Atmos. Chem. Phys. Discuss., 8, S3321–S3325, 2008
www.atmos-chem-phys-discuss.net/8/S3321/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S3321–S3325, 2008

Interactive Comment

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.

T. Zinner et al.

Received and published: 3 June 2008

Thanks for your efforts. We tried to address all your remarks. The discussions regarding natural variabilty vs. our model world's variability was extended, as you asked for, and we tried to put some of the points you found overstated into perspective. We extended one image with the additional information you demanded. Please find our reply to your points on the "independence of the test case" and your questions on the Bayes law addressed in the following details.

Please find our reply (–>) following each of your specific remarks:

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.

–> We changed the sentence to read "covering a wide range of natural possibilities". We added a small additional discussion paragraph into the "Conclusions".

—-

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.

–> Yes, you are right. Please see the new paragraph in the "Conclusions".

—-

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

ACPD

8, S3321–S3325, 2008

Interactive Comment

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.

–> Our test case is not only independent in the sense that we varied the adiabatic assumption, but it is also done with a different model version of the GCE and a completely new microphysical setup (see page 4274, line 14-17). By using the term "independent" so often we also wanted to emphasisze the fact that this is not simply one of cloud cases from the cloud/radiance database, but an additional one. This is why we would like to leave the term in the abstract and on p.4287. We removed it from p.4277, from the title to section 6, and from the legend to figure 10. We added an "almost" on p.4285.

—-

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.

–> We do use these water contents. We only use the adiabatic values for the derivation of the adiabatic effective radius. As for the the two unknowns: We fix the number concentration of cloud condensation nuclei N as stated on line 16, p.4276. We tried to clarify this with an extension of the last paragraph in section 2.2.1.

—-

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)

–> We included a few small graphs into figure 3 to show what you asked for. But we're not sure if the size is still readable. Wewould not want to inlcude a whole new set of

ACPD

8, S3321–S3325, 2008

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

images, as this is aready a long paper.

—-

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?

–> We tried this. Of course results are not good for these cases. The retrieval is limited to retrievals close to the maximum value in the database then with a very high uncertainty, as the measured combinations of reflectivities is a rare situation.

—-

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.

–> You are correct that this value will be small, if there are only few cases in the database with this forward simulated value of I. Thus the P(reff|I) will be large. But this integral has only one value for each pair of I. I.e., it will be large for each possible reff given this (pair of) I. But it is only the relative probability of having one reff1 compared to the probability for another reff2 (P(reff1 | I) compared to P(reff2 | I)) which one is interested in using the retrieval for the single measured I (pair). And the relative size of P(reff1|I) compared to P(reff2|I) does not change (which is the shape of the PDFs given in the figures 6 and 7). Thus it is just a normalizing factor.

—-

8) The figure 9 is depicted before the figure 8: they should be inverted. –> In all

ACPD

8, S3321–S3325, 2008

versions, I have at hand, it is in the right order.

—-

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