Review response to discussion article “Towards standardized processing of eddy covariance flux measurements of carbonyl sulfide” by Kohonen et al.

Reviewer comments in black
Author response in purple
Altered text in the manuscript in italic

Reviewer #2

Erkkilä (Kohonen) et al present a detailed and valuable analysis of the impact of various eddy covariance data processing options on the calculated ecosystem uptake of carbonyl sulfide (COS). They attempt to quantify the flux uncertainty deriving from the data processing, and they make recommendation for some of the options. The methods are sound and the manuscript is fairly well written and easy to follow. I do have some concerns about the analysis and interpretation of the results. In addition to the “major comments” of referee Wohlfahrt, with which I agree, I believe the paper could be made stronger by addressing the issues below.

Specific Scientific Comments
1. I disagree with the idea that the “processing uncertainty” reported here is actually an uncertainty in the calculated fluxes. Instead, it is a metric of the sensitivity of the calculated fluxes to different processing choices. Some of those choices are clearly better than others, and it doesn’t make sense to calculate the fluxes in a way that is known to be pretty good and in another way that is known to be pretty bad and then say that the difference between the two ways is the uncertainty in the flux. In particular, the following data processing choices are obviously bad: (a) the COS lag and RM lag methods, (b) the RF 30s detrending method, (c) omitting high-frequency correction, (d) omitting the storage flux, (e) determining a u* filter threshold before including the storage flux, (f) omitting gap filling (for cumulative sums). None of those methods should be included when assessing methodological uncertainty, as there is no uncertainty about the fact that those methods should not be used. Thus the “processing uncertainties” presented are misleading, in that they give an inflated impression of the real processing uncertainty in the EC method. Moreover, I think the total “processing uncertainty” in Fig. 12 is of no use even as a sensitivity metric, as it blends sensitivity to choices that are unclear with sensitivity to other choices that are very clear. (Similarly, the total uncertainty defined in Section 2.4.6 is of limited use because it blurs the distinction between stochastic half-hourly noise, which can be averaged out, and long-term systematic bias, which cannot.) So I would present instead (and show in Fig. 12) the flux sensitivities to the various individual processing choices. Then if the authors want to identify which processing choices are genuinely debatable and use their sensitivities to calculate a more meaningful overall processing uncertainty, they can do that. And then if they want to compare the magnitude of the systematic processing uncertainty (i.e. potential bias) to that of the random flux noise, they can do that too (But why? Over what noise averaging period is such a comparison meaningful?).
The uncertainty estimates presented here are based on the well established eddy covariance data processing protocols, presented in Sabbatini et al., 2018. We agree that processing choices listed in the comments are not as good as others. In this method, we calculate the fluxes using block averaging with planar fitting, block averaging with 2D wind rotation, linear detrending with planar fitting and linear detrending with 2D wind rotation. These are all very widely used processing schemes and not thought as obviously bad. The bad choices listed in the comment are not used for estimating processing uncertainty. The estimate of processing uncertainty is based on calculating the lowest and highest possible fluxes that come out from different (reliable) processing schemes and thus tells how much variation there can be in fluxes due to processing choices. The method is explained in detail in Section 2.4.6. However, as random error is dominating the total

uncertainty, even a loose definition of the processing uncertainty would not inflate the total uncertainty to a large degree.

2. The distributions of lag times in Fig. 3b and 3d are concerning. Why the spike at 2.7 s, in the wing of a broad peak centered on 3.2 s? The spike seems to suggest that the true lag was actually always 2.7 s, while the other retrieved lags were in error, perhaps due to some stochastic noise artifact. After all, if the true lag really were varying stochastically as suggested by the broad peak, then why would there be a preponderance of times when it was exactly 2.7 s? Was there perhaps a change in the experimental setup at some point during the measurement period? After seeing Figs. 3 and 4, I'm actually inclined to think that using constant lag is the most advisable option for these data. The authors instead recommend the DetLim method but do not justify that recommendation. In particular, it's unclear why the lag determined from CO2 should ever be any worse than that determined from COS (except when the CO2 flux crosses zero), given that both gases are measured by the same instrument and the CO2 almost always has a higher signal to noise ratio.
Thank you for pointing this out. We checked the lag time issue and the spike at 2.7 s and winged shape were due to an artefact caused by a lag time optimization tool used in the final flux calculation. In the revised manuscript we will show the lag times determined directly from the maximum covariance, which doesn't have this artefact. Using CO2 lag time is probably not any worse from DetLim lag, and we found a more clear lag time distribution for CO2 lag than DetLim lag. The DetLim lag method was established in the eddy covariance data processing protocol by Nemitz et al., 2018 for small fluxes, but to our knowledge the method is implemented here for the first time. We will add a more clear statement in the revised manuscript.

3. Surprisingly, and despite the statement on lines 344 and 350, Fig. 7a seems to show that the COS cospectra don't seem to have any high-frequency signal loss, unlike the CO2 cospectra. I can think no reason why that should be the case unless the large high-frequency instrument noise for COS is synchronizing by chance with high-frequency fluctuations in w. Given that the COS cospectra seem to match well with the temperature cospectra, it doesn't seem to make sense to use the CO2 cospectral correction (based on the mismatch between the CO2 and temperature cospectra) for
COS. Unless perhaps Fig. 7a mistakenly shows COS cospectra after correction?
We think that it was indeed by chance that the instrument noise was synchronizing with high frequency fluctuations. We have now made the same plot only for COS fluxes that surpass the random noise (covariance at least three times higher than the random noise) and the flux attenuation is more evident and closer to that of CO2.

Technical Corrections
- line 2: "the recent development" should be "recent developments"
Corrected

- line 21: "not being" should be "is not"
Corrected

- line 22: "for radiation-dependency" should be "for the radiation-dependency"
Corrected

- lines 60-61: The word "respectively" doesn't make sense here, as there's nothing for the analyzers to be respective to. I recommend changing "at 10 Hz from Aerodyne Research (Billerica,

MA, USA) and Los Gatos Research (San Jose, CA, USA), respectively" to "at 10 Hz, one from Aerodyne Research (Billerica, MA, USA) and one from Los Gatos Research (San Jose, CA, USA)".
We will revise the introduction based on reviewer comments, and have left this part out of the text to harmonize and make the introduction more compact.

- line 64: I would delete "a basis of EC measurements"
Deleted

- line 112: "a home-made" should be just "home-made"
Corrected

- line 149: "to some extent of weather changes" should be "to some extent weather changes"
Corrected as suggested

- line 156: "different" is superfluous, and so I would delete it
Deleted

- line 178: "others" should be "other reasons"
Corrected

- line 344: "compare Fig. 7a and 7b" should be "compare Fig. 7a and 7c"
Corrected

- lines 423 ff: "The uâĹŮ filtering is applied to conform the. . . does not make sense and I'm not sure exactly what you are trying to say here.
The whole revised sentence/chapter reads "*The u* filtering is applied to conform the assumption that fluxes do not go down under low turbulence conditions, as is the case for respiration of* CO_2*, but which does not necessarily apply to COS uptake. The u* filtering may therefore bias COS fluxes due to false assumptions. However, we did not see u* dependency disappearing even with a concentration gradient-normalized flux, so the u* filtering is applied here normally to overcome the EC measurement limitations under low turbulence conditions.*"