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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Heterogeneous chemistry: a mechanism missing in current models to explain secondary inorganic aerosol formation during the January 2013 haze episode in North China" *by* B. Zheng et al.

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I find this study very interesting and I can believe that heterogeneous chemistry might have a strong potential for SNA formation in very polluted conditions.

However, I have some doubts regarding the justification for the lower and upper limits used in the RH-dependent expression adopted for the uptake coefficient, γ. The values are taken from studies (K. Wang et al., 2012; Crowley et al., 2010; Shang et al. 2010; Wu et al., 2011) which all concern specifically reactions on $dust$ particles. But haze

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particles in Northeastern China in winter are primarily a mixture of organic and SNA aerosols, i.e. very different aerosols. Aren't there studies on the uptake by sulfates or organic aerosols? If not, the manuscript should at the very least, state clearly that the adopted uptake coefficients are rather arbitrary – in the case of SO_2, it is chosen in order to match the observed concentrations of sulfates.

Another point concerns the abrupt increase in sulfate concentration observed in haze conditions, 70–130 μg m^{-3} in a few hours, which is presented as argument for a large heterogeneous production. Aren't meteorological variations the main driver for such large changes? Although I acknowledge that the larger fraction of SNA in the total aerosol loading is a valid argument to the enhanced SNA formation in haze conditions.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 16731, 2014.