

# ***Interactive comment on “Modeling of aerosol property evolution during winter haze episodes over a megacity cluster in northern China: Roles of regional transport and heterogeneous reactions” by Huiyun Du et al.***

## **Anonymous Referee #1**

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Modeling of aerosol property evolution during winter haze episodes over a megacity cluster in northern China: Roles of regional transport and heterogeneous reactions

Du et al.,

The study used NAPQMS to re-produce the haze formation in North China Plain. They almost captured well the haze evolution and formation. Also, they did calculate the BC ageing processes in the haze formation in Beijing. Certainly, the model is very important to serve on haze formation in North China Plain. In particular, there are

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lots of the measurement data from the APHH-BEIJING which can further improve the model accuracy. As the result from this study, they struggled to understand how the sulfate formation and BC aging, although they still not be perfect on reproducing it. As the novel of this paper, I might suggest one minor revision. (1) Seemly, the authors have some wrong citation in this paper. For example, L61 Tang et al., (2016), In the reference, there are two Tang et al., (2016). I don't know which one should be cited here. L75-77, SIA mixed with BC. Not just revealed by Wang et al., (2018). There are different methods revealing it. You should pointed out it..Such as Wang et al., ESTL, (2017) 4 (11), 487-493; Peng et al., PNAS, 113(16), 4266-4271; (2) The study mostly considered the model could not re-produce the sulfate concentration although they did good nitrate and ammonium. Seemly, the authors think that the heterogeneous reactions should be dominant and missed very much in the model. I don't deny the claim. ALSO, the author should considered the primary sulfate emissions from the sources. As the recent study indicate the primary sulfate particles can be emitted from the household coal emissions. As I knew, the inventory from the household in rural areas still not good enough in the model. The authors should not miss the point in this study. For example, Zhang et al., JGR, 123 (22), 12,964-912,979. They found the coal burning in household can emit certain amounts of sulfates. (3) L24 Discovered, should change to revealed (4) L92-97. Introduction part, the description of participant should be avoid here. Probably, you can cite this paper. Shi et al, acp-2018-922, in ACPD. (5) L99, Sources of? Deleted of (6) L177 miss space before data You mentioned several times mixing states. I think that you should explain the mixing state in the model. Is this mixing state similar to Li et al, 2018, JGR, 121 (22), 13,784-713,798 or similar to Riemer et al., ACP, 13(22), 11,423–11,439. Seemly, they have different understanding on mixing state. How is your model you think? (7) L269-270. I don't think this is right reference here. The study worked on iron associated with ocean production. They didn't work on any aerosol particle in Beijing. (8) L321, along with (9) L379-389, I STILL want to emphasize the primary emissions of sulphates here., Seemly, you missed household emission in L500-502. (10) Section 4.4, the authors

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tried to understand the heterogeneous reactions. The possible reactions should occur on aqueous layer which is related to the particle phase anymore (Sun et al., JGR, 123 (2), 1234-1243; Kuang et al., GRL, 43, 8744–8750.). Also, in 513-515. (11) Here I still confused on the aqueous chemistry and heterogeneous chemistry. What are differences in the model? Could you please list them. Seemly, heterogeneous chemistry happened in aqueous layer of particles. (12) Figure 6, the pie is too small to see clearly them (13) Figure 12. The figure is not clear for me. (14) Figure 13 what is PSO4? In X-axis

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