Review of "Formation and composition of organic aerosols from the uptake of glyoxal on natural mineral dust aerosols: a laboratory study"

General comments

In this study, the authors investigated the heterogeneous reactions of glyoxal on real mineral dust in the CESAM simulation chamber, which is an upgraded vision of the previous studies. Multiple instruments were employed to perform complementary measurements of both gas and particle phases. Key influencing factors were carefully considered, including the presence or absence of irradiation and oxidant ozone, as well as varying humidity conditions ranging from <5% to 80% RH. The chemical composition of reaction products was characterized both online and offline, and uptake coefficients were calculated. This study highlights the substantial uptake of glyoxal by mineral dust and the resulting modifications in particle properties, including hygroscopicity, acidity, optical characteristics, and reactivity. The manuscript is well written. The contribution of this study is within the scope of ACP. There are only a few aspects that require further discussion, and the manuscript would benefit from reorganization, particularly in the results section, to enhance clarity and conciseness. Therefore, I recommend a minor revision before publication.

Major comments:

1. This study is impressive, with comprehensive techniques and results. However, the authors should refine the manuscript to ensure that key points are not overshadowed by excessive detail.

   For instance, while the methodology section contains valuable information, this is not a technical paper. Consider shortening this section and moving some details to Supporting Information (SI) if necessary. Additionally, a table summarizing the complementary functions of the instruments used would greatly enhance clarity.

   In the results section (3.3 Organic Composition), the authors provide a detailed discussion of organic fragments and molecules in the particle phase but lack clear linkages between them. Given the expected diversity of organic products, it is important to highlight which ones are most significant and why. For example, the role of oligomers and organic acids deserves emphasis. Reorganizing this section to focus on key products and scientific questions, rather than structuring it around the instruments used, would improve readability and impact.

2. Glyoxal concentrations play a potentially import role in the reactions. Why did the authors choose much higher concentration rather than atmospherically relevant values. i.e., 10-100 pptv? Was it due to technical limitations? Please provide an explanation.

3. Wall loss is a critical factor in accurately calculating the uptake coefficient and warrants further discussion. While its size dependence is addressed in the SI, it is important to clarify whether wall loss also varies with different RH conditions. Additionally, the authors state that the uptake coefficient in humid air is independent of concentration. To support this claim, please provide evidence or relevant references.

4. The authors did not observe uptake under dry condition. However, given that glyoxal is highly water-soluble—more soluble than methylglyoxal, which can undergo hydration in the gas phase even at <5% RH (Axson et al., 2010). One might expect some uptake to still occur in relatively dry air (though not necessarily at 0% RH)), but it did not. Can the authors provide potential explanations for this observation? Additionally, a closer look at the volume concentration curve, it shows a slight enhancement after glyoxal injection, though less pronounced than under humid conditions. I recommend exploring alternative methods to better illustrate and analyse the results in dry air.

Minor comments:

Line 116: Give full name of OA.

Line 127: Give full name of CESAM or add some explanation, e.g, the abbreviation in French for Experimental Multiphasic Atmospheric Simulation Chamber.

Line 181: Please show the FTIR spectra, e.g., in SI.

Line 457: Fig 1 a) RH=0%, is it real 0%? Or some value < 5% which is the lowest RH listed in table1. Please revise. Same comment for Fig. S4.1.

Line 485-486: Consider putting the result in presence of ozone in the SI.

Line 502: It is not clear why Fig 2 can confirm the statements above. Please reshape.

Line 573: Fig 4, please add the legend for the red and blue curves.

Line 599: The update and formation of OA process is reversible.

Line 610: Table 2: Do not use the symbol γ for OA formation. Consider using e.g., Y (yield) instead.

Line 625: Shen et al. (2016) also used α-Al_2O_3. Please add it.

Line 627: How long was the exposure time?

Line 631: Change "AlO_3" to "α-Al_2O_3".

Line 635: What are the values of update coefficients with BET surface area? Please calculate and compare them with the corresponding literature.

Line 685: Change 28 m/z to m/z 28. Same for 44 m/z and the other fragments. Please go through the whole manuscript and revise accordingly.

Line 688-690: Need more explanation for this observation.

Line 783: Do not use "seem to". Please change to another more appropriate word.

Line 929-936: Please add the comparison of uptake coefficients with Shen et al. (2016).

References:

Axson, J. L., Takahashi, K., De Haan, D. O., and Vaida, V.: Gas-phase water-mediated equilibrium between methylglyoxal and its geminal diol, Proc. Natl. Acad. Sci. U.S.A., 107, 6687–6692, https://doi.org/10.1073/pnas.0912121107, 2010.