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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Model development of dust emission and heterogeneous chemistry within the Community Multiscale Air Quality modeling system and its application over East Asia" *by* X. Dong et al.

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We thank the referee for a very thoughtful review and detailed suggestions to our manuscript. Incorporation of these suggestions helps to improve the quality of our manuscript significantly. Following are the responses to the reviewer's comments, and related revises have been incorporated into the updated manuscript.

(1). Comment: An important issue is that the Tong et al. paper detailing the dust scheme used in this study is currently not published. Perhaps it has been accepted

since submission, but without that manuscript in the literature first it is difficult to see how this work can be published. There are many details left out of the description of the dust scheme, probably because they are explained in Tong et al., which is all the more reason to wait to publish once that has been peer-reviewed. Response: The dust scheme used in the standard version of CMAQ and this study is the same, and the developments implemented in this study are all based on the standard version of CMAQ with FENGSHA dust scheme. FENGSHA is developed by Dr. Daniel Tong with his co-authors, and we are also expecting to see the publication of Tong et al., 2015. We agree that with the publication of Tong et al. may provide a more comprehensive description of the dust emission scheme. But the fundamental functions and algorithms are described clearly in our manuscript, and we also include more details about how the parameterization of friction velocity is improved in this study in the revised manuscript. So our updated manuscript can also provide a clear description of the scheme to make it easy to be understood. The major objective of our manuscript is to demonstrate how the improvements employed in this study can result in better performance of the original scheme in CMAQ, and also point out the potential remaining issues within it. Thus a fully explicit description of the original scheme is beyond our focus. In fact, a few publications (Appel et al., 2013; Fu et al., 2014) have already investigated the CMAQ model performance with FENGSHA scheme since the first release (CMAQv5.0 released in 2012), while our manuscript is the first one that provide detailed description of the functions and parameters. With the newly added information, the updated manuscript should provide a clear introduction of the scheme now.

(2). Comment: The work of Wang et al. (2012) implements two established dust schemes into the CMAQ model, including heterogeneous chemistry, and evaluates them over Asia. This paper is mentioned in the manuscript, but it would be valuable to provide a comparison because the baseline version of CMAQ performs so poorly when simulating Asian dust, and therefore most changes would improve upon the baseline. For example, what benefit does the FENGSHAA dust scheme bring that justifies including it, rather than one of the more established dust schemes? I think the hetero-

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geneous source mineralogy profiles are an improvement over the Wang et al. (2012) study, although the improvement in agreement with observations (shown in Figure 5 and Table 5) appears limited. Response: Our study focused on how to improve the FENGSHA scheme within the standard CMAQ model. We point out that the poor performance of baseline CMAQ was mainly due to the overestimation of threshold friction velocity, which is attributed to the double-count of soil moisture effect in the original parameterization. The dust scheme used in our study is different from the two used by Wang et al., (2012) in terms of the microphysical parameterization. Wang implemented two schemes, the Westphal scheme and the Zender scheme, both of which are well-established. We did not include detailed comparison between our work and Wang's work mainly for two reasons: First, the FENGSHA is developed by Tong et al. and adopted by U.S. EPA for the standard CMAQ version, our study is focused on the CMAQ model application and improvement. While the developments implemented by Wang et al. (2012) was not public available, we are unable to examine their model performance for our case study; Second, in our study, dust is speciated into 19 subspecies to make it compatible with the aerosol-related schemes within CMAQ modeling system, while Wang's work treated dust as a unique independent species. Thus comparing the work by Wang et al. (2012) is inapplicable and beyond the focus of our study. The reviewer raised a very interesting question about the benefit of FENGSHA as compared to other schemes. The most important specialty that distinguish FENGSHA from all other scheme is that it speciates dust emission into sub-aerosol species, while conventional schemes mostly treat dust as a unique species in the model. But without carefully designed schemes comparison study, it remains an open-ended question that how FENGSHA can benefit the model with better performance.

(2). Comment: Do the authors think that the assumption that the Gillette et al. field data is for zero soil moisture conditions will be a factor for other dust schemes? If so this perhaps needs pointing out. Response: The Gillette et al. data affects the FENGSHA scheme because the parameterization rely on initial threshold friction velocity which are influenced by the soil moisture. The double-count of soil moisture effect occured

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when Tong et al developed the FENGSHA scheme, thus the discrepancy is not related with the Gillette data. Although other schemes (such as the Westphal scheme) may also consider the impact of soil moisture, their parameterization methods are different and use independent field campaign data. So for our knowledge we donot think there is any well-established dust emission scheme may has the same issue as discussed in our study.

(3). Comment: Can the authors test the emissions and the agreement with observations using the GLDAS soil moisture data set shown in Figure 10? The authors state that there is no observational data between 2006-2010 over East Asia, but testing the GLDAS soil moisture seems like a relatively trivial test that would provide an answer to the open question of whether soil moisture explains the underestimate of emissions in the Taklamakan. Response: We did not perform simulation with the GLDAS reanalysis data in this study since it doesn't provide all the required variables for WRF simulation. GLDAS v2 data has 22 variables (http://hydro1.sci.gsfc.nasa.gov/data/s4pa/GLDAS/README.GLDAS2.pdf) while the FNL ds083.2 data contains 116 variables (http://rda.ucar.edu/datasets/ds083.0/). We do plan to use the GLDAS soil moisture and soil temperature data only with other inputs from FNL to perform test simulation, but this is not a trivial work if considering the inconsistency between GLDAS as an assimilation data and FNL as a reanalysis data. In addition, even GLDAS may drive model prediction (such as results shown in Haustein et al., 2013) closer towards observations, but it may not necessarily demonstrate the better quality of GLDAS, so we are looking for soil moisture observation data to address this issue.

(4). Comment: pg 35593 ln 5 - the talk of double-counting feels too specific for the abstract, unless the soil moisture issue is a general issue that the authors wish to bring to the attention of the community. ln24 - "revised" repeated. Response: We emphasize the double-counting in the "Abstract" section because this is the major factor that responsible for the underestimation of dust emission by the standard CMAQ model.

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As the previous model applications found out the underestimation issue but couldn't address the reason, it is necessary to clearly state it to help the research community realize and correct it in the modeling system. The repeated "revised" has been removed in the updated manuscript.

(5). Comment: pg 35595 ln8 - "deposition". Response: Thanks to the reviewer's comment, this sentence has been changed to "deposition and surface concentration" in the updated manuscript.

(6). Comment: pg 35599 ln 16 'soil moisture' ln 28 - diameter or radius? Response: When describing size bins of aerosols model usually refers aerodynamic diameter, so we did not specifically mention it in the manuscript.

(7). Comment: pg35607 ln2 - I can't see the two cities on Figure 1 Response: That was a typo, the locations of the two cities are added in Figure 2 in the updated manuscript, and description in the text is also revised.

(8). Comment: pg 35615 ln 1 - is the slightly increasing trend significant of not? Response: Dust emission is predominantly determined by the wind field which may vary from year to year as a result of the regional climate change. So with the only 5 springs in this study we are hesitate to reach solid conclusion regarding if the trend is significant or not, because longer term investigation is necessary to answer this question. But based on both the observations and model simulations, the year 2010 showed higher dust emission than 2006 due to the severe drought over East Asia. In the manuscript we mentioned the slightly increasing trend of dust to demonstrate that modeling bias of PM10 is affected by the simulation of dust emission.

(9). Comment: pg 35616 ln 21 - 'aerosols' ln 23 - how close are Duolun aand Yulin to source? The sedimentation of coarse particles will alter PM2.5/TSP with distance, do you take this into account? Response: The locations of Duolun and Yulin are marked in Figure 2 in the updated manuscript. Duolun is at the east edge, and Yulin is close to the southeast edge of Gobi Desert, so both of them are quite close to the source area.

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As coarse particles deposit faster than fine particles, the ratio of PM2.5/TSP at these two cities should be higher than that in the middle of the Gobi Desert. But unfortunately that's the only two sampling sites we can find that provide observation data, so we did not make solid conclusion about the PM2.5/PM10 ratio in the CMAQ model. And this is also the reason that we recommend the research community to devote more efforts to look into this uncertainty in the modeling system.

(10). Comment: pg 35617 ln7 - rephrase this, implementing development doesn't really describe anything. Response: We also realize that the original manuscript need to be rephrased to get better quality. In the updated manuscript, the sentence has been revised as: "The dust module in CMAQ has been further developed in this study."

(10). Comment: Figure and table captions could do with improving and cross-referencing. e.g. Table 5 does not mention what the model is evaluated against and should reference Table 4 for this, at least. Figure 3 doesn't include the time frame for the data comparison. Figure 5 should either include the statistics or the caption reference Table 5. Response: This is a very helpful comment and we appreciate it. In the updated manuscript, caption of Figure 3 is revised to indicate the time as "five-year average"; caption of Figure 5 is revised to reference Table 4 and Table 5.

(11). Comment: References Wang, K., Zhang, Y., Nenes, A. and Fountoukis, C.: Implementation of dust emission and chemistry into the Community Multiscale Air Quality modeling system and initial application to an Asian dust storm episode, Atmos. Chem. Phys., 12(21), 10209–10237, doi:10.5194/acp-12-10209-2012, 2012. Response: The DOI number of the reference has been added in the updated manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 35591, 2015.