

Reply to the comments of 1st Referee:

Comment1: 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 PM_{2.5} 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 μ 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 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 indepth 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:

Comment 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.

Response 1: We thank the Referee for the valuable comments and present here a response to his/her major concerns. While we agree with the reviewer on the quality of the presentation and will improve the introduction section and overall clarify the text, we disagree on the comments related to the technical and scientific merits of our study.

The paper discusses the inconsistencies we found in the WRF-Chem v3.2 code released on April 2, 2010. We cooperated with the model developers to test and implement those corrections in the newly released WRF-Chem v4.1.3. The main objective of the presented paper is to quantify the effect of those inconsistencies on model performance.

E.g., our findings explain why WRF-Chem overestimated PM₁₀ surface concentrations, and why realistic values of AOD were associated with too strong dust emissions and elevated dust surface concentrations. These discrepancies have been discussed in some previous studies: Kumar et al. (2014); Eltahan et al. (2018); Flaounas et al. (2017).

Our numerical experiments are specifically chosen to demonstrate the effect of those corrections quantitatively. It is not so important what region of the world is selected for our experiments except it should be a dusty region. We use a WRF-Chem experimental setup configured over the Middle East, one of the most significant dust source regions. We did not look at the specific dust event case. Different meteorological and surface conditions will not affect the results since we estimate relative biases, not absolute values.

Comment 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?

Response 2: Calculating PM_{2.5} and PM₁₀ concentrations require the integration of aerosol volume size distribution (approximated in GOCART by five bins for dust and by three bins for sea salt) over the radius r from 0 to 2.5 μm and from 0 to 10 μm , respectively. Integration could be done assuming that the size distribution is a function of r or $\ln(r)$. Coefficients d_{25} , s_{25} , d_{10} in eq.2 are obtained assuming that aerosol size distribution is a function of $\ln(r)$. This method is acknowledged in CAMS reanalysis (<https://confluence.ecmwf.int/pages/viewpage.action?pageId=153393481>) and is justified by the fact that an aerosol size distribution is a smoother function of $\ln(r)$ than r , and therefore numerical integration is more accurate. The default d_{25} , s_{25} , d_{10} in eq.2 were calculated incorrectly.

Comment 3&4: 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 this?

Response 3 and 4: The mapping of GOCART five dust bins approximation of the aerosol size distribution to the MOZAIC eight bins also requires the integration of aerosol size distribution over the radius r or $\ln(r)$. Consistently with our calculation of PM_{2.5} and PM₁₀, we assume that the aerosol size distribution is a function of $\ln(r)$. Table 4 compares bin's partitions calculated assuming aerosol size distribution is a function of $\ln(r)$ or r .

Comment 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?

Response 5: In the previous versions of WRF-Chem coupled with GOCART the mass flux of gravitationally deposited material (dust and sea salt) was miscalculated. The outgoing mass flux from the bottom of each grid cell was overestimated. As a result, the integral mass balance was violated. We presented in the paper a conservative finite difference scheme that correctly approximates the gravitational deposition. We conducted a numerical experiment showing that the old scheme violates the mass balance, and the new one does not.