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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.

Anonymous Referee #2

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

This manuscript investigated the main cause of the discrepancy between modeled and observed pollutant concentrations (ozone, CO, OC, and EC/BC), from the Mount Tai Experiment in June, 2006 (MTX 2006), using the Models-3/CMAQ model. They presumed that the main cause of the discrepancy would be from open residue burning. In order to prove this presumption, they carried out several sensitivity simulations with five different emission scenarios (with and without open yard burning together with 2000 and 2006 emission inventories).

Although the general research framework looks plausible, the details of the methodology employed are questionable. In this study, authors used an Eulerian modeling setting over East Asian domain (without nested-griddng over CEC) to prove their presumption. But, the modeling setting is too crude to prove their presumption. In case that this set of Eulerian modeling is used to evaluate/analyze the observational data from the point-measurement "networks", I think it would be a working idea in some sense. But, when a rather coarse-grid Eulerian modeling is carried out for evaluating the limited number of observational data from only a "single measurement location" like Mt. Tai, it could inherently contain large uncertainty. This uncertainty is even more enhanced by the fact that the open residue burning would be "spotted" spatially, not massive like Siberian or tropical forest fires. Also, the emissions from the open yard burning could be highly time-resolved, since they are rather controlled burning by peasants, unlike the massive, uncontrollable forest fires. But, authors carried out the CMAQ model simulations with a coarse-grid horizontal resolution, 80 km x 80 km. Vertical resolution would also be several hundred meters (because only 7 layers below 2 km were employed; I guess that many layers of the 7 layers are located even near the surface, probably below $\sim$1km, but Mt. Tai is $\sim$1500 m high). In addition, the open residual burning emissions are just "daily-resolved" in this modeling setting. Overall, with these modeling settings, I do not think that the accuracy of the "single point-location" measurement data can be evaluated successfully. In other words, for this study purpose the modeling settings should be much more sophisticated. Otherwise, it is likely that the CMAQ modeling could mislead to erroneous quantification and/or conclusions.

In addition to the reasons mentioned above, several factors could introduce further large uncertainties. (1) Although the MODIS fire data-base can provide daily hotspot map, it cannot catch all the fire events occurred during the study period. Particularly, fires under the clouds cannot be detected. Therefore, in the meteorological field analysis, the cloud distributions should be considered and discussed in detail for this type of study. (2) Current NMVOCs, POA (primary organic aerosols), and BC emissions in East Asia are highly uncetain. The uncertainties in these emissions frequently exceed

even more than ∼100%. Also, biogenic emissions are very poorly established in East Asia. In this study, 1ox1o GEIA emission for isoprene and terpene (1995) appeared to be used, but this emission is old, and has been reported to be overestimated greatly. Also, soil NOx emissions were neglected in this study, but the soil NOx emission is usually important in the areas, where the "agricultural residue burning" is active (because the main source of soil NOx is fertilizer). (3) The SOA formation of CMAQ model or current knowledge of the SOA formation, is not very accurate (probably, CMAQ-MADRID would equip with a better SOA formation scheme). Therefore, the uncertainty in the SOA formation scheme of the CMAQ model could introduce a large uncertainty in Fig. 2 and Table 3 & 4.

Multiplication of the uncertainties of the above-mentioned factors can affect the discrepancy between model values and observations on the top of Mt. Tai. I believe that authors should consider or, at least, discuss the uncertainties mentioned above. Overall, the current version of the manuscript does not include all the details mentioned above, and is far from the publication in this journal. Although I agree with the general framework of this study and the importance of studying biomass burning in East Asia, I recommend the authors to improve the work with a more sophisticated set of the model simulation, and then revisit the ACP.

Relatively minor points 1. Some awkward English expressions throughout the manuscript frequently hamper reading the manuscript.

2. In Table 1, since the CEC is such a polluted area and is located almost in the center of modeling domain, the influence of the boundary conditions would be of limited importance.

2. In the text and Table 3, only "correlation (r)" is analyzed, but in addition to correlation, "error" and "bias" are also important, and should also be analyzed.

4. In Fig. 2, why were not temperature and NOx, and SO2 concentrations included? Temperature is more important than, for example, relative humidity.

5. IMPORTANT: Also, in Fig. 2, there are differences between model-predicted and measured wind directions. In this type of study, even 1o difference in the wind direction at the receptor area (Mt. Tai) can result in huge differences in the source region (burning sites), as the difference in the angle tends to get larger along the backward trajectories.

6. Fig. 3, I think that if backward trajectory analysis (like HYSPLIT) from the Mt. Tai site is superimposed with the MODIS fire map, it would be much more appealing.

7. I also recommend authors to re-construct the manuscript. For example, I do not think that the detailed discussion from p. 22107:18 to p.22108: 28 is suitable for "Introduction".

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