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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Carbonyl sulfide in air extracted from a South Pole ice core: a 2000 year record" *by* M. Aydin et al.

Anonymous Referee #3

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I consider this paper an important contribution, which a) extends our historic record of a bigeochemically interesting gas considerably, b) adds further confidence into COS as a tool for paleoatmospheric studies, c) presents further evidence for a significant anthropogenic source that lead to a steep, unprecedented rise of atmospheric COS during the 19th and 20th centuries. This is a well-done paper that merits rapid publication. The paper presents new data on COS in air extracted from a South Pole ice core (SPRESSO). Previous records of COS from ice cores and firn air covered the era to 350 years before present (Sturges et al., GRL, 28, 2001; Aydin et al., GRL, 29, 2002; Montzka et al., 2007). This paper extends our paleoatmospheric record of COS to 2,155 years before present. The data are based on solid experimental work. The methods applied for drilling, extraction and analysis of gas and dating have been

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thoroughly investigated and are described in previous papers of the authors. The new COS record presented here confirms previous findings, that the preindustrial mean atmospheric mixing ratio of COS was roughly 25-30% lower than today (in the range of 300-350 pptv), and that it had a substantial natural variability on centennial time-scales (varying between approximately 300 and 400 pptv). This variability is yet unexplained, and likely is climate-related. The authors discuss this variability in the light of our current knowledge on the sources (photochemical production in seawater, volcanos, ..) and sinks (uptake by land plants, hydrolysis in seawater, OH reaction …) of atmospheric COS. They adopt several parameters as potential proxies for climate-related effects on the major biogeochemical COS fluxes, and relate published records of these parameters to their COS data (discussed in terms of deviation from the mean and from the trend). Radiative forcing due to solar variability is assumed as a proxy for the marine photochemical COS production, forcing due to volcanism as a proxy for volcanic emission of COS, hemispheric mean temperatures are assumed to affect hydroloysis lifetime of COS in seawater. Although the authors are able to explain part of their observed variability of COS on plausible arguments (like the positive anomaly of COS and volcanism during the little ice age cold anomaly), it is no surprise that their COS record as a whole does not co-vary uniformly with the proxy records. Given the complexity of the COS cycle and our currently poor quantitative knowledge the discussion necessarily is to a large extend speculative. More reliable proxies for the dominant source and sink terms of the COS cycle are needed to understand the observed variability.

MINOR SPECIFIC COMMENTS: Page 2, 1st paragr.: “there is some debate regarding the importance of COS compared to other stratospheric sulfate precursors …”: you might add the SPARC-report (Thomason and Peter (eds, 2006) as a reference, which presents some original new information on that in its modeling chapter.

Caption of Figure 2: I presume the open squares are the outliers ? If so, say so.

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