Anonymous referee # 1

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**: The main issue with the manuscript is the lack of a clear and detailed explanation of the methodology. The authors referred to the work of Tong et al. (2015) in various places in the Methodology section. This is not a published manuscript (under review in journal?) which makes it impossible for a reader to understand the details of the dust emission algorithm that the authors have used.

Response: The methodology of our study includes three major parts as described in the manuscript: first, section 2.1 introduces the development of the values of initial threshold friction velocity constant based on reanalysis of field measurement data; second, section 2.2 introduces the development of the source-dependent speciation profiles for Gobi and Taklamkan Desert; third, section 2.3 introduces the implementation of the dust heterogeneous chemistry. The reviewer's major concern is about the section 2.1. We agree with the reviewer that more details are necessary to clearly describe how the reanalysis of field data is performed, so the manuscript is revised according to this comment. Tong et al. (2015) is still under preparation to give an explicit description about the dust emission model and field measurement data. So in this manuscript, the fundamental equations and algorithms are briefly introduced. In the revised manuscript more necessary details are added to help readers to clearly understand the dust emission model (see the last paragraph of section 2.1).

(2). **Comment**: It is unclear how the double-counting of the soil moisture was addressed in this study. What was the procedure for revising the original threshold velocities? It is obvious from Fig. 1(c) that the process of modifying the threshold velocities are different for different soil and land use types. What value of the soil moisture was used for each soil and land use type to modify the threshold velocities? The whole procedure needs to be presented and scientifically justified.

Response: In the original field measurements, the value of the soil moisture in each sample was provided (e.g., see Table 2 of Gillette et al., 1980). These data were used to feed the Fecan formula to derive the would-be threshold velocity under dry condition. We have reprocessed the data for the soil and land use types measured by Gillette 1980 and 1982. In case of missing data for certain soil types, we have chosen the values with the soil composition closest to a measured type following the USDA soil composition diagram (Fig 1 of Gillette et al., 1980). We have now added the information in the revised manuscript (see the last paragraph of section 2.1).

(3). **Comment**: How (quantitatively) is the presence of non-erodible elements accounted for in calculating the threshold friction velocity and what value of the surface roughness was used?

Response: Non-erodible elements mainly include pebbles, rocks, and vegetation, while in this study only vegetation is considered. The land cover types (accompanied by soil types) determine the threshold friction velocity as calculated in Eq.(5) in section 2.1. The erodibility and roughness length used in this model only represents the potentially erodible particles such as slit, clay and sand by following the fundamental algorithm of Marticorena et al. (1997). So with this algorithm, only three land cover types are considered for erodible potential, include shrubland, mixed shrub/grassland, and barren/sparsely vegetated land. Surface roughness length (Z_{ruf}) is calculated in WRF based on vegetation fraction, while the surface roughness factor is calculated based on Z_{ruf}) (see response for comment#5)

(4). Comment: What is the value of coefficient A (scaling factor) in Eq. (1)?
Response: The value of A is set as 32.0 in this study.

(5). **Comment**: How is the surface roughness adjusting factor ($Z_{i,j}$) calculated?
Response: The surface roughness adjusting factor *Z* is calculated as:

$$Z = \frac{C_1}{C_1 \times lnZ_{ruf} - C_2}$$

The constants $C_1 = 32$ and $C_2 = -5$ used in this equation are derived from field measurement data from Gillette et al. (1980) and the relationship between roughness length and friction velocity described in Marticorena et al. (1997).

(6). **Comment**: The new dust speciation profiles are different for Gobi and Taklamakan. How does ONE default profile in CMAQ is replaced by these TWO profiles? Does user need to pre-define the regions where each profile is being applied?
Response: In the standard CMAQ, the default speciation profile is applied to the entire simulation domain. The two new profiles developed in this study are assigned separately to Gobi and Taklamkan based on a pre-defined map of these deserts. Yes, users need to pre-define the region where which profile should be applied.

(7). **Comment**: The vertical-to-horizontal dust flux ratio in Eq. (2) is based on the linear fitting of the measurements of Gillette (1979) by Marticorena and Bergametti (1995). The authors should note that the value of K based on this equation has the unit of [1/cm], while the rest of the formulation in the manuscript is in SI units. It seems that a factor of 100 is missed in the present calculations. Also, what would be the justification of using K=0.0002 for clay%>20%? Please comment.
Response: The unit of K is [1/cm] in Marticorena and Bergametti (1995) equation, and the factor 100 is implicitly reflected in the scaling factor A. The value of K (0.0002) for clay%>20% is used here following the recommendation of Marticorena and Bergametti (1995). We notice that this value may be subject to further changes when more measurements are made available.

(8). **Comment**: Eq. (5) is not from Fecan et. al (1999). There is no surface roughness study in their work.
Response: The reviewer is right, the Fecan et al. (1999) should be cited to support the

calculation of soil moisture adjusting factor in Eq.(6), not for Eq.(5). We have correct this in the updated manuscript.

(9). **Comment**: Based on Eq. (6), the soil moisture factor is 999.9 for Sm > Wmax. I think it should be the case only when Sm > Sl. Furthermore, the third relation should be used when Wmax < Sm < Sl. Please go through the conditions in Eq. (6) and revise them. Also, how is Sl (saturation soil moisture limit) determined?
Response: The reviewer is right about the conditions in Eq.(6). In fact the soil moisture S_m shall not exceed the saturation soil moisture limit S_l. We set the soil moisture adjustment factor f_{soilm} as 999.9 in the code of the model only to avoid computational abnormal values, thus it is not necessary to show this condition in the manuscript. So there are only two conditions considered, and the Eq.(6) has been revised in the manuscript as:

$$f_{soilm,i,j} = \begin{cases} 1.0, & for\ S_m \leq W_{max} \\ (1.0 + 1.21 \times (S_m - W_{max})^{0.68})^{0.5}, & for\ W_{max} < S_m \leq S_l \end{cases} \quad (6)$$

The value of S_l is determined based on a predefined lookup table for different landcover categories and soil types as shown below. The values in the table are mainly based on documentation of the North American Mesoscale (NAM) model.

Table Saturation soil moisture limit

Landcover Soil Type	Shrubland	Mixed shrub/grass land	Barren or sparsely vegetated
Sand	0.395	0.135	0.068
Loamy sand	0.410	0.150	0.075
Sandy loam	0.435	0.195	0.114
Silt loam	0.485	0.255	0.179
Silt	0.476	0.361	0.084
Lam	0.451	0.240	0.155
Sandy clay loam	0.420	0.255	0.175
Silty clay loam	0.477	0.322	0.218
Clay loam	0.476	0.325	0.250
Sandy clay	0.426	0.310	0.219
Silty clay	0.482	0.370	0.283
Clay	0.482	0.367	0.286

(10). **Comment**: The results deteriorate for Ca_2^+ when the new dust profile is used (Table 5). Please comment on the possible reasons.
Response: The evaluation statistics suggest that total $PM_{2.5}$ is underestimated at Duolun and Yunlin, but Ca_2^+ is overestimated under the default profile. So although the NMB value changse from 36.69% by default profile to -53.12% for Ca_2^+ by revised profile, it is highly possible that the revised profile provides a better estimation of the calcium mass contribution since the evaluation statics for trace metals should be consistent with that for total $PM_{2.5}$. The overall underestimation for $PM_{2.5}$ and trace metals should be due to the underestimation of fine mode aerosol mass contribution

(20%) in total dust emission. No solid conclusion is made at this point because the PM_{10} data is not available at Duolun or Yulin, although the total suspended particles (TSP) is measured and indicate the fine mode aerosol should have larger mass contribution (about 40% in TSP).

(11). **Comment**: In general, the information and texts in figures are very small and hard to read.
Response: The figures are revised to make the text bigger and easier to read in the updated manuscript. Figure 1 uses larger text font, and we also reduce the number of landcover categories shown in Figure 1(a), because only three categories will generate dust in the model. Figure 2 is revised to add text indicating the locations of Fudan Univ. observational sites and TAQMN site. Figure 7 and Figure 10 has added description in the subtitles to help illustrate the information of the figures. The rest of the figures have smaller font due to limited page size, but they are all drawn as vector graphics so readers can enlarge them to have a clear view.

(12). **Comment**: Please delete the repeated word “revised” on P. 35593 line 24.
Response: It is changed in the updated manuscript, thanks to the notification of the reviewer.

(13). **Comment**: In Eq. (3), the values of clay, silt, and sand should be in fraction not percentage.
Response: Yes, they should be in fraction. We have revised the text and Eq.(3) in the updated manuscript.

(14). **Comment**: In Fig. (2), the orange rectangle (Fudan observation) and the purple diamond (TAQMN) are hard to find. Please consider marking them within the figure.
Response: Figure (2) is revised to add text and larger markers to indicate the locations of observational sites.

Anonymous referee # 2

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**: The simulation period need to be clarified in the method section 2.4.
Response: The CMAQ model simulation period covers March and April from 2006 to 2010. This period is selected to represent the spring dust episode of East Asia. We have added this introduction into section 2.4 in the revised manuscript (see first paragraph of section 2.4).

(2). **Comment**: As the analysis is mainly focused on the spring time from 2006 to 2010, it would be better to explain how to initialize the model for each year.
Response: The regional model CMAQ uses daily initial concentrations and boundary conditions provided by a global model simulation with GEOS-Chem. The downscaling method is described in Lam and Fu (2009). GEOS-Chem simulation was conducted for 5 years from 2006 to 2010. We have added this information into section 2.4 in the revised manuscript (see second paragraph of section 2.4).

(3). **Comment**: Temporal coverage of observation data with screening criteria is also suggested to add in Table 4.
Response: Table 4 gives a brief introduction of the observations used in this study, and more details of these data are added into the revised manuscript in section 2.5. All the observations are collected to cover the simulation period March and April from 2006 to 2010, except for the data from Fudan Univ. network due to limited measurement efforts. The local data from Fudan Univ. has daily measurements only for 2007. We didn't apply additional screening criteria to the observations in this study because the data has already been examined before being officially release. The MODIS level2 AOD is filtered by GSFC before release. The API data is organized by China MEP and it has been screened before being published. The AERONET level 2.0 data used in this study is screened and quality assured mainly by the local agencies that organized the observational sites. The EANET data is screened and quality assured by Dr. Keiichi Sato (ksato@acap.asia) and Dr. Ayako Aoyagi (eanetdata@acap.asia) from the Asia Center for Air Pollution Research. The TAQMN data is screened and quality assured by Taiwan EPA before release.

(4). **Comment**: P35598 L17, I found difficulties to understand the function (6). Should that only apply to the case when Sm is > Wmax as stated in L11?
Response: The reviewer is right. W_{max} represents the maximum water holding capacity for each soil type. The soil moisture adjustment factor f_{soilm} is applied only when soil moisture S_m exceeds W_{max}. In addition, S_m shall not exceed the saturation soil moisture limit S_l, and the soil moisture adjustment factor f_{soilm} is set

as 999.9 in the code only to avoid computational abnormal values. So we have revised the function (6) as:

$$f_{soilm,i,j} = \begin{cases} 1.0, & for\ S_m \leq W_{max} \\ (1.0 + 1.21 \times (S_m - W_{max})^{0.68})^{0.5}, & for\ W_{max} < S_m \leq S_l \end{cases} \quad (6)$$

(5). **Comment**: P35602 L21, “the ACM2 PBL scheme” should be introduced at WRF part
Response: The reviewer is right, the WRF simulation uses ACM2 PBL scheme. The CMAQ model can define its own PBL scheme for vertical diffusion, so the vertical diffusion is configured by CMAQ again while running the. But usually the PBL schemes used by WRF and CMAQ will be the same to retain consistency. As the detailed description of the WRF configuration is described in Dong and Fu (2015a, b), we removed the description of CMAQ ACM2 PBL scheme in the revised manuscript.

(6). **Comment**: P35603 L20, I would suggest to clarify that Dust_profile, Dust_Chem and Dust_Chem_High were performed based on Dust_Revised.
Response: Yes the reviewer is right, the scenario Dust_Profile, Dust_Chem, and Dust_ChemHigh are all performed based on Dust_Revised. We have added this in the revised manuscript (see last sentence of section 2.4).

(7). **Comment**: P35605 L27, the sentence of “with relatively larger discrepancy in cities close to the Gobi Desert.” is confusing, please revise it.
Response: We intended to demonstrate that among all the cities which has API data, model evaluation results indicate relatively larger bias for those that are closer to the Gobi Desert. We realize that the original description is confusing and we have revised this sentence in the updated manuscript (see last sentence of the first paragraph in section 3.1)

(8). **Comment**: P35606 L1-2, is that based on daily records of all API sites from spring in 2006-2010?
Response: Yes, the evaluation statistics are calculated based on daily data pairs. We have added this information in the revised manuscript (see second paragraph in section 3.1).

(9). **Comment**: P35607 L1-2, “The two cities are close to the Gobi Desert, as shown by Fig. 1.”, Such information cannot be found in Figure 1
Response: The locations of the two cities (Duolun and Yulin) are shown in Figure 2. We have revised the description and Figure 2 in the updated manuscript.

(10). **Comment**: P35607 L19-20, need to clarify that these two observations (API and Huang et al. (2010)) cover the same time period?
Response: Huang et al. (2010) covers the period from March 20 to April 20 2007. In this study the same observation data are used to examine the simulated concentrations of trace metals and $PM_{2.5}$ from CMAQ. The spatial distribution of modeling bias shown in Figure 3(d) is averages of March and April from 2006 to 2010.

The main objective of this part is to demonstrate that CMAQ underestimates trace metals and $PM_{2.5}$ at near desert sites (Duolun and Yulin), and meanwhile it also overestimates PM_{10} at the near desert area. The time period of observations is described in the updated manuscript (see last sentence of section 2.5).

(11). **Comment**: P35608 L4-5, "O_3 (1st row), SO_2 (2nd row), SO_4^{2-} (3rd row), HNO_3 (4th row), NOx (5th row), and NO_3^- (6th row)", that doesn't match with the layout of Figure 6

Response: In the original manuscript we submitted, Figure 6 contains 6 rows of plots in portrait direction. The edited version, after being converted into PDF format, somehow distorted the figure to cause this problem. We are contacting the office to have this issue resolved. Thanks for the reviewer's comment.

(12). **Comment**: P35609 L10-12, "The elevation of NOx concentration should be attributed to the conversion of gas-phase HNO_3 back to NOx (Yarwood et al., 2005)" Since O_3 and OH is reduced, that might also account for the change in NOx and NO_3

Response: The reviewer is right, we have add this in the discussion (see first paragraph of section 3.3).

(13). **Comment**: P35635 Figure 2, the orange rectangles are hardly to find. It would be helpful to add the location of the Gobi and Taklamakan desert as well.

Response: The approximate area of Gobi and Taklamkan desert is marked in the revised figure. The location of orange rectangles are also revised to make them easier to be found (see Figure 2 in the updated manuscript).

(14). **Comment**: P35596 L3, "simulation" to "simulations"

Response: This is revised according to the reviewer's comment in the updated manuscript.

(15). **Comment**: P35597 L20, "elsewhere" to "in"

Response: This is revised according to the reviewer's comment in the updated manuscript.

(16). **Comment**: P35599 L23, "emission" to "emissions"

Response: This is revised according to the reviewer's comment in the updated manuscript.

(17). **Comment**: P35606 L4, "simulation without dust emission" to "no dust emissions"

Response: This is revised according to the reviewer's comment in the updated manuscript.

(18). **Comment**: P35607 L15, "one set of data pairs" to "Dust_Profile"

Response: This is revised according to the reviewer's comment in the updated manuscript.

(19). **Comment**: P35614 L28, “al” to “all”
Response: This has been revised according to the reviewer’s comment in the updated manuscript.

(20). **Comment**: P35615 L19, “comapred” to “compared”
Response: This has been revised according to the reviewer’s comment in the updated manuscript.

(21). **Comment**: P35617 L7, “Model development” to “Dust model”
Response: This has been revised according to the reviewer’s comment in the updated manuscript.

(22). **Comment**: P35617 L16, “bye” to “by”
Response: This has been revised according to the reviewer’s comment in the updated manuscript.

(23). **Comment**: P35628 Table 1, “in next section” to “in this study”
Response: This has been revised according to the reviewer’s comment in the updated manuscript.

(24). **Comment**: P35630 Table 3, “intial” to “initial”
Response: This has been corrected.

(25). **Comment**: P35631 Table 4, please check the title
Response: The title of Table 4 has been changed to “Observations used in this study”.

Anonymous referee # 3

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 heterogeneous 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 sub-species 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 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. 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. As coarse particles deposit faster than fine particles, the ratio of $PM_{2.5}$/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 $PM_{2.5}/PM_{10}$ 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.