Atmos. Chem. Phys. Discuss., 6, S4601–S4605, 2006
www.atmos-chem-phys-discuss.net/6/S4601/2006/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of a new convective cloud field model: precipitation over the maritime continent" *by* H.-F. Graf and J. Yang

H.-F. Graf and J. Yang

Received and published: 14 November 2006

Reply to reviewer 1: Jun-Ichi Yano

These more general and strategic remarks are most welcome since they give a chance to answer in a similar manner. Actually, since the publication of these comments we got into contact immediately and there will be, hopefully, a fruitful co-operation in the future, starting with a visit of JIY in Cambridge in January 2007. Thanks, ACPD!

In the NG2005 paper, which was based on Frank Nober's PhD, we aimed at showing a new potentially useful approach to convective cloud parameterisation. Unfortunately Frank left hard science for a permanent job since then. The work is now continued by my co-author Jian Yang and a new PhD student, Till Wagner. Jun-Ichi remarks that the components of CCFM are kind of arbitrarily put together. This is somehow right, but is based on history. The cloud model, which originally was based on a layout by Weinstein and MacCready has been developed in my own PhD some 30 years ago, it is well comparable with the Simpson and Wiggert 1969, but with extended microphysics as described in the article. Actually meanwhile the microphysics are further evolved to include aerosol effects, but still are bulk. The model was used since I tested it (as quoted in the article) against 3 years worth of data and it performed well for cloud top height and in some case studies of strong precipitation (never published) - just what we want to have. We use CAPE as closure; actually CAPE is to be used as much as possible by the resulting CCFM spectrum. Any other concept could be used and would have to be tested. At this time we are happy with CAPE since there are more grounds to be worked on. CAPE is changed by the mother model after each time step based on a number of processes, including convective heating. We know that the Lotka-Volterra-Equation is far from being optimal since it provides oscillating solutions. Till Wagner will improve this part and he is also thinking of using another cloud model since we feel that entraining parcels would not be optimal anyway on the long run. However, we always are constrained by the wish to include CCFM into a global climate model and, hence, to be very restrictive concerning the extra computer time this parameterisation will need.

As to the question why using the CCFM in a limited area model: As is always the case if you do something new and untested, the question is how to pay for that? This is the very reason why we tested CCFM in REMO: We got a project for this. Not actually to use CCFM, but a convection parameterisation including microphysics that can handle aerosol effects. So the easiest was to try CCFM since it was already coupled to ECHAM physics that is also used in REMO. We were surprised by the results and so we kept the model running. The test for performance of REMO-CCFM over the Maritime Continent was a necessary precondition for the aerosol work and the results were better than anticipated. Currently the aerosol effects paper is being prepared. Unfortunately this is the way we sometimes have to walk these days, but Brownian motion in some cases also finds some brownies. As described, we had to adjust the degrees of freedom of CCFM to the smaller domain and so CCFM is not as powerful as it potentially could be in a coarser grid.

One problem with CCFM (and any new convective parameterization, I am afraid) is that it must be integrated into a GCM that was developed with a completely different parameterization. We have to keep the interfaces (so they do exist, fortunately the new ECHAM5 makes life easier in this case) if we do not want to change a lot of code, probably buying in some other problems. Hence we will end up with an awkward mixture of approaches. The direct coupling of large-scale precipitation on convective via the background humidity seems to be one of these. If we get convective precipitation right we have too much large scale (possibly since REMO is too moist over the warm pool). We tested several approaches, including the development over time of the cloud spectrum (what makes sense at time steps of 5 minutes in REMO, makes less sense at 40 minutes in the GCM), but ended up with the most appropriate set of additional parameterisations as described in the article. More sophisticated development of the method will require additional funding (e.g. for running a CRM in parallel), but this is not secured so far.

In any case we currently continue testing and developing CCFM in the GCM domain since this is what it was developed for. Problematic is just that the column mode, as promising this is at first glance, does not provide the large scale (stretching over several columns) feedback of changed circulation due to the action of the convective clouds themselves. This is no problem for shallow clouds as tested in NG2005, but well so for deep convection. Hence, the tests are done in the global model.

We take the technical comments into account and revise the manuscript accordingly. Thank you, Jun-Ichi, we are looking forward to working together!

Reply to reviewer 2 (anonymous):

Thank you also for your comments!

Actually we did the proposed alterations to the maximum initial cloud radius, however due to the discussed problems with highly instable situations (squall lines) larger radii worsened the overall results. The set of parameters we used at the end are those to which REMO had to be “tuned” in order to receive the best possible results. We were careful tuning “at the second level”, i.e. using variables that are provided by the mother model, so that overall variability is not suppressed. Improvements could be achieved only by applying results from cloud resolving models (this holds for initial cloud radii as well as for the effects of vertical wind shear) and this is what we want to do in the future, provided funding is secured. However, we do not intend to do such studies for limited area models since these are not the ones for which CCFM was developed. Please see also our discussion above with reviewer 1.

The shift in winter maximum of precipitation by one month is actually due to few days of extreme precipitation at the end of January (the typical squall line problem) over North Australia.

We had a closer look at days with convective precipitation (in addition to all days) since CCFM first hand produces convective precipitation. As seen in the Figure, the large scale monthly mean hourly precipitation remains unchanged, be the days taken with convective precipitation or all days, meaning that there is large scale mainly when there is also convective precipitation. What happens is just an increase of precipitation intensity mm/hr due to the fewer hours of convective precipitation. Actually this is not very important information and might be omitted.

Over sea the clouds are generally smaller than over land due to the systematically smaller initial radii and TKE. Introduction of the dependence of initial maximum radius and vertical velocity at cloud base on mother model variables have led to the very clear improvement of precipitation distribution (see Figs 8 and above). However, we cannot be sure that planetary boundary height is always correctly simulated in REMO. At the end of the day CCFM cannot be (much) better than the mother model.

ACPD

6, S4601–S4605, 2006

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 10217, 2006.

ACPD

6, S4601–S4605, 2006

Interactive Comment