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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.

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Referee 1: General Comments: 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.

Author: For this study two different campaigns were chosen to get an idea what happens if the algorithm is used for a single-layer water cloud and for a mixed-phase multi-layer cloud. Some features can be seen and analysed, but the non-linearity of the pro-

cesses leads to complex context and deficiencies of the model need to be addressed but this is out of the scope of this paper.

Referee 1: 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 het- erogeneity are also much wider than just precipitation formation, also affecting other microphysical processes such as evaporation, accretion etc.

Author: We reworded this paragraph. However, precipitation formation is a non-linear process which is initiated by large particles. The evolution of the formation process is also affected by microphysical processes such as evaporation and accretion. Thus mean values are not representative and could lead to a delayed onset of precipitation.

Referee 1: 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.

Author: We added the cloud types in this paragraph. We have implemented this sub-column algorithm into the ECHAM5 global climate model to take subgrid variability of cloud cover and microphysical properties into account. Simulations with the Single Column Model version of ECHAM5 were carried out for one period of the Mixed-Phase Polar Arctic Cloud Experiment (MPACE) campaign dominated by mixed-phase multi-layer clouds as well as for a stratocumulus cloud observed during the Eastern Pacific Investigation of climate Processes (EPIC) campaign.

Referee 1: 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. We included the reference in the introduction.

Author: Pincus and Klein (2000) studied the unresolved variability in large-scale models and quantified some biases in process rates. They also pointed out the problems to include parts of the unresolved variability in global models.

Referee 1: 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.

Author: We already reworded this paragraph, because one referee from the JGR asked us to list the different possibilities and so we did the list for an easier understanding.

Referee1: p9341 How is the precipitation fraction treated in the REF simulation (no sub-columns)?

Author: The precipitation fluxes represent grid-cell averages, while the accretion processes as well as evaporation of rain and sublimation of snow depend on the fraction area, , of a grid-cell covered with precipitation. Our approach for estimating is a slight modification of that employed by Tiedtke (1993) as defined in Jakob and Klein (1999). It is a weighted sum of the precipitation from above and newly formed precipitation in the regarding layer.

$$C_{Pr}^{k} = max(C_{Pr}, \frac{C^{k}\Delta Pr^{k} + C_{Pr}^{k-1}}{\Delta Pr^{k} + Pr^{k-1}}) \quad (1)$$

$$C_{Pr} = \begin{cases} C^{k}, & \text{if } (\Delta Pr^{k} \geq Pr^{k-1}) \\ C_{Pr}^{k-1}, & \text{if } (\Delta Pr^{k} < Pr^{k-1}) \end{cases} \quad (2)$$

The total precipitation flux is the sum of rain and snow. If the change in precipitation flux from one layer to another is larger than the precipitation flux in the layer above than the total precipitation fraction is equal the cloud cover. If the change in precipitation flux from one layer to another is smaller than the precipitation flux in the layer above than the total precipitation fraction is equal the precipitation fraction the layer above.

Roeckner, E., Bäuml, G., Bonaventura, L., Brokopf, R., Esch, M., Giorgetta, M., Hagemann, S., Kirchner, I., Kornblueh, L., Manzini, E., Rhodin, A., Schlese, U., Schulzweida, U., and Tompkins A. : The atmospheric general circulation model ECHAM5, Part I: Model description, Report 349, Max Planck Institute for Meteorology, Hamburg, Germany, 2003. Tiedtke, M. (1989): A comprehensive mass flux scheme for cumulus parameterization in largescale models. Mon. Wea. Rev., 117, 1779-1800. Jakob, C. and Klein, S. A. (1999): A scheme for parameterizing ice-cloud water content in general cirulation models. J. Atmos. Sci., 47, 1865-1877.

Referee 1: 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?

Author: By using the PDF of the Tompkins scheme, only information about the PDF of total water (water and ice) is available which can not be easily split up into a PDF for water and ice separately. Further assumptions need to be done. Therefore measurements from the atmosphere are taken into account for the distributions of mass and number concentration of water and ice.

Referee 1: 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.

Author: We have rewritten the text. A random number decides which corresponding value on the x-axis is taken and allocated to the given cloudy sub-box. As highlighted in Fig. 2 the random number 0.5 gives a value of 1 mg/kg in liquid water content, respectively 0.7 gives a value of 2 mg/kg.

Referee 1: *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

Author: Yes, it is right, there is no stochastic element to the mean properties. But since the properties must be conserved the properties of the last sub-box can not be chosen randomly because the system would get out of balance. First we kept the more or less random distribution and also tried to allow a certain variability for the last values but since the mass budget was unbalanced I could not run the model anymore. In the end we decided that the last value can deviate around 5

Referee 1: 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)

Author: We changed it to: LWP in simulation HET is lower due to higher precipitation rates presumably because HET allows precipitation formation from the upper end of the LWP distribution in contrast to the uniform distribution of HOM. Therefore the observed characteristics of LWP are better captured.

Referee 1: 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.

Author: We added: Since the stratocumulus deck is a relatively thin single layer cloud

and the number of sub-columns is chosen large enough we expect that REF and HOM would be very similar.

Referee 1: 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.

Author: We added: The overestimation in LWP is reduced in simulation HET as compared to simulations REF and HOM (Fig. 4). The average LWP in HET is 104.36 g m^{-2}, while REF and HOM show much larger values deviating stronger from the observations (Tab. 3).

Referee 1: 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?

Author: How should I plot POP with ECHAM output? From the model output we get diagnosed precipitation.

Referee 1: 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?

Author: Sedimentation leads to a reduction of cloud ice in some parts of the cloud (sub-boxes with larger amount of cloud ice). Therefore the small amount of ice which remains in the layer may sublimate or is not enough to form a cloud anymore and leads to a clear sky part reducing the cloud cover in this layer.

Referee 1: p9349, line 24 Is there a reference for the WANG retrieval?

Author: Yes, Wang (2007), we added this. Wang Z. 2007. A refined two-channel microwave radiometer liquid water path retrieval for cold regions by using multiple-sensor measurements. IEEE Geosci. Remote Sensing Lett. 4: 591–595.

Referee 1: 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.

Author: We added: The retrievals do not distinguish between ice and snow as they are based on radar reflectivity data. The model estimates of IWP do not include snow at the surface. Differences between observations and model results could therefore partly arise from this discrepancy.

Referee 1: *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?

Author: The idea was to show that there is convergence. As you said the results for HOM100 and HOM500 are very close to each other. But since the method is quite time consuming we used 20 sub-columns with the justification that the average values only deviate little as compared to HOM100 and HOM500. The largest differences occur for 21 sub-columns due to the distribution of cloud cover over this 21 sub-boxes because the sub-boxes are only completely cloudy or cloud free.

Referee 1: 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?

Author: In case of HET, the non-linearity of the system can be seen undoubtfully if you look at HET40, but also here the results for HET20 and HET60 are quite similar. For the HET runs we did not do more different simulations.

Referee 1: 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?

Author: The cloud fraction overlap is the same everywhere and it is calculated with the maximum-random overlap assumption, but depending on the vertical correlation of cloud properties they are differently distributed for the sensitivity studies, that is right.

Referee 1: p9355, 2nd para Would not expect much impact for stratocumulus case (as it is a thin single layer cloud). Could include this statement.

Author: We included this statement. Figure 9 shows the changes in cloud properties for different assumptions of the vertical correlation for the EPIC campaign. We did not expect much impact for the stratocumulus cloud as it is a thin single layer cloud. It can be seen that changes occur but they are smaller than the differences between the simulations REF and HOM and the differences when changing the standard deviation.

Referee 1: 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?

Author: I do not understand the question, because we did not confute this statement. If the correlation is low and the IWC in the uppermost layer is small, then the sub-boxes below are filled more or less according to the random distribution, so we will get sub-boxes with high IWC and sub-boxes with low IWC distributed homogeneously. Large ice crystals will sediment and lead to enhanced accretion. Sublimation will take place in cloud free layers below. If there is a high correlation and only small IWC in the layer below then only a small number of sub-boxes or only one will have a high IWC below. Only in this sub-box sedimentation takes place and sublimation in the cloud free layer below. Maybe more precipitation is formed here but a large part of the precipitation sublimates in the lower levels before reaching the surface.

Referee 1: 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!

Author: We added a statement: Our method coupled to the radiation routine as introduced by Räisänen et al. (2004) should save computing time as compared to the use of a high-resolution model (Grabowski, 2001) because only the cloudy columns will be divided into sub-columns and not the whole dymanics and cloud resolving model physics need to be calculated.

Referee 1: 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..."

Author: We included the evaporation in our outlook. Furthermore it might be possible to improve the precipitation formation and evaporation and reduce errors in the radiation budget and distribution of precipitation.

Referee 1: 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.

Author: We reworded the paragraph. Compared to the superparameterization of

Grabowski (2001), the algorithm used in this study to include subgrid variability is not deterministic but statistical and does not involve coupling to the dynamics, but primarily addresses microphysical processes of stratiform clouds. The sub-column algorithm is applied for each time step without using the information inside the sub-boxes the time step before and has therefore no time-dependency. The sub-columns representing one column of a GCM are horizontally independent.

Referee 1: 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.

Author: We added some other processes which are affected by the sub-column algorithm. We introduce inhomogeneities in the microphysical properties of stratiform clouds, which then affect many microphysical processes like precipitation formation, accretion and evaporation.

Referee 1: 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

Author: We changed it to "log-normal distribution". We use a log-normal distribution with prescribed standard deviations for IWC, LWC, ICNC and CDNC taken from atmospheric measurements for the defined cloud properties instead of using constant values for each cloudy sub-box as done by Jakob and Klein (1999).

Referee 1: p9363, table 2 There is little sign of convergence, only for HOM100 and HOM500.

Author: We added a sentence in the analysis. In general, the results converge for an increase in sub-columns, which is also true for simulation HET. High convergence can be seen for the results of HOM100 and HOM500. Table 2 summarizes examples for the heterogeneous distributions (HET20, HET40, HET60).

Referee 1: p9364, table 3 Have the SigmaCDNC and SigmaLWC columns been swapped, except for the HET row? Or have I misunderstood something?

Author: Yes, you are completely right, the columns have been swapped. We changed this!

TECHNICAL COMMENTS Referee1 : Introduction, p9337, line 18 "...permits to increase..." -> "...permits an increase in..." p9340, line 13 "primary" -> "primarily"

Author: We changed the sentences.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C6228/2011/acpd-11-C6228-2011-supplement.pdf

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