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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A statistical subgrid-scale algorithm for precipitation formation in stratiform clouds in the ECHAM5 single column model" *by* S. Jess et al.

Anonymous Referee #2

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Review of "A statistical subgrid-scale algorithm for precipitation foramtion in stratiform clouds in the ECHAM5 single column model" by S. Jess, P. Spichtinger and U. Lohmann

This paper describes the implementation of an algorithm for the treatment of sub-grid scale clouds in the ECHAM5 Single Column Model which applies the microphysical processes to a number of sub-columns within each grid-box. The impact of sub-grid heterogeneity on the microphysical processes is evaluated by comparing a heterogeneous distribution of cloud mass and number concentration (HET) with a homogeneous distribution (HOM) and the one-column reference (REF). Results are presented for two

case studies; a stratocumulus case (EPIC) and a mixed-phase multi-layer case (M-PACE).

GENERAL COMMENTS

The impact of representing heterogeneity on microphysical process rates is a worthwhile contribution to the advancement of cloud and precipitation parametrization as there are many questions and uncertainties for model parametrizations in this area. This paper is a first attempt at showing the impacts of including heterogeneity in the microphysics parametrization for specific case studies and evaluating the differences against observations, and this is the new and novel aspect of the paper. However, the problem is that the evaluation of the two case studies only weakly provides enough quantitative information to draw firm conclusions. I did ask myself the question, "Can I be convinced from the two SCM case studies that there is enough useful information to determine the benefit of the HET approach?". The answer is, just about. Although there is not enough data to show that HET is statistically better or worse, there is information from the comparison of the three different approaches and the sensitivity studies to say something about the behaviour of the different algorithms. There are clearly some SCM forcing issues for the observational cases that overwhelm the differences from the model sensitivity during certain periods. This is problematic but is often a feature of SCM forcing datasets. The author's must be wary about overstating the fit with observations in the light of the SCM forcing deficiencies and I would ask them to check these comments throughout. In addition there are a number of places where causes of errors/differences are speculative and it would strengthen the paper if more evidence was provided to substantiate and explain these differences. Specific comments are included below.

SPECIFIC COMMENTS

p9336, line 4, "...precipitation formation is initiated by large particles." Please reword this first paragraph. Firstly, it is due to the non-linearities in the precipitation formation process which mean the mean values are not representative. But the impacts of heterogeneity are also much wider than just precipitation formation, also affecting other microphysical processes such as evaporation, accretion etc.

Abstract, p9336, line 15 You mention the two observation campaigns, but it would be informative to also mention the dominant cloud types here as well as this is very relevant information for context of the evaluation result, i.e. stratocumulus for EPIC and mixed-phase multi-layer cloud for MPACE.

p9337, Introduction I'm surprised there isn't a reference to Pincus and Klein (2000), "Unresolved spatial variability and microphysical process rates in large-scale models", JGR, as this is probably one of the most relevant papers.

p9339 line 26 onwards "There are different possibilities to include subgrid-variability into a large scale model...". Essentially (1) is about prognosing the sub-grid variability in terms of a PDF, (2) is about using sub-grid variability information (whatever its source) in the physical processes (radiation, microphysics), and (3) is about combining the above two and coupling with dynamics. The way it is written it seems like these are three different possible approaches, but certainly (1) and (2) address different parts of the sub-grid variability problem. This paper is addressing (2) only. Perhaps taking these points out of a list format and rewording slightly would make the logic flow better.

p9341 How is the precipitation fraction treated in the REF simulation (no sub-columns)?

p9342, section 2/3 and p9344, section 4.1 I note that you are running the model with the Tompkins (2002) cloud scheme which in principle prognoses the PDF of cloud water etc., but this information is not used, lognormal distributions are used instead. Could you include an explanation of why the information potentially available from the Tompkins scheme is not used?

p9342, lines 8-10 Sentence not too clear. Could be made more readable to relate the 0.5 value to 1mg/kg and 0.7 value to 2mg/kg. Could you rewrite.

*p9343, second para So there is no stochastic element to the *mean* liquid or ice properties as there is in the Raisanen algorithm, as the last cloudy sub-box is adjusted to conserve the in-cloud mean properties? The results from section 5 show this results in multiple iterations and reverting to the homogeneous assumption between 10-22 % of the time. Have you looked at the impact of not making this last cloudy sub-box adjustment and instead leaving a certain level of randomness? Maybe some randomness is not a bad thing?

p9347, line 18 Could you include a sentence explaining why LWP is lower in the HET simulation, i.e. presumably because HET allows precipitation formation from the upper end of the the LWP distribution (in contrast to the uniform distribution in HOM)

p9348, section 4.2.1 Presumably we expect REF and HOM to be very similar in this case as the SCu deck is a relatively thin single layer cloud. Presumably if the cloud was one level thick and the number of columns was large enough the two would be identical? Worth pointing this out somewhere in this section.

p9348, line 20 Figure 4 is very noisy, although you can just about see the signal. Including some numbers in the text here (e.g. average LWP) would help to quantify the improvement in HET compared to HOM and REF. I don't get anything from Figure 5, as there appears to be no significant relationship or differences. In fact, I would suggest getting rid of Figs 4 and 5 and just quantifying the average LWP. Alternatively if you want to include some info on the relationship between precip and LWP, you could think about an alternative way of showing Fig 4. and just get rid of Fig 5. Maybe LWP versus POP (probability of precipitation) would give a clearer picture for figure 4?

p9349, line 9 Can you explain why the vertical thickness of the cloud is reduced in simulation HET due to sedimentation. Is this again due to the fact that the upper end of the distribution has a higher sedimentation rate (but then the smaller end will have a lower sedimentation rate than HOM?

p9349, line 24 Is there a reference for the WANG retrieval?

p9350, line 11 Presumably the IWP from the SHUPE-TURNER method does not distinguish between ice and snow as it is based on radar reflectivty data. As the model IWP does not include the snow contribution (which can be a significant contribution), there will inevitably be a significant under-estimate of IWP compared to the observations and it is not a fair comparison. The authors infer this, but need to state it more explicitly in the text. At the moment the phrase "The model estimates of IWP do not include snow at the surface" is confusing. The whole vertical profile of snow is missing from the IWP.

*p9352, section 5.1 on number of sub-columns Section 5 shows sensitivity to the number of columns used in the algorithm, which from the table looks as though there is convergence at around 100 columns for HOM, but the main results are based on experiments with 20 columns. Is this sufficient? Also, (line 10) there is clearly not enough information to show that that the HET results have converged at all. The fact that HET 40 is so different to HET20/60 highlights the non-linearities in the system I understand the requirement to run with as few as sub-columns as possible for computational reasons, but you need to do more to show that 20 columns is sufficient and you would not get significantly different results/conclusions with a higher number of sub-columns. Do you have more HET runs with different numbers of sub-cols to show this?

p9355, first para So cloud fraction overlap is the same, but the cloud properties are different? With HETcor=0 essentially random overlap of properties and HETcor=1, maximum ovelap?

p9355, 2nd para Would not expect much impact for stratocumulus case (as it is a thin single layer cloud). Could include this statement.

p9355, lines 18-21 I'm not convinced by this argument. Why couldn't a low correlation between levels also result in IWC sublimating below cloud?

p9357, line 11 Main reason this method is computationally cheaper than Grabowksi (2001) is because it doesn't have the dynamics and rest of the CRM physics, not just because only the cloud columns will be divided into sub-columns!

p9357, line 12 Precipitation evaporation can be as important to represent correctly, so would suggest "...to improve precipitation formation and evaporation and reduce errors in the radiation budget..."

p9340, line 14 "the sub-column algorithm has no time dependency". I understand what you are trying to say in terms of the contrast with the Grabowski parametrization, but there is time dependency in the algorithm due to varying input profiles. Could this be reworded.

p9340, line 17 "We introduce inhomogeneities in the microphysical properties of stratiform clouds, which then affect precipitation formation" But the inhomogeneities affect more than just the precipitation formation, as the whole microphysics is modified. Suggest reword.

p9342, line 3 Presumably you meant "log-normal distribution", rather than "log-normal size distribution" as it is a distribution of the water contents and representative number concentrations? There seems to be some confusion

p9363, table 2 There is little sign of convergence, only for HOM100 and HOM500.

p9364, table 3 Have the SigmaCDNC and SigmaLWC columns been swapped, except for the HET row? Or have I misunderstood something?

TECHNICAL COMMENTS

Introduction, p9337, line 18 "...permits to increase..." -> "...permits an increase in..."

p9340, line 13 "primary" -> "primarily"

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9335, 2011.