Dear referee reviewer #2:

We are very grateful for your constructive comments and suggestions on our manuscript “Multi-model simulations of springtime dust storms in East Asia: Implications of an evaluation of four commonly used air quality models (CMAQ v5.2.1, CAMx v6.50, CHIMERE v2017r4, and WRF-Chem v3.9.1)”. After carefully discussions with other co-authors, we have revised our manuscript according to this point-to-point response letter.

The revised manuscript with revision mode has been supplemented to this response letter and presented in the same compressed zip file. The revised or added contents are listed as follows (words in red are the responses):

1. Title. To better reflect the content of the article, I suggest to change the title to "Multi-model simulations of a springtime dust storm over Northeast China: Implications of an evaluation of three commonly used air quality models (CMAQ v5.2.1, CHIMERE v2017r4, and WRF-Chem v3.9.1)" due to the following reasons: 1. Only one dust storm is considered. 2. Referring to East Asia is misleading as only a comparably small subregion of East Asia is considered. East Asia in contrast comprises two of the earth’s major deserts, Taklamakan and Gobi, which are not at all subject of the study. 3. The evaluation of CAMx is limited to identifying that practically no emissions can be produced from within the model domain due to the MODIS based desert mask applied in the emission scheme which precludes emissions from regions that are not barren or sparsely vegetated. While this is an important conclusion, no further evaluation of CAMx is presented and thus the present title is misleading. Considering that some efforts where made to adjust the emissions of other models, it would have been interesting to see results from CAMx after expanding the mask to include other landcover types, but I understand that this might be beyond the scope of the study. In that case I recommend to simply adjust the title, to clarify in the abstract that three models are evaluated and (as before) to discuss in the main text why CAMx is not one of them.

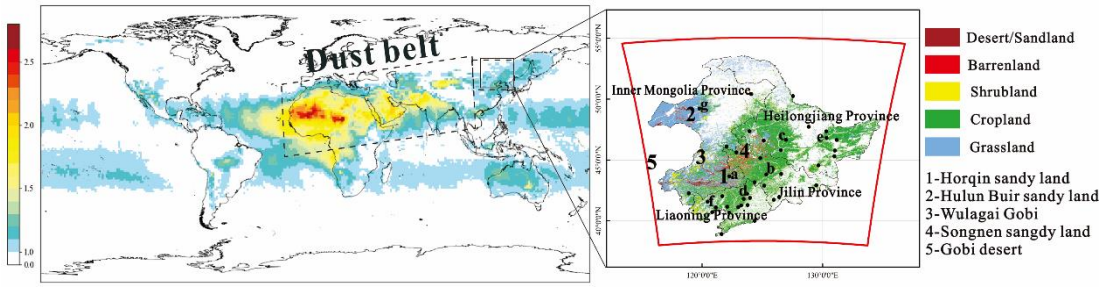
Response: We want to thank the reviewer for the constructive and insightful advice. According this comment and the suggestion from Reviewer 1. The further simulation and analysis of CAMx were implemented, and provided in Section 3.5 and the supplement file of the manuscript. As the dust mask used in CAMx showed no coverage in NEC area, the seasonal dust source map (G12_0.1_seasonal) was adapted to replace the original dust mask file as it had the best performance among those source maps in the WRF-Chem model. Therefore, the title is changed to “Multi-model simulations of a springtime dust storm over Northeastern China: Implications of an evaluation of four commonly used air quality models (CMAQ v5.2.1, CAMx v6.50, CHIMERE v2017r4, and WRF-Chem v3.9.1)”

2. Page 1, line 27. "to simulate dust storms in East Asia" should read "to simulate a dust storm over Northeast China", see above

Response: It is revised as “This study applies and evaluates four widely used regional air quality models to simulate dust storms in Northeastern China.”

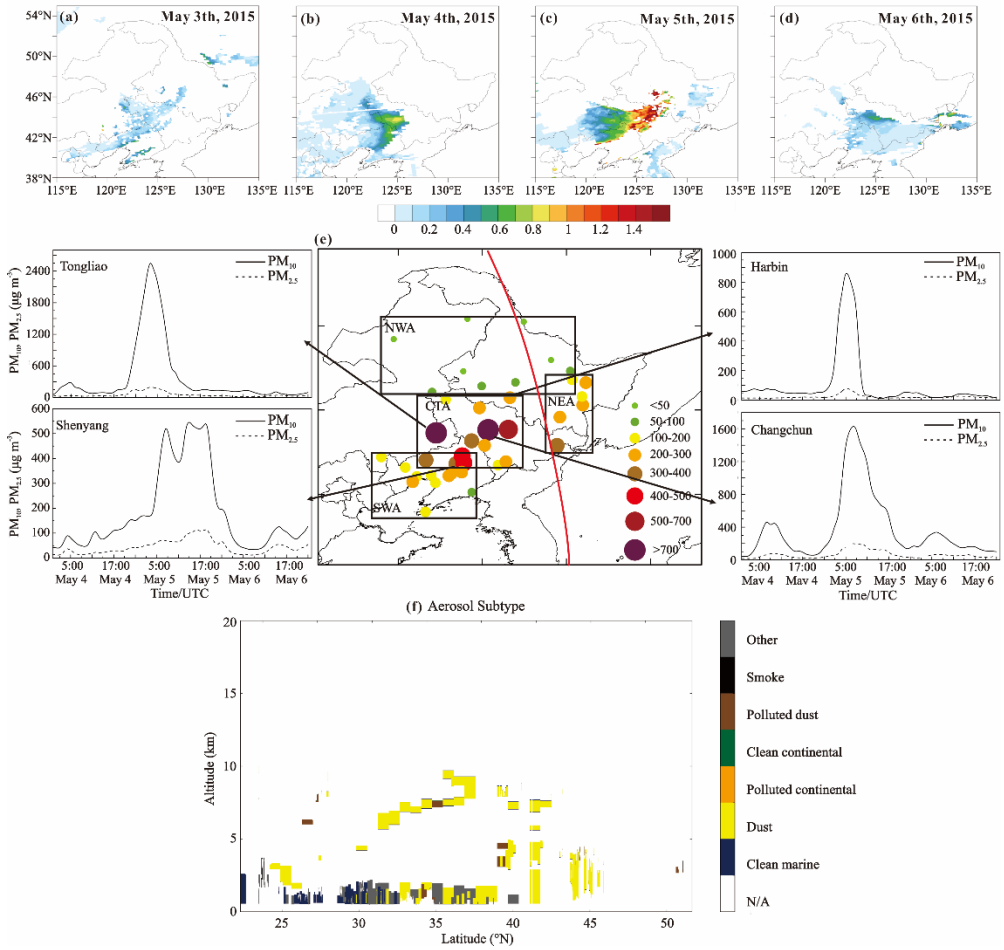
3. Page 5, line 8. The line in Fig. 1 is hardly recognizable as being dark blue.

Response: Thank you for your reminding. We find that the color blue didn’t present well when converting to PDF format. So we changed it to color red and the revised figure is showed below.



4. Page 7, Fig. 1. Please label the regions with CTA, NWA, NEA and SWA. The CALIPSO path can hardly be identified as being blue as mentioned in the caption.

Response: The labels of subareas of CTA, NWA, NEA and SWA were added on the figure and the CALIPSO path color was changed to red. The revised figure is showed below.



5. Page 8, line 20. "would" or "did"?

Response: It has revised to “This revision could increase the saltation flux by a factor of 2 or more.” According to the description in LeGrand et al. (2019).

Refference

LeGrand, S. L., Polashenski, C., Letcher, T. W., Creighton, G. A., Peckham, S. E., and Cetola, J. D.: The AFWA dust emission scheme for the GOCART aerosol model in WRF-Chem v3.8.1, Geosci. Model Dev., 12, 131-166, https://doi.org/10.5194/gmd-12-131-2019, 2019.

6. Page 10, line 11. "omitting the effect of soil moisture" should read "omitting the term supposed to account for the effect of soil moisture"

Response: Thank you for your very helpful suggestion. This sentence is revised as “The major modifications were omitting the term supposed to account for the effect of soil moisture on dust emission”

7. Page 10, line 13. "maximum", not "minimum"

Response: Thanks for your comment, it should be “maximum” and had been revised in our new manuscript.

8. Page 17, line 5. In Figs. 2 and 6 it is not possible to identify trajectories, not even the approximate direction of the outflow from the source regions can be identified in Fig. 2, it is therefore hard to make this comparison. Neither do I expect any difference, as the WRF wind fields should be quite realistic.

Response: Thank you very much for pointing out this problem. We find that this part could not properly expressed. According to the observations, the large areas of NEC such as northern Liaoning, Jilin and eastern Heilongjiang Province were influenced by this dust episode while the simulated results of CHIMERE did not show an obvious impact on northeastern NEC (eastern Heilongjiang Province). This is one of the differences between CHIMERE simulation and observation. It should be simulated and observed patterns rather than trajectories. Therefore, we revised this sentence to “The simulated dust showed its impact on the eastern areas like Jilin and northern Liaoning Province (Fig. 6a~c), while northeastern NEC (such as eastern part of Heilongjiang Province) were also observed to be influenced by this dust episode (Fig. 2).”

9. Page 17, line 11. "The most striking feature of the model results was their concentration" should read "The most striking discrepancy between the model results was in their concentration level" or similar.

Response: It has revised to “The most striking discrepancy between the model results was in their concentration level.”

10. Page 17, line 15. "might" should be deleted

Response: Thanks again. This is revised to “This difference might have arisen because the KOK scheme was mainly built on fragmentation theory”

11. Page 18, line 7. The sentence "With further comparison ..." needs rephrasing

Response: It is revised as “Comparing to observations and simulated results of WRF-Chem and CHIMERE, the dust simulated by CMAQ was only short-distance transported southeastwards to….”

12. Page 19, Eqs. (1) and (2) and Section 3.4 in general. Please define all variables and make sure to use units (e.g. for the p limits in Eq. (2)). The discussion would benefit from some revision because it is hard to follow what is used in (a) the different models (b) the literature cited and (c) in the present study. E.g., it would help to mention both, model name and the related citation next to each other where applicable and make use of active voice.

Response: The definitions of variables and parameters used in equations were explained more detailed in the manuscript. Moreover, we also named the methods described in literature or used in models: the formula described in Lu and Shao (1999)

is named as LS99 and a version of LS99 modified by Kang et al. (2011) and introduced in CMAQ since version v5.2 by Foroutan et al. (2017) is called F17. The formula involving p for calculating α described in Shao (2004) is named as S04. This part is now revised as "The formula is expressed as follows:

$$\alpha = \frac{F}{Q} = \frac{C_\alpha g f \rho_b}{2p}(0.24 + C_\beta u_* \sqrt{\frac{\rho_p}{p}}) \quad (1)$$

where f is the fraction of fine particles contained in the soil volume, p is plastic pressure, in the range of 10^3~10^7 N m^{-2} (Gillett, 1977; Callebaut et al., 1985; Rice et al., 1997), ρ_b and ρ_p are the bulk soil and soil particle densities with unit of kg m^{-3}, g is the gravitational constant in m s^{-2}, u_* is friction velocity in m s^{-1}, and C_α and C_β are constants. Here the formula described in Lu and Shao (1999) is named as LS99 and a version of LS99 modified by Kang et al. (2011) and introduced in CMAQ since version 5.2 by Foroutan et al. (2017) is called F17. The formula involving p for calculating α according to Shao (2004), namely S04, can be described as:

$$\alpha = c_y \eta_{f,i}[(1-\gamma) + \gamma \frac{p_m(d_i)}{p_f(d_i)}] \frac{g}{u_*^2}(1 + 12u_*^2 \frac{\rho_b}{p}(1 + 14u_* \sqrt{\frac{\rho_b}{p}})) \quad (2)$$

Where $p_m(d_i)$ and $p_f(d_i)$ are respectively the fully and minimally disturbed dust fraction in bin d_i, and $\eta_{f,i}$ is the fully disturbed dust fraction. ρ_b=1000 kg m^{-3} is bulk soil density. γ is a function specified as $\gamma = \exp[-(u_* - u_{*t})^3]$ where u_{*t} is threshold friction velocity. c_y is a dimensionless coefficient which is set to be 1×10^{-5}, 4×10^{-5}, 5×10^{-5}, 3×10^{-4} for different soil textures and locations in Shao (2004); then, values of soil plastic pressure p in the range of 10^2 to 10^4 N m^{-2} were obtained via matching with observed dust flux and friction velocities. This formula is now used in WRF-Chem v3.9.1."

13. Page 19, Eq. (2). Unless u_* is about 1, the RHS has a discontinuity at p = 3e4 N m^{-2}. The two cases on the RHS are limiting expressions for large and small p, it seems to be problematic to apply them on adjacent p intervals, and not use the full expression for intermediate values of p. Where does this distinction of cases and the threshold of p = 3e4 N m^{-2} come from?

Response: The threshold of p = 3e+5 N m^{-2} (however, it was miswritten as 3e+4 N m^{-2} in the manuscript) was from the description of Shao et al. (2004) "For p >3e+5 N m^{-2}, σ_m becomes negligibly small (<0.1) under normal wind conditions, implying that saltation bombardment is insignificant in such circumstances and aggregates disintegration is the main mechanism for dust emission." Nevertheless, the reference doesn't clearly present the threshold value of the equation and no explanation about how the latter part of the equation ($\alpha = 168 c_y[\eta_{mi} + (1-\gamma)\eta_{ci}]\left[\frac{1000}{p}\right]^{\frac{3}{2}} u_* g$) established is provided, and it has not been used in the air quality model. We read the reference again and found that equation 2 in the manuscript was improperly introduced. The equation 6 in Shao et al. (2004) which showed as

$$\alpha = c_y \eta_{f,i}[(1-\gamma) + \gamma \frac{p_m(d_i)}{p_f(d_i)}] \frac{g}{u_*^2}(1 + 12u_*^2 \frac{\rho_b}{p}(1 + 14u_* \sqrt{\frac{\rho_b}{p}}))$$

is used in WRF-Chem model and it doesn't need piecewise p values for calculation. Therefore, this part was revised and the discussion about the threshold of p is removed. Now it is shows as "Note that the fitted c_y and p defined above could only be used in S04 and not in LS99 and F17 with different physical parameters. For example, the fitted value of 5000 for p (silty clay loam) in Table 3 of Shao (2004) was used as p of sand in Kang et al. (2011). To correct the overestimated p used in the vertical flux calculation of LS99, Kang et al. (2011) reported that a modified C_α was recalculated based upon c_y (which is used in S04). However, to our knowledge, no method based on physical evidence is available to complete this conversion. Moreover, the source code of Shao_2004 in WRF-Chem only uses prescribed values $p = 3 \times 10^4$ and $c_y = 1 \times 10^{-5}$

without considering the soil textures. As both of their values varied widely over soil types and locations, the mismatch in part of the study domain would lead to difference in magnitude, no matter in CMAQ or WRF-Chem."

14. Page 21, line 13 to 15. Please mention that you use the AGO scheme

Response: Thank you for your helpful suggestion. It is revised as "here only the outputs simulated by AGO scheme with *ierod*=3 (mixed USGS and MODIS) were chosen for further validation."

15. Page 20, line 19. The sentence "Furthermore, ..." needs rephrasing

Response: It is rephrased as "Furthermore, comparing to the sandblasting for the clay and clay loam, the dust originated from aerodynamic entrainment (which was not taken into account by the present dust model) was significantly constituted up to 28.3% and 146.4%, respectively"

16. Page 22, line 3. Please mention that you analyse hourly values.

Response: This sentence is revised as "…and normalized mean error (NME)) for the hourly data of 12 simulations and observations at 40 ground-based monitoring sites in NEC were calculated…."

17. Page 22, line 9. "was shown" should read "is shown"

Response: It is revised to "is shown"

18. Page 22, line 10. "abscissa" should be replaced by "distance to point OBS"

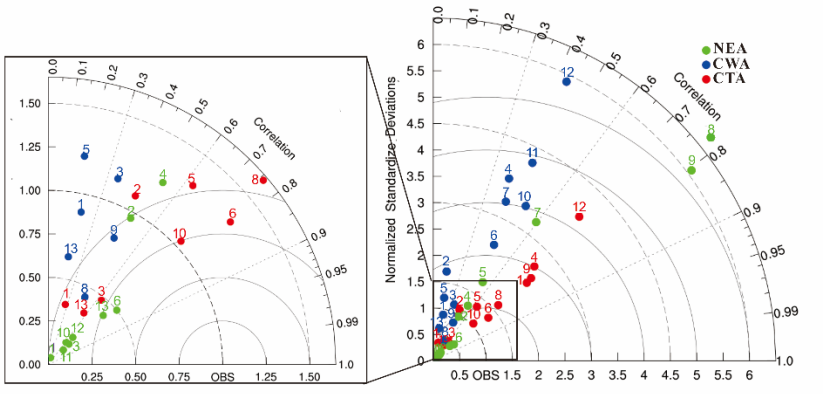
Response: It has been revised to "…while the RMSE (distance to point OBS) measures differences between the modeled and observed PM_{10}…."

19. Page 23, line 6. "Thus, NSD..." should read "Thus, NWA..."

Response: It is revised to "Thus, NWA was not included in the Taylor diagram."

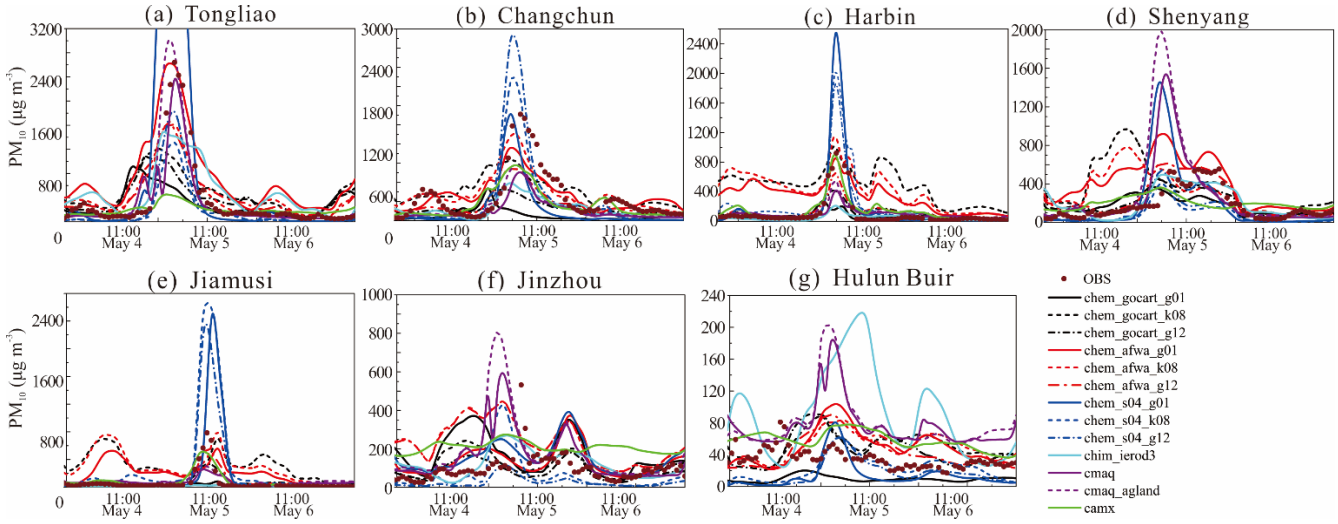
20. Page 23, Fig. 9. The colours for NEA and CWA are hard to distinguish

Response: Thank you for this helpful comment. The color of NEA was changed from purple to green and the revised figure is showed below.



21. Page 24, Fig. 11. It might be worth to enlarge the figure and refine the colours

Response: This figure had been enlarged and we also carefully refined the colors. It now shows as:



22. Page 26, line 25. "best near" should read "perform best close" or similar

Response: It is revised as “All simulations performed best near the dust source areas and degraded in accuracy….”

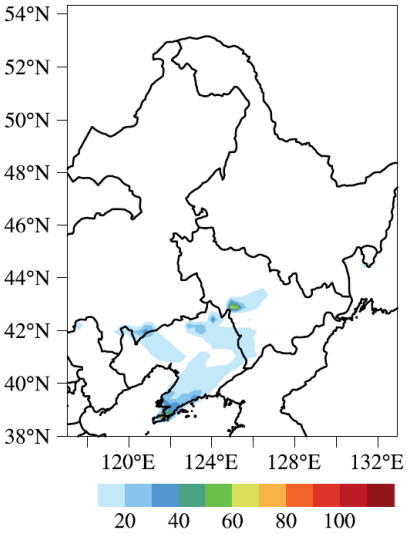
23. Page 27, line 3. This clearly is not related to the resolution but simply a matter of allowing emissions from areas not classified as desert or sparsely vegetated by refining the landcover mask.

Response: Thank for your very helpful suggestion. This sentence is revised as “A dust mask including dust emissions from regions not classified as “barren or sparsely vegetated” in CAMx should be developed by refining the land cover mask in future works.”

24. Page 27, Author contributions. Please make sure that the order of the initials of each contributor is consistent with the author names on the title page

Response: Thank you for your valuable suggestion. The part of Author contributions is now revised as “MS, XZ and CG performed the majority of the source code reconfiguration of WRF-Chem, CHIMERE, CMAQ and CAMx, and initially designed the numerical simulations to carry them out. DQT, AX, WG and CX provided help for the simulation designation. LH provided support for conducting the CAMx model. HZ and SZ provided advices on the selection and usage of observational data. MS, XZ and DQT led the analysis of the simulations, and SIE, XW, XL and MD provided professional advices. SM and XZ wrote the paper and all authors read, revised, and approved the final manuscript.”

In addition, we conducted reproducibility tests of our simulations and found an error in the WRF-Chem section. We have run the WRF-Chem model with UOC_Shao2011 scheme and the daily mean PM_{10} on May 5th, 2015 simulated by UOC_Shao2011 is provided below. It showed that spatial pattern and concentration level had little similarity with the observations. Considering its unreasonable results, then we selected UOC_Shao2004 for the subsequently simulations. It means that the actually used dust scheme in manuscript is UOC_Shao2004. However, the texts expressed the used scheme as UOC_Shao2011 by wrong. Therefore, we corrected this into UOC_Shao2004 in the manuscript.



Thank the reviewer again for the constructive criticisms that have helped us to improve our manuscript. We have tried our best to improve the manuscript and made changes in the manuscript. These changes will not influence the content and framework of the paper. And here we do not list the changes but marked in the revised manuscript.

All in a word, via these evaluation works, we hope to do some contributions to the community for enhance the dust forecast ability on regional scale in air quality models.