Geosci. Model Dev. Discuss., 6, C568–C570, 2013
www.geosci-model-dev-discuss.net/6/C568/2013/

Geoscientific Model Development Discussions

Interactive comment on "A refined statistical cloud closure using double-Gaussian probability density functions" *by* A. K. Naumann et al.

Anonymous Referee #2

Received and published: 16 May 2013

General comments:

The authors improve an existing formulation of a statistical cloud closure using double-Gaussian probability density functions by Larson et al. (2001). The improvement is achieved by allowing asymmetric dependencies of σ_i/σ on skewness sk, which was symmetric in Larson's original. A further improvement is achieved in the formulation of the liquid water flux where an old parametrisation by Cuijpers and Bechtold (1995) has been extended such that the skewness of the distribution of the quantity s is taken into account. The new parametrisations are derived from LES model simulations, tested against data, compared with the Larson etal. and Cuijpers and Bechtold parametrisations, and tested for their performance with varying spatial resolution of the parent model. Finally, three formulations of the autoconversion rate are equipped with the new

and old distributions of s and tested against each other. This all looks quite complete and interesting and should be published if my bad feeling that training and testing data are more or less the same is shown to be wrong.

A few points for improvements (major comments):

1) It is not quite clear whether and how the "training data", in particular data from the RICO simulations, differ from the test data, which seem to be from RICO simulations as well. Of course, if training and testing data are the same, the model derived from the training data will produce good results, but without guarantee that it works with independent test data as well. This is a critical point and some of the following comments are due to a related problem.

2) (page 1094): The double Gaussian has 5 free parameters (here $a, s_1, s_2, \sigma_1, \sigma_2$). I do not understand that the number of free parameters should change when s_1 is expressed as a function of the remaining 4 parameters plus the 3 parameters $\bar{s}, \sigma, sk$, which are then 7 free parameters. Evidently they cannot all be independent. Please clarify.

3) (page 1096 and Figure 3): In Figures 3a and 3b it is shown how the new parametrisation (Eq. 4) is derived by fitting parameters to data from LES model runs. Figure 3c shows how this new parametrisation has been tested. But there is a problem. Instead of using an observable quantity like LWC, optical thickness, radar reflectivity, the quantity σ_1/σ is shown. It seems to me that here the test data are treated in the same way as the training data and thus they should show similar behaviour, isn't it? Further, as σ_1/σ is not an observable quantity (probably), it must have been derived from simulations and thus it seems again that training and testing data are closely related which would render the results useless.

4) (figures 5 and 6): these figures are not very useful as again non-observable quantities (i.e. model quantities) are plotted against other non-observables. For testing I expect to see plots with quantity y modelled against quantity x observed, usually giv-

ing a cloud of data points scattered more or less around $y = x$ or $y = a + bx$. The scatter around the $y = x$ line then allows statements about the quality of the model. Here I have problems to estimate a quality. In figure 6, all lines look very different to the concentrated patch of data points, but I dont know what it means. In contrast, figure 7 looks much more useful, showing measured (?) profiles against modelled ones with different parameterisations. This is understandable, but the testing in the former figures is not. This and the corresponding text should be improved.

Minor points:

1) (page 1092, line 21): "surface fluxes", please say what is flowing (heat, vapour?).

2) (Equation 8, 2nd line): what is the difference between $\bar{q}_l$ and $\bar{s}$? According to the equation they should be identical? If so, please state it.

3) (page 1103, lines 7-9): first "deterministic PDF" sounds strange, but contrasting it to a "stochastic approach" sound even more so. Perhaps you can find better expressions.

4) (page 1103, line 12): "In these moist cases...".

5) (page 1106, line 11): I know what a joint pdf is, but what is a two-point pdf?

6) (table2, footnotes): check brackets in the definition of RMSE.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1085, 2013.