

Interactive comment on “Fast heterogeneous N₂O₅ uptake and ClNO₂ production in power plant plumes observed in the nocturnal residual layer over the North China Plain” by Zhe Wang et al.

Anonymous Referee #2

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The paper reports on measurements of N₂O₅ and ClNO₂ on a mountain top site in the North China Plane (NCP), and examines the chemistry of N₂O₅ to ClNO₂ conversion in power plant plumes that were observed during the project. This study is a very useful addition to the growing literature on this important chlorine activation pathway. In general the paper is clear and very well written and should be publishable pending the handling of the following comments and questions.

General Comments I would like to see a better description of the aerosol particle characteristics and chemistry. For example, surface area, organic fraction, in addition to nitrate and chloride could be included in Figure 1. This would be particularly useful

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since this is likely the major difference between the environment in this study relative to the studies in Europe or North America. It would also be helpful if instead of mass concentration, some of the correlations in figures (Figure 11, Figure S1) could be also done with molar concentration, which is how the lab studies (Bertram and Thornton, 2009, Roberts et al, 2009) were parameterized.

Specific Comments Abstract, Line 12. I know what you mean when you say effect the next day's photochemistry, but someone not familiar with ClNO₂ would first need to know that it photolyzes to yield chlorine atoms, so some additional explanation would be good here. Abstract Line 18. A brief phrase describing how you got the uptake coefficient and yield would be good here. Abstract, Line 22. When you use the word “determined” it sounds like a measurement. It would more accurate to say ‘estimated’ or ‘modeled’. Page 3, Line 2. Not sure what is meant when you say “the field determination of (phi) is limited”. Do you mean that there are not very many reported determinations of (phi) from field measurements? Page 3, Line 13. The Thornton et al., 2010 reference should be included in this list. Page 4, Line 19. It would be more proper to say ‘iodide ion chemical ionization mass spectrometry with a quadrupole mass spectrometer’. Page 11, Eq. 6. The term dN₂O₅/dt should really be the loss rate of N₂O₅, which are corrected shown in the next two terms in the equation. Page 11, Lines 15-20. The big problem with this analysis is that it assumes that the growth rates that are inferred from Figure 9 correspond to the actual kinetic time within the plume. There is no way to know if that is true. The features in Figure 9 could be due to something completely different, e.g. a gradual shift in wind direction so that the plume as gradually influencing the site, starting with the dilute edge. There is simply no way to know what the physical circumstances were, with the evidence at hand. Another approach needs to be found, or the analysis should be abandoned. Page 11-12, Eq. 7 and Lines 1-2. With the equation written as is, k' would then be 1/450, not 450 as stated, to match the parameterization of Roberts et al., 2009. I believe the correct expression was used to generate the points in Figure 10a since Eq. 7 as written would generate phi's that were quite a bit <1. Page 12, Eq. 8. Same problem as Eq. 6,

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$\text{dN}_2\text{O}_5/\text{dt}$ is not the proper term here.

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