

Interactive comment on “Implementation of aerosol-cloud interactions in the regional atmosphere-aerosol model COSMO-MUSCAT and evaluation using satellite data” by Dipu Sudhakar et al.

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Reviewer Comments:3

The authors present a numerical study on aerosol, clouds and radiation with mutual interactions and compare the results with satellite derived data. . This is an important topic, since all cloud-related processes pose a severe problem in weather forecast and climate modeling. The paper contributes to the ongoing research by examining the effect of mutual interactions of these processes and the improvement of atmospheric

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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 mid-latitude 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?

ANS: Even though SB scheme is for mixed phase clouds, heterogeneous ice nucleation is not included in the official version of COSMO. Also, Seifert et al., (2012) has demonstrated the importance of heterogeneous ice nucleation by adding Philip et al., (2008) parameterization (not available with official version). Hence, to reduce the uncertainty in aerosol cloud interaction, we have restricted our analysis to liquid phase clouds only. Moreover, our main objectives are to modify the fixed CCN in two-moment scheme with online coupled COSMO-MUSCAT model and incorporate COSP satellite simulator in it. In future we will be addressing aerosols as INP. Additionally, cloud ice optical properties on 17 February shows that (Figure 1), the study area is dominated by liquid phase clouds rather than ice clouds.

In satellite observation, cloud droplet number concentration can be derived from MODIS cloud optical depth τ_c and effective radius r_e (Quaas et al., 2009), which is

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given by,

$$N_d = \alpha \tau_c^{0.5} r_e^{-2.5} \quad (1)$$

where $\alpha = 1.37 \times 10^{-5} m^{-0.5}$. In the above equation the lower limit of τ_c and r_e are constrained to 5 and 2. This would result in low CDNC value across the domain (Figure 6c).

•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

$$\lambda = \left[\frac{\pi \rho_w N \Gamma(\mu + 4)}{6 \rho q_c \Gamma(\mu + 1)} \right]^{\frac{1}{3}} \quad (2)$$

with ρ is air density, ρ_w is bulk density of liquid water, q_c is 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

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

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

ANS: The cloud droplet size distribution is represented by gamma function, which is used with drop diameter.

$$\phi(D) = N_o D^\mu e^{-\lambda D} \quad (4)$$

where D is droplet diameter, λ is slope parameter and μ is spectral shape parameter.

Whereas the effective radii for droplets and cloud ice are obtained directly by dividing the third and second moments of the size distribution given by (Morrison et al., 2008)

$$r_e = \frac{\Gamma(\mu + 4)}{2\lambda\Gamma(\mu + 3)} \quad (5)$$

Equation 5 is corrected to

$$\delta = \frac{3\rho q_c dz}{2\rho_w r_e} \quad (6)$$

- *The nucleation rate (7) is connected with supersaturation S. Small but inevitable errors in vapor 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?

ANS: In COSMO model, nucleation rate is parameterized as a function of grid scale supersaturation and vertical velocity. It uses saturation adjustment to calculate S. Also, It is logical to use nucleation scheme explicitly depending on supersaturation in combination with saturation adjustment, which is done by applying an operator splitting method (Seifert et al., 2006). For SB parameterization the arbitrarily chosen small droplet mass is given by $1 \times 10^{-12} kg$, and corresponding size of freshly nucleated drop is $6.2 \mu m$. A detailed explanation is available in Seifert et al., 2006.

- *Problem of averaging.*

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 necessary. See Specific Points.

ANS: COSMO and COSMO-MUSCAT models are incorporated with COSP satellite simulator (Bodas-Salcedo et al., 2011). The variable such as, cloud water path, cloud optical depth, effective radius and sulfate aerosol number concentration are derived from COSP satellite simulator, which are vertically averaged.

To produce similar output to satellite data, COSP requires grid mean vertical profile of temperature, humidity, hydrometer mixing ratio, cloud optical thickness and emissivity, surface temperature and emissivity from the model. It produces the output comparable with satellite data in three steps. First it address the mismatch between model and satellite pixel, second vertical profiles of individual sub-columns are passed to each instruments and finally COSP statistic module gather the output from all instruments (Bodas-Salcedo et al., 2011). The above paragraph is included in the revised manuscript (Section 2.2).

- *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*

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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.

ANS: There was an error in Figure 4, which is corrected in the manuscript. In the corrected version, there is correlation between cloud optical depth, effective radius and liquid water path. The difference between COSMO-MUSCAT and COSMO-2M would more relevant than COSMO-2M.

- *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 much smoother distributions than COSMO-2M, in particular Fig. 5. Do you have an explanation?*

ANS: In the interactive model (COSMO-MUSCAT) the general guess has been replaced by C_{ccn} calculated using equation 8.

In the revised manuscript, Figure 6(a to c) has been modified. In order to compare the model simulation with satellite observations, we have used equation (9) to compute model N_d , as the COSP simulator can provide cloud optical depth and effective radius similar to MODIS satellite.

In COSMO-2M, we have used intermediate aerosol ($C_{ccn} = 3.0 \times 10^8 \text{ m}^{-3}$), when it comes to COSMO-MUSCAT interactive simulation, it uses gridded C_{ccn} information from MUSCAT.

From Figure 6d it is noticed that the maximum value of sulfate aerosol number concentration is in the order of $3.0 \times 10^8 \text{ m}^{-3}$, however the droplet activation is controlled by several other meteorological properties such as vertical velocity and micro-physical

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links.

- *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.*

ANS: This part has been revised.

- *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?

ANS: The wording and the comparisons are revised in the manuscript.

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-cloud and cloud radiation feedbacks.*

I.33: What is the outcome of Seifert et al. (2012)?

ANS: Introduction is revised. I.33: Seifert et al. (2012) reported that in COSMO model, radiative aerosol induced effects are more relevant than effect on precipitation. They have shown that, one-moment scheme has a strong positive bias in maximum 2m temperature. This difference between one-moment and two-moment scheme may partly explained by different cloud-radiation coupling.

• p. 5, subsection 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?

I. 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?

ANS: subsection 2.1.1. is revised to 2.2.

More information regarding the satellites are included in the manuscript. p5, I.9 has been rephrased: However, a meaningful evaluation of modeling with satellite observations is challenging because of the difference in the model variables and the satellite retrievals.

To produce similar output to satellite data, COSP requires grid mean vertical profile of temperature, humidity, hydrometer mixing ratio, cloud optical thickness and emissivity, surface temperature and emissivity from the model. It produces the output comparable

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with satellite data in three steps. First it addresses the mismatch between model and satellite pixel, second vertical profiles of individual sub-columns are passed to each instruments and finally COSP statistic module gathers the output from all instruments (Bodas-Salcedo et al., 2011). Since COSP is running online with COSMO model, it is able produce output similar to model simulation (in every hour).

An important aspect of COSP satellite simulator is positional errors due to mismatch between meteorological regimes in the observation and models, which is not considered.

- *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.*

ANS: Revised as suggested.

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

ANS: Revised.

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

ANS: Revised.

- *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)?*

ANS: P7, l.2: this line is removed from the manuscript. P7, l.30: We have also included cloud droplet number concentration N_d as a diagnostics of the model via COSP

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satellite simulator (MODIS simulator in COSP).

- *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.*

ANS: Revised

- *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.

ANS: P7, l.11, rephrased to: In satellite, it varies between 5 to 54 and in model between 5 to 45, with maximum values observed over similar geographical regions. However, the satellite derived cloud optical depth and liquid water path are overestimated while comparing with model (COSMO-MUSCAT) outputs.

P7, l.14: this sentence has been removed.

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

ANS: Revised as suggested.

- *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 re by $10\mu m$ 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.

ANS: Revised

- p.7 l. 25. Again: Not 'slight' and 'little'.*

ANS: Revised

- p.8 l.3. 'cloud microphysics are modified'. If this is worth mentioning, then please be more precise*

ANS: Revised

- p.8 l.3. Please explain what you mean by 'better agreement'. Allgemeine FRAGE!!*

ANS: This part is revised: Even though, the satellite derived N_d has poor spatial distribution, the N_d values are underestimated while comparing with COSMO2M and it is overestimated while comparing with COSMO-MUSCAT

- 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.

ANS: In COSMO-2M N_{ccn} is a function of S , whereas C_{ccn} kept constant. In the coupled model constant C_{ccn} in the two-moment scheme is replaced by gridded C_{ccn} proxy from MUSCAT, which is four dimensional.

- p.8 l. 9pp. The aerosol NUMBER (not 'mass') concentration is given in Fig. 5c. Could you please comment on the fact, that Sulfate is so much larger than N_d for COSMOMUSCAT? Is the result of Boucher and Lohmann (1995) transferrable to your model concept?*

ANS: The main objective of this paper is the replace the constant C_{ccn} , in

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COSMO2M(COSMO two-moment), with interactive aerosol from MUSCAT model. Since the MUSCAT model is available with aerosol mass concentration (in this case sulfate aerosols), we have used Boucher and Lohmann parameterization to calculate C_{cen} number concentration from mass concentration.

- 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?

ANS: This paragraph has been revised. clear grid means, cloud free region, which is also revised in the manuscript.

- p. 8, Section 3.3. l. 25. '(20 to 20 Wm^{-2} ' ?? 'some regions': Please precise Fig. 6. The colorbars are differently scaled for most of the subfigures. Sometimes this is straightforward (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.

ANS: Revised to Northern part of the domain. Color scale of Figure 6 & 7 are revised. Line 27/28 has been removed from the manuscript.

- p. 9 l. 7pp

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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'.

ANS: Revised as suggested.

- p9 l21. Missing reference.*

ANS: Revised as suggested.

- If a paper is written by two authors, please cite as 'A and B (1999)'*

ANS: Revised as suggested.

- p.11: Citation of IPCC is incomplete.*

ANS: Revised as suggested.

- Please check ALL figures w.r.t. wording within the plots and in the legends. E.g., in Fig. 2 'Temperature', in Fig.3 'MUCAT', in Fig. 4 g-i 'CSOMO2M'.*

ANS: Revised as suggested.

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Fig. 1. COSMO-MUSCAT cloud ice optical properties on 17 February 2007.

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/gmd-2016-186/gmd-2016-186-AC4-supplement.pdf>

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-186, 2016.

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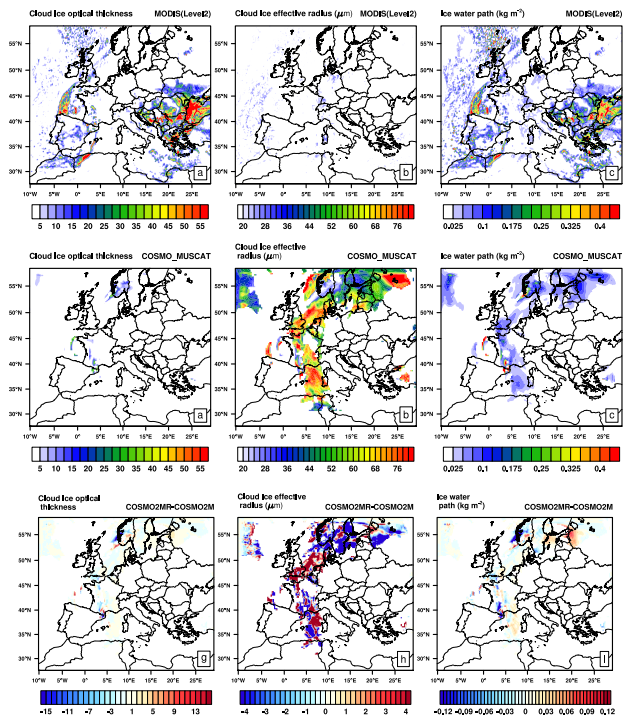


Fig. 2.

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