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Interactive comment on "Numerical framework and performance of the new multiple phase cloud microphysics scheme in RegCM4.5: precipitation, cloud microphysics and cloud radiative effects" *by* Rita Nogherotto et al.

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First, we would like to thank all the reviewers for their careful reviews and constructive comments, which helped to improve the quality and clarity of the paper.

Anonymous Referee #1

The paper describes the implementation of an improved cloud microphysics scheme for stratiform clouds within the RegCM4.5 model. The scheme introduces a prognostic representation of cloud water, ice, rain and snow in the model improving the physical basis for simulating mixed phase clouds and microphysical processes. The per-

formance of the model is evaluated using the COSP simulator and comparing cloud radiative forcing to observational estimates. The paper is interesting and well written and requires only few changes. Since a few pieces of information are missing in the paper, prohibiting a comprehensive understanding of the results, I recommend major revisions.

Main comments: 1. It is not clear to me whether you have tuned the model after introducing the changes to the microphysical scheme. I assume that the original model was tuned with the SUBEX scheme to reproduce the radiative budget within the area covered. Could it be that the tuning forces the model to simulate a high amount of high clouds to balance a spurious heating in the model? And, in case you did not tune the model with the new microphysics scheme, you may allow the model to simulate a more realistic cloud field? Please give details on the model tuning and its implication for the results.

The model was tuned after introducing the new microphysical scheme, we wanted to test the best performance given by the new parameterization and afterwards compare it with the pre-existing scheme. The presence of an overestimation of high clouds using the SUBEX scheme, however, was not very sensitive to the SUBEX parameters, therefore clouds pattern wouldn't be much different by tuning the model before introducing the new scheme. The problem of excessive high level cloudiness has been a long-standing one within the RegCM system.

2. Why do you use random overlap? Most large scale models use maximum random overlap. Of course it depends on the layer thickness which overlap is more appropriate. How many of your 23 layers are in the troposphere and what is the resulting vertical resolution in the troposphere?

We thank the reviewer to point this out: we have actually used the max-random overlap assumption but there was a mistake in the text. Most of the 23 layers are located in the troposphere, as the model top is at about 50 hPa. In the mid-troposphere the vertical resolution is about 0.75 sigma (i.e. about 75 hPa) , since higher level density is placed in the boundary layer.

3. The differences between dX/dt and deltaX/delta t : don't seem to be defined clearly enough – see comments further down (5. and 8.).

More clarifications about this in comments 5. and 8.

Minor comments:

1. You may want to give the full name for the SUBEX scheme when it is first mentioned.

Done.

2. Figure 1: What is the process that converts rain into cloud liquid or snow into cloud ice? You probably want to get rid of the arrow head pointing up. The arrow pointing from snow to water vapor should say sublimation and only point up. The evaporation arrow should only point up.

Done. This was a mistake.

3. Equation 2: the sums should go from y=1 up to m and not from x=1.

Done.

4. Text before the equation after equation 2 (which is not equation 3!): You say it is an nxn matrix. But n is your time step! If you want to be consistent with equation 2 it should be an mxm matrix. It should also say 'm = 3 category system' instead of 'n=3'.

Done.

5. Equation 4: L should have an index x instead of x being in brackets. I think it should be dT/dt instead of deltaT/deltat? Why do you subtract the source of q_x due to convective outflow and due to sedimentation from dq_x/dt? These sources of water/ice should be a sink in temperature in the same way as dq_x/dt. If there is a source of q_x due to sedimentation and convective detrainment, then dT_L/dt should not be =0.

We thank the reviewer for this comment as there was a mistake in eq. 4, now corrected with deltaT/deltat. We subtract the convective detrainment term and the advective flux since they do not represent changes in temperature due to the latent heating with changes of phase of water in the scheme itself. Recall that the T_L budget is just used over the cloud scheme, since the processes are solved both implicitly and explicitly. We thus need to use the conservation in T_L before and after the scheme to work out what T change is associated with the change in the family of qv, ql, and qi. Any "source" of microphysics variables that is *not* the result of phase change within the microphysics has to be accounted for, otherwise it will result in a superfluous change in temperature. Recall that the impact on condensation in the convective updraughts is already accounted for in the temperature budget of the convection scheme itself. We have modified the text to: "where Lx is the latent heat (of fusion or evaporation depending on the processes considered), Dqx is the convective detrainment and the third term in the brackets is the sedimentation term. We subtract the convective detrainment term Dqx and the advective flux terms to the rate of change of species qx (due to all the processes) because they represent a net TL flux not associated with latent heating with changes of phase of water in the scheme itself."

6. Equation 5: Please give values/expressions for p, alpha and gamma.

Done.

7. Line 157-160: Not only do the time scales need to be fast but the ice crystal number needs to be high as well.

Good point, although I would clarify the reviewers point in stating that it is not precisely a case of needing fast-timescale *and* high ice crystal number concentrations, since the former is a function of the latter. In the cases where homogeneous ice nucleation dominates, the assumption is very reasonable. However, even if the case of heterogeneous nucleation it is fairly reasonable, since in most updraughts, if the IN number concentration is low, homogeneous nucleation will kick in anyway. The limited (albeit slow) depositional growth of the isolated IN prior to the homogeneous nucleation threshold being reached can be ignored at this level of approximation without impact. If IN are numerous enough to shut off homogeneous nucleation then the timescale is roughly on the same order as a GCM timestep and the assumption is still reasonable. We have added a statement to this effect.

Added in the text: "As stated by \cite{tompkins:07}, this is very good assumption if ice nucleation is predominately homogeneous in nature, although even if heterogeneous nucleation predominates it is still reasonable, since to cut off homonucleation completion IN concentrations need to be of an order of magnitude that results in the growth timescale is similar to a typical global model timestep (Kärcher and Lohmann 2003)"

8. Equation 6: If the left hand side includes large scale advection already, then it is not clear to me why there is a second term on the right hand side.

The equation is correct, the left hand side is the total derivative of the saturation mixing ratio (see IFS documentation). Please find all the steps of the equation in Figure 1.

9. Equation 8: this equation together with the diagnostic cloud scheme removes any supersaturation relative to ice. In line 177 you say 'condensation is a source of ice as homogeneous freezing takes place.' – it should say 'deposition' and the remainder of the sentence should be reformulated explaining that homogeneous freezing would only take place at high ice supersaturations but here in connection with the diagnostic cloud scheme deposition is handled just as condensation removing any supersaturation instantaneously.

We thank the reviewer for the comment and we have riformulated the sentence in this way: "and all the increase of cloud amount is a source of cloud water unless the process occurs within cold clouds, in which case deposition occurs and ice forms. Due to the diagnostic treatment of the cloud fraction, homogeneous freezing takes place and removes any supersaturation instantaneously."

10. Equation 11: You talk about evaporation due to turbulent mixing but you do not mention that by not resolving ice supersaturation you neglect the fact that there could be also an increase in ice mass due to turbulence.

The reviewer is correct and we were aware of this shortcoming already and now explicitly identify it as a caveat of this first implementation in the text.

Changed the text to: "A very simple treatment of turbulence mixing is adopted in this first version of the scheme that duplicates the approach of Tiedtke (1991) by treating turbulence as a sink of cloud water. As discussed by \cite{Tompkins:02} and \cite{Tompkins:05}, the sign of the turbulent impact on cloud water is only correct if the total water mixing ration $q_t= q_l+q_i$ is smaller than the saturation mixing ratio, otherwise mixing leads to an increase in cloud water. The intention is to correct this when a PDF-based cloud cover parametrization is later implemented."

11. Equation 12: The value of alpha does not seem to matter. The source term for any q_x is here D.

We thank the reviewer for the comment: there was a mistake in the equation, now corrected as follows:

$(\partial q\_x)/\partial t=\alpha(T)D\_x$

12. Equation 15: What are the values of b_1, P_loc and c_0 ?

We added the following text after equation (15): "where c0 = 1.67 10−4 s−1, b1 = 100 (kg m−2s−1) and Ploc is the local cloudy precipitation rate."

13. Later on in the text you do not mention which autoconversion formulation you use. It is not clear to me why you need all 4 alternative formulations here in the paper if you (presumably) only use one.

We wanted the paper to describe the scheme in all its features and options. Showing tests of all the autoconversion parameterizations was however beyond the scope of this work. We added in the text: "Here, the default autoconversion parameterization is set to the Sundqvist's scheme (eq. 15) and sensitivity studies using different autoconversion schemes need to be carried out for specific applications."

14. Table 1: You probably want to list the ISCCP simulator in the last 2 lines as well.

Done.

15. Line 299: Please complete the information for this citation in the publication list.

Done.

16. Table 2: How large is the interannual variability in global mean total cloud fraction? Are the differences of the simulated coverage significantly different from the observations or could the simulations be a member of the distribution of observed cloud coverages? I assume you have quite a few years of data from the ISCCP observations and could easily check this. Similarly in table 4 you should be able to give an estimate for the variability of CRF. I assume that the differences in SW and LW fluxes are huge compared to the interannual variability but for CRF_tot it is not that obvious anylonger. Please note that the fourth row should say TOA CRF_SW : : :.. and not LW.

Thanks for pointing out this issue. Figure 2 compares the average of observations for the period of available data (1983-2007, panels g) and h) with the analogous fields for our analysis year (2007). It can be seen that 2007 is generally representative of the long term climatology, suggesting that the inter-annual variation in global mean high, medium and low total cloud fractions is not large and it is reasonable to choose only one season for detailed analysis. We have modified the fourth row.

17. Figure 4e should say MIC Medium JJA

Done.

18. Figure 7: In the CERES data, is the white in the very south in DJF and the very north in JJA missing values or really values close to zero.

We thank the reviewer for this comment. We re-made the three radiation plots with grey areas depicting missing values and added it in the caption.

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$$
\begin{aligned}
\frac{dq_{sat}}{dt} &= \frac{\partial q_{sat}}{\partial T}\frac{dT}{dt} \\
&= \frac{\partial q_{sat}}{\partial T}\left[\frac{\partial T}{\partial t} + \omega\frac{\partial T}{\partial p}\right] \\
&= \frac{\partial q_{sat}}{\partial T}\frac{\partial T}{\partial t} + \left[\frac{\partial q_{sat}}{\partial T}\omega\frac{\partial T}{\partial p}\right] \\
&= \frac{\partial q_{sat}}{\partial T}\frac{\partial T}{\partial t}\Big|_{diab} + \frac{\partial q_{sat}}{\partial p}\Big|_{ma}\omega
\end{aligned}
$$

Fig. 1. Passages of the equation for the total derivative of the saturation mixing ratio

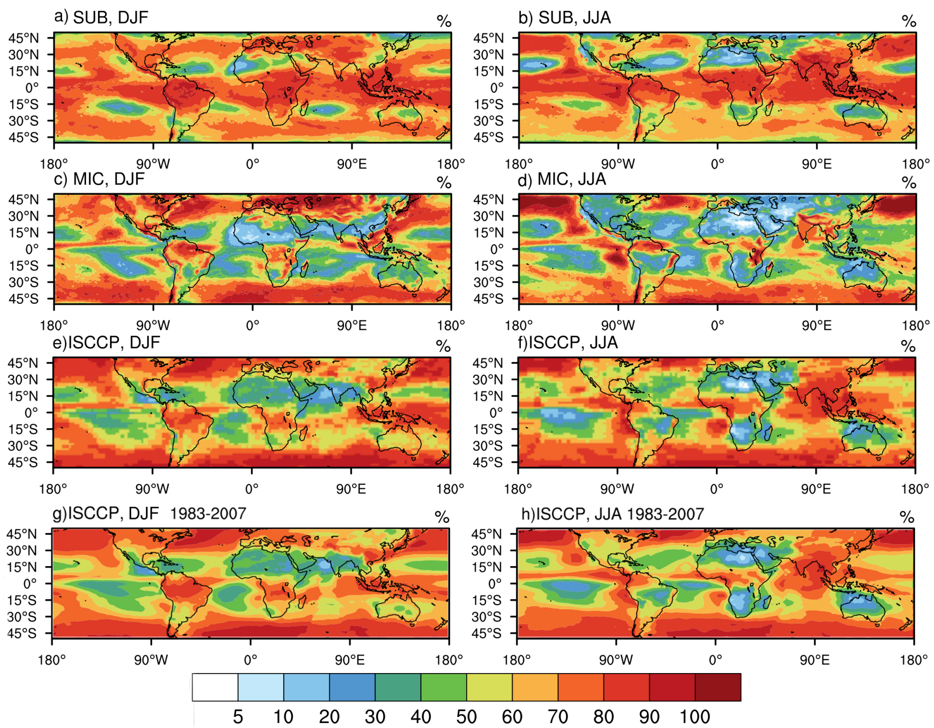

Fig. 2. Same as Figure 3 of the paper but with two additional panels (g) and h)) with the average of total cloud for the period of avialble data (1983-2007).