Implementation of aerosol-cloud interactions in the regional atmosphere-aerosol model COSMO-MUSCAT and evaluation using satellite data

In this study the spatial and temporal covariance of aerosol concentrations and cloud condensation nuclei (CCN) concentrations is introduced in the MultiScale Chemistry Aerosol Transport Model (MUSCAT). For this purpose an empirical relationship between the sulfate mass and CCN concentrations, which was introduced by Boucher and Lohmann (1995), is implemented.
Furthermore, the warm-phase cloud microphysics is coupled to the radiation scheme using a widely accepted empirical relationship between the cloud optical thickness, the effective radius and the vertically integrated liquid water content within an atmospheric layer.
Their new model version is evaluated against satellite retrievals of cloud microphysical and macrophysical properties, as well as radiative flux measurements at the top-of-the-atmosphere and at the surface using satellite simulators implemented in the model. The evaluation is performed for 1 day on Feb. 17 2007.

Given that uncertainties with respect to aerosol cloud interactions remain high, testing alternative approaches in treating these processes is of interest to the community. Therefore this paper could be published in GMD following major revisions of the manuscript.

Major Revisions:

- Whilst the paper gives a detailed introduction of the general benefits of regional models with online coupled aerosol-cloud, aerosol-radiation and aerosol-cloud-radiation interactions, such as WRF-CHEM (Grell et al. 2005), COSMO-ART (Bangert et al. 2011), COSMO-M7 (Zubler et al. 2011) and COSMO-ART/M7 (chapter 2 of: http://e-collection.library.ethz.ch/eserv/eth:48845/eth-48845-02.pdf), I would like to be given further motivation regarding the advantages of their particular approach and have it contrasted to existing approaches.

  My questions raised here, could guide such a discussion:

  1) Why did you use the Boucher and Lohmann's (1995) empirical relation, rather than implementing newer approaches using Koehler theory that have previously been applied in COSMO-ART/COSMO-M7 such as Abdul-Razzak & Gan (2000), or Nenes & Seinfeld (2003)? What is your justification for only considering sulfate and ignoring nitrate contributions to CCN?

  2) Whilst the relationship between cloud optical depth, effective radius and vertically integrated liquid water content is a commonly used diagnostic, it is wavelength independent. How did you deal with this issue when implementing this approach in a radiation scheme that computes the radiative transfer within 3 SW and 5 LW bands covering the entire wavelength spectrum?

- Furthermore, a short motivation should be included regarding the chosen case study. Are all the clouds shown boundary layer stratocumulus?

- The model evaluation is performed on a single day. The authors argue that the forecast skill decreases with increasing lead time. Could you obtain better agreement and therefore obtain a

longer evaluation period if you performed nudged simulations? Otherwise, a brief justification should be given that 1 day is a sufficiently long time period for your evaluation.

General Comments:

- This paper talks about the coupling of aerosol-cloud interactions and aerosol-cloud-radiation interactions. However, it should be stated clearly (already in the abstract) that these interactions are only included explicitly for warm-phase cloud processes.

- The paper often talks about changes to the COSMO model in general. However, the modifications are applied not to the COSMO model in general, but to the COSMO version of COSMO-MUSCAT. Whilst I agree with the authors that CCN variability and aerosol-cloud-radiation coupling is not provided in the officially released code versions, such developments have already been included in other COSMO versions (COSMO-ART, COSMO-M7, COSMO-ART/M7). They are therefore not new to the COSMO code itself.

- The paper is written in good scientific English. However, some sentences require re-writing (some of which are listed below). Furthermore, articles are missing in a few places.

Specific Comments:

P2L6: Last sentence needs rewriting.

P2L15: Sentence needs rewriting:“This approach, however,...”

P2L31-32: Delete last sentence of this paragraph. It is mis-leading as all of the mentioned aerosol-coupled regional models include a coupling of the cloud microphysics to the radiation.

P3L22:Sentence needs rewriting:”In this study, the COSMO model...”

Equation 5: Change 1.5/2 to ¾.

P4L23: Sentence needs rewriting: “IN the COSMO model, the aerosol...”

P5L1-11: This discussion needs some clarification: How is the Smax issue raised overcome? You raise the issue of ignoring updraft velocity in the Boucher and Lohmann formulation, however, is this then not introduced by dS/dz*w in Eq.7 (which admittedly is a very simplistic formulation of this relationship).

P5L20ff: Here you list all satellite products used for evaluation. Also include CERES here, as you use it later.

P5L27: I suggest to delete: In the upcoming… You have 1 ISCCP figure, 2 Modis and 1 CERES. That is not massively unbalanced.

P5L30: I suggest to delete last sentence of this paragraph.

P5L25-30: Please rephrase. The point regarding satellite biases could be formulated like: “One should keep in mind that the satellite products, just like models, are prone to biases. Comparisons of satellite retrievals with … have shown that…. Nonetheless, spatial correlation of the cloud structures are well represented.”
P6L2: Sentence needs rewriting: “As the forecast time...”

P6L10: Results are only shown for 17th. Not 15th – 25th. Please include description of the cloud types of this domain (altitude, phase, average thickness, surface precipitation). When did you start your simulation, cause the 2nd day would be the 16th if the simulation were started in the morning of the 15th? Also, why are is the 16th not included in the analysis?

P6L22-25: First 3 sentences should go into section 2.1.1 (methods).

P7L1-4: First 3 sentences should go into section 2.1.1 (methods).

P7L6: “The top panel shows….” This should be in Fig. Caption.

P7L19: Please check units.

P7L25-30: In this discussion quantitative statements should be included. For instance, the area mean changes + variability could be determined. It would help determine the signal from the noise in figures g to i.

P8L14: First sentence of paragraph need rewriting/clarification.

P8L19: Maybe rephrase title, because aerosol-cloud radiation interactions were already discussed in previous section (cloud optical thickness). Suggestion: “Impacts on radiative balance”
In general this discussion is not very precise. It would help the discussion if you relate observed changes in e.g. the SW fluxes to the decrease in cloud optical thickness…

P8L30: Please rephrase. It is an initial approach to modify COSMO-MUSCAT only.

P9L7: Finding 1 should be removed as it is not discussed in the paper.

P9L13: Paragraph missing after point 2.

P9L22: Last sentence needs rewriting and one reference is missing.

P10ff: A few references need changing. Some paper titles use capital letters.
IPCC reference is incomplete

Fig1: More detail should be given in the caption. I think this figure could be improved/clarified.
Why are the “emissions/land use” and M7 stand-alone and not connected?
The figure could be clearer regarding the structure of the code. Are RACM and MUSCAT separate modules? Will M7 not be embedded in MUSCAT?
Is the output really completely separate?

Fig2: I personally would not include all “H” and “L” markes. I would simply mark the center of the dominant low pressure and high pressure systems.

Fig3/Fig4: Looking at these figures optically, I would arrive at different cloud fractions for Fig3b and Fig4d. Areas where the total cloud fraction of the model is 100%, the diagnosed optical thickness is 0, or just very small?
Does the COSP simulator include subgrid-scale cloud water? If not, is that justified for cloud optical thickness?

Fig4: Maybe regional/domain means + a measure of variability could be given to highlight the results more quantitatively?

Fig5: What is going on with the MODIS cloud droplet number estimate? Why is the spatial pattern so different than in Fig4a-c. Are these clouds just too shallow to obtain a good number estimate?

Fig6/Fig7: I find the color scale depicting the fluxes very misleading. I would suggest using a pure blue color scale for LW fluxes and pure red for SW fluxes.