

Interactive comment on “Simulation of Heterogeneous Photooxidation of SO₂ and NO_x in the presence of Gobi Desert Dust Particles under Ambient Sunlight” by Zechen Yu and Myoseon Jang

Anonymous Referee #1

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This is an interesting study where the SO₂ and NO_x oxidation on mineral dust was investigated by means of simulation chambers, and simulated using the Atmospheric Mineral Aerosol Reaction (AMAR) model. Different dust particles (Gobi desert GDD and Arizona test dust ATD) were considered and their differences in reactivity and buffer capacity are discussed. Overall this paper is well written and addresses an important topic (mineral dust is an important category of aerosols), I would therefore recommend its publication once the authors have had a chance to discuss the comments (some are major) raised below.

C1

I have a conceptual problem with the AMAR model, which takes into account processes in three phases: the gas phase, inorganic salt-seeded aqueous phase and dust phase, implying that uptake is treated as absorption (according to Henry's law). However in many studies involving the uptake of trace gases on mineral dust, a Langmuir type behavior has been reported, showing an adsorption behavior that could typically contradict the assumption of absorption. Also, water and several gases have been shown to exhibit competitive adsorption properties, going against the absorption assumption. Could it not be that these two assumptions would correspond to two completely different humidity regimes? Maybe the authors could comment on that, and strengthen their assumptions in the manuscript.

Concerning the determination of the photoactivation parameters, have you checked how much is simply due to bleaching of the dye? That is a commonly reported issue in photocatalytic degradation of dyes on TiO₂. In addition, the organic compound may also directly react with the electron-hole pair changing the reaction mechanism given in (R2) to (R5). Do you have any indication that this is occurring with the selected dye? By the way, did you perform any elemental analysis of the two samples, or just for the GDD as ATD has a known composition? This should then provide a basis for explaining the difference in the photoactivation parameters.

Sulfate is very often considered as a poison for surfaces, as it passivates very rapidly reactive surfaces. However, the outcome of the AMR does show that (probably due to the absorption assumption discussed above). Have you built in some capacity to have surface saturation or not at all?

Figure 4 seems to show that the model does capture the nitrate formation at longer times. How do you explain this? Is there any renoxification process taking place in this system?

I would recommend to the authors to review their paper and clarify a few basic assumptions in their work.

C2

