Atmos. Chem. Phys. Discuss., 4, S1358–S1360, 2004
www.atmos-chem-phys.org/acpd/4/S1358/

Atmospheric Chemistry and Physics Discussions

ACPD
4, S1358–S1360, 2004

Interactive Comment

Interactive comment on "A new convective cloud field model based on principles of self-organisation" *by* F. J. Nober and H. F. Graf

Anonymous Referee #3

Received and published: 26 July 2004

This manuscript proposes a new method for the parameterization of convection in the GCMs, derived based on the ideas of cloud spectrum and quasi-equilibrium suggested by Arakawa and Schubert (1974). To provide a spectrum of clouds using predicted GCM parameter fields at a given GCM grid, the authors have adopted a 1D Lagrangian cloud plume model driven by two external parameters: i.e., initial cloud radius and vertical velocity at the cloud base. Quite uniquely, the authors have also attempted to introduce the cloud-cloud interaction into their model by using an equation for describing the competition of biological species in population dynamics. The scheme seems computationally efficient. Although the dynamics of the included 1D cloud model is highly simplified, certain important physical and chemical processes of cloud particles and aerosols could be incorporated explicitly in the model. The authors compared the result of their method with that of the current ECHAM scheme as well as those of LES

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models using an ARM shallow convection case.

The topic of this paper is apparently suitable for ACP and the results presented are original. The paper is relatively well-written and reasonably organized. It is my personal opinion that the method suggested by the authors might be able to provide us with a unique approach to implement some of the ideas suggested by Arakawa and Schubert 30 years ago. Thus, it is very important and necessary for the authors to document their idea and approach on the ACPD.

However, I have to indicate that the work of this research still needs a few critical additions in order to not just propose some ideas but provide necessary evaluation for the method itself.

The major concern of the method is the statistical significance of the cloud spectra calculated by using the cloud plume model and various initial profiles predicted by a real world GCM. As the authors indicated in the discussion section, the model seems predicting a spectrum biased toward small clouds. In addition, the cloud-cloud interaction would much rely on the cloud spectrum. Particularly, this interaction would be quite sensitive to the appearance of "big" clouds because of the dominances of these clouds in the evolution of environmental profiles. Unfortunately, the current evaluation only provides a comparison with observation and LES model results for a shallow convection case in a selected cite. The result of this case study is useful but not convincible. Therefore, it is extremely important for the authors to evaluate their model in a global base in order to test the representations of convective clouds with different sizes in the model.

Thus, due to the incompleteness of the current work, I suggest accepting the paper for publication only when the authors include results from a carefully planned global-scale evaluation as suggested above.

Several Specific Comments:

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In Page 3674, Line 15, the authors mentioned that "The entrainment rate is the same as in the original setup ...". This is not enough to describe the model. I suggest the authors to include more detailed descriptions of the entrainment rate along with other empirical assumptions that are critical to the result of such a dynamically-simplified model.

In Page 3675, first paragraph, the authors described that the TM and HM are neutral profiles from the last time step. Is that true that the "after-convection profiles" in the model are assumed to be neutral instead of stable ones?

In the same page, Line 25, "meteorological situations" should be "meteorological conditions".

In page 3677, the authors used CAPE to describe "cloud potential energy content", should the CAPE better be CPEC?

In page 3679, Equation (7), isn't it true that the Kij can be treated either "locally" or "globally"? What are the assumptions the authors adopted during the calculation of Kij? And what are the justifications in physics or dynamics for these assumptions? Also, a distribution of K needs to be provided.

In page 3681, the authors claimed that (in the second paragraph) the combination of w and R they had adopted in the test "leads to a reasonable spectrum of possible clouds". Wondering based on what dataset the authors have drawn such a conclusion?

In the end of Page 3684 the authors stated that "The same bias can be identified in the mass flux curve (Fig. 7)". What is the bias the authors referred to and how could one identify the bias from Fig. 7?

PDFs of cloud top height and Wmax calculated by proposed model against observations or LES results should be provided for the evaluation.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 3669, 2004.