

The reviewed manuscript presents a method for collection and concentrating OCS (the most abundant atmospheric sulfur species) from air. The concentrated OCS sulfur isotopes are then measured by IRMS. Having such measurement method is an important contribution. The results seems robust, and the manuscript is usually well written.

The main problem I see regarding the suitability of this method for atmospheric sampling is related to samples preservation. So far, it is shown only for 7 days, which is not always enough to get a sample (i.e. from ship cruise or remote location) back to the lab and to analyze it. Also, no tests were done for preservation effect on isotopes. The authors themselves recognized this point as missing and wrote that "Further investigation of preservation of OCS concentrations and isotopic integrity during the storage of the adsorption tubes with respect to storage temperature and materials of the adsorption tube are currently underway."  
I recommend that the results from these investigations will be included as part of this paper.

Detailed comments:

Page 1 (abstract), Line 17 – "7 inch tubes (1 cm<sup>3</sup>)" – better to give both dimensions in cm.

Page 1 (abstract), Line 17 "preserving samples" – need to add "up to 7 days", or to whatever the new investigations show.

Page 1 (abstract), line 20 – should be "lack of diurnal variations".

Page 1 (abstract) line 20 and Page 11 line 6 – It appears here as this method enables measurements also of <sup>33</sup>S in air samples. In fact, this has worked out only for standards, and an interference prevented measuring this in air. Please correct.

Page 1 (abstract) – Portability is reported here based on the sample tubes. But the size and weight of the sampling system is also a main issue. Please report this, at least in the method section.

Page 3, line 30 – Please explain the need for both glass-beads, Tenax TA, and Porapak N. On which of those is the OCS trapped? All of them?

Page 4, line 7 – "less than -110C". Can you be more specific? It can be important for someone trying to use this method. Also, if you get as low as liquid N<sub>2</sub> temperature, than some O<sub>2</sub> (having higher boiling point) will be liquefied.

Page 6, line 5 – Rephrase. It should be explained in a different sentence that standard A is not pure OCS because of ...

Page 7, line 11 – Refer to figure 3a.

Page 7, line 23-24 – Not clear. Please rewrite.

Page 8, Line 16. This is only true if the blanks always have atmospheric values. Please consider how it will affect the uncertainty in atmospheric measurements, if the blanks are in the expected range of OCS sources.

Page 9, line 3 – The variability in concentration reported by Montzka (2007) is seasonal, not diurnal. Any possible explanation for the diurnal variability, and how it is related to what reported for other sites?

Page 9, line 12 – Is this 1 permil change (not this small) is also accompanied by a change in concentration? Can this be related to the blank problem? Please explain in the text.

Page 9, line 19 – What do you mean by “reasonable signals”

Page 9, line 20 – “We earlier discussed”, where? In previous papers by the same group? I tried to read this in Hattori et al. (2015), but it is not clear to me. Please explain in more detail here how this isotopic balance was done.

Also, if both the earlier value and the new value agree with the SSA value, then this is not a strong constraint for atmospheric OCS d34S.

Page 10, Line 3 – Please report the magnitude of these effects, and estimate how this should influence the atmospheric OCS.

Page 10, line 29 – These “further investigations” seem critical to establish the method. Are there new results since the manuscript was first submitted?

Page 11, line 26 – Please be more specific. Instead of “some shortcomings in terms of sample amount” write down the ratio of sample amounts in the two methods.

Page 11 - The conclusion section mentions for the first time another method (by Said-Ahmad, 2017). The mentioning of other existing methods should be done in the introduction, and comparison with other methods should be in the discussion, and include advantages and disadvantages. Similarly, writing in the last lines of the paper that the current method has the advantage of enabling carbon and oxygen isotopes measurements seems out of place, since this is not demonstrated in the current manuscript. However, this can be mentioned in the discussion.

The discussion should also refer to the new paper by Angert et al., mentioned in the online discussion. Are the atmospheric values reported by the two papers identical, considering all experimental uncertainties? If not, is this a methods issue, or a real geographic effect?

Figure 3a – Maybe better to show versus sampling time (and not run number). Also the 10min is black in the legend and gray in the figure.

Figure 8 – Better to start the y-axis at 80%. Also, need to show longer preservation periods.