This manuscript quantifies the sulfate formation pathways from 4 to 22 December 2019 in Nanjing by proposing a new method of simultaneously measuring sulfur and oxygen isotope compositions. The authors conclude that sulfate in $PM_{2.5}$ is mainly from a secondary source with SO_2 homogeneously oxidized by OH and heterogeneously oxidized by H_2O_2. Overall, the manuscript is well-written, and the method is reasonable. I have a few points that could be addressed to strengthen the manuscript and some minor comments.

General Comments:

1. The method seems applicable, but the authors need to explain the calculations better. I find it hard sometimes to understand how the result is derived. For example, the authors mentioned that the $\delta^{18}O$ value of primary sulfate is about 38 ‰ in Line 296 before they pointed out it was based on Formula (5). That is confusing. Why there are contribution ranges on each day in Table 1, instead of a single number like in Table 2?

2. Is it possible to add more data points in Figure 7? It seems three are not robust enough to rerive the linear relationships.

Minor Comments:

Line 69: Define RH here instead of in Line 186.

Line 98/320: I did not find references related to Holt et al.

Line 168-170: I am not sure why high CO is indicative of local emissions. It can be transported by a long range.

Line 192: What does the negative -2.9 mean here? Is it possible to have negative values?

Figure 3: Legend of PM2.5 is wrong. Should be sulfate.

Line 249: The average of 51.6% seems just a little higher than 50%. I suggest to say that most of the days (seems 7 out of 11) have more than 50% contributions from heterogeneous oxidation.

Figure 7: Are the three dots corresponding to three kinds of water? Better to describe it in the texts or figure title.

Line 324: $f_{SS\text{-}OH+}$ should be $f_{SS\text{-}OH}$+