

Interactive comment on “Is the photochemistry activity weak during haze events? – A novel exploration on the photoinduced heterogeneous reaction of NO₂ on mineral dust” by Tao Wang et al.

Anonymous Referee #1

Received and published: 13 June 2019

This study evaluates the formation of nitrite and nitrate by light-induced heterogeneous reactions of gaseous NO₂ on TiO₂ used as a proxy for mineral dust. The obtained laboratory results are compared with the behavior of nitrite and nitrate from sampled ambient aerosols. There are number of previous studies on this topic and the results from this study add to the current knowledge on this topic. This study is within the scope of Atmospheric Chemistry and Physics. The work presented here is overall well done, however I am still reluctant to follow the authors' general conclusion in the importance of illumination conditions for nitrite and nitrate formation, mainly due to a

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few shortcomings of the study that need to be addressed and clarified before the paper should be considered for publication.

General comments: (1) The light intensities are given in mW cm^{-2} . How were these values measured? Are they integrated values in certain wavelength range? How do they correlate with the solar actinic flux? (2) The NO_2 mixing ratio of 15 ppm is extremely high. The authors mentioned Langmuir-Hinshelwood mechanism but in a number of previous studies it was demonstrated that the NO_2 uptake coefficients decrease with the increasing NO_2 mixing ratios up to 100 ppb. Higher mixing ratios than 100 ppb do not influence the uptake coefficients. (3) Generally, the data presented in this work had no error bars, therefore no uncertainties of those data can be evaluated. I generally feel that more solid evidences are need to arrive the conclusion.

Specific comments:

Line77: It is not mentioned what brand was used for the ultrapure water and how was tested the purity of the water? Line 79: It is not mentioned the purity of the air bottle. What is the level of VOCs in this bottle? Line 119: How was derived this equation? Where does it come from? Line 125: It would be better the equation to be presented as $(2.1 \pm 0.2) \cdot 10^{15}$ Line 131: "Based on earlier finds"...should state "Based on earlier findings" Line 169: The wavelength should be ≤ 390 nm. Line 172: The dot on hydroxyl radical should be on O atom and not on H atom. Line 197-198: These two questions are not properly formulated. For example "Whether" does not fit here. I am not native English speaker but I think the English language usage must be substantially improved in the core of the manuscript. Line 235: On which basis is this statement that "the irradiation tends to be weaker in winter"? Where is this applied? What solar zenith angle, latitude? Etc. Lines 293-295: The authors stated "low and high NO_2 concentration of 9.20 ppm and 21.45 ppm. These values are not concentrations but mixing ratios and both values are extremely high. Lines 313-314: This definition is very strange, "polluted aerosols" and "carcinogenic aerosols". Please change this.

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Supporting Information

On Y axis should be the intensity or spectral irradiance. It depends what values the authors measured and how were these values measured (see my general comment above).

Table S1: It is stated the velocity of SO₂ instead of NO₂. Table S2: The presentation of data is not scientific here. Please change.

Conclusions and Implications

I would be very careful to claim the importance of sunlight during the haze events, as you merely tested a few aerosol samples, and they cannot represent all aerosols types. Note your experimental conditions are often not atmospherically relevant (high NO₂ mixing ratios and light intensities). The findings should not be over-interpreted and stated with caveats.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-315>, 2019.

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