Geosci. Model Dev. Discuss., 8, C66–C71, 2015
www.geosci-model-dev-discuss.net/8/C66/2015/

Geoscientific Model Development Discussions
Open Access

Interactive comment on "A new chemistry option in WRF/Chem v. 3.4 for the simulation of direct and indirect aerosol effects using VBS: evaluation against IMPACT-EUCAARI data" *by* P. Tuccella et al.

Anonymous Referee #1

Received and published: 23 February 2015

This paper describes how an existing aerosol scheme in the WRF-Chem model has been coupled to radiation and clouds to include feedback processes on meteorology. The authors then simulate the atmospheric conditions over a month long period and compare the predicted meteorology, trace gases, aerosols, with measurements. Measurements are from one surface site and a series of aircraft flights. They also examine aerosol-radiation-cloud effects by comparing predicted AOD, COD, and CWP with satellite measurements. Finally a sensitivity simulation is done that removes secondary organic aerosol (SOA) to examine its impact on aerosol-radiation-cloud effects.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

The organization and presentation is clear, but I think it is missing some additional description and context, included in my comments below, before the manuscript is suitable for publication. Given that the journal is GMD, the description of the modeling components/approach is presently a little weak.

Major Comments

1) NOSOA simulation: Page 814, line 11: Given relatively small size of domain 3, I would assume a large amount of SOA from the outer domains could be transported into and across domain 3 during a 30-h simulation period. Thus, the NOSOA simulation does not necessarily have no SOA, but a reduced amount of SOA. It would have been better to turn off SOA entirely and suggest that this simulation be repeated. In fact, I would prefer if the entire 2 week period was repeated to assess the impact of SOA on AOD, COD, and CWP. Examining only one day, it is difficult to really conclude that including SOA is clearly improved when evaluating predicted AOD, COD, and CWP.

2) Context and "connecting the dots": In general there is a lack of discussion on the errors in aerosol concentrations, particle number, and CCN and whether the results are consistent. For example, particle number is somewhat too high, but that should be related to mass that is too high. The authors show that OM is too low but SO4 is too high – but is the total mass too high as well? Is that consistent with the errors in simulated aerosol number and consequently CCN? Also particle number and CCN will be influenced by aerosol components (BC, other inorganics) that are not sampled by existing measurements (AMS instrument). In addition, the discussion of the simulated regional variations of AOT, COT, and CWP should be put in the context of the known biases in the aerosols – which are only evaluated at one point in the domain. It is hard to know how errors in aerosols elsewhere in the domain contribute to the differences between observed and simulated AOT, COT, and CWP.

3) Insights from aircraft sampling: An additional figure is needed that show the aircraft flight tracks. It is hard to judge the spatial variability of simulated aerosols. I do not know

GMDD
8, C66–C71, 2015

if the aircraft flew in one particular region or throughout domain three. Readers should not have to go to other papers for this information. I certainly appreciate the summary statistics on all the aircraft flights, but it would have also been useful to include one or two "interesting" flights that shows relatively large SOA predicted by the model that corresponds to observed organic matter and relatively higher CCN.

Specific Comments:

Page 793, line 3: change "scheme" to "schemes"

Page 793, line 3: RACM/MADE/VBS is presented as a new chemistry option, when in fact the gas-phase coupling with aerosols and VBS for SOA was existing in WRF-Chem. As stated elsewhere the new part is the coupling with aerosol direct and indirect effects. I suggest a new name that better conveys the differences with the older scheme – perhaps "RACM/MADE/VBS/AE" where AE stands for aerosol effect.

Page 793, line 10: Change "correlation" to "correlation coefficient"

Page 793, line 14: Change "observed mass" to "observed concentrations". Mass and concentration is not the same thing.

Page 793, lines 14-17: There are many uncertainties in the treatment of VBS that likely contribute to errors as well that should be mentioned, here and where VBS is introduced in the model description.

Page 795, line 3: In addition to the Grell reference, include Fast et al. (2006) and Chapman et al. (2009) here which presents the first coupling of aerosols to radiation and clouds alluded to in the previous sentence. The Grell paper does not have such coupling.

Page 795, line 23: Many readers unfamiliar with WRF-Chem will not know what "traditional" means. Please be more specific.

Page 796, beginning of section 2.1. It would be useful to indicate that WRF-Chem is a

GMDD
8, C66–C71, 2015

community model, and such, has many options for trace gas chemistry and aerosols. The authors are using one particular scheme for each.

Page 796, line 23. I know the VBS approach is described in Ahmadov et al. (2012); however, I think some description is needed in this paper. Either here or in the supplemental material that describes its overall approach. There are many treatments of SOA now available, including various VBS approaches that are by no means the same. The main purpose of the paper is to examine the impact of SOA on aerosol radiative forcing parameters, so some description of the SOA treatment is warrented.

Page 798, lines 4-6: This sentence is awkward. Change to "The Lin and Morrison microphysics schemes in WRF/Chem version 3.4 include the prognostic treatment of the cloud droplet number."

Section 2.1: the authors should describe some of the cloud-aerosol interactions that are missing in the model that may or may not have an effect on their simulations. This would include aqueous organic chemistry, ice-borne aerosols, etc.

Page 798, line 26: Please state why this period is chosen. There is no motivation for this period yet. I presume they want to use the measurement set described in the next section, but that has not been described yet. Also why is this period and dataset particularly useful to evaluate the model for their purpose of investigating the impact of SOA on radiative forcing parameters.

Page 799, line 5: This sentence could be improved. Perhaps "A series of 30-h simulations were performed on each day starting at 00 UTC, with the first 6 h discarded as model spin up."

Page 799, line 10: Here the authors mention a 13-day spin up period. So this contradicts the first sentence of the paragraph. It sounds like the simulation period is for the whole month, but the authors will be focusing on days after May 14 after the chemistry spin up period. The paragraph needs to be rephrased to clarify this point better.

GMDD

8, C66–C71, 2015

Section 3.1: This results section talks about surface aerosol measurements used, but there is no discussion here on the measurements. The authors should include what type of instrumentation was used.

Section 3.3: Please state the horizontal spacing of the pixels used in the analysis.

Comment: Could have used data assimilation to improve meteorology on domains 1 and 2 to help improve meteorology on domain 3 and thus confidence in aerosol-radiation-cloud interactions.

Section 4.2: The authors present gas concentrations in terms if ug/m3. It would be better to convert these to ppm or ppb to be more consistent with other comparisons in the literature.

Page 805, lines 6-11: I am assuming the RACM mechanism may be too simplistic to represent HONO well. In addition, errors in simulated HONO may not translate significantly into errors in simulated aerosols that are the focus of the paper. What would be more important here is some evaluation of VOCs that likely contribute to SOA formation. The authors should provide some context on this subject in this section.

Page 805, line 27: Here beings the discussion evaluating the surface aerosol predictions. However, it is not clear how the measurements are compared to the model and additional text is needed to clarify their methods. Are they measurements from an AMS instrument? If so the cut off is normally 1 um, but often it could be as low as 700 nm under certain conditions. The model uses a modal representation of aerosols, so how are the simulated concentrations compared to the measurements. If the authors are using the entire Aiken and accumulation mode, they may artificially introduce a bias in the comparison.

Page 806, line 1: Please be more specific than just “consistent”. Consistent in what way? I presume statistics is meant here.

Page 812, section 4.5: It would be very useful if the comparisons in this section were

GMDD

8, C66–C71, 2015

extended to include droplet effective radius from MODIS. This is a parameter important for radiative forcing, since aerosols will lead to smaller droplets and higher albedo that would alter the radiation budget. The authors would have to screen periods with high clouds (cirrus) so the results focus on warm clouds with liquid cloud droplets.

Page 814, line 20: I assume “ticker” should be “thicker”.

Figure 7 and 8: Need to define what blue and red denote, and include units for y-axis.

Interactive comment on Geosci. Model Dev. Discuss., 8, 791, 2015.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper