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Geoscientific Model Development Discussions

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Interactive Comment

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.

Anonymous Referee #2

Received and published: 7 January 2014

The paper describes changes to the cloud scheme within the GEOS model in detail and includes an extensive evaluation of the simulated climate and several sensitivity studies. The model description is partly difficult to follow since some background information is missing, equations are not always discussed and variables not always introduced. In particular more detail on the cloud cover and aerosol schemes are needed to understand the new cloud scheme. I recommend publication after revision of the paper.

Main comments:

Please give more detail on the cloud cover scheme. Is it a diagnostic or prognostic

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Interactive Discussion

Discussion Paper

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 delta_q is prognostic then delta_q must be calculated before calculating f_c in equation 6. What is the equation for delta_q. Please add more detail and clarify.

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.

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.

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

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freezing thresholds and number concentrations.

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

Minor comments:

S_crit is not defined in sect. 3.5 but in 2.3.3. Please change text accordingly.

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 delta_q'. I can't see how you get to q_mx without dash within the bracket. Please give some more detail.

Please define all variables (e.g. equ. 32, 35, 37) and add discussion to e.g. equation 31.

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?

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.

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

Interactive comment on Geosci. Model Dev. Discuss., 6, 5289, 2013.