

Harris et al. report their measured sulfur isotope fractionation factors for the main oxidation pathways of SO<sub>2</sub> to sulfate: OH in the gas-phase and H<sub>2</sub>O<sub>2</sub>, O<sub>3</sub> and O<sub>2</sub> catalyzed by transition metals in the aqueous-phase. These measurements have not been done before, so this paper fills a huge hole in figuring out how to interpret δ<sup>34</sup>S of sulfate aerosol and SO<sub>2</sub>. The paper is very well written and I have only some very minor comments detailed below. I recommend publication after these minor comments have been addressed.

Note that I am not intimately familiar with their experimental laboratory techniques other than the isotope measurements themselves. However, from what I can tell the experimental technique seems very reasonable and the authors seem to have carefully considered possible interferences.

More detailed comments are below:

Equation 9: Define “*n*”

P23963 L13: Change “reactions” to “reactants”

P23970 L11: Remove “just”

Figure 6 and associated discussion in text: I don’t know what a facsimile model is and don’t understand what this figure is trying to demonstrate.

P23977 L18-19: 0.6 ppb background of what species?

Paragraph beginning on P23981 L19: Interpretation of the ice core data in Alexander et al. (2003) is speculative. Combined observations and modeling of Δ<sup>17</sup>O suggests that metal catalyzed oxidation by O<sub>2</sub> is negligible in the southern hemisphere during the Holocene (Sofen et al., 2011). Although it is likely more important in the glacial period due to the increased dust source, it may still represent a very small fraction of total sulfate formation. Based on your measured fractionation factors, can you estimate how much of an increase in the metal catalyzed oxidation pathway is needed to explain the observations reported in Alexander et al. (2003) based on this explanation alone? Is this reasonable?

Table 2: Don’t need to write “by Shuhei Ono (2010)” since he is an author.