

Interactive comment on “Effect of NO_x, O₃ and NH₃ on sulfur isotope composition during heterogeneous oxidation of SO₂: a laboratory investigation” by Zhaobing Guo et al.

Anonymous Referee #3

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Review of “Effect of NO_x, O₃ and NH₃ on sulfur isotope composition during heterogeneous oxidation of SO₂: a laboratory investigation” by Guo et al.

The topic of the paper is relevant for interpretation of isotope ratios of sulfate in the atmosphere. Unfortunately, the presentation and analysis of the paper is insufficient in my view for publication in a scientific journal. This relates to many categories that I consider important for a scientific publication, and I can only list a few points here:

Maybe most importantly, for all key figures, the numbers in the text disagree with what I see in the figures.

Examples:

C1

L 104: As shown in Fig. 2, the yield of SO₄²⁻ ranged from 0.0097 to 0.7795 g and the values of δ³⁴S were 2.9–4.8 ‰.

I see no sulfate concentration above 0.6 g in figure 2, and no delta values below 3.5 ‰.

L129/130: The yield of SO₄²⁻ ranged from 0.0081 to 0.6712 g with the δ³⁴S values of 1.6–2.9 ‰ (Fig. 3).

I see no sulfate concentration above 0.6 g in figure 3

Line 157: yield of SO₄²⁻ ranged from 0.0237 to 0.9469 g with the δ³⁴S values of 0.8–4.3 ‰ (Fig. 4).

I neither see the very small or very high sulfate values mentioned in the text in Fig. 4. The range in figure 4 (1.2–2.5 ‰) is very different from what is written in the text.

I wonder what is going on there. This looks like either the authors have not checked the consistency between text and figures and need reviewers to find this out, which is not acceptable, or a crucial piece of information is missing.

The methods section provides almost no information on experimental conditions and setup, I have to guess how experiments were conducted. I wonder whether there is any relation of the samples collected outside and the lab experiments. I understand from the description that the concentrations of the reactants are in the % range, is this then representative for the atmosphere? Why were the various flow rates used? How are samples collected in the lab experiments? These are only a few questions.

In the discussion, the authors completely mix the interpretation of their results with what was found in previous studies and it is not possible to clearly understand when they are reporting a result from a previous study and when they put the results of their experiments in light of previous findings. I think this paper would require a clear separation of results and discussion. And then still a careful distinction on what is new from the present paper and what has been found before.

C2

Quantitatively, the epsilon value of the overall process (1.3 ‰ line 192), which enables source partitioning, falls completely from heaven. Where does it come from? Does the evaluation include the assumption from the literature that the source isotopic composition is 2.7 ‰ $\pm$ 2 ‰ (line 184)? That would result in a huge error. In the present version of the paper, Figure 5 shows some synthetic Rayleigh fractionation curves, but I see no relation to the data, other than the unexplained value of 1.3 ‰

All the data shown in Figure 1-4 should have associated error bars.

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