordinate system Eulerian (fixed to ground) or Lagrangian (floating with the wind)?

vi) Near bottom of p. 8614: For the LES simulation considered here, the algorithm features virtually no bias in cloud volume but a clearly statistically significant (although slight) bias in cloud fraction. What is the physical reason for the latter? For example, is the bias sensitive to the coefficient (now 0.95) used to desribe vertical correlation?

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vii) As the initial motivation behind this paper is to generate cloud fields for radiative transfer tests, it would be pertinent to show how the radiative properties of generated cloud fields compare to those for the original LES field. One relevant issue is the overall bias of the algorithm. Another interesting question is the variability of the results. Comparisons of computed and observed radiative fluxes for aircraft measurement campaigns are plagued by limited sampling (typically just a few legs in a cloud), and simulated aircraft measurements like those considered here could be very useful for quantifying the sampling problems.

This comparison could possibly be extended to some other approach for deriving cloud field structure, such as Los and Duynkerke (2000) or Räisänen et al. (2003) (cited in the paper). After all, a new more complicated algorithm is justified only if it produces better results than earlier and simpler approaches. Calculations for a single wavelength using either the Monte Carlo method or Independent Column Approximation might be enough to illustrate these issues.

TECHNICAL COMMENTS

- p. 8614, line 14: the horizontal resolution (43 m) and vertical resolution (39 m) stated here fall below the ranges given at the bottom of p. 8613 (50-250 m for horizontal resolution and 50-100 m for vertical resolution, respectively).

- p. 8617, line 2: "large wavenumbers": do you mean "large wavelengths"?

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