Atmos. Chem. Phys. Discuss., 2, S267–S269, 2002
www.atmos-chem-phys.org/acpd/2/S267/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparing forward and inverse models to estimate the seasonal variation of hemisphere-integrated fluxes of carbonyl sulfide" *by* A. J. Kettle et al.

Anonymous Referee #1

Received and published: 25 July 2002

This paper describes the use of a simple two-box model (Northern and Southern Hemisphere boxes) to deduce the hemisphere-integrated COS flux from published COS time-series data. Hemispheric fluxes are calculated as a linear combination of a steady-state solution and a time varying perturbation. By applying two independent approaches to determine the latter, one that makes no assumption about the functional form of the total column COS time series and one that forces it as a cosine function, the authors derive a consistent steady-state COS flux from the Northern to the Southern hemiphere. This behaviour has superimposed on it seasonal fluctuations in hemispheric sources and sinks. Overall the model outputs agree well with published COS data, however there are unresolved issues regarding the nature of Northern Hemisphere COS removal (i.e. plant/soil uptake during the boreal summer

vs oceanic uptake in the boreal winter).

This paper clearly constitute a valuable contribution to the subject area, however my major criticism is that the text is in general overlong and grammar/ sentence construction is in places a little clumsy and repetitive. Together these serve to detract from clarity and impact. Largely the problem is not that too much irrelevant information is included (although some content could be cut, see below), but rather that the text is simply not concise enough. The authors should attend to this, removing unnecessary levels of detail in order to maximise the scientific impact of this work. I could certainly en visage shortening the text by about 20% without loss of impact.

There are also some specific issues that the authors might like to consider:

Introduction: Page 579, line 9. The authors state that "the impact of COS on the chemistry and radiation budget of the stratosphere is not necessarily of major importance". This might be better if phrased "the role of COS on the chemistry and radiation budget of the stratosphere is still open to some debate".

The statement immediately following (lines 10 and 11) seems a little weak as justification for the study.

Methods: Page 580, line 25. What is the nature of the uncertainties relating to the source/sink terms in Fig 1b? This requires a short explanation.

Page 581, line 27 onward. This section on CO2 seems overlong. Although there are clearly similarities between COS and CO2 atmospheric behaviour which it is useful to point out, the level of detail afforded to the case for CO2 seems unnecessary and irrelevant. I recommend shortening this to a few lines only.

Constraining information: Page 590, lines 9-12. It is not necessary to list all of the ground stations in the text as these are listed in Table 1. Because this table clearly shows location co-ordinates I do not believe that Fig 4 is necessary and it should be deleted.

ACPD

2, S267–S269, 2002

The use of "data" in the singular (e.g. "data is") and "none" in the plural (e.g. "none is") are both incorrect and should be rectified.

Figure 8. Is this needed ?

Interactive comment on Atmos. Chem. Phys. Discuss., 2, 577, 2002.