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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 #1

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This manuscript presents an algorithm to represent subgrid variability and its influences on microphysical process rates. The algorithm generates sub-columns whose properties are fed into a micophysical parameterization. The algorithm has two variants, one of which accounts for within-cloud variability (HET), and one of which does not (HOM). The algorithm is compared in single-column mode to reference simulations (REF) and observations from marine Sc (EPIC) and mixed-phase Arctic stratus (M-PACE A).

The algorithm is not particularly novel, it appears to be somewhat computationally inefficient, and it improves the solutions only marginally if at all. Based on the figures presented, there apparently remain separate, major errors in the single-column model

that ought to be fixed before an in-depth comparison is made between REF, HOM, and HET. The current model errors overwhelm the small signal (improvement due to HET) that the authors seek. Hence the comparison with observations cannot conclude much about the advantages of HET. Furthermore, the text in places doesn't fully and accurately characterize what is displayed in the figures.

MAJOR COMMENTS:

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.

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.

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.

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).

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.

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?

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.

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.

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?

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 produces precipitation on Oct 16, which disagrees with the observations (see Fig. 6).

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.

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.

p. 9351: "As compared to the reference run, the time is increased by 25 to 27% for the simulation 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 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.

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."

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."

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 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.

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.

MINOR COMMENTS:

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.

p. 9344: "The standard deviations for the distributions are taken from measurements 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?

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