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Atmospheric Measurement Techniques Discussions

Interactive comment on "Eddy covariance carbonyl sulphide flux measurements with a quantum cascade laser absorption spectrometer" *by* Katharina Gerdel et al.

Anonymous Referee #1

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General Comments

Gerdel et al. present a careful consideration of eddy covariance data processing in the specific case of carbonyl sulfide (OCS) data from a popular, commercially available instrument. It is important to pay attention to this kind of methodological detail, and I respect the authors' work here. However, as to the manuscript itself, I have to say that I do not think it presents substantial new concepts, ideas, methods, or data, and therefore I cannot recommend it for publication in AMT. Please do not take the following explanation as demeaning the authors or their work; that is not my intent. I just need to be clear about why I would reject this manuscript.

The manuscript aims to assess whether the instrument in question can make "defensible EC COS flux measurements", but what does "defensible" mean? The authors do not report the accuracy of the EC OCS fluxes, and so I came away from the manuscript with no more or less confidence in them than I had before. The validation (by comparison to another type of instrument) is restricted to CO2 and H2O measurements. Instead of considering EC OCS accuracy, the authors focus on random noise, and the real question that the manuscript addresses is: can the noise in EC OCS measurements with this instrument be reduced via high- and low-pass filtering? The answer is of course yes: filtering out noise makes data less noisy. If the noise filtering techniques were new and innovative, their performance might be worth reporting, but as the manuscript itself says, the techniques are common.

The closest the authors come to the issue of bias (and therefore, in my mind, defensibility) is when they imply briefly in Section 3.3 that the high-frequency noise in the OCS mixing ratio tended to be correlated with the vertical wind velocity signal such that the cospectra were biased high. But they do not offer any evidence for this surprising claim, or any ideas for how such a thing might occur. The figures do not illustrate it.

The characterization of the noise in the OCS mixing ratio reported by the instrument (i.e. the Allan plots in Fig 2) does not appear to be new either; I believe the manufacturer itself has done this kind of analysis and freely shares it. That the characterization was done in the field here might add some novelty, but the authors do not say whether the noise was different in the field than in the manufacturer's labs.

This work seems to belong in the methods section of an article that actually makes use of the calculated EC OCS fluxes. In that case, details of how the data processing was done would be important to enable others to reproduce the work. I hope the authors have such a manuscript in the pipeline; I would look forward to reading it.

Specific Comments

I offer the following additional comments in the hopes that the authors might find them useful.

- I was a little concerned about how all laser spectrometers (for CO2 isotopes, N2O, CH4, OCS) were lumped together at times (e.g. page 8, lines 15-17), as if the present analysis ought to apply to all of them – but not, say, to an IRGA. What matters here is the noise and drift in the mixing ratio measurement, not whether the infrared light in the instrument came from a heated filament or a laser. The noise and drift considerations for Patrick Sturm's CO2 isotope QCLAS were very different than those for the present OCS QCLAS, which is measuring a comparatively tiny spectral line.

- Regarding the high-pass filtering: an alternate approach is to correct the drift in the OCS mixing ratio before beginning EC calculations. The OCS mixing ratio drift results from slow changes in the spectral baseline (i.e. the zero offset), which is reset periodically by the QCLAS's auto-background feature but changes in between those resets. By comparing measurements of the same gas (a standard tank or even the atmosphere) just before and just after an auto-background reset, one can determine how much the OCS zero level had drifted since the previous reset and make a linear correction (though the drift might not always be so linear). When it comes to the EC fluxes, this method is probably similar in effect to using linear detrending but ought to be better because real trends in the OCS mixing ratio would not be filtered out.

- Regarding the approach of Wienhold et al. (1995) to quantify the SNR, it always seemed fundamentally flawed to me. If you look at a plot of covariance vs lag time, you typically see oscillatory patterns because the eddies are quasi-periodic structures. So the variability in the covariance as you scan the lag time is not merely noise: much of it results from the quasi-periodic nature of the signal. You should be able to test this by comparing the noise estimate obtained this way when the signal (i.e. the real eddy flux) is large (e.g. at midday) to when it is near zero (e.g. at night for H2O and OCS). If the noise seems to decrease as the signal decreases, then the method may be flawed in the way I suggest.

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