Atmos. Chem. Phys. Discuss., 2, S533–S535, 2002
www.atmos-chem-phys.org/acpd/2/S533/

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.

A. J. Kettle et al.

Received and published: 23 October 2002

We appreciate efforts of the referees and editors in taking the time to review this manuscript. Most of the comments were positive, and there were some suggestions for improvement. We have addressed the comments of the referees on a point by point basis in a separate document addressed to the editor. We provide a summary here for the ACP readership.

Both referees were impressed by the aims and objective of the work. Referee 1 has stated that 'this paper clearly constitutes a valuable contribution to the subject area', and Referee 2 has stated that 'overall ... this is a valuable contribution to the field, and the authors have shown that the slightly unconventional modelling approach is a good tool'. The strength of this paper is that it presents two independent means to deduce

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the seasonal variation of the hemisphere-integrated fluxes of COS. That COS fluxes might have a seasonal cycle was suggested in a previous manuscript using databases and forward modelling approaches. However, the magnitude of the seasonal cycle was very uncertain. This inverse modelling approach has found broad agreement with the earlier work with a much smaller uncertainty. Although we have developed the theory for COS, the approach can probably be applied to any chemical tracer whose atmospheric lifetime is longer than the interhemispheric exchange time. There are a number of chemical species whose atmospheric mixing ratios are well-known but whose sources and sinks are poorly constrained.

The referees have noted some possible weaknesses in the work. Referee 1 thinks that the work is overlong and that it might be reduced by 20% without loss of impact. Referee 2 did not altogether agree with this and said that the manuscript was densely written but that this was appropriate given the nature of the work. On the basis of the initial comments of the Referee 1, we shortened, condensed, and corrected the manuscript before it appeared on the ACP website. We believe that this current version of the manuscript is more readable than the version that we first submitted to ACP. I guess the 20% of the manuscript that might make it outwardly 'hard to read' and 'densely written' would include the sections with all the equations in which the theory is developed. We are reluctant to cut out these sections because we have not found the theory developed in other publications (including the landmark box-model studies of CO by R. Newell in the early 1970's). The style of the analysis and the structure of this approach is patterned broadly on the textbook of boundary layer meteorology by R. B. Stull, a rigorous but interesting description of how to interpret oscillations in any geophysical data (he develops theory specifically for the atmospheric boundary layer) and infer their origin. The mathematical theory presented in our paper will help guide other researchers in investigating temporal trends in their data.

The referees have suggested specific changes and improvements to the manuscript, and these have each been addressed. The first paragraph has been modified to em-

ACPD

2, S533–S535, 2002

phasize the motivation for the manuscript and the benefits of the approach. The nature of the uncertainties in the forward approach (concern of referee 1) has been addressed at length in the first manuscript, which is in press in the Journal of Geophysical Research. Referee 1 has suggested inconsistent subject-verb agreement for the words 'data' and 'none', and we have gone through the Word document and corrected the error. Three other grammar errors have been identified and corrected. Referee 2 and Uli Poeschl had some difficulty reading some figures, and these have been replotted with the largest font practical. Referee 1 has suggested that Fig. 4 (showing a map of all 7 stations) might not be needed given the table of the spatial coordinates. We have retained the figure because it presents spatial distribution of the stations in the clearest manner. The reader can immediately see how the stations are clustered to each other (i.e., how the time series if one station should similar with others) and how close the stations are to the equator (i.e., how they might be influenced by cross-hemispheric flows). This is not as clear with only the table of latitude and longitude coordinates.

Referee 1 suggested that the CO2 might be removed, but referee 2 declared that it 'is quite a crucial part of the rationale of the whole work, and is fine'. COS behaves similar to CO2 in some respects. Because so much more is understood about the biogeochemical cycles of CO2, it forms a departure point from which to understand the atmospheric variation of COS.

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