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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of a new emission inventory on CAM5 simulations of aerosols and aerosol radiative effects in eastern China" *by* Tianyi Fan et al.

Anonymous Referee #1

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General comments

The manuscript by Fan et al. (acp-2016-802) discusses the newly emission inventories that were applied in CAM5 model. The simulated AOD, aerosol concentrations and aerosol radiative effects with the newly emissions are compared to the simulations based on IPCC AR5 emission inventories. They found the updated emissions incurred a few improvements in the simulations. The work suits the scope of the journal. The newly emission data applied in the models were appropriate and were basically clearly described. The manuscript was well written.

Unfortunately, I am having difficulty in understanding how this work in current state demonstrate evidence of novelty or advancement in the science or approach. Firstly,

the aerosol improvements due to emissions are quiet small, particularly in aerosol optical properties. This result is to some extent as expected. The author is encouraged to dig his/her own topic to extend the discussion and improve the scientific significance.

For example, China SO2 emission have been declined since 2006 while NOx and NH3 have been increasing. What's the changes in aerosol radiative forcing over China in the decade as the change in emission structure? Is it significantly different from the simulation uncertainty as using the different emissions of MEIC and IPCC AR5?

Secondly, there are large model biases in winter which cannot be explained by emissions alone and the author has no discussion. The model has a systematic bias. In fact, recently, lots of model studies claimed the importance of sulfate production through heterogeneous reactions of SO2 on deliquescence preexisting particles catalyzed by transition mental ions, which can increase PM2.5 concentrations and the mass fractions of secondary inorganic aerosols in the wintertime hazes of northern China (Wang et al. 2014; Huang et al., 2014; Zheng et al, 2015; Chen et al., 2016; Dong et al., 2016). The coexistence of NO2 and SO2 also promotes sulfate production (He et al., 2014; Wang et al., 2014). These chemical mechanisms are not appropriately considered in most models. I'm not sure that CAM5 model has well considered the sulfate chemical mechanisms in China, which is extremely likely not. The model bias should be further discussed, and the implications to the conclusion should be mentioned.

Thirdly, the observations in Figure 9 are susceptible. The observations show minimum winter sulfate in northern China cities, which is totally opposite to the general recognition that aerosols are higher in winter than in summer. An explanation is required.

Specific comments

Line 113-115, What are the emission amounts in sectors of shipping, agricultural waste burning, waste treatment and natural biomass burnings? Are they high comparing to MEIC emissions?

Line 130-140, I don't fully understand this paragraph. Did the model use BVOC from MEGAN model in MOZART-4 and use anthropogenic VOC from MEIC?

Line 140-142, Did the factor of 1.4 also apply to the natural biomass burning emissions to derive SOAG?

Line 147, Is there particle number emissions in the AR5 emission inventories?

Line 170, The simulated ADRE is a "all-sky" value while the observation-deriving ADRE in Line 187 is "clear-sky". Need to state the discrepancy.

Line 211, It might be more appropriate to show concentration results (Section 3.2) before AOD results (Section 3.3).

Line 240, why not show the results by aerosol components?

Line 249, "while the observed maximum extends further north" According to Figure 5, it seems that the observed AOD maximum is in the south of the simulated maximum. Please rewrite this sentence. Line 250-251 have the same problem.

Line 249, "This model maximum is mostly due to dust aerosol . . ." It seems that the maximum AOD in the earlier summer is about half less than the observation, and the maximum AOD is from dust. Thus, the anthropogenic AOD is quiet low comparing with MODIS. That is, CAM5 model heavily underestimates AOD in China and this cannot be explained by emissions alone. Besides to the emissions, the author need to mention other causes (e.g. missing nitrate, particle size distribution, aerosol hygroscopic growth, etc.) that account for the large AOD bias.

Line 329-330, "The sulfate concentrations in northern China . . . are characterized by the summer maximums. . ." In Figure 9, at northern cities, the minimum wintertime sulfate in observations are susceptible. As the observations were collected from literature measurements that were carried out in different periods, the observations in summer and winter may not be comparable. The comparison uncertainty should be admitted.

Line 343-349, in my opinion, gas-phase oxidation of SO2 is not the main pathway for sulfate production. Aqueous oxidation in droplet/cloud water should be more important. At 35°-40°N, the maximum sulfate difference between MEIC and AR5 in summer is also due to the high ambient humidity. Besides, if CAM5 can capture the wintertime high concentrations, the largest sulfate burden difference could be in winter than in summer.

Technical notes

If possible, mark the data range (i.e. Min, Max) in Figure 7 and 9.

References

Chen, D., Z. Liu, J. Fast, and J. Ban, 2016: Simulations of sulfate-nitrate-ammonium (SNA) aerosols during the extreme haze events over northern China in October 2014. Atmos. Chem. Phys., 16, 10707-10724, doi:10.5194/acp-16-10707-2016.

Dong, X., J. S. Fu, K. Huang, D. Tong, and G. Zhuang, 2016: Model development of dust emission and heterogeneous chemistry within the Community Multiscale Air Quality modeling system and its application over East Asia. Atmos. Chem. Phys., 16, 8157-8180, doi:10.5194/acp-16-8157-2016.

He, H., and Coauthors, 2014: Mineral dust and NOx promote the conversion of SO2 to sulfate in heavy pollution days. Sci. Rep., 4, 4172, doi:10.1038/srep04172.

Huang, X., Y. Song, C. Zhao, M. Li, T. Zhu, Q. Zhang, and X. Zhang, 2014: Pathways of sulfate enhancement by natural and anthropogenic mineral aerosols in China. J. Geophys. Res., 119, 14165-14179, doi:10.1002/2014JD022301.

Wang, Y., and Coauthors, 2014: Enhanced sulfate formation during China's severe winter haze episode in January 2013 missing from current models. J. Geophys. Res., 119, 10,425-10,440, doi:10.1002/2013JD021426.

Wang, Y. S., et al. (2014). "Mechanism for the formation of the January 2013 heavy

haze pollution episode over central and eastern China." Science China-Earth Sciences 57(1): 14-25.

Zheng, B., and Coauthors, 2015: Heterogeneous chemistry: a mechanism missing in current models to explain secondary inorganic aerosol formation during the January 2013 haze episode in North China. Atmos. Chem. Phys., 15, 2031-2049, doi:10.5194/acp-15-2031-2015.

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