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

The authors present a numerical study on aerosol, clouds and radiation with mutual interactions and compare the results with saltellite derived data. . This is an important topic, since all cloud-related processes pose a severe problem in weather forecast and climate modelling. The paper contributes to the ongoing research by examining the effect of mutual interactions of these processes and the improvement of atmospheric models. This is worth to be published. The presentation is concise, length and number of figures are appropriate. However, the presentation is partly very vague and not consistent throughout the paper. The differences between the resp. data fields are inspected by eye, but not quantified. Therefore, it is difficult to follow the conclusions. Errors in some equations may be typos.
Yet, before publication, I suggest some substantial revisions. Please see the major points and specific remarks below.

Major points

- 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?

- Equations (3) - (5)
  (3) holds if the cloud drop size distribution is used with the internal coordinate drop diameter D, not radius r. 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.
  (5) requires some explanations as for the inherent assumptions to be reproduced by the reader. A familiar model for the optical thickness (see e.g., Salby: Atmospheric Physics. Academic Press, 1996, Eq.(9.45)) gives

  $$\delta = \frac{3}{2}\frac{\rho q_c dz}{\rho_w r_e}$$

  which differs by a factor of 2 from (5).
  Please clarify.

- The nucleation rate (7) is connected with supersaturation S. Small but inevitable errors in vapour concentration q_v signify huge relative deviations in S. Can you estimate the resulting uncertainty in the nucleation rate?
  Do you have a full prognostic equation for supersaturation S or do you use saturation adjustment to calculate S? In the second case, some more information is required for the calculation of the nucleation rate by (7). How do you get a supersaturation $S > 0$ despite adjustment?
  The uncertainty of calculation of S occurs in all schemes using an equation such as (7).

I wonder wether it is helpful to introduce more physical details on the nucleation rate as long as the basic property S carries such an uncertainty.
Please comment.
The size of a freshly nucleated droplet is to be prescribed. What do you assume?

- Problem of avaraging.
  p. 7, Figs. 4,5. 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 given 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 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. See Specific Points.

- Drop number concentration, liquid water content and path, optical thickness, and effective radius are interrelated, not independent of each other. Fig. 4 shows a strong correlation between optical thickness and cloud water path, as expected. The effective radius distribution shows a different pattern, somewhat inversely to the drop number concentration in Fig. 5; for same liquid water content, a lower N_d means a larger r_e, see e.g. the relationships (3) - (5). This relation should be taken into account in the interpretation of Figs. 4 and 5. 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 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 the influence - in opposition to the influence of the full interactive treatment with MUSCAT? COSMO-MUSCAT seems to result in smuch smoother distributions than COSMO-2M, in particular Fig. 5. Do you have an explanation?

- The aerosol-cloud-radiation interaction is an important point, since it affects directly the energy budget. Unfortunately, the discussion is limited to a description of Figures 6, 7, and no information on the cloud related parameters of COSMO-2MR are given. Either this aspect should be strengthened or skipped.

- The wording and the comparison can be more straightforward and more precise throughout the paper. Please work over the whole text. This concerns in particular the data intercomparison, which is done on a subjective basis phrasing like 'the differences are small'. Please quantify your statements for objective conclusions. Otherwise, e.g., the conclusion of superiority of COSMO-MUSCAT is not a priori clear from the case study, in particular since the difference between the MODIS data and each model result is larger than the difference between two model versions.
  Please also interpret systematic differences in terms of the model modifications.
  Might it be possible that parts of the differences between data from simulation and satellite are due to a) different cloud distributions and b) different instants of time used for the daily average?

Specific points

- Introduction: The section can be written in a more compact way. In particular, the 1- and 2-moment schemes should be discussed primarily with regard to the aerosol-clooud and cloud radiation feedbacks.
  l.33: What is the outcome of Seifert et al. (2012)?

- p. 5, subsubsection 2.1.1. should read 2.2.
  This short para has the character of an introductory explanation, but none of the methods is explained. Please give some more informations, e.g. in form of a short table as overview of all satellite data sources (ISCCP, CALYPSO, CERES, MODIS ...?), including informations of spatial and temporal resolution for the averaging aspect. Do you use all mentioned satellite data?
  l. 19: I do not understand 'the assumptions for the satellite retrievals' in this context.
  COSP is important for the paper. Please explain what the simulator does, at least the input and output data, and what kind of errors may occur.
  What kind of spatial and temporal averaging is done? E.g., how many output times do you have for COSP- and for satellite data to determine a daily mean value? Can the averaging procedures produce a bias in the results, maybe the difference in daily averaged cloudiness in Figure 3?
  What is the physical interpretation of a 'daily mean cloud cover'? 12h cloud free plus 12 h full cloud cover results in 50% cloudiness?

- p.5 Section 3.1: The synoptic situation should be described for the situation on 17 February, the day of the later discussion and evaluation.

- p.6 l. 16. 'Northerly wind'? Fig. 2 shows mostly south-westerly winds over the Atlantic.

- p.6 l. 18-19. Please precise the sentence 'The cold continental air mass ...'.

- In Section 3, you use 3 version of the COSMO model and several satellite data sets for mutual intercomparison. Please make clear everywhere, which respective data sets are compared, and break the passages of different intercomparisons. Please use always the same expressions. E.g. p.7 l. 2. Which two model versions? What is the 'MODIS simulator' (also l. 30)?

- Section 3.2 (in particular) contains inconsistencies in wording and notation compared to the rest. E.g., optical depth δ vs. τ_c, COSP satellite simulator vs. MODIS satellite simulation? Please unify.

- p.7 l.11pp. The spatial structure of the fields are similar. On the linear scale, I would not agree to 'slightly larger' (l. 11) or 'slight underestimation' (l. 18). I am well aware that both data sets are subject to many sources of error, hence a similar field structure and a similar order of magnitude should be acceptable, but not whitewashed.
  p. 7 l. 14pp. The strongest differences do not occur near the Atlantic coast, but in the most western part of the domain. I have the impression that the model does not catch these clouds. Please clarify.

- p. 7 l. 19. Correct unit of cloud water path.

- p.7 l.20pp Fig.4 g-i. I do not follow your interpretation. The differences should be seen in relation to the signal. The least (relative) difference should be seen in the LWP, since the amount of condensate is primarily determined by other than microphysical processes and is to be seen in relation to the change in cloud ice. The sequel of e.g., red and blue

bands over the Biscaya may be a phase shift. A decrease of r_e by 10μm is of the order of the signal, not a 'slight reduction'.
Please precise. I agree with your conclusion of l. 27-28. However, I cannot see the superiority of COSMO-MUSCAT from the presente material.

- p.7 l. 25. Again: Not 'slight' and 'little'.
- p.8 l.3. 'cloud microphysics are modified'. If this is worth mentioning, then please be more precise.
- p.8 l.3. Please explain what you mean by 'better agreement'. Allgemeine FRAGE!!
- p.8 l.6. Fixed $CCN = 300\text{cm}^{-3}$ 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.
- p.8 l. 9pp. The aerosol NUMBER (not 'mass') concentration is given in Fig. 5c. 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?
- p.8 l.14pp Please revise the para.
  'the model exhibits more clear grid points.' What do you mean?
  '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 (2012) or similar ones. You would need a different tool.
  'the satellite may overestimate the retrievals.' What do you mean?
- p. 8, Section 3.3. l. 25. '(20 to 20 W m^{-2}'?? 'some regions': Please precise.
  Fig. 6. The colorbars are differently scaled for most of the subfigures. Sometimes this is straightforeward (e.g., a and f vs. b and f), sometimes, however, confusing (e.g. a vs. c, j vs. l). Please unify the scaling.
  Please also consider to plot the net UPWARD LWF to have the colors consistent to the SWFs, e.g., blue for weak differences. 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.
  l. 27/28. I cannot follow the statement 'the differences are neither systematic nor large'. Please interpret the radiative flux differences also in terms of the cloud properties.
- p. 9 l. 7pp
  Please check the conclusions with regard to the above points for more precise statements.
  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. Please clarify.
  Conclusion 2. Precise the 'modified model simulation'.
- p9 l21. Missing reference.
- If a paper is written by two authors, please cite as 'A and B (1999)'.
- p.11: Citation of IPCC is incomplete.
- Please check ALL figures w.r.t. wording within the plots and in the legends. E.g., in Fig. 2 'Temeperature', in Fig.3 'MUCAT', in Fig. 4 g-i 'CSOMO2M'.