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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.

S. Jess et al.

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Referee 3: I. The fit between simulations and observations is poor. However, the authors do not provide basic statistics on the quality of the fit. The authors should provide numbers regarding the biases of REF, HET, and HOM as compared to observations. Additionally, they should calculate the anomaly correlations with respect to data and comment on them. Based on the figures, it appears that any advantage of HET over HOM or REF will be dwarfed by the gap between the simulations and the observations.

Author: You were right. The fit is not so good and there should be done more statistics. Since it is out of the scope to add more statistics and analysis we decided not to publish

Full Screen / Esc

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Interactive Discussion

Discussion Paper



the paper in ACP. But anyway we want to answer your questions below.

Referee 3: II. The text gives a rosier picture of HET than do the figures. This is problematic because some researchers might read only the text but not scrutinize the figures. Abstract: "Results with the new algorithm show an earlier onset of precipitation for the EPIC campaign" The claim of earlier and more accurate onset of precipitation contradicts Figure 3 in the paper. In the second case (EPIC), the precipitation is (nearly) zero for most of the first couple days of all three simulations (REF, HOM, and HET), in contrast to observations, and all three schemes (REF, HOM, and HET) miss the first observed episode of surface precipitation on the 17th. It is true that HET provides an accurate prediction of the magnitude of the 2nd episode of surface precipitation, which occurs on Oct 18 (as do HOM and REF), but HET's simulation of surface precipitation is nonetheless inaccurate overall, e.g. see Oct 20-22.

Author: The precipitation formation begins earlier. This precipitation leads to a decay of the cloud but evaporation in sub-cloudy layers is the reason that only small amount or no precipitation reaches the surface at the beginning. As you said the first precipitation event is not captured at all by all three simulations but the magnitude for the precipitation event on October 18th and 19th is better as compared to measurements. Figure 3 shows an earlier onset of precipitation on October 16th where no precipitation is formed for REF and HET. In this case it is not in accordance with observations but the sentence "Results with the new algorithm show an earlier onset of precipitation for the EPIC campaign" is true. Precipitation starts earlier but at the beginning it is not more accurate because no precipitation was observed. So maybe we can conclude from the model simulations that some precipitation is formed by simulation HET but too early because the precipitation event took place on October 17th while no precipitation is formed for simulations REF and HOM. For October 20th to 22nd HET does not show a better agreement with observations. The overestimation is reduced on October 20th but the average is the same as for REF and HOM, because there are more time steps with precipitation at the surface because more precipitation is formed due to the non-

Full Screen / Esc

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Interactive Discussion

Discussion Paper



Interactive
Comment

linearity of the system. There are maybe just some sub-boxes producing precipitation while the mean properties are too low to form precipitation.

Referee 3: Abstract: "Results with the new algorithm show . . . higher conversion of liquid to ice for the MPACE campaign, which is in better agreement with the observations than the original version of the ECHAM5 model." Fig. 6 shows that the new algorithm (HET) does not have greater IWP than the other (REF or HOM) simulations.

Author: Surface precipitation includes snow and rain. Because more liquid is converted to ice and snow in simulation HET, liquid water path is reduced. But since the ice falls out as snow it is not anymore added to the ice water path in the model, and only a small amount of ice can be seen in the IWP on October 6th. Regarding the average values for simulation HET IWP is even smaller than for HOM, but due to the earlier ice formation in case of HET less liquid water is available for freezing and some of the ice is converted into snow and fell out as precipitation. We reworded in the abstract: Results with the new algorithm show an earlier onset of precipitation for the EPIC campaign and a thinner cloud with a dynamic cloud top for the MPACE campaign, which reduces the liquid water path in better agreement with the observations than the original version of the ECHAM5 model.

Referee 3: p. 9347: "In general, LWP in simulation HET shows better agreement with [EPIC] observations than simulations REF and HOM" True, but HET is still quite inaccurate, particularly on Oct 17-18, where HET predicts nearly zero LWP while observed values peak at more than 200 g/m³ (see Fig. 3).

Author: Yes, that's right but for the second part of the period an improvement can be seen in the simulations. As there is precipitation formed at the beginning of the period LWP reduces stronger than in case of REF and HOM because the cloud is only present in one layer at the beginning of the simulation. We reworded this sentence. In general, LWP in simulation HET shows better agreement with observations during the second part of the period than simulations REF and HOM which can also be seen in the time

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

averaged values for the whole period in Table 3.

Referee 3: p. 9348: "The relationship between precipitation and LWP is closest to the observations for simulation HET." In Fig. 4, there isn't much resemblance between any of the simulations and the observations. Fig. 3 shows that HET grossly underestimates LWP on Oct 17-18 and grossly overpredicts precipitation on Oct 20-21.

Author: As we tried to explain, it can be seen in Figure 3 that LWP is improved as compared to simulations REF and HOM. There are still deficiencies as compared with observations, but LWP is clearly reduced which is way to high for simulations REF and HOM. Regarding Figure 4 it can be seen that that the tendency goes into the right direction. While LWP is too high in REF and HOM, the frequency of lower LWP values is increased in HET which can also be seen in the observations. Of course it is quite difficult to compare a grid-averaged value of a GCM with a point measurement, so it is more important to look at tendencies than to compare absolute values.

Referee 3: p. 9349: "Hence LWP in [the EPIC] simulation HET is closer to the measurements than simulations REF and HOM." I see that HET slightly improves LWP, but I also see that huge errors remain in LWP and in all the other fields (see Fig 3). Can the authors quantify the improvement with statistics?

Author: We compared averaged values from measurements and observations and we looked at the relationship between LWP and precipitation as well as CDNC and precipitation.

Referee 3: p. 9349: "By including cloud inhomogeneities an earlier precipitation formation and therefore a reduction of the cloud life time is triggered due to sedimentation of ice crystals in simulation HET." Fig. 6 contradicts this sentence. Fig. 6 shows that including cloud inhomogeneities does not trigger appreciably earlier precip or a reduction in lifetime.

Author: Looking at Figure 6, the cloud present in a pressure level from 600 – 700

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hPa has a reduced cloud life time due to sedimentation, while the cloud exists more or less over the whole period for simulation REF and HOM. Or if we consider the mixed-phase cloud as a unit we would explain that the vertical extension of the cloud is reduced in HET due to sedimentation of cloud ice. Also here we talk about precipitation formation and not about precipitation at the surface. But looking at Figure 6 slightly more precipitation is formed at the beginning And the model diagnoses a small part as snow.

Referee 3: p. 9349: "The high overestimation of precipitation in all simulations at the beginning seems to be an initialization problem while the absence of precipitation on 7 October may be caused by not considering the large-scale advection of hydrometeors in the forcing data." If so, there are errors of large but imprecisely known magnitude influencing the simulations. When such errors are present, it is impossible to know whether an improved agreement with observations is due to improved physics or compensating errors. Before this manuscript ought to be regarded as publishable, the authors need to diagnose the errors in initialization and forcing and mitigate them in some way.

Author: This work was generated during a PhD and it is out of the scope of this work to correct model errors, but we count possible initialization and forcing problems. It is completely right that it is worth to investigate in model development before further studies are done with the model.

Referee 3: p. 9350: "Although amounts are small, the inhomogeneities produce more snow than simulations REF and HOM in better agreement with the observed light snow showers (not shown)." The authors should certainly show evidence. From Fig. 6, it does not appear that HET produces greater IWP than HOM or REF. Can the authors quantify the improvement?

Author: The table attached gives total precipitation (totprec) for all three simulations as well as the diagnosed snow for the simulations and the percentage of the diagnosed

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

snow as compared to total precipitation. In case of HET 25As written in Klein et al. (2008): "Ice crystals fall from the liquid layers and may reach the ground in the form of light snow or snow showers." Looking at the table nearly no precipitation falls as snow in simulation Ref and HOM, while at the beginning of the simulation and during the last time steps a part of the precipitation is diagnosed as snow in simulation HET which can only be qualitatively related to the observations.

Referee 3: p. 9356: "For the EPIC field campaign the new algorithm was able to produce higher precipitation rate earlier and a reduced LWP in better agreement with the observations." In fact, the HET produces precipitation on Oct 16, which disagrees with the observations (see Fig. 6).

Author: We reworded this sentence to make more clear that LWP is only in better agreement with observations. For the EPIC field campaign the new algorithm was able to produce a higher precipitation rate earlier. The LWP was reduced consequently in better agreement with the observations.

Referee 3: p. 9356: "Especially for the EPIC campaign, the inhomogeneities in simulation HET are necessary to form more precipitation." It does not follow, as the authors state, that inhomogeneities are necessary. Instead, there may be a flaw in the authors' microphysics scheme.

Author: Regarding the precipitation evolution for REF and HOM in case of the EPIC simulation, it can be seen that there are only marginal changes in precipitation between the two simulations. The sub-column algorithm alone can not account for changes in the precipitation formation in this specific case because there is only a single layer cloud. But in simulation HET the inhomogeneities in the microphysical properties produce precipitation in some parts of the cloud, so the horizontal definition of cloudy and cloud free sub-boxes is not enough for a single layer cloud because no accretion increases the probability of precipitation formation but horizontal variability in cloud properties changes the possibility to form precipitation somewhere inside the cloud.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

Referee 3: III. It is unclear what is the computational cost of the algorithm, but it appears to be expensive. p. 9343: "After the distribution of the cloud variables all microphysical processes are calculated for each sub-column separately." This sounds expensive. If $N=20$, and all grid boxes are cloudy, then I would expect the cost of computing the local microphysical processes (but not advection and diffusion of hydrometeors) to increase by a factor of about 20. However, the breakdown of the computational cost is not discussed clearly in the manuscript, even though some overall computational times are listed on p. 9351. Perhaps there are other large costs in the code that makes the sub-columns look relatively cheap.

Author: First of all the sub-column algorithm is only used for cloudy layers. Cloud free layers are split fast into sub-boxes if it is needed for vertical interactions. Especially in the EPIC case where only one cloud layer is present at the beginning of the simulation, the costs are not so high while for the mixed-phase cloud which is present in some layers. The cloud microphysics is only a small part of the whole simulation. Considering the cloud microphysics only the algorithm is expensive but if you consider the whole simulation then the CPU is not that much increased. In case of the MPACE campaign the CPU time for the cloud microphysics and cloud cover scheme only is for simulation HOM about 3 times increased while the CPU time for simulations HET is about 5 times increased as compared to simulation REF (REF 0.19s, HOM 0.56s, HET 0.87s). For the EPIC campaign the CPU increase is about the same but a bit smaller if only the CPU of the cloud microphysics and cloud cover scheme is used (REF 0.32s, HOM 0.77s, HET 1.51s). The simulation HOM needs about 2.4 times more CPU time and the microphysics of the sub-column algorithm HET is 4.7 times enhanced as compared to the reference version.

Referee 3: p. 9351: "As compared to the reference run, the time is increased by 25 to 27simulation with 20 sub-columns." What does this CPU time include? Radiative calculations? What percentage of the overall runtime in the REF, HOM, and HET simulations is spent doing microphysics? The paper needs a table that lists the costs of

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

the major components of the model. From the numbers presented, it appears that microphysics is a small percentage of the total model cost, thereby making the method feasible.

Author: Radiative calculations are included in the CPU time but the percentage of the microphysics is added in the answer above. Again: In case of the MPACE campaign the CPU time for the cloud microphysics and cloud cover scheme only is for simulation HOM about 3 times increased while the CPU time for simulations HET is about 5 times increased as compared to simulation REF (REF 0.19s, HOM 0.56s, HET 0.87s). For the EPIC campaign the CPU increase is about the same but a bit smaller if only the CPU of the cloud microphysics and cloud cover scheme is used (REF 0.32s, HOM 0.77s, HET 1.51s). The simulation HOM needs about 2.4 times more CPU time and the microphysics of the sub-column algorithm HET is 4.7 times enhanced as compared to the reference version. Example of one simulation:

Timer report:	calls	t_{min}	$t_{average}$	t_{max}	t_{total}
total	1	3.737s	3.737s	3.737s	3.737s
radiation	73	.00690s	.00721s	.00757s	.52636s
cloud and cover	1154	.00002s	.00131s	.06664s	1.510s
cloud	577	.00126s	.00259s	.06664s	1.494s

Referee 3: IV. The authors appear to conflate hydrometeor size distributions and spatial distributions. If I understand correctly, the authors' scheme attempts to account for spatial variability; this is related to, but separate from, variability in particle sizes. The excerpts below are confusing. Please clarify them. Abstract: "Cloud properties are usually assumed to be homogeneous within the cloudy part of the grid-box, i.e. subgrid-scale inhomogeneities in cloud cover and/or microphysical properties are often neglected. However, precipitation formation is initiated by large particles. Thus mean values are not representative and could lead to a delayed onset of precipitation."

Author: Since mass and number concentrations are linked, the droplet and ice radii

[Full Screen / Esc](#)
[Printer-friendly Version](#)
[Interactive Discussion](#)
[Discussion Paper](#)


change from sub-box to sub-box in each layer. We reworded this paragraph: Cloud properties are usually assumed to be homogeneous within the cloudy part of the grid-box, i.e. subgrid-scale inhomogeneities in cloud cover and/or microphysical properties are often neglected. 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 3: p. 9338: "The precipitation formation in ECHAM5 is currently calculated using mean values of the cloud condensate in the cloudy parts of the grid-box. However, mean values in a grid-box are not representative for the formation of precipitation as all collection processes start with the large particles in the cloud and more than one cloud could occupy a grid cell of a GCM."

Author: Since mass and number concentrations are linked, the droplet and ice radii change from sub-box to sub-box in each layer. If one sub-box contains a large mass and a small number then the average size of the cloud droplets is larger than a sub-box with the same mass but an higher CDNC. Since we use a double moment cloud microphysics we change the size of the droplets and crystals by changing mass and number concentration.

Referee 3: V. The paper contains several speculations about causes of the errors in their simulations, but the paper provides little hard evidence. It would probably be fruitful to invest more time into diagnosing model errors and bolstering several of the manuscript's claims with evidence. p. 9347: "Since the vertical resolution of the model is around 500 m, the vertical extension of the cloud was equal or less the vertical resolution of the model. Hence, small amounts of precipitation formed inside the cloud caused its decay. The period on October 17th with low cloud cover in all simulations could be due to too low relative humidity in the forcing data (Posselt and Lohmann, 2008)." The almost complete absence of cloud during a couple days of the EPIC simulation is a glaring error. Its source ought to be conclusively identified and corrected

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

before lesser effects such as the within-cloud subgrid variability are addressed. If this case requires higher resolution in order to produce cloud, then the resolution ought to be increased.

Author: To correct the errors of the model is out of the scope of this paper.

Referee 3: p. 9349: "The high overestimation of precipitation in all simulations at the beginning seems to be a initialization problem." The authors should demonstrate convincingly, using the initial conditions and available data, that there is an initialization problem.

Author: At this stage we can only give hints what are the problems of the absence of precipitation because this work was done during a PhD. Before using these campaigns for further model studies improvements of the model physics should be done before.

Referee 3: p. 9343: "For example the precipitation formation process via the warm phase is proportional to the cloud liquid water mixing ratio to the power 2.5 and inversely proportional to the number concentration of cloud droplets to the power 1.8." Because of the inverse proportionality to number concentration, the onset of precipitation is not earlier if there is little variation in mixing ratio but great variation in number concentration.

Author: The idea is that we have a horizontal distribution of cloud properties instead of a homogeneous one. So we have sub-boxes inside a cloud where the cloud properties are such that precipitation formation is possible. In case of a liquid cloud, it can be a sub-box with a large mass and a small number concentration leading to large cloud droplets and therefore a higher probability to form precipitation. The other case where more precipitation may be formed than in the reference simulation or in simulation HOM is when we have a moderate mass but a small number concentration. Then we also large large droplets initiating precipitation formation.

Referee 3: p. 9344: "The standard deviations for the distributions are taken from mea-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

surements over Canada analyzed by Gultepe and Isaac (2004) for CDNC and Gultepe and Isaac (1996) for LWC. . . . For the ice properties the values were calculated from measurements of frequency distributions of data from the Interhemispheric differences in cirrus properties from anthropogenic emissions (INCA) campaign over Punta Arenas in Chile and Prestwik in Scotland during 2000 (Gayet et al., 2004) and data from Schiller et al. (2008)." How do the prescribed values of the standard deviations compare with observations from M-PACE B, M-PACE A, and EPIC? How much do these standard deviations vary across different clouds in the atmosphere? Is it realistic to use a constant value of the standard deviation globally?

Author: We did not look how prescribed standard deviations compare with observations, but I fear that the data available is not enough for statistics. All studies we looked at only distinguished stratiform from convective clouds or stated that the data is mainly sampled among stratiform clouds. Of course it is not very realistic to use a constant value for the standard deviation all over the atmosphere, but since no measurements are available for different regions like tropics, mid-latitudes and Arctic regions including seasonal cycles it is at least a first improvement to include some subgrid variability on a global scale.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/11/C6239/2011/acpd-11-C6239-2011-supplement.pdf>

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

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