

Overall, the authors have done a good job addressing the concerns I raised regarding the original manuscript. Moving many of the figures and tables to the Supplement has made the manuscript substantially easier to follow and has sharpened the focus on uncertainties associated with age-of-air calculations. The addition of Section 4.3 also more clearly highlights differences in the origins and transport histories of the sampled air masses, helping the reader interpret the variability observed among the individual AirCore profiles. The additional references provide useful context, particularly regarding historical trends in OCS.

Despite these improvements, the primary take-home message of the manuscript remains largely unchanged from the initial submission. I therefore reiterate my assessment from the first round of review that this work provides a useful contribution by highlighting uncertainties in age-of-air estimates arising from both experimental uncertainties and the choice of calculation methodology. Developing robust and internally consistent age-of-air records is important for identifying and interpreting long-term changes in the Brewer–Dobson circulation. Overall, the revisions have improved the clarity and presentation of the manuscript and have more effectively communicated the significance of these uncertainties.

I have no concerns with the manuscript being published in its current form; however, I have a few minor comments that I believe would further improve the manuscript (see below).

AR: We thank the Referee for the positive feedbacks and for the effort spent in providing another round of careful comments. Below, we have provided point-by-point responses to the comments. Overall, we are confident that these modifications have improved the manuscript's quality and readability. We thank the Referee for the insightful discussion.

Minor Comments

Line 40: Add “primarily through tropical upwelling” to clarify the primary pathway for transport into the stratosphere.

AR: It has been added.

Line 52: Consider revising to: “CH₄ oxidation serves as a major source of background atmospheric CO.”

AR: It has been revised as “*Its major sink is oxidation by OH• in the troposphere, which serves as a major source of background atmospheric CO (Lelieveld et al., 1998; Saunio et al., 2025)*”.

Line 61: I suggest changing “lifetimes are defined as” to “lifetimes are inferred from.”

AR: It has been modified.

Line 63: “For species with only stratospheric sinks, atmospheric and stratospheric lifetimes are equal (Brown et al., 2013; Volk et al., 1997).” While this statement is correct, it may be misleading in this context because it does not apply to either OCS or CH₄. I suggest clarifying this distinction.

AR: The specification has been added: “For species with only stratospheric sinks, atmospheric and stratospheric lifetimes are equal (Brown et al., 2013; Volk et al., 1997), *while for species such as COS and CH₄ these lifetimes do differ*”.

Line 69: I suggest omitting the somewhat subjective statement “reducing contamination risks during field campaigns,” as anomalous measurements can still occur in individual AirCore profiles.

AR: The sentence has been removed.

Line 85: It would be useful to note that AirCore profiles can provide substantially higher vertical resolution than ground- or satellite-based remote-sensing retrievals in the upper troposphere. This advantage has been demonstrated for carbon monoxide by Gurganus et al. (2026; see their Figure 10).

AR: The following sentence has been introduced: “Knowing the analyzed number of moles it is possible to link each aliquot to the sampling altitude and, therefore, retrieve the collected altitude profile (Karion et al., 2010; Membrive et al., 2017; Tans, 2022). *These profiles can provide higher vertical resolution compared to remote sensing techniques (Gurganus et al., 2026).* The profiles of this study were retrieved following the approach described in Membrive et al. (2017), except for the one collected with University of Bern’s double-sided AirCore.”

Section 2.4.2: Please add a statement noting that global OCS measurements remain very sparse, which contributes considerable uncertainty to estimates of the tropospheric OCS burden.

AR: This has been specified as follows: “*Global COS measurements remain very sparse and therefore the uncertainties associated to the tropospheric COS burden remain generally high (Hu et al., 2021; Kaushik et al., 2026).* In this study, to obtain representative tropospheric mole fractions for COS, N₂O, and CH₄, we followed the approach described in Andrews et al. (2001), which was previously used in other studies on age of air estimations (Andrews et al., 1999; Boering et al., 1996). Andrews et al. (2001) defined the stratospheric boundary conditions of CO₂ by taking 12-months running means from surface data collected at Mauna Loa (19 °N, MLO) and American Samoa (14 °S, SMO), delayed by two months.”

Section 2.5: I suggest moving the discussion of the Volk et al. (1997) method to the beginning of Section 2.4, where the Plumb method is introduced. Introducing an additional analysis method after the data have already been discussed makes the methodological development somewhat disjointed.

AR: Following this suggestion as well as the Editor’s comments, Sect. 2 has been split into Sect. 2 “Datasets” and Sect. 3 “Methodology”. Sect. 3 now presents first all steps regarding the methodology of Plumb and Ko (1992), followed by all steps needed to implement Volk et al. (1997) and finally the assumptions behind the N₂O stratospheric lifetime estimate.

Figure 4: Consider removing the outlier profiles that are excluded from the analysis in Section 3.2.1 (KRNa and SOD3-b).

AR: We understand the reasons behind this suggestion. However, we prefer keeping the profiles considered outliers in Figure 4, as we believe this figure provides an indubitable visual proof of the different behavior of such profiles when compared to the ones adopted in lifetime calculations. The figure supports the paragraph explaining the data refinement performed before calculating the stratospheric lifetimes.

Line 329: A more detailed discussion of the differences in the ACE-FTS tropopause CH₄/N₂O slopes is warranted, as these differences appear to produce the most significant discrepancy in the inferred lifetimes between the Plumb and Volk methods.

AR: Following the Referee’s suggestion, the following sentences were added within the discussion over the CH₄ stratospheric lifetime estimates (Sect. 5.2): “These latter estimates are associated with larger uncertainties with the Volk et al. (1997) method due to larger uncertainties regarding the extrapolated slope at the tropopause for these datasets. This is due to the lower number of datapoints over which the linear regressions are performed, which affects in particular the bootstrapped values at the tropopause retrieved following Volk et al. (1997). *Moreover, the slopes at tropopause level inferred from ACE-FTS data show larger differences between different latitudes. We speculate that this may be due to the more formally correct nature of the Volk et al. (1997) method, which can better capture stratospheric transport features. In fact, as will be illustrated in Sect. 5.3, it is possible that the AirCore samples we collected at polar latitudes still reflect mid-latitude air features, while ACE-FTS averages over long periods may be more representative of polar air masses*”.

Line 434: Please insert the missing reference.

AR: The reference referred to a Figure that is now in the Supplement, and has been corrected.

Figure 9: I find the introduction of partial-column abundance at this point as a metric for comparing ACEFTS and AirCore unnecessarily complex. A comparison based on mean lower-stratospheric mixing ratios would, in my view, be more intuitive, although I recognize that variations in tropopause height may make such a comparison difficult to define consistently.

AR: We acknowledge that introducing partial column abundances as a last point in the discussion may add a layer of complexity to the message this study wants to communicate. However, as the Referee highlighted, we believe that such metric provides a more consistent estimate than one based on lower stratospheric mixing ratio, due to variations in tropopause height which could affect the accuracy of these estimates.