Atmos. Chem. Phys. Discuss., 7, S4458–S4466, 2007
www.atmos-chem-phys-discuss.net/7/S4458/2007/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The influence of cloud top variability from radar measurements on 3-D radiative transfer" *by* F. Richter et al.

F. Richter et al.

Received and published: 29 August 2007

Thanks to all referees for their comments on our article. There have been some valuable remarks which we hope to incorporate in an adequate way in

the revised version, which motivated us to rethink some aspect of our work and improve our final version significantly.

Here our responses to the specific comments:

Referee #2

Specific comments:

1. 'The three types of cloud top geometry are considered in the paper. It is not clear why the thin cloud with homogeneous top is not considered.'

Analysing three months of radar and additional data, there has not been found a cloud fitting to the category thin and homogeneous. Therefore, it lacks in the analysis.

2. 'The IAAFT algorithm is used to build 2D field cloud top from 1D data distribution. It is not obvious that time series measurements can be used to emulate 1D distribution. It seems that the time series of cloud top measurements depends on underlying surface and radar location. Which duration of radar measurements time series is used?'

The concept of transforming time series to spatial data is widely used under the frozen turbulence assumption meaning that cloud fields are advected over the sensor without a change in driving forces within the advection process (e.g. Evans and Wiscombe, 2004). This implies indirectly that there is only a small dependency of underlying surface and radar location. Time series have been measured over a quite homogeneous location with just small differences in topography. The temporal resolution of the data is nearly 10 seconds.

Technical correction:

1. 'The formula for non-adiabatic LWC is not presented explicitly.'

There is a formula for the calculation of the subadiabatic LWC in the final version.

2. 'The value of power spectra slope -5/3 (-1.8 and -2.0) is written without sign - in the paper text.'

This is changed in the final version.

Referee #1

1. 'My main concern is the conclusion of large deviation from -5/3 power law, since the three sample clouds are all simulated from one dimensional time series with IAAFT algorithm. Is it possible to calculate the spectrum power from real cloud data? At least in Fig.1, I can feel (maybe I am wrong) the difference in variability between the

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simulated cloud and the one dimensional time series of the real cloud. I am not familiar with IAAFT, but the authors mentioned that IAAFT is similar to the method of Baker & Davis (1992), as I know in Baker Davis (1992) the cloud is simulated using the inverse Furious transformation based on a certain assumed spectrum power value. In other words the simulated cloud field should follow the same spectrum power law as the assumed value.'

The power spectra used in this study have been calculated from measured cloud top data. It might not become clear from the text that the IAAFT is in this way different to Fourier methods used by Barker and Davies (1992) that beneath the slope of the power spectrum also the PDF of the time series is considered. Barker and Davies also did not take into account the differences between 1D and 2D power spectrum discussed in Austin et al. (1994), whereas Evans and Wiscombe (2004) faced this problem. Concerning your feelings that measured and simulated cloud tops are different in variability, there might be some causes. Displayed is just one slice of the cloud field, but the mean slope of power spectra in all directions have to follow the predefined one. Furthermore, the predefined PDF does not have to be recovered in a single line of the cloud top field, but the overall PDF of the field has to follow the given one. Additionally the vertical resolution of the cloud field has been refined according to the measurements.

2. '8088, the authors had summarized the progress in cloud simulation, one work by Raisanen et al. (2004, QJRMS p2047) should be included, since to my knowledge such cloud generator method is the most plausible scheme for climate models.'

This article is an enhancement for our work and a valuable addition to the article of Randall et al. (2003) and fits well into the overview of current research in cloud treatment in climate modeling.

3. '8089, line 6, in order to make a clear description, it is better to add a sentence: the temperature ranges of the sample clouds are all above 263 K.'

This sentence is added in the final version.

4. ’8090, lines 18-25, it is not very clear, what does it mean that gamma is the spectrum of 2-D field?’

This becomes not very clear from the text. Gamma is the slope of the power spectrum of a 1D spatial series, where the Fourier coefficients are infered from. These coefficients are distributed on a 2D field according to the symmetry for isotropic fields describe for example by Pardo-Iguzquiza and Chica-Olmo (1993). With these coefficients a backward transform is performed and yields in a field where the mean slope of the power spectra of every row and column is around beta. This is changed in the text.

5. ’8091, it is better to give a brief definition for ‘subadiatic’. It is not clear why LWC is subadiatic but the effective radius is adiatic. As we know LWC, effective radius and particle number concentration are physically associated together.’

The relationship of LWC, effective radius and number concentration depends on processes which are included and on the scale clouds are resolved with. For example Baker and Latham (1980) introduced the concepts of homogeneous and inhomogeneous mixing, where the first one takes place when all droplets within a certain volume are affected by evaporation whereby all droplets are reduced in size but the cloud droplet number concentration is kept constant. As a consequence the effective radius is reduced. In the inhomogeneous case, entrained air is not dispersed over the overall volume and droplets in the affected region are completely evaporated until saturation. All droplet radii are still present but the cloud droplet number concentration is reduced. In this scheme the effective radius stays the same. In both schemes the liquid water content decreases compared to the adiabtic LWC. The existence of the mixing schemes and in-between stages are weakening the associations between liquid water content and effective radius at least in small scale observations. Some combinations of these microphysical parameters for maritime and continental clouds observed by insitu measurements can be found by Miles et al. (2000).

6. ’8091, line 10, it is confused for suddenly appearing of a weighting function, weight

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for what?’

The weighting function weights an adiabatic LWC profile to a subadiabatic one. A short formula is added to the text describing this step more precisely.

7. ’8093, the authors mentioned that Rayleigh scattering is included in the Monte Carlo simulation, but how about the gaseous absorption? For solar radiation O3, H2O and C2O are important. I am not sure the Rayleigh scattering can be simulated well without including the gaseous absorption. The authors should present a simple explanation or include the gaseous absorption in their Monte Carlo calculations. Today many Monte Carlo models can account for the gaseous absorption. Another question is about the cloud droplet optical property. Which parameterization is used? I assume the authors not using Mie calculation for droplet optical properties directly.’

The Rayleigh scattering has been parametrized due to Bucholtz (1995). Gaseous absorption is neglected due to simplicity like it have been done in various Monte Carlo cloud studies before (e.g. Loeb et al (1998), Varnai (2000)) Cloud properties have been calculated by complete Mie calculations by using a modified gamma distribution with an alpha of 6. These calculations have been performed to avoid differences between widely used approximations and complete Mie calculations described by Räisänen et al. (2003).

8. ’Fig. 4, it is not clear that the homogeneous cloud refers smoothing out only the cloud top turbulence structure only or smoothing out both of the turbulence structure and the vertical inhomogeneous structures for LWC and effective radius. If it is latter, the difference in albedo might be due to the vertical inhomogeneity. I believe Li et al. (1994, JAS p2542) is the first work studying the radiative impact corresponding to cloud vertical inhomogeneity. That work is better to be mentioned here in order to show a comparison which effect is more important.’

Homogeneous cloud means that cloud top is flat, but the pofiles of liquid water content and effective radius are the same as the profiles for the inhomogeneous clouds. That

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means that there are slight deviations for every column in liquid water path and cloud optical depth. Even if the homogeneous cloud would also have uniformly distributed microphysical and optical properties within the column most of the differences would have to be assigned to cloud top differences because cloud field are overcast. Li et al. (1994) found just very weak dependencies of reflectances from the vertical distribution of the microphysical and optical properties for overcast cloud fields but increase for broken cloud fields. Our intention was to describe in one way the cloud top structure as realistic as possible and because of this a more realistic vertical cloud structure has been applied and in the other way we tried to leave the cloud top structure as the only origin for radiative differences by avoiding horizontal inhomogeneities of optical properties. Also cloud properties in a certain level are the same for the homogeneous and the inhomogeneous case.

9. '8094, line 18-19, the sentence of "enhances the increase" is not very clear, increase of what?'

is be changed to 'enhances the albedo increase'

10. '8094, could the authors simply illustrate definition of penetrate depth, it seems very important in their discussion.'

The penetration depth is used as a measure for the lowest z-position photons reach on their path through the clouds. This position mirrors the optical properties of the so far travelled path. If there are areas of higher extinction values in the upper part of the cloud photons are subjected to more scattering events in this area and therefore, the chance to be scattered back to the detector is higher and photons does not reach deeper cloud areas. Photons entering the cloud in eroded parts of the cloud top are not subjected to these areas of high extinction and therefore reach deeper parts of the clouds where in our profile extinction coefficients are lower and mean geometrical free pathlength increases. A brief definition of the penetration depth is added in the final version.

11. 'Fig. 7, I do not understand the difference bars shown in Fig. 7. If the number of injecting photon is large enough the reflection should be fairly determined. The relative error is proportional to 1/pN, where N is the number of injecting photon. Please present a clear description.'

You are right, the difference bars are quite large for the homogeneous cases. There have been injected 10ˆ6 photons but the Monte Carlo model is equipped with the local estimate approach for example described by Barker et al. (2003). This approach enables to calculate reflectances with a smaller amount of injected photons by tracking secondary photons released on every scattering event on their direct way to the detector. The classical approach where just the primary photon is tracked needs larger amounts of photons that every detector pixel is hit by enough photons with the predefined entering direction. The quoted error calculation might not be valid for the local estimate approach but difference bars should indeed be smaller. Our only explanation mentioned in the text so far is that they are caused by the random nature of the Monte Carlo model and that more photons would result in a more robust result. Concerning the local estimate approach we will add a reference to the text.

12. 'The discussion of this work is main for boundary cloud which has big impact on radiation budget in climate models. I wonder if such heterogenous cloud effect can be considered in climate models through a proper parameterization. Could the authors bring out some idea in their revised version? This is beyond the scope of this work, just a curiosity of mine.'

Its difficult to include the described effects in parametrizations for climate models. The motivation for this work was more initiated from the remote sensing view. As an outlook the authors would suggest to first measure longer time series of cloud top variability by radar measurements and investigate in possible relationships to other atmospheric variables for the option of parametrizations. Also anisotropy effects of cloud properties due to wind effects described by Hinkelman et al. (2005) for LES simulations should be investigated by means of measurements. The primary goal should be to incorpo-

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rate these effects in remote sensing retrievals. How these small scale effects could be included in atmospheric models is not clear to the authors. The most advanced concept for including cloud inhomogeneities known by the authors is the concept of Randall et al. (2003) nesting cloud resolving models within the model grid. Maybe parametrizations of cloud top variability could be included in these models.

Jane Hurley

1. 'Is there any kind of insolation other than solar? (p.8088,line 23)'

This is changed in the final version.

2. 'Re: p.8089, line24,25 Do you account for smaller scale variability or how do you know it doesn't affect radiative transfer?'

The smallest scale is determined by the resolution of the cloud radar. If scales become smaller variabilities might not be reflected in the radiation results due to the effect called radiative smoothing caused by horizontal photon transport between adjacent cells.

3. 'In table 7, the sopes should be negative ...'

This is changed in the final version.

4. 'Hard to understand exactly how you carry out all of this ...'

We hope that our results are illustrated comprehensible...

5. 'You have compared a MC scheme with realistic cloud structure to homogeneous cloud top and shown, not surprisingly, significant differences. But suppose, instead of a homogeneous cloud top, the cloud top structure were represented by a combination of "homogeneous" facets at various viewing angles (ie. neglecting the fact that photons leave the cloud top at one angle and may re-enter a different part of the cloud) - how well cloud this represent the MC results?'

If we understand you right you would approximate the cloud top variability by partly homogenenous areas which are connected to each other by angles which are so large that for photons leaving the cloud re-entering of the cloud in another part becomes unlikely. This might be an interesting question but is not leading to the goal because this work has been driven from the remote sensing view where cloud tops in the retrieval algorithms are treated as homogeneous.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 8087, 2007.