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Interactive comment on "Improving dust simulations in WRF-Chem model v4.1.3 coupled with GOCART aerosol module" *by* Alexander Ukhov et al.

Anonymous Referee #1

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This manuscript examined some inconsistencies with the use of the Goddard Chemistry Aerosol Radiation and Transport (GOCART) aerosol module in the fully coupled WRF-Chem model. The authors identified that 1) the diagnostic output of PM2.5 surface concentration was underestimated by 7% and PM10 surface concentration was overestimated by 5% due to the incorrect representation of the dust and sea salt coefficients; 2) the contribution of sub-micron (0.1 – 0.46 ïĄ■m) dust particles was underestimated in the calculation of optical properties with the consequence of underestimated AOD by 25-30% because the finer dust particles were not accounted for in the Mie calculations; and 3) an inconsistency in dealing with gravitational settling that led to the overestimation of the dust column loadings by 4-6%, PM10 surface concentrations by

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2-4%, and the rate of gravitational settling by 5-10%. The authors further examined the impacts of boundary conditions on PM10 surface concentrations using the MERRA-2 reanalysis. These are all useful aspects of the WRF-Chem model and certainly help the improvement of the WRF-Chem simulations. However, this manuscript lacks in-depth technical and scientific analyses and is rather poor scientifically. All the analyses were based on one dust case (1-12 August, 2016) over the Middle East which calls into question the applicability and effectiveness of the code rectifications in other regions and in other dust cases under different meteorological and land surface conditions. Besides, I have several major concerns as listed below:

1) The Introduction section was poorly written. It is clear that the authors have read and cited a lot of references on the subject of dust sources, dust impacts and dust modeling but the Introduction section was written in such a way that it was hard to gain a clear idea of why the inconsistencies occur and what the latest developments are in dealing with them and how the authors would like to address them. The Introduction section needs to be improved substantially.

2) How were the “correct” dust and sea salt coefficients, d_25, s_25, d_10, in Equation 2, determined? The authors mentioned that they used the natural logarithm of particle radii but what was the rationale behind that? Was that determined from empirical relationships or lab experiments or field measurements or just trial and error? Are there any references for that?

3) I commend the authors for identifying the underestimation of the AOD by the neglect of the sub-micron dust particles and their effects but I am concerned that the authors did not provide any logic behind the modifications of the corresponding numbers from MOZAIC bins (1, 2, 3, 4, 5, 6, 7, 8) to GOCART dust bins (DUST1, 2, 3, 4, 5). Please provide scientific evidence or references to support this work. Otherwise, what the authors have done is not convincing at all. 4) The authors changed the calculation of bin concentrations of dust and sea salt from using the functions of particle radius to using the functions of natural logarithm of radius. Again, what was the rationale behind

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this?

5) I did not understand how the inconsistency in the gravitational setting of dust and sea salt led to the increase of their total mass in the atmosphere. The authors mentioned that “Instead of transport the dust and sea salt mass between the layers, the default finite-difference scheme transport their mass mixing ratios not taking into account the dry air density variation with the height”. Does this mean that dust and sea salt mass can’t be transported across the layers? If there are vertical motions or turbulence dust and sea salt can certainly move up and down. Then where did this overestimation come from?

6) The English of this manuscript needs to be improved.

My minor concerns are listed below:

1) On Line 21-22 of Page 8 the authors stated that “The model erroneously pushed more dust into the atmosphere to fit the observed AOD”. Was this in reference to the model default values or the model runs that assimilate the observed AOD. I don’t quite understand this. 2) I did not understand this statement on Line 16-18 of Page 10: “In both runs, the magnitude and temporal evolution of the AOD time-series are well correlated with the observed AERONET AOD at all sites only in the absence of dust events or when the AERONET AOD is below 1”. I am wondering what the authors wanted to convey here with this statement. 3) The ALL_OK run was just one of the model realizations. It is not appropriate to treat it as truth and designate the differences from it as biases.

4) How useful is the Merra2BC utility in general sense? Since the MERRA-2 reanalysis is essentially a global atmospheric reanalysis generated by an atmospheric circulation model with the incorporation of trace gas constituents and aerosols. It is not a fully coupled atmosphere-chemistry model and I would think that the so created gaseous and aerosol species data may not be as useful as those from fully coupled chemistry models such as MOZART-4.

5) The authors mentioned that Figure 4 shows the averaged AOD time-series and scatter plots obtained from the ALL_OK and NON_LOG_046 runs. Instead, I found the spatial patterns of AOD and their differences.

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