Review of revised version of **Implementation of aerosol-cloud interactions ......**
by S. Dipu Sudhakar et al.

The authors present the revised version of a numerical study on aerosol, clouds and radiation with mutual interactions and compare the results with saltellite derived data.
Although, the paper shows some improvement compared to the 1st version, it is still far from publication. Still, the presentation is partly vague, requires physical interpretation, lacks some corrections according to the suggestions in previous review, and requires a substantial improvement of the language. It is strongly recommended to follow all comments. Slanted fonts stand for citations from the 1st review.
Yet, before publication, I suggest another considerable revision.

Comments

- The following 2 questions have not been answered:
  In midlatitude winter I expect that the ice phase plays an important role in the development of clouds and precipitation (Bergeron-Findeisen effect!), and you use the Seifert and Beheng (2006) scheme for mixed phase clouds. The paper, however, is devoted to the liquid phase alone.
  Please discuss the effect of the modified treatment of drop nucleation on the ice phase properties, since a modification in one path of condensate formation is connected with an opposing trend in other path(s).
  How do you determine the effective radius under cloud free conditions?
  Do you use the scheme of Seifert and Beheng (2006) in its warm cloud version? If Yes, then please argue for the neglect of the ice phase in midlatitude winter. If No, then please explain the changes in the ice phase properties in the whole model domain when changing the nucleation treatment. See p.6 l15 'screened for liquid phase clouds only'.
  Next, concerning r_e. Do you assume a lower limit of r_e for cloud free conditions (see question above), as suggested in your response? If Yes, $r_{e,min} = 2\mu m$ (see p.6 l15), this - together with $\tau = 5$ - results in $N_d = 5.4 \times 10^9 m^{-3}$ (Eq. 9). Something goes wrong here.......
  Please clarify.

- Equations (3) - (5) as in the first review:
  The reviewer is familiar with the calculation of the moments and related properties from the cloud drop size distribution $\phi(D)$. Then, (4) follows as

$$\lambda = \left[\frac{\pi \rho_w N \Gamma(\mu + 4)}{6 \rho q_c \Gamma(\mu + 1)}\right]^{1/3}$$

  with ρ air density, ρ_w bulk density of liquid water, q_c mass fraction of liquid water, N number of drops per volume, as was already explained in the 1st review.
  Please revise and check the relationship $\lambda(N, q_c)$ used throughout the paper.

- The following questions have not been answered:
  Problem of avaraging.
  p. 7, Figs. 4, 6(new). Cloud water path is a property defined for the whole air column. Cloud effective radius, cloud droplet number concentration, and sulfate aerosol number concentration are defined locally, and for a grid point model the data are interpreted to be representative for the grid cell. For which level are the presented data relevant? If they are vertical averages, please discuss, how the vertical average is calculated, how cloud free layers are considered, how the result is to be interpreted, etc. This point is

even more complicated for the local variable r_e, which depends nonlinearly on the local variables N and q_c. Likewise, optical thickness is defined for a certain layer of thickness dz, maybe the layer where the respective r_e holds. The presented fields (do they hold for the whole column?) depend on the averaging method.
The same question arises for the daily averaging procedure and concerns also liquid water path. It concerns both, model and satellite data.
Please explain, and correct the discussion where neccessary.
The reviewer is aware that you use the COSP satellite simulator. The question of averaging, however, is not answered, and the added text p. 5 l.27pp is not helpful in this context.

- As before: interpretation of Figs. 4, (new) 6.
Drop number concentration, liquid water content and path, optical thickness, and effective radius are interrelated, not independent of each other. The correlation may be positive or negative, see e.g., (5) states $\tau \propto LWP$ and $\tau \propto 1/r_e$, while Fig.4 suggests on first glance only the first relation. Please interprete the graphics in terms of these interrelations.
Again: For the discussion of the improvement of COSMO-MUSCAT to COSMO-2M it would be helpful to include the COSMO-2M-fields in Fig. 4 besides (or instead of) the difference fields.

- The following questions have not been answered:
The choice of the parameters C_{ccn} (p. 4 bottom) is a good general guess, however, not a universal constant. Did you do a similar run with modified C_{ccn}-values to check its influence - in opposition to the influence of the full interactive treatment with MUSCAT?
COSMO-MUSCAT seems to result in much smoother distributions than COSMO-2M, in particular Fig. 5 (new: Fig. 6). Do you have an explanation?

- Please go through the whole paper carefully. Avoid repetitions, strengthen the physical interpretation, improve the verbal presentation, eliminate errors in grammer and spelling.

- p.7 l.9: wrong unit of rainfall amount.

- p.7 l.22, Fig. 4: You give the range of data for cloud optical depth with a minimum of 5 for both satellite and model data. This does not agree with the figures: Huge white areas occur, and white stand for data less than 5 according to legend. If on the other hand, you prescribe $\tau = 5$ as minimum (p.6 l.15), than refer to that chosen threshold.
A similar problem concerns the effective radius (l. 26) with white standing for $r_e < 2$ μm, that is the mentioned minimum value.

- p.8 l.7pp. Please clarify: The under-/overestimation refers to the frequency of the respective value of liquid water path or optical depth.
Does the model really overestimate 'low cloud' or does it overestimate the frequency of low optical depth cases?
Please clarify for all 3 properties.

- I appreciate the inclusion of fig. 5 for the different probability distribution functions. How do you define here the PDF? Unit?
The interpretation is given as a description of higher/lower PDF and the conclusion that the PDFs are similar for the single day and the period. Unfortunately, an interpretation of the differences/coincidences in the structure of the PDFs for the model and the satellite data is missing. It would be interesting to look for reasons of the shift in PDF for liquid water path, the more frequent occurrence of low τ in COSMO, and the preponderance

of r_e around 10 μm in COSMO, the peak in the MODIS-PDF for cloud optical depth between 150 and 200, the drop of COSMO-PDFs to 0 around LWP = 20m $\tau = 50$, $r_e =$ 30 μm, and many more features.
What are the PDFs in the COSMO-2M case? This may help the interpretation.

- p.8 l.19p. I do not see this.
  p.8 l.20p: Something goes wrong with the sentence.
  Please clarify.

- p.7 l.20pp (new: p.8 12pp), Fig.4 g-i. You describe what is seen in the figures, but you do not give a physical interpretation. As suggested in the 1st review, the differences should be seen in relation to the signal, and then you find differences of 50% of the signal. If you use the full Seifert and Beheng schene, then the difference in LWP should be seen also in relation to the change in cloud ice concentrations (not only locally but in the whole domain). The sequel of e.g., red and blue bands over the Biscaya may be a phase shift.
  p.8 l.19p. I cannot see the superiority of COSMO-MUSCAT from the presented material.
  p.8 l.20p. Sentence unclear. (i) Any explanation is missing. (ii) If you compare two models differring in 2 parameterizations, you cannot trace back the differences simply to the microphysics parameterization. Please clarify.

- p.8 l.29-31. Under-/Overestimation - in comparison to the what?
  l. 31pp. '... explained by cloud microphysics modification.... MUSCAT-model.' I cannot see an explanation why N_d should be reduced in the MUSCAT-model. Please explain.

- See 1st review. Old: *p.8 l.6, new: l.32 Fixed CCN= 300cm^{-3} in COSMO-2M? This is in contradiction to Section 2.1, telling N_{ccn} is given as function of S.*
  l. 32. Similar: 'constant cloud condensation nuclei profile'??
  Please clarify.
  You have answered the question in your response, but you did not clarify anything in the paper. Please precise your wording and distinguigh clearly between the properties N_{ccn} and C_{ccn} as well as to talking about CCN (an abbreviation to shorten the text).

- As 1st review:
  p.8 l. 9pp, new: p.8 l.34pp. The aerosol NUMBER (not 'mass') concentration is given in Fig. 6c. Could you please comment on the fact, that $N_{sulfate}$ is so much larger than N_d for COSMO-MUSCAT? Is the result of Boucher and Lohmann (1995) transferrable to your model concept?
  Please make clear which parameter you are talking about and give a precise explanation.

- As 1st review: *p.8 l.14pp, new: p.9 l6pp. Please revise the para.*
  'The model is unable to capture sub grid scale cloud patterns': A subgrid scale cloud cannot be captured by the microphysics parameterization of Seifert and Beheng (2006) or similar ones. You would need a different parameterization tool. '
  You talk of the 'coarse resolution' of the model. I would not call a mesh size of 28 km 'coarse'. More important: Please tell the resolution of the satellite data when you compare the resolution.
  If the snow cover can be ignored - how are the satellite retrievals affected by the snow cover?
  Please use a more precise wording.

- As 1st review: *p. 8, new: p.9, Section 3.3. l. 25 new: l. 19. '(20 to 20 W m^{-2})'??*
  Fig. 7. The colorbars are differently scaled. Sometimes this is straightforeward, but

sometimes, however, confusing. Please unify the scaling insofar as to use the same scaling at least for SFC and TOA net down SWR. Same for Fig. 7.
Fig. 7 a-d contains is repetition of Fig. 6 e-h. Use the difference fields COSMO2M rad minus CERES instead.
Are figs. 7 i and k really different?
Once again: *Please interpret the radiative flux differences also in terms of the cloud properties.*

- p. 10
  Now you have a contradiction in conclusions 1 and 2. Conclusion 1 - 'modification has only a minor effect'. Conclusion 2 - COSMO MUSCAT shows an improvement in the cloud microphysical properties.
  Please clarify.
  What is the outcome of the PDF analysis?
  From the 1st review for Conclusion 1: *If you refer to the model runs COSMO-2M and COSMO-MUSCAT, please say so. Then, this statement does not agree with p.7 l. 20-29 (new: p.8 l.12-21). Please clarify.*
  Conclusion 3. You can find differences in the model runs with and without the effect on radiation. How do you know that the new approach gives results closer to reality?