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Interactive comment on "Development of two-moment cloud microphysics for liquid and ice within the NASA Goddard earth observing system model (GEOS-5)" *by* D. Barahona et al.

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Referee $\#1$

We thank the reviewer for his/her comments on the manuscript. Answers to the comments are provided below.

"**Section 2.3 Please give a short summary and overview of the different sub-sections to follow. Also, it would be helpful to mention at the beginning, that the modifications to the MG08 scheme are listed. Overall this section could profit from a bit more physical background in addition to the equations from**

parameterizations. Also it is hard to distinguish between already existing parameterizations and new developments. Please clarify this."

Additional details have been included to provide a more complete physical background in each Section. The following paragraph has also been included at the beginning of Section 2.3.:

In GEOS-5 clouds are divided into stratiform (cirrus, anvils and stratocumulus) and convective. Stratiform clouds are formed by in situ condensation and anvil detrainment. The stratiform scheme of Morrison and Gettelman (2008, hereafter MG08) was implemented into GEOS-5, as part of a rewrite of the cloud scheme. Since MG08 allows for ice supersaturation, and accounts for activation of aerosols based on a sub-grid vertical velocity, other aspects of the cloud scheme were updated. The calculation of cloud fraction and large scale condensation was modified to account for supersaturation with respect to ice and microphysical processing (Section 2.3.1). The new scheme uses CCN activation and ice nucleation parameterizations of Fountoukis and Nenes (2005) and Barahona and Nenes (2009b), respectively (Section 2.3.2 and 2.3.3). A parameterization of sub-grid vertical velocity was also developed (Section 2.3.4) and MG08 was modified to account for the effect of pre-existing ice crystals on cirrus formation (Section 2.3.5). Finally a new microphysical scheme for convective clouds was implemented (Section 2.4). These modifications represent a complete overhaul of the cloud microphysics of GEOS-5.

"**Section 2.3.2 The homogeneous nucleation (Koop et al. 2000) is only mentioned in one sentence and even though it is said to be dominant (Sec. 3.5). At the beginning of the explanation for heterogeneous nucleation ($T < 235K$) please motivate the additional implementations to Ph13, i.e., accounting for the competition between the nucleation modes and preexisting ice crystals.**"

The inclusion of Ph13 was not done to account for competition between nucleation modes nor preexisting crystals. This is accounted for in the Barahona and Nenes (2009) (BN09) parameterization. Instead, Ph13 is used to inform BN09 on the availability of heterogeneous ice nuclei. Ph13 was selected as it includes the most recent experimental results on freezing of dust, organics and black carbon. Similarly, the parameterization of Koop et al, (2000) is used in conjunction with BN09 to describe homogeneous nucleation. To address the reviewer's concern we have expanded the Section 2.3.3 including additional details on the BN09 parameterization.

"**Section 2.3.5 Where is the vertical velocity accounting for preexisting ice crystal included into the ice nucleation scheme? Please clarify the relation between Ni,pre (preexisting ice crystal concentration) and Nc,nuc from Eq. (16) (nucleated ice crystal concentration).**"

$N_{i,nuc}$, corresponds to the ice concentration nucleated in a time step calculated by BN09. $N_{i,pre}$ is the ice crystal concentration left in the grid cell from previous time steps. The effect of preexisting ice crystals on ice nucleation is parameterized as a reduction in the subgrid scale vertical velocity (wsub, Appendix B) which is an input to the BN09 parameterization. To address the reviewer's concern, the explanation the BN09 parameterization and its relation to preexisting ice crystals has been expanded.

"**Also, the accounting for preexisting ice crystals by reducing the vertical velocity was also applied in the ice nucleation parameterization by Kärcher et al. 2006. Maybe this should be mentioned.**"

Kärcher et al. 2006 accounted for preexisting ice crystals by reducing vertical velocity using a numerical approach, instead of the analytical approach used in this work. This

is now explicitly mentioned in the paper.

"**Section 2.4.3 Please note the existence of graupel in the convection scheme earlier. It is also noteworthy that ice and snow are treated as a single species. This was not clear, as previously only cloud ice, liquid, rain and snow where mentioned.**"

The following paragraph has been added to the section: Precipitation is generated within each convective parcel and is assumed to reach the surface during each time step. The remaining condensate is then detrained into anvil clouds following Eq.(29). Ice water in convective cumulus is likely to exist as graupel, snow and ice crystals, affecting the formation of precipitation. Following Del Genio et al. (1996) a simplified treatment is proposed where total ice is partitioned between ice and snow (assumed as a single species) and graupel. The two species are differentiated by their terminal velocity. This partitioning is prescribed as a function of temperature and used to calculate the formation of ice precipitation within convective clouds. For ice crystal growth and detrainment a single ice species is assumed.

"**Section 3.2 Please explain the two mechanisms on a more physical basis. How does the restriction of ice nucleation to supersaturated regions lead to ice supersaturation?**"

Restricting cloud formation and nucleation to $S_i > S_{crit}$ implies that supersaturation must be built before new clouds can form. The term $P(S_i > S_{crit})$ also reduces the nucleated ice crystal concentration therefore reducing the water vapor relaxation time scale. These two effects lead to sustained supersaturation at cirrus levels ($T < 235K$). This explanation is now included in the section.

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"**S$_{crit}$ is the critical saturation ratio and is therefore not larger than 1 and dimensionless. In this section S$_{crit}$ is rather used as the critical relative humidity, analogue for the clear sky saturation ratio.**"

In this work S$_{crit}$ represents the minimum level of saturation required for ice nucleation. It is dimensionless but can be greater than one since ice nucleation requires supersaturation to occur. S$_{crit}$ is calculated by the ice nucleation parameterization and determined by the aerosol properties and the dynamical forcing. It is defined locally and therefore different from grid cell clear sky saturation ratio. The section has been expanded including additional details on the cloud fraction scheme.

"**Figure 2: Please be more accurate about what data is used. In the plot caption it states, that it is annual data. Which year? Does Fig. 2 include cloudy and clear sky saturation? Are values only above $S_{crit} > 100\%$ considered, as there are hardly any values around saturation at $S_{crit} = 100\%$.**"

The figure was obtained from 6-h instantaneous output over a 3-year subset (2002-2005) of the NEW run. By definition $S_{crit} > 100\%$ since ice nucleation is not possible without supersaturation. S$_{crit}$ depends on the local vertical velocity and the characteristics of the aerosol at the scale of individual cloudy parcels ($\sim$ 100 m – 1 km). We have included additional explanation in the text (Sections 2.3.3 and 3.2) to clarify the meaning of S$_{crit}$. Additional details have been included in the caption of Figure 2.

"**Also I would expect a smoother profile for an annual global frequency distribution. What is the source for the outliers? Please discuss the reason for the high critical saturation ratios, especially the peak of S_{crit} at 150% in the left plot.**"

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There is no smoothing of the profile because instantaneous output is used. The peak at 150% and the highest S_{crit} values correspond to low temperature regions (red regions on the right panel of Fig. 2) with high vertical velocities and low aerosol concentration, common around the tropopause. The S_{crit} distribution is also determined by the availability of IN and the temperature; in general high IN concentration leads to low S_{crit} (Barahona and Nenes, 2009a). The peak in S_{crit} at 150% results from a lack of IN at very low T.

"**According to this plot the homogeneous nucleation regime (** $S_{crit} \sim 140\%$ **) is very dominant.**"

Not necessarily. The predominance of homogeneous nucleation depends on whether a cloud is actually formed at those conditions. Although high values of S_{crit} are very frequent for $P < 50$ hPa (Figure 2, left panel) most cirrus form between 100 hPa and 300 hPa (e.g., Figure 8) where $S_{crit} \sim 110\% - 130\%$. At these vertical levels S_{crit} is relatively high ($\sim 130\%$) in the Southern Hemisphere but lower in the Northern Hemisphere. Thus homogeneous freezing is more predominant in the Southern Hemisphere. This is in agreement with Fig. 7. The above discussion has been included in the paper.

"**Figure 3: What time interval is investigated? Are the MOZAIC and AIRS data only for clear sky as well?**"

MOZAIC and AIRS data span from 2002 to 2004. Yes, the retrievals mostly cover clear sky conditions.

"**Are the high clear sky saturation ratio values above** 160% **and the outlier at** 190% **physical? The blue and red shades of the left plot turned purple and pink in my print. Legend labels are missing for the middle and right plot.**"

The values are indeed physical. High values of supersaturation are obtained at very low temperature ($T \sim 200$ K) likely caused by low water vapor relaxation rates and lack of cloud formation. A similar behavior is observed in the satellite retrievals indicating that this is not an artifact of the model. Also the model is restricted so that S_i is always below its value at water saturation. The figure has been updated to avoid referring to specific colors. Captions have been added to the middle and right plots.

"**What might be a reason for the maximum supersaturation at 700 hPa and 60S in the GEOS-5 Output (middle plot)?**"

We thank the reviewer for pointing this out. This behavior was caused by the presence of liquid water at $P > 600$ hPa. We have corrected the Figure removing such instances. The new plot is slightly different from the original however the conclusions of the work remain the same. The discussion has been updated accordingly.

"**Please discuss possible reasons for the vast differences of S**$_{crit}$ **(Fig. 2 left) and clear sky S**$_{i,c}$ **(Fig. 3 left).**"

We do not expect the distributions of S_{crit} and $S_{i,c}$ to be similar. S_{crit} can only assume values greater than 100% and is defined by the ice nucleation threshold calculated by the ice nucleation parameterization. $S_{i,c}$ is the grid scale clear sky saturation ratio. Unlike S_{crit}, $S_{i,c}$ is influenced by large scale dynamics, temperature and microphysical processing, and only indirectly by the aerosol properties and the subgrid scale vertical

velocity.

"**Section 3.4 Please be more critical in discussing Fig. 5. Also, in Fig. 5 the middle plot using the ARGACT CCN parameterization is not mentioned in the text.**"

The biases of GEOS-5 regarding cloud droplet number are already discussed in the Section. The following text has been added to the Section to emphasize the limitations of the current GEOS-5 configuration: CDNC can be influenced by CCN activation parameterization and the aerosol size distribution. The GOCART model used uses a single moment aerosol microphysics, and some uncertainty may result from assuming a fixed size distribution to obtain the aerosol number concentration. The impact of this assumption is discussed in Section 5. The sensitivity of CDNC to the CCN activation parameterization was studied by implementing the Abdul-Razzack and Ghan (2002, ARG) activation parameterization (Fig 5, middle plot) and is analyzed in Section 4.

"**Section 3.5 What would be a possible explanation for the wave-like pattern in Fig. 6d?**"

Further analysis of the pattern shows that it is due to coarse vertical resolution. The post-processing subroutine maps the 72 model levels to 48, with only 8 vertical levels between 700 hpa and 300 hPa. This is noticeable when plotting the contribution of convective detrainment to Nc because detrainment at subfreezing temperature conditions is relatively infrequent. This limitation is now explicitly acknowledged, however it does not affect the conclusions of the paper. The plot has been smoothed out to minimize this effect.

'**Please introduce the three plots in Fig. 7 and specify in the text, which one is referred to when discussed.**"

The following text was added to the section: Figure 7 (left panel) shows the spatial distribution of ice crystals concentration nucleated in cirrus ($T < 235$ K), weighted by cloud fraction. The spatial distribution (also weighted by cloud fraction) and zonal mean of the contribution of heterogeneous ice nucleation to $N_{c,nuc}$ are shown in the middle and right panels of Fig. 7, respectively.

"**The 30% contribution of heterogeneous nucleation cannot be seen clearly in South America.**"

This refers to the west coast of South America. The statement has been clarified.

"**Section 3.6 What is the regarded time range for all figures in this section?**"

Model results span over the 10 years of simulation. CloudSat retrievals are plotted for the period 2007 to 2008. These statements have been added to the Figure caption.

"**Why is there such a big reduction in total ice condensate in the tropics with GEOS-5 NEW in Fig. 8?**"

There may be several reasons for it. The operational version of GEOS-5 uses a saturation adjustment scheme that creates ice cloud at ice saturation whereas the new microphysics restricts ice formation to supersaturated regions. The parameterizations of sublimation, autoconversion and accretion subroutines, as well as the time scale

of ice crystal growth were also replaced by new formulations that besides total mass take into account particle size and number concentration. The formulation of cloud fraction was also modified. Deep analysis of the effect of each of these factors on ice water content is out of the scope of this work and may overextend the paper. It will be addressed in future studies.

"**Section 3.7 Please discuss the different frequency peaks at different temperatures in Fig. 11 for NEW. Are these the different nucleation modes?**"

The peaks result from coarse vertical resolution (about 50 hPa) between 700 hPa and 300 hPa, and are an artifact of the post-processing subroutines. The plot has been smoothed out to minimize this effect. Nucleation modes are not used in the ice nucleation parameterization. Instead, heterogeneous ice nucleation is described using a continuous spectrum obviating the need for prescribed thresholds.

"**Section 3.8 As stated earlier, please explain what the COSP package is. Is MODIS data already used in this package?**"

Satellite data is not incorporated in COSP. The following paragraph has been added to Section 3 to clarify the usage of COSP: The Cloud Feedback Model Intercomparison Project Observation Simulator Package, COSP, (Bodas-Salcedo et al. 2011) was used to compared model output to satellite retrievals. COSP uses the model output to simulate the retrieval of satellite platforms, minimizing in this way errors from the sampling of the model output when comparing against satellite observations.

"**Section 5 In line 15: The critical supersaturation in Eq. (15) only indirectly accounts for sub-grid scale dynamics and aerosol properties.**"

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S_{crit} is calculated by the nucleation parameterization which accounts directly for subgrid dynamics and aerosol properties. We have added expanded section 2.3.1 to better explain the relation between S_{crit} and ice nucleation.

Technical Corrections

"**- p. 5298, line 5-16: please be a bit a more precise about the the mathematical derivation of the Eqs. (10) and (11). How does q mx from Eq. (8) relate to qmx from Eq. (10)? How does qt appear in Eq. (11) as qmx was eliminated?**"

A new appendix have been added detailing the derivation of Eq. (11).

"**- p. 5302, line 10: What is Shom ? Did you use a numerical fit based on Koop et al. 2000 as done by, e.g., Kärcher and Lohmann 2002, Ren and MacKenzie 2005?**"

Shom is calculated using the correlation of Ren and MacKenzie (2005).

"**- p. 5302, line 20: are the equations (Eq.(17)-(19)) for the mixed-phase regime all based on Ph13? If so please mention.**"

Ph13 is only used for condensation/deposition ice nucleation. Ice nucleation by immersion and contact freezing is treated explicitly following the correlations of Murray et al. (2012) and Niemand et al. (2012). This is now clarified in the paper.

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"**- p. 5305, line 6: what value is assumed for λm (lm in the free troposphere)?**"

It is estimated as 10% of the boundary layer height from the previous time step (Molod et al. 2012). This is now mentioned in the revised paper.

"**- p. 5312, line 19: is the graupel equation not used for Eqs. (28) and (29)?**"

Since graupel is not detrained only the total ice (ice/snow plus graupel) is used in Eq. (28). The partitioning between ice/snow and graupel only affects the generation of precipitation. This is now clarified in the Section.

"**- p. 5315, line 19: 'that for the conditions of Fig. 3', what are the conditions? Is this only upper troposphere?**"

For Figure 3 the statement refers to $P > 200$ hPa. It has been clarified.

"**âĂć p. 5321, line 18: stated in line 13 is that LWP is not generated by COSP, how come the annual average of GEOS-5 COSP is used here? In the plots the LWP mean of GESO-5 is 37.1 g m−2 , in the text it states 60g m−2.**"

The version of COSP implemented in GEOS-5 simulates MODIS retrievals but does not have a CloudSat simulator. The different values of LWP result from using different satellite simulators along with the same model output. If the COSP MODIS simulator is used then LWP as seen from MODIS would be 60 g m^{-2}. Model native output is however 37.3 g m^{-2}. This has been clarified in the revised paper.

"**- p. 5321, line 26: the IWC from GEOS-5 is lower**"

The statement refers to IWP (not IWC). IWP from GEOS-5 is 27.1 g m^{-2} and CloudSat 25.8 g m^{-2}.

"**- p. 5321, line 28: what is ISCCP?**"

It is International Satellite Cloud Climatology Project. The abbreviation has been clarified.

"**- p. 5324, line 24: word missing 'the MODIS COSP simulator'. Is the annual mean for CTL or NEW?**"

The sentence refers to the NEW run. This has been corrected.

"**âĂć p. 5359, Figure 2: Which year is this? For the zonal mean the label for x legend is missing. In caption: Solid lines (right) represent the annual mean tropopause pressure. I think you mean the bold solid line, while the solid lines are contour lines.**"

The figure was obtained from 6-h instantaneous output over a 3-year subset (2002-2005) of the NEW run. Labels have been added to the picture and the caption corrected.

All other technical corrections have been made.

References

All references are similar as in the original paper.

Referee #2

We thank the reviewer for his/her comments on the manuscript. Below we address the reviewer's comments.

"**Please give more detail on the cloud cover scheme. Is it a diagnostic or prognostic scheme; is the variance of the uniform PDF fixed? On page 5295 you say that it is based on Slingo (1987) and that it follows a total-water-PDF approach. The Slingo scheme is a relative humidity scheme augmented by an additional predictor based on vertical velocity. Of course a PDF can be written connected with this cloud scheme (as done in equation 3) but presumably the PDF depends only on relative humidity and has a fixed variance. On the other hand you calculate the change in PDF width due to microphysical processes in equation 10. If $\triangle$q is prognostic then $\triangle$q must be calculated before calculating f_c in equation 6. What is the equation for $\triangle$q. Please add more detail and clarify.**"

The proposed scheme can be qualified as semi-prognostic. To make an initial estimate of f_c a total water PDF scheme with prescribed variance (parameterized in terms of RHc) is employed. This is fully diagnostic since RHc does not depend on state variables. The convective contribution to f_c however is fully prognostic and depends on the detrained mass flux parameterized using a Tiedke style approach (Tiedke, 1993). All microphysical processes are calculated using the initial estimate of f_c. Processes of precipitation and evaporation may lead to a variance that is inconsistent with the distribution of total water in the cell. Since total water and total condensate are known after the microphysics, then a new variance consistent with the new state of the system (hence a new cloud fraction) can be calculated by inverting Eq. (5). This may have the limitation that the microphysical processes were calculated using an initial estimate of

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f_c instead of its final value, however ensures consistency between f_c and q_c at the end of the time step.

The above discussion has been introduced in the paper. The derivation of Eq. (10) is now explicitly detailed in a new Appendix.

"**The introduction of the variable S_{crit} allows cirrus to form at higher relative humidity consistent with ice nucleation and therefore resolves ice supersaturation. But the clouds also disappear at high relative humidity in your scheme since a diagnostic cloud cover scheme does not allow cirrus to persist at lower total water content after a nucleation event at high relative humidity. This has of course an impact on the ice supersaturation frequency, cloud cover and cloud properties in your model. This fact should be pointed out in the paper and the impact discussed.**"

This is an excellent point. Indeed the cloud fraction scheme may reduce cloud fraction if $q_t < S_{crit} q_s^*$. However if the cell is still supersaturated (i.e., $q_t > q_s$) water vapor deposition onto preexisting ice offsets this change and restores f_c (f_c is assumed to increase proportionally to the change in q_i from deposition). This is done along with the partitioning of condensate before the microphysics. Thus cirrus persist even if $S_i < S_{crit}$. This explanation has been added to the section.

"**How do you achieve consistency between cloud cover and ice crystals if cloud cover is decreasing with decreasing supersaturation (equation 6) and ice crystals are presumably still existent since air is still ice supersaturated? If cloud cover decreases with decreasing humidity then ice crystal density should increase unless they also sublimate at ice supersaturation. Please discuss.**"

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As mentioned above this apparent inconsistency is resolved by water vapor deposition onto preexisting ice crystals. This is done before the microphysics along with the partitioning of new cloud condensate.

"**One of the main uncertainties in simulating aerosol effects on clouds is the uncertainty in ice nucleation thresholds of different aerosols and their abundance. I did not find any mention of the assumptions made in the model even though these have a large impact on the simulated cloud properties and the competition between heterogeneous and homogeneous freezing and S_{crit}. Please discuss uncertainty due to those assumptions. In the discussion of your results you often comment that errors may be due to uncertainty in the dynamical forcing but they may also be due to uncertainty in freezing thresholds and number concentrations.**"

The BN09 parameterization differs from other approaches in that it does not use prescribed freezing thresholds for heterogeneous ice nucleation. Instead, a continuous function called the heterogeneous nucleation spectrum which describes the ice nuclei number concentration as a function of T and S_i is employed. The nucleation spectrum can have any functional form. In this work the recent formulation of Phillips et al. (2013) which arguably includes the most updated experimental results is employed. Similarly, immersion IN are described as a continuous functions of temperature. It is acknowledged that there is still uncertainty in the formulation of the nucleation spectrum. This is now explicitly mentioned in the paper and its impact discussed.

"**I couldn't find any information on your aerosol scheme. Is the aerosol loading prescribed or is an interactive scheme coupled?**"

The following paragraph was added to the paper for clarification: The Goddard Chemistry, Aerosol, Radiation, and Transport model, GOCART, is a single moment global aerosol transport model. GOCART runs interactively with the GEOS-5 model as described in Colarco et al. (2010). It includes components for dust, sea salt, black and organic carbon, and sulfate aerosols. Scavenging of aerosol mass is based on a convective mass flux approach. Aerosol properties do not change upon droplet and ice crystal activation.

"**What happens when aerosols activate/nucleate or droplets/ice crystals evaporate/sublimate? Are those aerosols available for activation/nucleation again and do their properties change?**"

The mass-based scavenging of aerosols in GOCART prevent the recirculation of IN. Preactivation of IN is not taken into account. Further research is required to elucidate the impact of these factors on clouds on a global scale.

Minor comments:

"**S_{crit} is not defined in sect. 3.5 but in 2.3.3. Please change text accordingly. Please give some more detail. Please define all variables (e.g. equ. 32, 35, 37) and add discussion to e.g. equation 31.**"

Corrected.

"**Related to the main points above: I cannot follow the derivation of equation 10. If you substitute $q'_{mx} = q'_t + .5\Delta q = q_t + \Delta q_c + .5 * \Delta q'$ in equation 8 then you**

should get a far more complicated expression for $\triangle$q. **I can't see how you get to** q_{mx} **without dash within the bracket.**"

This is a typo, it is supposed to be q'_{mx}. It has been corrected. A new appendix has been added detailing the derivation of equations in Section 2.3.1

"**Is your equation 32 consistent with the generation of total condensate calculated by the convective parameterization? Don't you need to include at least a factor that makes sure that** dq_i/dt **is always smaller than** dq_{cn}/dt**?**"

We thank the reviewer for pointing this out. The model is implemented so that dq_i/dt is always lower than dq_{cn}/dt. The equation has been updated accordingly.

"**Could you please comment on the structure of figure 6d. It looks like convection can be only happening in certain temperature intervals or its probability is much higher than at other temperatures.**"

This is caused by coarse vertical resolution. The model has about 15 vertical levels between 700 hPa and 300 hPa however they are remapped in post-processing so that they number of levels is reduced to only 8. This is now explicitly acknowledged in the paper, however it does not modify the conclusions of this study. The plot has been smoothed out to minimize this effect.

"**In figure 5 (MODIS) is the grey color missing data?**"

Data for latitude higher than $60°$ has been excluded from the analysis since MODIS

LWP is very uncertain in those regions. This is now explicitly mentioned in the Figure Caption.

References

All references are similar as in the original paper.

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