Reply to Short Comment by W. Eugster

W. Eugster (hereafter WEug) puts forward a number of criticisms, which can be summarized in the following 3 points:

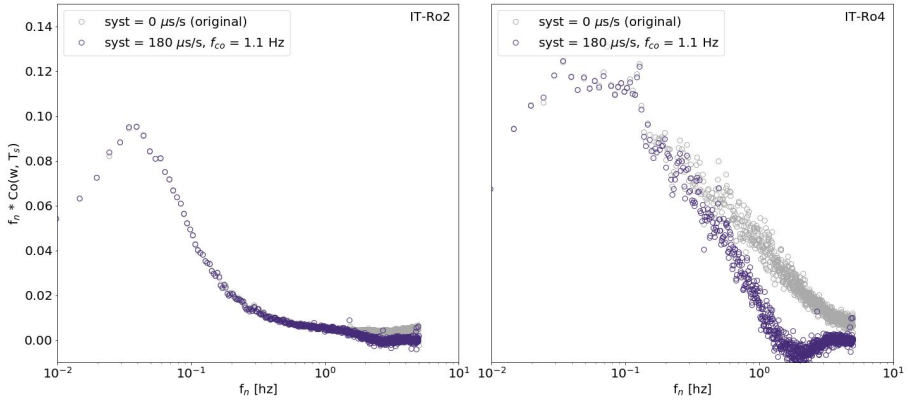
1. WEug criticizes the fact that we supposedly suggest to use mixed analog-digital acquisition systems instead of fully digital systems. The first 4 pages of the comment elaborate on this topic using a data acquisition system that he proposed in the past as a contrasting example.
2. At pages C4-C5 WEug proposes a mathematical demonstration as well as a simulation and a practical example, supposedly showing that "drift [*which we identify as the major source of flux bias*] is not the issue".
3. At pages C5-C7 WEug criticizes our Fig. 7 because, in his opinion, it depicts (possibly wrongly calculated) co-spectra in the wrong manner.

In the last paragraph, WEug criticizes our choice of the Journal and proposes to reject the paper.

Following our replies to each point:

1. WEug misunderstood the message we want deliver with this paper. By no means do we suggest using mixed analog-digital acquisition systems. We merely start from the observation that in traditional analog-digital EC systems, data synchronization was not an issue the researcher needed to be too much concerned of, because the instruments' manufacturers essentially solved it and methods exist for correcting for the (potentially) involved spectral losses in post-processing. Conversely, when assembling a custom fully digital system – which is often necessary in applications, for which industrial-grade integrated systems do not yet exist – care must be taken to get the synchronization right. Therefore, the entirety of the paper is devoted to describing and demonstrating (via simulations starting from real data) potential flux biases resulting from poorly designed fully digital acquisition. However, several times we clearly state that fully digital systems are to be preferred. As an example, at Pag. 2, Lines 12-25 we highlight the limits of analog-digital systems and the following Lines 26-30 explain how fully digital acquisition overcomes those limits. Furthermore, the opening line of the Conclusions reads: "Undoubtedly, modern EC systems should log high-frequency data in a native digital format […]". It appears that Referee #1 and #2 - as well as Ivan Bogoev in his Short Comment - didn't misunderstand this crucial point, so we don't deem it necessary to modify the paper for this aspect, unless the Editor requires it.

2. WEug misunderstood the drift issue we describe and simulate. The entire paper discusses *relative* frequency drifts, i.e. the drift of the clock of the sonic relative to the clock of the gas analyzer (or vice-versa). He instead describes, simulates and exemplifies an *identical* absolute drift of the two clocks, which of course results in no *relative* drift and no significant errors. This is evident from both Eq. 2 of his Short Comment and from the discussion that follows it. Again, the Referees and the author of the second Short Comment appear to have understood what we have done and its rationale, so we don't deem it necessary to make any modifications, unless prompted by the Editor.
3. WEug misunderstood what Fig. 7 shows and therefore derived irrelevant conclusions. We do appreciate, though, that the description of the figure could be improved. For that figure, we used data from 2 sonic anemometers and computed co-spectra between vertical wind component (w) and sonic temperature (T_s), the grey circles in Fig. 7. We then modified T_s to simulate a *relative* drift and computed co-spectra again, the purple circles. The aim is to show the low-pass nature of the filter that the relative drift implies and how the same filter leads to significantly different losses depending on the co-spectral shapes. We note that:
    - No data from a gas analyzer is involved, so considerations about correlated noise etc. (beginning of Pag. C6 of the Short Comment) don't apply.
    - In our opinion, a log-log plot is better suited to visualize spectral losses at high frequencies (which is the aim of Fig. 7) because it emphasizes the lower y-axis ranges, where attenuation occurs. However, due to the stark difference in attenuation, a semi-log plot works as well in this instance - see the following figure, where we plotted the same data on a linear y-axis:



Here we also expanded the axis ranges to show the entirety of the data. In this version of the Figure one can see that at high frequency the transfer function is characterized by damped oscillations around zero (the same can be guessed by looking at Fig. 6a of the manuscript,

bottom-right corner). This is an aspect we did *not* want to emphasize in the paper, because we believe it has no practical implications (oscillations occur where signal is already minimal) and only distracts from the main message of the paper. This is also the reason why at Pag. 7, lines 1-2 we state that we used a transfer function model "that was found to reasonably approximate the data obtained for all drifts at all sites *in the most relevant frequency range*". The model captures the transfer function behavior well at frequencies where co-spectral content is significant and, in particular, where the cut-off frequency is found.

In any case, addressing a comment by Referee #2, we suggest to replace this Figure with the one proposed in the Reply to Referee #2, Comment 6.

In addition, WEug writes: "*My experience as a reviewer is that often "cospectra" of the kind shown for IT-Ro2 in Fig. 7 are simply due to erroneous calculations of the cospectrum. I cannot double check this hypothesis with the IT-Ro4 data shown at right in Fig. 7".* We need to point out that, upon his request, on April 17, 2018 we shared with WEug, by email, the data and the code used to generate Fig. 7 (with no response from him until the publication of his Short Comment), therefore he had with him everything needed to "*double check [his] hypothesis*".

Finally, we find the suggestion to submit to a different Journal somewhat unusual, considering that the Editor already evaluated the paper to be appropriate for publication in Biogeosciences Discussion and that WEug posted his Short Comment after the Referees have submitted their reviews, which show a full understanding of the discussed issues and of the content of our manuscript.