

Interactive comment on “Kinetics and mechanisms of heterogeneous reaction of NO₂ on CaCO₃ surfaces under dry and wet conditions” by H. J. Li et al.

T. Zhu

tzhu@pku.edu.cn

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We would like to thank the reviewer for the constructive suggestions to improve our manuscript. We have implemented all the comments and suggestions in the revised manuscript. Below please find a detailed point-by-point response to each comment.

1. The manuscript repeatedly mentions that SEM analysis of the particles was performed (e.g., Abstract line 10; page 7118, line 21; page 7119, line 7). However, no details are given in the experimental section and no data/results are presented. The authors assume a cubic shape for CaCO₃ particles, which becomes important for calculating the uptake coefficient at higher RH. An SEM image should be included in the

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supplemental information and discussed in relation to the assumed cubic shape.

Response: The SEM image of CaCO₃ is provided in the revised manuscript. We have also measured the size distribution of grinded CaCO₃ particles using a laser particle sizer; based on this result, we calculated the surface area of CaCO₃ to be 0.19m²/g, we used this value in the discussion of the revised manuscript.

2.The authors calculate that 3.4-3.6% of the oxygen atoms correspond to -Ca(OH) sites (page 7120, line 26). Since XPS analyzes the bulk sample, it would be more insightful to estimate the percentage of surface sites corresponding to -Ca(OH) sites versus -Ca(CO₃H) sites.

Response: The X-ray of XPS could only penetrate into about 10 nm depth of particles, hence the results of XPS reflects the composition information of particles surface, not the bulk sample. The XPS shows the oxygen atom ratio of Ca(OH):CaCO₃ is 100:7.3, unfortunately with this technology we could not separate CaCO₃ and -Ca(CO₃H).

3.The NO₂ concentrations used in these experiments are orders of magnitude higher than found even in the most polluted regions (4.81×10^{15} to 1.22×10^{16} molecules cm⁻³ for RH < 10% versus 5×10^{12} molecules cm⁻³ in extremely polluted regions and more typically 2×10^{11} molecules cm⁻³). This significant difference should be acknowledged and discussed in the paper. This also has great implications for the low RH studies, in which the authors conclude that N₂O₄ is the reactive species. The concentration of N₂O₄ in their experiments is never calculated – yet they range from 5.7×10^{12} to 3.7×10^{13} molecules cm⁻³, which is considerably higher than equilibrium concentrations at tropospheric levels (1×10^4 molecules cm⁻³ in remote regions and 6×10^6 molecules cm⁻³ in extremely polluted regions). If uptake coefficients were calculated for N₂O₄ at low RH, does this reaction likely compete with HNO₃?

Response: We agree that the NO₂ concentration used in the laboratory is higher than that in the real atmosphere. To meet the detection limit of the instrument (DRIFTS), we have to use this level of NO₂ concentration in the laboratory experiments. We

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addressed this difference in the revised manuscript. When discussing the implication of the laboratory results to the atmospheric chemistry, we have pointed out: 1) the reaction with N_2O_4 is only happened under dry condition, under this condition, NO_2 reaction is not as important as HNO_3 in the troposphere; 2) under wet condition, NO_2 reacts directly on the surface and no N_2O_4 is needed, the importance of the reaction of NO_2 on CaCO_3 particles in the troposphere is comparable to that of HNO_3 .

4. Figure 1. It appears that the black squares (left axis) and open circles (right axis) are the same data with the left and right axes scaled differently, making the data redundant. The right axis should be scaled to correlate with the left, thus requiring only one set of data points. The line appears to be arbitrary; instead, the authors should include the BET best fit line. The authors should also review Al-Hosney, H. A., and Grassian, V. H. *J. Phys. Chem. A*, 101, 1266-1276, 1997. This article presents an extensive discussion of surface adsorbed water that the authors should consider in their discussion.

Response: In the revised manuscript, this figure will be used as supporting information. The data are from the same data, but the two axis have different meaning, the left axis is based on directly measurement of infrared absorption, the right axis is the number of water monolayer based on BET fit results. With the right axis we could better describe the relationship between RH and the number of water monolayer.

5. Figure 2 and Figure 10. It appears that these two figures present the same data, merely plotted using different variables (although Fig. 2 has one less data point). This presentation is redundant. I suggest eliminating Figure 10 and moving Figure 2 along with its discussion (page 7121, line 13 through page 7122, line 7). The effect of surface adsorbed water on the reaction with NO_2 might fit more naturally after Section 3.3.

Response: Figure 2 and Figure 10 are based same dataset, but have different meaning. The figure 2 is to describe the influence of water vapor on reaction rate; this is

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helpful for the discussion on reaction mechanism, while Figure 10 discuss the impacts of RH on uptake coefficient.

6. Figure 4 and page 7123, lines 20-24. These plots appear to follow normal saturation curves consistent with the formation of a "passivation layer," where calcium nitrate products remain immobile and unreactive. Such plots could be fit with an appropriate saturation model; thus, the benefits of dissecting these kinetic plots into three regions remain unclear.

Response: Figure 4 shows the nitrate production rate, not the absorption rate of NO_2 . The nitrate production rate is the results of multi-steps. Based on the measurements, we believe that the reaction mechanism and reaction order were changing during the reactions processes. At the initial stage, the absorption of NO_2 was the rate determining step, one can use normal saturation curves; however, at the third stage, formation of the reactive surface sites was the rate determining step. Based on this reason, we could not simply use saturation curves to fit the results.

7. Figure 5. When calculating $\ln\{\text{dNO}_3/\text{dt}\}$ for region I of the 4.81×10^{15} molecules cm^{-3} data set, were all data points between 0 and 50 minutes used? Clearly the number of reactive surface sites is decreasing during this time period as evidenced by the saturation behavior. Given this observation, it would be best to fit the initial linear portion (e.g., 0 – 15 minutes) of this data to determine $\ln\{\text{dNO}_3/\text{dt}\}$. This analysis, however, would increase $\ln\{\text{dNO}_3/\text{dt}\}$ for the first two data points in Figure 5, resulting in a lower slope. Such an analysis might significantly challenge the concluded second order w.r.t. NO_2 for this reaction, which is already a stretch based on the slope 1.63 ± 0.23 . Given the uncertainty in this analysis, I am greatly concerned by the strong mechanistic conclusions presented for low relative humidity. The authors need to carefully consider the interpretation of their data, and should revisit this analysis and their mechanistic conclusions.

Response: We agree with the reviewer's comment. In the revised manuscript, we

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carefully re-analyzed the results, added one point, used the slope of the initial linear portion (e.g., 0-10 minutes) of this data to determine $\ln\{d\text{NO}_3^-/dt\}$, and we obtained a slope of 1.69 ± 0.19 .

8. Figure 6. Why are Kubelka-Munk units used in this figure? Why are absorbance units used everywhere else? Although Kubelka-Munk units theoretically apply to diffuse reflectance spectroscopy, several studies note the utility of absorbance units for adsorption studies and heterogeneous reactions. The authors should be consistent with the presentation of their data or they should include a discussion in the text explaining any differences.

Response: We agree with the reviewer's comment. In the revised manuscript, we changed the figure with absorbance units.

9. Figure 7 & Figure 8. Nitrate formation in Figure 8 appears linear for the NO_3^- ν_1 peak at 1048 cm^{-1} . The analysis in Figure 7 suggests saturation behavior for the NO_3^- ν_3 peak at 1344 cm^{-1} . Why is there a discrepancy? It is also very interesting to note that the surface adsorbed water levels off after 30 minutes, especially since calcium nitrate is of interest given its hygroscopic properties.

Response: In Figure 7 we would like to illustrate the formation of NO_2^- , so we did not display the NO_3^- ν_1 peak at 1048 cm^{-1} . The NO_3^- absorption at 1688 cm^{-1} is overlapped with that of H_2O at 1636 cm^{-1} , Gaussian peak fitting was used and thus has high uncertainty. In the revised manuscript, we used an IR spectrum with full scale. The water absorption at $3000\text{--}3600\text{ cm}^{-1}$ shows clearly increasing trend. The NO_3^- ν_3 peak at 1344 cm^{-1} is overlapped with the CO_3^{2-} absorption at $1380\text{--}1560\text{ cm}^{-1}$, so the NO_3^- absorption at 1048 cm^{-1} is more reliable for concentration analysis. In the revised manuscript, we used 1048 cm^{-1} for NO_3^- concentration both in Figure 7 and 8.

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