Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-186-RC1, 2016

Geoscientific Model Development Discussions

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.

Anonymous Referee #1

Received and published: 20 September 2016

The authors present two extensions to the regional atmosphere aerosol chemistry model COSMO-MUSCAT. As a first extension, the authors now use the two-moment scheme from COSMO (Seifert and Beheng). The cloud condensation nuclei (CCN) information needed by this scheme comes from MUSCAT (instead of constant, prescribed CCN profiles), following Boucher and Lohmann 1995 and taking sulfate mass as a CCN proxy. The second extension concerns the cloud optical depth in the radiation scheme, which now accounts for droplet-size, via the cloud effective radius, following Martin et al. 1994.

To evaluate the effect of the two new features on code performance, the authors consider a ten day test period (February 15 to 25, 2007). Of this period they focus, however, mostly on one single day (February 17). The simulations are run in forecast mode, thus are not nudged. Variables considered for the evaluation are different cloud related quantities (cover, optical thickness, effective radius, water path, droplet number concentration) and shortwave and longwave net radiation at the surface and the top of atmosphere (TOA). Comparison is done among different model versions (COSMO2M, COSMO2M.rad, COSMO-MUSCAT) and with satellite data (CERES, MODIS, ISCCP) and typically comes in the form of maps.

The authors find significantly improved performance of the new code version when comparing modeled and satellite based cloud effective radius and cloud droplet number concentration. Improvements are less pronounced for other quantities like cloud optical depth, cloud water content, or cloud fraction.

The topic - effect of more elaborated aerosol-cloud treatment in a regional climate model - is of interest. A number of corresponding models exists (e.g. WRF or also COSMO, Zubler et al. 2011), yet given the complexity of the topic a larger number of models whose results can then be compared are clearly desirable. The study thus is of interest.

The study fits the scope of GMD, but requires major revisions to meet GMD standards.

Major points:

1) Precision and / or clarity of statements could generally be improved. Two examples in the following, more can be found under 'minor points' below.

Evaluation is currently done essentially via comparing maps and arguing that things look similar or that there is a slight increase, minor change, a largest change etc. What are the numbers behind such statements? Only few are given. Regional averages, variability, correlations, scatter plots etc. would allow for better quantitative comparison of the different models among themselves and with the satellite data.

What is the resolution (space and time) of the satellite data you use for evaluation? Are model averages based on data from each model time step or based on output data? If output: hourly or less frequent?

2) You state that you have run the model for 10 days in forecast mode, February 15 to 25, 2007. But you use only one day (February 17) for model evaluation. Why? More importantly, is a one day forecast enough to evaluate the different models? The model may still, above all, be in an adapting stage after only one day (see e.g. Cossu Hocke, GMD, 2014). This may also apply to aerosols, a key element in your study, but with lifetimes on the order of days. While the forecast mode of your simulations makes comparisons more difficult as time evolves, you may still check whether, for example, CCN and cloud optical thickness evolve in concert over the ten days of your simulation. Please comment on possibilities and limitations of your one day forecast comparison. Or re-run your simulations in nudged mode an compare them over a longer time.

Minor points:

p.2, l.30-35: As you point out that other groups have already coupled COSMO with two-moment cloud microphysics to an aerosol module, including droplet-size aware radiation. Please explain to the reader how your work differs from these existing, closely related approaches.

Section 2.1 I find rather difficult to read as information on different codes (COSMO, COSMO-MUSCAT, and MUSCAT) as well as different code versions (current version, several other versions) is tightly interleaved. It is not always obvious whether what is stated applies to COSMO, COSMO-MUSCAT, the former or present code etc. For example, p.4, l.23ff: to which model do these equations now apply? COSMO2M? If so, how do the CCN numbers given here (1.26e9 and 1.0e8) go together with the 300 mentioned p.8,l.6? I further guess, but this is not really clear from the text, that equation 7 also applies to COSMO-MUSCAT, but with Cccn taken from MUSCAT and probably

k and Smax the same as in COSMO. Please clarify.

p.4, eq.3: What is Gamma?

p.5,l.3: 'aerosol mass concentration information from the MUSCAT model'. Where does MUSCAT get that information from? Are aerosol emissions prescribed? If so, where from? Or concentrations? What are these emissions / concentrations?

In section 3, evaluation against satellite products: is snow cover an issue?

p.6, l.6: 'with radiation coupled with microphysics': you mean the cloud effective radius following Martin et al. 1994 (your equations 3 to 5) is used? But for a fixed CCN number of 300?

p.6,l.27: Unless you have further evidence that for the concrete case ISCCP indeed underestimates cloud cover over the Atlantic, a fairer formulation may be that besides the model having a problem it could also be that the satellite has a problem.

p.7, l.2: 'In the two model versions...' You consider three model versions, COSMO2M, COSMO2M.rad, and COSMO-MUSCAT. While by and by one finds out what two versions you mean here, please state so explicitly.

p.7, l.3ff: Can you comment further on this screening for the liquid phase in MODIS and the models? How dominant is the liquid phase in either one?

p.7, l.9: 'In both cases it varies between 2 and 50...' Does the real quantity vary in that range or just your colormap? Also, figure 4a shows clearly much more red color than figure 4d. Reducing the comparison of the two panels to their range skips this aspect. In that sense, the patterns are not similar, as claimed in the text. Also, in wide parts where there is substantial cloud optical depth, the satellite based value is about twice as large as the model value. I would not call this a slight difference but a factor of two. This is another example where precision could be improved (major point 1).

p.7, l.14 ; High values for cloud effective radius are also seen over land. And, as above (p.6, l.27), it is not obvious that the flaw is with the satellite data.

p.7, l.21: What do you mean by 'largest impact'? Largest in what sense? Change in mean? Median? Percent or absolute? Per grid box?

p.7, l.25: 'slight increase': what is slight?

p.7, l.26: 'The cloud optical depth shows a variation in the range of +/-20...' Variation over what? Spatially? Within a model? Please clarify.

p.8, l.8: There is a wide region (red in figure 5d) with CCN of at least 300, i.e., the prescribed CCN value in COSMO2M. Yet the CDNC in COSMO-MUSCAT is much lower than in COSMO2M also in this region. Why?

p.8, l.14: 'While comparing with high resolution MODIS satellite products...': These have not yet been introduced, I think.

Section 3.3.: It would be interesting to elaborate a bit more on radiation. For example, the differences between COMSMO2M and COSMO2M.rad seem to be larger over sea than over land. True? Do the large differences (more downward SW and upward LW at the surface especially over sea) go hand in hand with reduced cloud optical thickness? Change in cloud effective radius? Cloud cover? Regarding the comparison with CERES: what area means of CERES and models? Given that you look at February (little radiation, snow cover, short days) and a cloud cover close to 100 percent over wide regions: how reliable are CERES surface fluxes?

p.8, l.30: 'This paper presents an initial approach to the modification of Seifert and Beheng (2006) two-moment scheme in the COSMO model.' This is not true. Other groups have done this before, e.g. Zubler et al. (2011) whom you cite.

p.9,l.2: Maybe state that this parameterization takes sulfate mass as a proxy, it is not a full grown aerosol module like SALSA, M7, etc.

p.9, l.8: 'In terms of the cloud distributions, this modification has only a minor effect.'

Given that you compare the second day of forecast simulations in winter, this is not truly surprising. To what degree is this finding just due to large scale weather conditions / your initialization?

p.9, l.9: What means daily averaged when you consider only one day in the first place?

p.9, l.10: 'The modified model simulations are in broad agreement with satellite observations.' I would argue that all your simulations are in 'broad agreement'. However, you see some improvements (as you state) in your modified version.

p.9, l.15: '...only minor changes in terms of the radiation budget were found.' Looking at figure 6, i-l, I would not call these changes minor. In wide regions they are on the order of a factor of two.

Figure 1: Why does it say "M7 to be implemented"? In the reference you cite, Wolke et al. 2012, it is stated that M7 is implemented. Please explain. And if M7 does indeed not form part of your model version, remove it from Figure 1.

Figure 2: What is the data source?

Figure 3: Does cloud cover from COSMO2M, and possibly COSMO2M.rad, look similar to cloud cover from COSMO-MUSCAT?

Figure 4: While the figure is useful, some more quantitative comparisons would also be useful, e.g. area means, variability, scatter plots... For example, what is the are mean cloud water path in 4i? And how does it compare wit the area mean of 4f? Figure 4i looks as if there is above all a change in spatial distribution of the cloud water path, not of total cloud water path (area mean). The same question may be asked for figures 4g and 4h.

Figure 4: How different would the figure be if you were to compare COSMO-MUSCAT with COSMO2M.rad? Put differently, are the differences mainly due to the variable CCN or also to the size-aware radiation?

Figure 5c: Point out that this is not a MODIS product but a derived quantity. Also, why is there hardly anywhere a CDNC greater than 10? After all, there is cloud cover all over the place and CDNC=10 or smaller is very low.

Figure 6: On the western boundary of the domain, there seems to be a boundary effect. Can you comment?

Figure 6 j-l: Given that the color scale is saturated in wide regions in these plots, why not take it larger? Possibly even -40 to +40, as in 6i?

Figure 7, a-d: Same figures as in figure 6 e-h. No need for duplication. You may consider replacing these panels with corresponding ones from COSMO-MUSCAT, so one has all three models and CERES shown.

Figure 7 6: What time is shown? February 17? 24h mean? Color scale in the SFC SW plots could be reduced to maybe 200 instead of 260 to fully exploit the range of the color table.

It maybe worthwhile to point out somewhere that you only changed the model but did not (yet) re-tune it, e.g. to get reasonable 2m temperature or precipitation. You show that the different codes give, for example, different cloud optical properties. But this does not imply an overall better model performance.

The language needs brushing, there are a number of sentences that do not work on the language level. I give only two examples.

p.2, l.6/7: "Although regional models do not describe part of the large scale feedbacks which are included in GCMs, regional modeling allowing for an optimal compromise.

p.8, l.8/9: "From figure 5c, the maximum aerosol mass concentration observed over south eastern Europe, on the contrary Nd shows less.

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