

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).

### **Minor Comments**

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

**Line 52:** Consider revising to: “CH<sub>4</sub> oxidation serves as a major source of background atmospheric CO.”

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

**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 CH<sub>4</sub>. I suggest clarifying this distinction.

**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.

**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).

**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.

**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.

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

**Line 329:** A more detailed discussion of the differences in the ACE-FTS tropopause CH<sub>4</sub>/N<sub>2</sub>O slopes is warranted, as these differences appear to produce the most significant discrepancy in the inferred lifetimes between the Plumb and Volk methods.

**Line 434:** Please insert the missing reference.

**Figure 9:** I find the introduction of partial-column abundance at this point as a metric for comparing ACE-FTS 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.