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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Remote sensing of cloud sides of deep convection: towards a three-dimensional retrieval of cloud particle size profiles" *by* T. Zinner et al.

Anonymous Referee #3

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General comments:

This paper describes and shows the feasibility to retrieve cloud particle phase and size along the cloud size and top by accounting for the 3D complex structure of clouds. In the context of cloud-aerosol interaction, it is indeed very challenging to be able to retrieve the cloud particle size profile. This paper follows a paper describing the concept (Marshak et al., 2006) of using passive visible and near infrared measurements to retrieve these properties in case of 3D convective cloud thanks to the Bayesian theorem. Here, the paper goes further as a large database of much more realistic cloud simulated with a cloud resolving model is built. This paper is very convincing on the

possibilities of using such method, but the authors should explain more clearly in the text the limitations of that method and be more cautious with the conclusions considering the hypothesis done to build the database (see the first three points below).

Specific comments:

1) In my point of view, the major limitations of this method are the difficulty of building a representative database and the connection that exists between results and database. This is also true with others methods and particularly with the assumption of homogeneous cloud, but, I think it should be mentioned more specifically in the paper, maybe in the conclusion. With the same idea, I find the sentence "covering the range of natural possibilities" (page 4272, line 3), a bit too much overstated.

2) The release of some assumptions done to create the database would lead, in my point of view, to larger distribution of radiances in function of effective radius and thus to larger standard deviation in the retrieval. This should also be discussed somewhere in the paper. I think, for example, to the effects of the sub-pixel variability or to the assumption made on the effective radius growth, which, even with the introduced Gaussian noise, seems to be almost the same for a given level; this leads to have almost the same profile along the line of sight and the same neighboring pixels and certainly too a more narrow distribution that it would be for real cloud.

3) Concerning the independent test case, I would not call it "independent", as the cloud used to do this test is created in the same way that the main database. I do not think that the change in the value s (p. 4277, line 3), modify a lot the cloud. For me, an independent test case would be a cloud created with another model and without the adiabatic assumption to see how this method behaves if some horizontal variabilities happens in the cloud effective radius. It is certainly too long to do but a least the mention "independent" should be removed and only "test case" should be written. I noticed it: last line in the abstract; p.4272, line 15; p. 4277, line 3; title part 6, p. 4285, line 3; page 4287, line 22; legend figure 3.

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4) The cloud model gives a mass content which should allow to derive the liquid water content and the ice water content of the cloud. In my understanding, you do not use it as it is written (p. 4276, line 13), that "wad can be derived from value of temperature and pressure at cloud bottom height", is it true? If yes, why do not using the value of liquid water content given by the model? I assume that it is because you have two unknowns N and Re. For clarity, it should be explain.

5) Figure 3: The effective radius variation is not really readable. To better show it, it can be interesting to add a vertical and an horizontal cross-section of Re respectively in function of z and x (for example, near x=30km and near z=3 and 8 km)

6) As I pointed out in 1), the model used to create the database can be biased and thus not represent completely the natural variability of cloud. In the paper, the range of cloud droplet effective radius is between 6 and 16 micron (figure 6), what would happens if the true value of effective radius in the cloud is out of this range, for example if it is 20 micron?

7) Page 4283, line 12: In my understanding, the integral of denominator is not only a normalizing factor. It is the probability of having this value of radiances in the entire database. Thus, if few cases in the database give the measured values of radiances, the conditional probabilities P(reff/I) will very strong and the confident in Reff would be very good.

8) The figure 9 is depicted before the figure 8: they should be inverted.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4267, 2008.

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Interactive Comment