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Interactive comment on "Impact of open crop residual burning on air quality over Central Eastern China during the Mount Tai Experiment 2006 (MTX2006)" *by* K. Yamaji et al.

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The authors are deeply grateful to the referee for his/her review providing excellent suggestions for improvements of this manuscript.

General comments:

One of my major concerns is about the uncertainty and confidence of the simulation results. Although authors discussed the reasons for the discrepancies between the simulation results and the observations, almost all discussions are very vague without evidence. For example, authors raised several reasons for the failure of the model in capturing the polluted episode of 12-13 June, such as the inaccuracy of emission infor-

mation, the ability of the model to reproduce the polluted episode due to the vertical and horizontal resolutions, the ability to simulate boundary layer evolution, and the boundary conditions (Page 22120-22121). But they did not provide any further information on these points. This leaves a question that, to what extent, the comparisons of the model simulation and the observation at the top of Mt. Tai about the first polluted episode and the monthly-averaged results are meaningful. As mentioned by the authors, the model is insufficient to reproduce "atmospheric phenomena" relevant to an isolated mountain such as Mt. Tai (Line 23-25 Page 22120). The confidence of the comparisons sounds very low. Another major concern is the application of observations at the top of Mt. Tai in the comparison with the simulation to investigate the "impact of open crop residual burning on air quality over the CEC during MTX2006" (the title of this paper). The top of Mt. Tai is a very special place within CEC. Observations there cannot reflect the general air mass characteristics of the CEC. Without a comparison with an observation at a ground site, the impact is difficult to be properly investigated. In addition, the abstract and text are tedious. There are several repeats. Many descriptions are difficult to follow. Words and grammars need to be checked carefully. I list some specific comments and technique corrections for reference.

Reply to General comments:

We understand that the two major concerns are about the vague discussion for uncertainty and confidence of the simulation and about insufficient comparisons with observations. We will deeply discuss considering referee's comments and suggestion in our revised manuscript.

As mentioned by the referee, for the first problem, this manuscript had concerns to give readers impression that this model simulation may be too poor to reproduce the observations. To avoid this, we focus on the contribution of the open crop residual burning to the observed O3, CO, BC and OC at the summit of Mt. Tai and the value of the daily gridded emission inventory for this simulation in the sections of results and discussions. In the revised manuscript, the remaining uncertainty and confidence of the simulation results will be discussed with their clear evidences. Although the referee pointed out that authors raised several reasons for the failure of the model in capturing the polluted episode of 12-13 June, such as the inaccuracy of emission information, the ability of the model to reproduce the polluted episode due to the vertical and horizontal resolutions, the ability to simulate boundary layer evolution, and the boundary conditions (Page 22120-22121), however, this part (Page 22120-22121) did not mean that these potential errors caused the failure of 12-13 June. Indicated in our manuscript (for example, Lines 18-23 Page 22117), we suggested that a possible cause of the failure of 12-13 June was the temporal distribution of emission. On influence of the inaccuracy of emission information on the failure of the model in capturing the polluted episode of 12-13 June, we will focus on uncertainty of emission timing of biomass burning. As mentioned by the referee, additionally, the influence on the simulated daily results owing to the normalization of fire data is very important for this study. We will discuss these in the revised manuscript. For the potential errors in the model result, for example for underestimation of CO, we will indicate that the boundary CO concentrations from a GCM, CHASER (CHemical AGCM for Study of atmospheric Environment and Radiative forcing) are likely to underestimate, because simulated CO concentrations by the GCM tended to be lower than observed one in June at almost sites of the Northern hemisphere (Sudo et al., 2002). On the other hand, the vague discussions (e.g. about model errors caused by modeled vertical and horizontal resolutions) without the clear evidences will be deleted from the revised manuscript. We plan to make these vague discussions clear in the next work.

For the second one, we understand a part of that the limited comparison between model and observation at the summit of Mt. Tai is too weak to investigate the whole Central Eastern China using the model. However, as mentioned by other papers published in this special issue (e.g. Kanaya et al., 2008), since the site is free from local sources as it is located at 1534 m high, we believe that the top of Mt. Tai had regional a representativeness for the Central Eastern China, with much reduced spatial inhomogeneity compared to a urban site. Meanwhile, we think that urban sites at the Central

Eastern China with outstandingly high emission rates of atmospheric pollutants are not befitting for locations with regional representativeness there. Furthermore, publicly available ground-based observation data in this region are very limited. As the reviewer pointed out, however, to evaluate more the model's reliability, we will show comparisons between model and observation in the revised manuscript using observational data at other sites, e.g., Mondy (51.40 N, 101.00E, 2000m asl) , Mt. Hua (110.09E, 34.49N, 2064ma.s.l.), Mt. Huang (118.15E, 30.14N, 1836ma.s.l.)., Xinglong (40.42N, 117.4E, 940m asl) and satellite data, OMI NO2 (Irie et al., 2008), both of which can help discussion on regional representativeness.

Finally, we will re-submit the revised manuscript to Atmospheric Chemistry and Physics after English check.

Specific comments:

Specific comment 1. Abstract (page 22105-22106): Geographical location of CEC in latitude and longitude is necessary.

Reply to Specific comment 1. The geographical location of the Central Eastern China (CEC) will be added in Abstract.

Specific comment 2. Abstract (page22105-22106): It needs to mention that observation results at the top of Mt. Tai are applied in the comparison with simulation. The geographical position and altitude of Mt. Tai need to be shown.

Reply to Specific comment 2. The geographical position and altitude of Mt. Tai will be added in Abstract.

Specific comment 3. Introduction: Also in the following descriptions, names of provinces of China and geographic regions such as CEC and the North China Plain are used. It is very difficult for readers who are not familiar with the district divisions of China to follow descriptions including these names.

Reply to Specific comment 3. Provinces of China used in this manuscript will also be illustrated in the revised manuscript .

Specific comment 4. Line 2 - Line 4 page 22110: Reasons for the adjustments in this study need to be addressed more clearly because this is the base of this study. The descriptions here sound that the adjustments still have a lot of problems. Readers need to know these uncertainties in order to correctly understand the results of this study. By the way, readers who are not familiar with the district divisions of China cannot understand the geographic positions of the provinces from the names.

Reply to Specific comment 4. The adjustments have been mentioned in Line 15 page 22109 - Line 4 page 22110 based on a published paper (Yan et al., 2006). The district divisions of China will be illustrated in the revised manuscript.

Specific comment 5. Line 25 page 22112 - line 9 page 22113: Details of allocating the annual emissions in province level into daily gridded database are necessary. How were the data of land cover, population distributions and etc. applied in the inventory preparation? How was the normalization of 5-day fire data conducted? Does the data processing have any influence on the simulated daily results which are investigated in this study?

Reply to Specific comment 5. On the spatial allocation with 0.5 degree from the annual province level emissions, we used both gridded data, area data from the Gridded Population of the World version 2 dataset and land cover map, to detect the province and land use type occupied by each grid (0.5 degree). On the emission timing (temporal distribution with 1 day), then, we used both data of fire data and land cover map to detect the burning timing of biomass in the open area. We will indicate in detail these in the revised manuscript. The way of the normalization of 5-day fire data will be indicated, and then the influence on the simulated daily results owing to data processing will be also mentioned in the revised manuscript.

Specific comment 6. Section 3 (page 22114 - 22115): As mentioned in the major comments, the confidence of the model design and the comparisons of simulation

results with the observations at the top of Mt. Tai need to be addressed. The sensitivity experiments are not for the evaluation of the model ability.

Reply to Specific comment 6. We believe that these sensitivity experiments themselves are meaningful for the evaluation of the modeling results, which may be subject to input data, anthropogenic and biomass burning emissions and boundary concentration. As mentioned in the referee's general and specific comments, however, additional discussion for uncertainty and confidence of the model simulation is needed based on clear evidences. Therefore, it will be added in the revised manuscript.

Specific comment 7. Line 17 page 22116: The reason of using the simulated results at about 1000 m to compare with the observations at the top of Mt. Tai (the altitude is 1533m) is necessary.

Reply to Specific comment 7. It will be added in the revised manuscript.

Specific comment 8. Line 26 page 22116 - Line 4 page 22117: Are these results stated according to the simulation?

Reply to Specific comment 8. We now think that this discussion (in Line 26 page 22116 - Line 4 page 22117) is not suitable in this manuscript. This part will be deleted in the revised manuscript.

Specific comment 9. Line 3-Line 11 page 22118: It is very difficult to follow these descriptions.

Reply to Specific comment 9. This part means that, 'On the model's reproducibility of CO in June, simulated day–to–day variation is reasonable comparing to observed one with the correlation coefficient of 0.536. However, the simulated CO is lower than observed one, e.g., the modeled monthly averaged concentration (313.1 ppbv) is much lower than observed one (567.7 ppbv). Even for daily concentration in the latter half of June, when biomass burning activity was low, the simulated CO is much lower than observed one by around 150–500 ppbv. These suggest that the model's underestimate is largely affected by the other reason except emissions from biomass burning (eg. CO emissions from energy sectors, CO inflow from this model boundary, and secondary CO).' This part will be changed to state it more clearly. The language check by a native speaker will be made well before resubmitting.

Specific comment 10. Line 8 - line 11 page 22120: Authors here attribute the reason for the discrepancies between the simulation and the observation to the inaccuracy of emission data. But if the model cannot properly simulate the upward flow along the slope, which took the pollutants to the top (authors have mentioned that the model cannot reproduce the local weather conditions and also report in the next paragraph that the model often failed in simulating the upslope motion of polluted air mass), evidences are necessary to show that the discrepancies are not caused by the poor ability of the model.

Reply to Specific comment 10. As mentioned by the referee, evidences are necessary to show that the discrepancies between model and observations, which is not caused by the poor ability of the model. For the upward flow along the slope, the polluted air masses at the summit of Mount Tai were not always affected largely by climbing along the slope. In fact, the intensity of local wind flows at Mt. Tai was not as strong as other mountainous regions (e.g. Alps), because the observed prevailed wind direction on nighttime and daytime were quite similar. Therefore no abrupt change in wind direction appears at Mt. Tai, which indicated a weak mountain-valley wind flow. It should be noted that our model reproduced the general features, but omitted some detailed ones. On the other hand, the observed ozone showed a weak valley between 14:00-17:00, with amplitude of only 3 ppbv for O3. Our simulation did not capture it. We will mention the magnitude of this error in the revised manuscript.

Specific comment 11. Line 23 page 22120 - Line 1 page 22121: These descriptions sound that comparisons of the simulation results with the observations at the top of Mt. Tai in this study are not appropriate.

Reply to Specific comment 11. As mentioned in 'Responses for General comments', this part had concerns to give readers an impression that this model simulation is too poor to reproduce the observations. To avoid it, this part will be revised, as mentioned above.

Specific comment 12. Line 4 - line 9 page 22121: Several reasons are raised without clarifying which one should be responsible for the discrepancies between the simulation and the observation. This makes the meaning of the following descriptions about the inter-comparisons between model experiments very questionable.

Reply to Specific comment 12. As mentioned in 'Responses for General comments', we will modify the uncertainty and confidence without evidences. As mentioned above, these sensitivity experiments themselves are valuable for the evaluation of the modeling results, so we will retain this part.

Specific comment 13 Line 16 - line 20 page 22123: The discussion is very vague.

Reply to Specific comment 13. This part will be modified.

Specific comment 14 Line 29 page 22123 – Line 2 page 22124: How did the smooth hotspots and total biomass burning emissions cause the differences between the model simulations? This is very important for the comparisons of the model experiments.

Reply to Specific comment 14. As mentioned in Responses to Specific comments 5, we will be mentioned.

Specific comment 15. Subsection 4.2.3 page 22125: This part could be largely simplified because of the lack of open crop residual burning.

Reply to Specific comment 15. This part will be simplified.

Responses to Technical corrections:

These pointed out by the referee will be corrected in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 22103, 2009.