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Geoscientific Model Development Discussions

Interactive comment on "Towards an online-coupled chemistry-climate model: evaluation of COSMO-ART" *by* C. Knote et al.

Anonymous Referee #1

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This paper describes the performance of a regional weather prediction model, COSMO, when it has been coupled on-line with a version of the RADM photochemical mechanism and the MADE / SORGAM treatment for aerosols. Four cases studies, each for a different season, are evaluated with operational surface meteorological, trace gases, and particulate data. The performance of the model is over-stated several places in the manuscript. The title of this manuscript is apt, since there are clearly missing pieces to the model that the authors acknowledge, such as wet removal of gases and aqueous-phase chemistry. Aerosol indirect effects are not included as well. While, the title is apt, some of the conclusions regarding the ability of the model to be used as a climate model need to be changed since climate-relevant calculations have not been evaluated. In addition, the text does not describe whether COSMO-ART will be coupled with ocean / sea-ice / ecosystem models which are also important components of climate

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models. While the paper contains a useful evaluation useful for publication in GMD, there first needs to be significant changes to the text as described in the comments below.

Major Comments:

1) Many of the conclusions regarding the performance of the model are over-stated. Examples are included the specific comments below. Many of these statements are too broad and lack specificity. The authors need to rephrase the text so that better reflects the performance as depicted in the figures. On a positive note, the authors do provide sufficient material so that the reader can judge for themselves how well the model performs.

2) While the title of the paper is apt, the model has a long way to go before it has all the necessary components needed as part of a climate model. The model does include aerosol direct effects but there is insufficient description on how those processes are treated and an initial evaluation of those processes. The model is only compared to AOD, which is not the same as aerosol direct effect. Aerosol indirect effects apparently have been incorporated but not used in this study. Since this study is trying to demonstrate the utility of using COSMO-ART as a chemistry-climate model, some discussion of the findings of Bangert et al. (2011) is warranted in the present work. The omission of wet removal of trace gases and aqueous processes is problematic for some other variables, such as sulfate. It would suggest that the next paper written using COSMO-ART should essentially repeat the present work, but include the processes that are currently missing in the model.

Specific Comments:

Page 1811, line 8: I am not convinced that the present work is the first regional chemistry model application in Europe. Perhaps I am getting hung up on the word "comprehensive" which could imply that most other studies employ more limited amounts of data. But then where is the line between "limited" and "comprehensive"?

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Page 1811, lines 12 and 13: This sentence is self-contradictory. First it says the model reproduces the bulk properties well, but then states there is a clear tendency to underestimate PM mass (itself a bulk property). The first part of this sentence is also vague. It would be useful to be more specific and also be specific on the bias in the PM mass.

Page 1811, line 13: It is consistent that an underestimation of PM mass will lead to an underestimation in AOD. However, errors in aerosol composition and size distribution also affect AOD. Simple statements like this tend to mislead the reader into thinking that if predicted PM mass was correct then AOD would be correct for the right reasons.

Page 1811, line 21: Please be more specific as to what "simulated well" means. Adding some numbers would help.

Page 1815, line 24: Since the purpose this manuscript is to demonstrate the ability of the model to simulate climate processes, it would seem very important to present some description of how aerosols affect radiation. For example, how are the aerosol optical properties handled? Has this been done in a previous study and just not cited here? Why not show a plot depicting the impact of aerosols on radiation when compared to AERONET data over Europe? Presumably including aerosols would improve the radiation calculations?

Page 1816, lines 1-14: The text discusses that MOZART is used to provide the boundary conditions for trace gases, but not aerosols. It is not surprising that aerosol species between MOZART and the present model are different, but it would seem that some sort of mapping could have been made. This would not have been a difficult task.

Page 1821, section 3: I found some of the discussion relating to the figures a bit confusing. The problem is that some figures show meteorology, trace gases, and aerosols, but the text first describes the meteorology, trace gases, and aerosols in that order. So the reader needs to go back to previous figures – not a natural progression. This is just an organizational issue and I am not sure the best way to fix this. It would be useful at the beginning of this section to state that some of the figures contain multiple

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parameters that are not all described at once. That way the reader knows up-front that discussion of the material will come later. Similarly, I was expecting some elaboration on the model results in Section 3, but some of this material was covered later in Section 4. Again some introductory material to Section 3 to explain how the results are presented and discussed would be very useful.

Page 1821, lines 14 – 22: The authors explain that the free running model simulation performed as well as cases when data assimilation or reinitialization is used. Given the simulation periods (multiple weeks) and large mesoscale modeling domain size, this is hard to believe without some proof. I believe that with only lateral boundary condition forcing, the model can perform qualitatively well but there is likely room for improvement. Significant research and development has been conducted on data assimilation and operational design. The operational community has been arguing for years that data assimilation improves forecasts, so the statements here seem to contradict how operational models are run.

Page 1821, line 23: A reference seems in order for this statement.

Page 1821, section 3.1: The meteorological evaluation presented in this study employs only surface stations. Where comparisons with upper-air measurements, such as radiosondes, made? It would seem that boundary layer depth is another critical meteorological parameter needs to be evaluated since this will affect vertical mixing of trace gases and aerosols. I suggest adding some additional analyses using upper-air data. If not, please add text somewhere in the manuscript describing that the model has not been evaluated with data aloft how that would affect conclusions regarding trace gases and aerosols.

Page 1822, line 16: Are the results in Table 2 for the entire period (day and night periods). It would be useful to be specific.

Page 1823, section 3.2.1: Are there any black carbon measurements that can be used to evaluate the model? For direct radiative forcing relevant for climate applications, this

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is a critical component. Even a small amount can have a profound effect on the relative contribution of scattering and absorption in the atmosphere.

Page 1823, lines 25-29: No VOCs measurements are used to evaluate the model in the present study. VOCs are not normally measured routinely, but are often measured during field campaigns. Where there any VOC measurements available during the campaigns mentioned in Section 2.3 for some of the simulation periods? A lack of VOCs affects the interpretation of simulated, but also simulated SOA.

Page 1824, section 3.2.2: It is true that the model does qualitatively resemble the spatial distribution in the measurements; however, the number of stations is rather limited in many regions so it is difficult to draw conclusions in portions of the modeling domain, especially over water. The evidence that the ship emissions are too high is very slim. The simulated trace gases and aerosols in along the ship tracks may seem unreasonable, but there are no measurements to confirm that conclusion. One would also need to know how well the model simulates mixing within the marine boundary layer.

Page 1825, lines 24-29: I assume the results described here are not shown, and it should be stated somewhere they are not shown.

Page 1829, line 1-7: The authors should compae their results with diurnal cycles shown from other PM models, such as McKeen et al, (2007).

Page 1829, line 13: Some specificity in the “better performance” would be useful for the reader.

Page 1830, line 10: I assume the boundary conditions for dust is a large fraction here, but I assume this is discussed in Section 4?

Page 1831, line 12-13: What about simulated aerosol water and errors in simulated composition that will also affect simulated AOD. AOD is tricky because a model can simulated the values well but for the wrong reasons that are difficult to assess fully.

Page 1832, line 11: This sentence states that the simulated PM is reasonable, but then goes on to show that nitrate is too high and OA is too low. Therefore, the model produces the "right answer for the wrong reasons". This fact needs to be included somehow in this sentence.

Page 1833: line 4: Need another word for "bursts" – not sure this terminology is correct.

Page 1836, line 20-21: This statement is overly optimistic. There are some stations with relatively large differences. Even subtle differences in the size distribution will have an impact on direct radiative forcing though radiation. Indirect radiative forcing via CCN activation also depends on size distribution. Since this paper is describing a model that ultimately will be used to simulate climate effects, this is pretty important. Simulating size distribution is perhaps more difficult for models than simulating total particulate mass, which is a critical parameter for air quality models. Same comment for line 17 on the next page.

Page 1839, line 11: Here it is stated that OA is underestimated by a factor of 2, but the difference between observed and simulated OA in the figures looks less than that at first glance.

Page 1839, line 27: Have the authors looked at satellite-derived fire emission products? They could establish whether a significant number of fires occurred within the modeling domain for their simulation periods. This could establish how important this source could have been for the simulated trace gases and aerosols.

Page 1840, line 4: The authors discuss work on emissions that will be included in the future. What about work to better represent the mix of POA dn SOA that is also described in this paragraph?

Page 1842: line 6: Here it is stated that the nucleation rates are too high, but earlier on page 1835 the authors show aerosol number too high and attribute that to primary emissions of small particles and discuss that the Kerminen and Wexler (1994) param-

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eterization produces a low fraction of new particles. Most nucleation parameterizations produce too few particles, and this parameterization would be consistent with previous studies. So, I am a bit confused by the statement on page 1842 when compared to earlier discussion.

Page 1843, line 17: Not much meteorological evaluation is presented in this study, so to say it was “very well simulated” is a bit of a stretch. It was well simulated in terms of the surface meteorological parameters examined. An evaluation of the upper-air conditions, boundary layer structure, cloud distribution, precipitation amounts, etc. is needed to make such a broad statement.

Page 1843, section 4: At the end of this section, a discussion on a lack of chemical data aloft is needed. Surface quantities, used exclusively in this study, depend on mixing from above. Why not pick a simulation period that includes field campaign data aloft?

Figure 1843, section 4: The number of stations is rather limited. Are these stations the only ones that contain data that is readily available to scientists? From a North American perspective, I would have expected more data to be collected routinely. However, it may not be readily available from some countries. Please elaborate.

Page 1844, line 21: The authors state that the model is suitable to simulate climate interactions. However, they have only shown predictions of AOD which is only one component contributing to of aerosol radiatve forcing calculations. Other factors include single scattering albedo and angstrom component, but neither is evaluated in this study. Aerosol indirect effects are likely to be larger than direct effects, but the authors have not yet included those calculations in the model. Therefore, I do not think it is appropriate to state that the model is suitable for climate applications until the direct effect is evaluated in more detail with additional aerosol optical properties and when the aerosol indirect effects have been included and evaluated. Perhaps the authors mean to state that the basic model framework is now suitable to begin implementing and evaluating feedback processes?

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Table 1: Units of the quantities needs to be included for all variables. Only temperature units are included.

Table 2: I assume that the N50, N150, and N250 are measurements made for a size range. So does N50 mean N50-100, etc? Please clarify.

Figure 2: The labels indicating the number of stations used are unreadable and covered over by other material. The authors should change the scale to make all of the text legible. The model and measurement label below the wind direction plot is meaningless, since only the biases are shown for that panel.

Figure 7: It might be useful to include a non-linear scale to better depict the differences between the simulated MODIS values when AOD is low.

Figure 8: I appreciate the authors showing many time series plots since they are more enlightening than just statistics. But for this figure it might be better to reduce the number of panels. I suggest focusing on a few (4-6) stations and then show the simulated and observed values for all of the cases, rather than for one case in the main part of the text. The rest of the material can be moved to the supplemental material. This just means exchanging portions of Figure 8 with the equivalent plots in the supplemental material.

Table 6, supplemental material: I suggest moving this table to the main part of the paper.

Interactive comment on Geosci. Model Dev. Discuss., 4, 1809, 2011.