The study by Alessandro Zanchetta and coauthors deduces stratospheric lifetimes from vertical profiles of carbonyl sulfide and methane from AirCore measurements and ACE-FTS satellite observations. The analysis is sound and rather comprehensive, comparing slightly variant methodologies of estimating lifetimes and pointing out caveats with respect to, e.g., regional sampling and observation altitude. The manuscript is generally well presented and well written and fits into the scope of ACP.

Authors' response (AR): *We thank the Referee for the positive comments and for the generous effort in providing this detailed feedback. We provide our point-by-point responses below.*

One major aspect that, in my opinion, should be improved is the length of the paper and the amount of information presented. Specifically, I wonder if all 13 figures and 11 tables are needed. In that context, please consider the following:

- Table 2 contains important information, but it is not essential for the main message of the paper and could be shifted to a supplement.
- Table 3 also contains information that should be provided by all means, but the paper would not suffer from this being included in a supplement rather than the main body of the paper.
- Figure 4 could also be shifted to a supplement. It is important to have this information, but how these profiles lead to the actual burden numbers is not visible in the figure, so it is not essential for the main paper.
- Figures 5 – 7 and 8 – 10 take up a lot of space. I wonder if it is not possible to combine these into two figures (one for COS and one for CH_4) without overloading them.
- It is good to show all results and important parameters for individual flights and analyses in Tables 4 – 6 for COS and 7 – 9 for CH_4. But the important pieces of information are given and discussed in the text and these long and detailed tables would be better off in a supplement. Also, the information displayed in Figures 11 and 12 is already given in Tables 5/6 and 8/9 for COS and CH_4 respectively.
- Does Figure 13 add new information compared to Table 10? In my opinion, the graphical representation of these data has limited added value, so showing just the table would be sufficient. But if you want to keep the figure, then the table with the more detailed information could be moved to a supplement.

AR: We agree that many of these tables and figures are not essential additions to this manuscript. Consequently, the following modifications have been implemented:

Table 2 is now Table S1 in the Supplement.

Table 3 is now Table S2 in the Supplement.

Tables 4-6 and 7-9 are now Tables S3-S5 and Tables S6-S8 in the Supplement, respectively.

Figure 4 is now Figure S19 in the Supplement.

Figures 5-7 have been incorporated in one multi-panel figure, now Figure 4 (a-c).

Figures 8-10 have also been incorporated in a single figure, now Figure 5 (a-c).

We thank the Referee for this suggestion and agree that Table 10 can be moved to the Supplement, where it can be found now as Table S9. We would nevertheless like to retain Figure 8 (previously Figure 13) in the main text, as it offers an intuitive visual summary of a core finding, while the table provides the detailed numerical support in the Supplement.

Besides the number of figures and tables, there is quite a bit of redundancy in the test as well. Essentially the same numbers for COS and CH_4 lifetime and stratospheric sink estimates from the two different methods (Plumb and Ko, 1992 vs. Volk et al., 1997) and for polar vs. mid latitudes are given in a similar way in Section 3 (lines 297 – 310 and 339 - 347), Section 4 (lines 380 - 382 + Section 4.1 and Section 4.2) and Section 5 (lines 506 - 516). While this information does fit into the flow in each individual Section, it may be worth avoiding some of this redundancy by cross referencing.

AR: We agree that cross referencing helps improve the readability of the manuscript. We now present the results in Sect. 3.2.2 for COS and Sect. 3.3.2 for CH_4. Throughout Sect. 4 (and its subsections) the lists of results have been replaced with references to their respective Results sections. We have kept the numerical values in Section 5 (Conclusion), to keep them readily accessible for readers and to keep a direct link with the Abstract and the Introduction.

Note that these suggestions are not meant to push the paper below a certain page or word limit because shorter is always better. But I had the impression that the take-home messages of the study are somewhat hidden underneath the wealth on information shown.

Besides the general recommendations to make the paper more concise, I have a few specific suggestions and technical corrections:

Introduction:

The introduction almost exclusively focuses on COS, while in the title and the rest of the paper, CH_4 is treated in parallel. I suggest adding one or two paragraphs on CH_4 in the introduction as well.

AR: We agree that the introduction lacked a discussion of CH_4. Accordingly, we have added the following paragraph to the introduction (Lines 48-59):

Methane (CH_4) is the second most important anthropogenic greenhouse gas in terms of radiative forcing, after carbon dioxide (CO_2) (Myhre et al., 2014). Its current tropospheric abundance is over 1900 ppb and has been constantly increasing in the past 20 years (Thoning et al., 2022). CH_4 is produced by both natural and anthropogenic sources. Among these, some of the major natural sources are wetlands, fresh waters and oceans, while the anthropogenic include livestock farming, agriculture, landfills and fossil fuels (Saunois et al., 2025). Its major sink is oxidation by OH• in the troposphere (Lelieveld et al., 1998; Saunois et al., 2025). With an average atmospheric lifetime of about 9 years, CH_4 can enter the stratosphere (Rohs et al., 2006; Saunois et al., 2025), which acts as a CH_4 sink. There, this gas reacts with stratospheric-abundant oxidating species such as excited atomic oxygen, $O(^1D)$, radicals such as OH•, or undergoes photolysis (Rohs et al., 2006). Moreover, stratospheric CH_4 chemistry affects ozone (O_3) concentrations and the oxidation of stratospheric CH_4 makes it the main sources of stratospheric water vapor (Han et al., 2025; Rohs et al., 2006). However, uncertainties in the magnitude of chemical loss of stratospheric CH_4 remain large due to variability in atmospheric transport and chemistry (Lelieveld et al., 1998; Portmann et al., 2012; Saunois et al., 2025).

Lines 39/40: an explicit reference for the COS tropospheric lifetime would be appropriate. And for COS being able to reach the stratosphere, a more original reference such as Crutzen (1976) would be more appropriate than the ones cited.

AR: We thank the referee for noticing the missing reference to the original study. The reference to Crutzen (1976) has been added.

Lines 43 – 45: Again, the chosen references appear a bit arbitrary. For example, Vernier et al. (2011) don't explicitly discuss the COS contribution but rather the effects of cumulative sulfate injection from volcanic eruptions. I suggest the Kremser et al. (2016) review paper, where the tropospheric budget and stratospheric sink of COS is discussed in detail and relevant literature is cited.

AR: We acknowledge the mismatch between the used reference and the presented information. We have replaced the reference to Vernier et al. (2011) with Kremser et al. (2016) in the revised version.

Methods:

Lines 100 – 105: It would be nice to show a comparison between the actual AirCore and ACE-FTS profiles. It's not essential to actually show this in the paper, so such a figure could go into the supplemental material.

AR: We agree that a comparison between AirCore and ACE-FTS could be an interesting addition. However, since this has already been presented and extensively discussed in Zanchetta et al. (2026 – see Figs. 9 and 10), we would prefer to refer to the existing literature and have therefore included the following sentence in the revised version:

The resulting 502 mid-latitude and 1681 polar latitude ACE-FTS profiles were averaged for each latitudinal interval. The resulting two averaged profiles **(see Figs. 9 and 10 in Zanchetta et al., 2026)** were then treated identically to the AirCore-derived profiles [...]

Line 114: I tend to agree that this is probably a fair assumption, but maybe you can add one sentence to explain why it is justified for the gases considered.

AR: the sentence has been modified as follows:

For simplicity**, given the smaller stratospheric abundances of the species when compared to the tropospheric portion of their profiles**, we assume that the ratio of the atmospheric mean mole fractions can be approximated by the ratio of representative tropospheric mean mole fractions.

Line 132 – 134: I find this explanation a bit confusing. Did you use the Prather et al. estimate of 116 years for the full period, or did you apply a 2.1 % per year correction, even though you state that the trend is not significant? Please clarify!

AR: the sentence has been modified as follows:

Moreover, the N_2O lifetime has been reported to depend on solar activity, which causes a variability of approximately 7 % (Prather et al., 2015). We therefore chose to infer N_2O lifetime in 2019, 2021 and 2023 from the 116 ± 9 years estimate of Prather et al. (2015), given the small –2.1 ± 1.2 % per decade decreasing trend (Prather et al., 2023) is not significant over such a short time span **and that any variability due to solar activity lies within the reported uncertainty.**

Lines 163 – 170: In my opinion, limiting the analysis to reasonably low N2O mole fractions so that a tropospheric influence is minimized is important. This is a fundamental difference to the study by Karu et al., who analyzed only data with N_2O > 317 ppb due to the altitude limitation. This may explain why that study compares least well to your analysis and other COS lifetime estimates in the literature, and it is worth highlighting that here or in one of the later sections.

AR: We agree that this would be indeed a major cause of discrepancies between this study and the one presented in Karu et al. (2023). This has been addressed in the Discussion section (see further comments, later in this document).

Results:

Lines 287 – 288: Either here or in Section 2.3, please add the actual number of ACE-FTS profiles that were averaged.

AR: this information was already present in Sect. 2.3 (now Lines 127-128):

The resulting 502 mid-latitude and 1681 polar latitude ACE-FTS profiles were averaged for each latitudinal interval.

Line 329: For CH_4, why are the uncertainties for satellite data as large as for campaigns, different than for COS?

AR: We thank the referee for pointing out this difference between the two species. We speculate that one reason behind this difference may be found in the QCLS precision, which is relatively lower for COS than for CH_4. Therefore, when comparing the averaged ACE-FTS profiles, the effects of instrumental precision are probably more noticeable in the COS results than in the CH_4 ones. Moreover, we observe that the COS-N_2O local slopes obtained with ACE-FTS "flatten" already at N_2O > 200 ppb, while for most AirCore profiles this happens at higher N_2O levels. On the other hand, ACE-FTS and AirCore profiles exhibit more similar behaviors for CH_4, which may be reflected in the obtained uncertainties.

Discussion:

Lines 393 – 397: I think that the statement that Karu et al. (2023) "introduced" another point of uncertainty is misleading here. They did not introduce this but, due to the very limited altitude range of commercial aircraft, had to use data with a very limited altitude (and consequentially N_2O, see my comment above) range. In my opinion, this warrants more discussion and explains why their lifetime estimate differs from your and other studies.

AR: Yes, we have tried to make this difference clearer as follows:

A further discrepancy between the studies arises from the different altitudinal ranges covered by the observations. Our profiles cover altitudes roughly between 15 and 22 km, comparable with the highest altitudes observed by Krysztofiak et al. (2015) and Barkley et al. (2008). While Toon (1991) and Toon et al. (2018) measured COS vertical profiles up to 40 km, **Karu et al. (2023)'s observations were limited to between 10 and12 km altitude. At such low altitudes, the mixing of lower stratospheric with tropospheric air is very likely to bias the tracer-tracer correlations and the consequent lifetime estimates.**

Lines 404 – 405 and Table 10: I suggest including stratospheric sink estimates from the important modeling studies by Brühl et al., (2012) of 35 Gg S/yr and Sheng et al. (2015) of 40.7 Gg S/yr. These papers do not explicitly state stratospheric lifetimes, but that also goes for some of the cited papers (e.g. Weisenstein, 1997).

AR: The suggested references have been added to the Table.

Lines 439 – 440: Please discuss this in more detail (cf. my previous comments on the comparison with the Karu et al. estimate)!

AR: We have highlighted the discrepancies with the study of Karu et al. (2023):

In the case of Krysztofiak et al. (2015) and Karu et al. (2023), the slopes of the tracer-tracer regressions were reported, and it was possible to recalculate the stratospheric lifetime and sink of COS using new estimates of the COS and N_2O tropospheric burdens. **It is**

important to notice that in the case of Karu et al. (2023), due to the limited altitudinal range of the observations, the tracer-tracer regression may have been biased by mixing with tropospheric air, which has probably affected the final lifetime estimates. The newly calculated lifetime results obtained with our method [...]

Technical corrections:

Line 206: it should be “advocate” instead of “advocates”

Line 233: Please replace the term “evident criticalities” (e.g. by “atypical behavior” of “distinctive behavior”)!

Line 252: This reference is meant to be Figures 5 – 6 and 8 – 9, correct?

Line 290: Figures and tables should be referenced in order of appearance. So please refer to Table 5 prior to Table 6 or swap the numbering of these tables!

Lines 326 – 327: There seems to be a grammar issue. I suggest: “The application of the Plumb and Ko (1992) method leads to an average slope at mid latitudes of 0.2537 ± 0.0022, while at polar latitudes it is 0.2381 ± 0.0063.”

Line 492: It should be “applied in this study”.

AR: All the technical corrections have been implemented.